# Global China, Global Crisis:
# Falling Profitability, Rising Capital Exports
# and the Formation of New Territorial Industrial Complexes

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## Abstract

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This dissertation explores the relationship between what has been called the "long crisis" or the "breakdown" tendency of the capitalist economy, driven by a constant, intensifying struggle to address the twin problems of falling profitability and rising surplus population, and changing geographies of Chinese industry both within and beyond China. Particular attention is paid to the current state of Chinese global outbound FDI and the prospective linkage between these capital exports and the production of new territorial production complexes in less industrialized countries, with a case study in Dar es Salaam, Tanzania. The Great Recession of 2008 acts as an important nexus, since the response to the crisis within China marks an important pivot in domestic industrial trends and the beginning of an acceleration in outbound FDI. Its aftermath

also illustrates the necessary theoretical linkage between the two related, but not identical, meanings of the term “crisis” prevalent in the literature reviewed here: The first is crisis in its momentary or immediate sense, referring to individual economic crises that have a relatively clear beginning and end. The second is crisis in its “long” or secular sense, referring to systemic breakdown tendencies inherent in capitalist accumulation. The distinct shifts in the domestic and international geography of Chinese industry, seemingly triggered by the 2008 crisis, can be better understood through the combination of both the momentary and secular dimensions of the concept, only properly visible when capitalism is appraised as a global system.

# Reading Guide

Since this is a long work with multiple points of focus, it will be helpful to offer a guide for interested readers. This guide explains the overall structure of the document, offers a preview of its contents and indicates where readers with specific topics of interest in mind (i.e. those interested in the Marxian theory of crisis versus those interested in demographic shifts or industrial relocation in China) should direct their attention first. In part, this guide can be thought of as a series of interlinked abstracts for each chapter of the dissertation. But it also attempts to offer some suggestions about the potential reading order. Overall, the dissertation is divided into two parts, preceded by an introduction. The **Introduction** situates the work within the discipline and briefly sketches the author's philosophical and political commitments. Those interested in the relationship of geography—and specifically quantitative economic geography—to the philosophy of science and communist political thought will find the introduction useful in its own right, beyond its framing of the dissertation as a whole.

Of the two main parts of the document **Part I**, which includes Chapters 1 and 2, is a wide-ranging theoretical review focusing on the nature of crisis and its manifestations in space. Those interested in theories of crisis, breakdown and the character of the global economy should focus their attention here. **Part II**, which includes Chapters 3 through 5, analyzes Chinese economic trends in profitability, industrial relocation and capital exports abroad, finishing with a case study of Chinese manufacturing firms in Dar es Salaam, Tanzania. Those interested in a conjunctural analysis of recent economic and demographic trends in China, as well as their consequence for the structure and prospects of the global economy, should focus their attention

here. Overall, any individual chapter can be treated as a stand-alone document, despite periodic references backward to the body of theory reviewed in earlier chapters. This reading guide will therefore approach the work chapter by chapter, excluding the introduction summarized above.

## Chapter 1 – Mainstream Accounts of Crisis

The first chapter focuses on the most recent and systematic theories of crisis that have developed or reemerged within the discipline of economics following the Great Recession. Specifically, I seek out immanent critiques that attempt to explain the deeper roots of the crisis and which attempt to account for the "secular stagnation" that it unveiled, as opposed to the myriad attempts to explain the crisis as a purely incidental phenomenon that can be understood without reference to other crises or secular trends in the global economy. Here, "immanent" delimits the scope to theories that arise from within the conceptual and practical frameworks of both economics as an academic discipline and as an occupation beyond the academy.

The contributions of a few main thinkers are emphasized: **Section 1** of the chapter focuses on Richard Koo, foremost expert on the Japanese crisis, whose theory of "balance sheet recession" became influential in the early 2010s. Koo later attempted to synthesize his theory of economic crises with mainstream models of structural development, emphasizing the key role played by the Lewis Turning Point and the challenges faced by "mature" economies thereafter. This section also includes a critique of the recent work of Matthew Klein and Michael Pettis, who are influenced by the "balance sheet" approach. **Section 2** addresses the work of Robert J. Gordon, a mainstream economist at Northwestern whose work focuses on large-scale macroeconomic trends. In particular, the section focuses on Gordon's long-run growth

accounting measures for the US economy since the Civil War, which demonstrate long-run productivity stagnation. **Section 3** finishes the chapter by briefly addressing some of the major positions associated with Modern Monetary Theory (MMT), which has seen a recent boom in popularity. This section is focused less on a systematic analysis of particular MMT thinkers' work and more on examining the hidden presumptions and occluded consequences of the fiscal policies that are the focus of attention for MMT as a whole. In particular, I explore the potential international consequences of such policies and the disguised imperial presumptions that underlay the theory.

Beginning with this immanent critique allows me to coherently identify the shared ideological horizon of such theories, as traced out by a series of topics that are consistently gestured to by various authors but never explained or approached. Foremost among these is an acknowledged decline in the profitability of investment as a whole, but especially within the "productive" sector—defined differently by different authors, sometimes implying any form of non-financial investment, though more often referring to "industry" or "manufacturing" specifically—and a concomitant rise in various types of financial activities. The result is a situation in which portfolio investment in various financial assets has overtaken direct investment in industry (or even productive services, R&D, etc.) within global capital markets. Similarly, these immanent critiques gesture toward fundamental changes at the level of structural development and the technical composition of production, even going so far as to suggest that "secular stagnation" is the product of inherent technological limits or the plateauing of "development" as such. This ideological horizon is significant not because it implies that such theories are merely incomplete, but instead that their vantage is limited in a much more fundamental sense. The identification of concrete but disavowed limits thereby gestures toward a

more all-encompassing critique of the more basic presumptions—and in fact the entire epistemological approach—leading to this theoretical dead-end in the first place.

## Chapter 2 – Marx and the Communist Theory of Crisis

In this chapter, I proceed to explore the critical theory of Marx and subsequent interlocutors, emphasizing the revival of various aspects of Marx's own incomplete theory of crisis following the Great Recession. Though this is often spoken of as a revival of "Marxism," the term is deceptive since it refers not to a single systematic theory but instead to several divergent attempts to reconstruct, reclaim and/or return to Marx's critique of political economy in an environment now unfettered by the 20th-century competition between various "Marxist" claims to orthodoxy, each of which was defined by its relationship to the "official" orthodoxy established in Russia after the ascent of Stalin. The absence of similarly limiting political conditions has allowed for a much more open engagement with once-heterodox interpretations of Marx. Meanwhile, the progress of publishing the entirety of Marx and Engels' writings—halted after its original editor David Riazanov was deposed and executed in the course of Stalin's rise to power—began again in the 1960s and has accelerated in recent decades, resulting in the full publication (completed in 2012) of all the drafts of *Capital* in their various forms alongside all other completed works and unfinished manuscripts related to its production. This has provided a new basis for thinkers attempting to carry on the critical tradition of Marx's own work, regardless of how they conceptualize this project.

My focus here is not on historiographic issues, however. Chapter 2 attempts to engage only minimally with active debates in the sub-field that has (only somewhat jokingly) been

referred to as "Marxology," and which concerns itself with the way in which these original texts ought to be understood, whether they contain a complete or incomplete theory, whether or not this theory can then be discovered through textual analysis or whether it must be (and then, whether it should be) reconstructed out of incomplete materials, and to what extent the work of earlier self-described Marxist scholars drawing from much more limited (and more heavily edited) writings of Marx is useful to this project. While this scholarship is an invaluable resource and all theorists indebted to Marx also make inherent "Marxological" presumptions (whether implicit or explicit) the stakes of this historiographical project are distinct from those of Marx himself, who sought an understanding of capitalism via a systematic critique of both: a) its self-presentation in various deceptive social forms, which are then taken at face value and synthesized into a body of theory allegedly making sense of economic reality but, in fact, obscuring that reality—this body of theory was then known as "political economy" and is today more widely distributed across now-distinct social science disciplines, among which economics[1] plays a similarly central (but not identical) role; and b) the actual core "laws of motion" that become evident in the process of this critique. Equally essential to understanding this critical project, however, is the fact that it was not an end unto itself. For Marx, the entire critical method was only given real force through its connection to a much more expansive social-historic goal: the revolutionary destruction of capitalism by the organized proletariat, which, at the same time, would begin the species-scale gestation of communism—a stateless "human community" defined by general social control of collective productive capacity, rationally meeting the needs of the species and reestablishing an auspicious metabolism with the non-human world.

[1] And, to add to the confusion, a revival of "political economy" itself, often even referring to itself as "Marxist" despite its purely classical (i.e. Ricardian) framework. It is no longer uncommon, for instance, for "political economists" and "political ecologists" to write development reports for the World Bank.

The review of Marxian theories of crisis in Chapter 2 emphasizes this point, framing crisis theory not within the historiographical limits of "Marxism" as either a coherent or conflictual orthodoxy but instead within the grander scope of the communist project, which both precedes and exceeds the work of Marx himself. At the same time, the critical method evident in his own work remains central.[2] In contrast to the immanent theories of crisis that retain the basic presumptions of economics as a discipline, then, Chapter 2 explores theories that are unified by the fact that they stand outside of "economics" proper and critique it from without, not only utilizing a fundamentally different methodology and focusing on different units of analysis but also understanding economics as such to be one ideological outgrowth of the social forms generated by capitalism itself. It is incorrect to refer to such theories as "Marxist economics" or "Marxist political economy," and the work of scholars who favor such descriptors is referenced more sparingly and with frequent disclaimers, though such work is nonetheless important as a point of reference in recent debates and as a source of empirical data. Similarly, the work of "critical theorists" whose goals are purely academic and whose portrayals of theory emphasize its purely intellectual dimensions and thereby sever it from any relationship to the communist political project are treated with a symmetrical suspicion. The most thorough attention in this chapter is given to scholars who themselves have maintained their fidelity to the necessarily insurrectionary dimensions of communism and whose work has exerted the greatest influence among those practically involved in and intellectually concerned with the character, limits and prospects of recent and forthcoming cycles of mass unrest across the globe.

[2] This is another area where the narrow focus of the "Marxologists" remains relevant, since the intentionally limited scope of their inquiry is well-suited to intricate excavations of this critical method—illuminating the real, lived process through with Marx produced his body of theory.

The organization of this chapter is more topical and less focused on particular theorists, though certain thinkers are drawn on more than others insofar as they have made important contributions to each of the topics. **Section 1** of this chapter focuses on recent debates around "automation" and the revival of interest in the central role of the "surplus population" in Marx's own work. Recent interventions by Aaron Benanav and Jason Smith are particularly important points of reference here, alongside work by Paul Mattick (Jr and Sr) and the summaries of Marxian debates offered by Simon Clarke. **Section 2** focuses on global industrial competition stoked by the twin problems of profit and population, as well as its relation to the secular trends in technical change hinted at by mainstream thinkers such as Gordon. Here, the work of Robert Brenner and Makotoh Itoh is particularly important, since both thinkers emphasize the role of technical change, demographic shifts and intensifying international competitive conditions in the rise of "trade wars" and financial crises. **Section 3** offers a brief overview of theories of imperialism, with an emphasis on contemporary theories centered on the role of global labor arbitrage. Here, the work of Intan Suwandi, John Smith and Tony Norfield are important points of reference, though this section offers less detail than the others. **Section 4** finishes the chapter by returning to questions of geography, with a focus on the regional consequences of these global trends. The section begins with a rejection of the most popular approach to such questions as offered by "radical" or "Marxist" geographers, such as David Harvey, arguing that Harvey's eclectic interpretation of Marx's work lacks rigor and is only given consistency by its Keynesian political presumptions. In contrast, this section focuses on concrete questions of the relationship between crisis and regional development explored by economic geographers, epitomized in the emergence of new territorial production complexes, as theorized by Michael Storper and Richard Walker.

### Chapter 3 – The Profitability of Chinese Industry

This chapter is the first of Part II, pivoting away from theoretical concerns and toward a conjunctural analysis of actual trends in Chinese economic development. The chapter focuses on the question of profitability within Chinese industry, drawing from existing literature on the nature of the "Chinese bubble," the state's response to the Great Recession and the continuing threat of crisis. This section is not primarily a review of secondary literature, however. Its focus is on contributing original data analysis to the existing picture. **Section 1** explains the underlying data and the methods that will be used. **Section 2** begins the analysis of this data with a calculation of the prevailing rate of profit within the Chinese economy. In this section, I have compiled the most wide-ranging collection that currently exists of all major, recent attempts to measure the rate of profit in China (in both English-language and Chinese-language scholarship), compared them to one another, contributed my own suite of measurements and derived from this a handful of mean profit rate measures. From this, I am then able in **Section 3** to periodize profitability within the Chinese economy, break down profitability by size, ownership or corporate structure of enterprise and decompose it into component measures. All of this gives some sense of the uneven distribution (geographically and categorically) of recent declines in profitability otherwise obscured in economy-wide measures.

### Chapter 4 – The Changing Geography of Industry and Profitability

This chapter picks up where the last left off. Though not primarily a review of secondary literature, the chapter aggregates a wide variety of pre-existing secondary sources, ranging from

academic articles and brief surveys conducted by economic consulting agencies to the data collected by the Chinese Ministry of Commerce, recipient countries' national statistical bureaus and by academic organizations such as the China Africa Research Initiative or by widely cited think tank projects, such as the China Global Investment Tracker. It is also bolstered by some limited in-person field work conducted in Guangzhou and Shenzhen in 2014 and 2015. **Section 1** decomposes profitability by region and province, comparing these trends to similar data on the growth or decline of total enterprises, electricity consumption, output and net profits. The goal is to capture both the overall contours of China's changing industrial geography and to identify the driving factors behind these transformations.

This analysis of domestic trends is then followed in **Section 2** by a similar look at the increase in Chinese outward foreign direct investment over the course of the past two decades. While any appraisal of Chinese outward FDI certainly entails some discussion of China's central role in the build-out of global logistics infrastructure in recent decades and the changing patterns of global trade, my focus is more narrow: an examination of all the major sources of data on Chinese *industrial* investment overseas and the relationship between this investment and the potential emergence of new territorial production complexes in countries that have thus far been marked by low levels of industrialization. This section closes with an overview of global demographic trends, focusing on the sites that have been host to the largest quantities of Chinese outbound FDI. These trends are then linked backed to the earlier, theoretical consideration of the capitalist "law of population" in order to speculate about the geographic prospects of future crisis and development. The chapter ends by identifying the likely future importance of Sub-Saharan Africa for continuing capital accumulation globally and arguing that, due to its location along the

Indian Ocean and historic ties with the Middle East and South Asia, the East African region will likely lead in early industrialization.

## Chapter 5 – A New Territorial Industrial Complex?

Informed by the conclusions of the preceding chapter, Chapter 5 zooms in from the larger global picture to focus on the role played by Chinese investment in East Africa. Dar es Salaam, Tanzania, is taken as a case study, since it is likely already the region's largest urban complex and is projected to grow rapidly over the coming decades. Previous surveys have also shown the country to be an active center of Chinese manufacturing investment, but one in which very little accurate data exists on the phenomenon. This chapter opens in **Section 1** with a brief appraisal of some competing theories on how to understand industrialization in sub-Saharan Africa and how to properly conceptualize the region's incorporation in global value chains. It then provides an overview of Dar es Salaam's recent history of rapid infrastructural development and real estate speculation, all deeply related to the flows of portfolio capital examined earlier in the dissertation. **Section 2** then gives an overview of the basic patterns of employment in the city and the character of its industrial zones and their neighboring slum settlements. Finally, **Section 3** details the results of my own field work (conducted Jan-Feb 2020), which involved an on-foot industrial survey of all the major factory districts in Dar es Salaam. The general findings hint that Chinese investment in Tanzania is simultaneously exaggerated (due to its role in a few prominent real estate projects) and underreported (due to the small average size of Chinese firms operating in the country).

## Suggested Reading Order

The ideal reading order depends on the reader's desired focus. In conclusion, I will outline a few possible reading orders, according to the major points of interest in the document:

Readers primarily interested in the ***geographic aspects*** of the work and its framing within the discipline of geography are advised to read the **Introduction**, followed by **Section 4** of **Chapter 2**, **Section 4** of **Chapter 4**, and the entirety of **Chapter 5**. They may also be interested in the geographic data presented in **Section 1** of **Chapter 4**.

Readers primarily interested in the ***theoretical aspects*** of the work should read **Chapter 1** and **Chapter 2**. These chapters summarize the debates on "economic" theory that the dissertation is positioned within. The higher-level, philosophical context for such debates is presented in the **Introduction**.

Readers primarily interested in the ***Chinese case*** will find the most useful content in **Chapter 3** and **Chapter 4**. If the reader just wants a summary of the findings, I suggest reading the introductory and concluding sections of both chapters, while skimming the graphics. Those looking for detailed methodology will find it explained in the body of the chapters and in the attached appendices.

Readers primarily interested in the ***Tanzanian case*** or in the general ***role of Chinese outbound investment*** should read **Section 2** of **Chapter 4** and the entirety of **Chapter 5**. It may help to refer to **Section 4** of **Chapter 2** here as well, since that is where many of the key theoretical terms are introduced.

## Acknowledgements

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## Note on Foreign Languages

This dissertation makes use of both Chinese and Swahili. Interviews were conducted in both languages. Chinese scholarship and statistical resources are referred to in several locations. Words from both languages will periodically be reproduced throughout the text.

### Chinese

For Chinese scholarship, authors are named according to the Chinese convention, placing surname first—unless the printing of their name clearly indicates an alternative preference, as when an author has an English first name. In the bibliography, this amounts to nothing more than the removal of the comma separating first and last names.

For Chinese words used in the text, two conventions are used. In general, anywhere that a Chinese term is referred to directly or requires clarification in a quote, Chinese characters will be used. However, when a term is commonly known by its Chinese name within the scholarship and is intended to be read in the flow of the English sentence, toneless *pinyin* will be used instead. This is the case for terms such as *hukou*, referring to the household registration system in China, which has a large body of English-language literature affiliated with it.

### Swahili

Swahili requires very little transliteration or explanation. It is conventionally written in the Latin alphabet. The only unfamiliar convention is the noun-class system insofar as it defines singular vs. plural status. This status is indicated at the head of the word in Swahili by a

transformation of the initial syllable or the addition of an initial syllable. For instance, “factory” in the singular is *kiwanda*, while “factory” in the plural is *viwanda*, “car” in the singular is *gari*, while “cars” in the plural is *magari*. The exact conventions depend on the noun-class. These conventions are not important to explain here. In general, the singular or plural will be used to match the definition given in English. In some cases, both will be communicated using a slash. For example: *ma/gari* to indicate car(s).

Whenever a new Swahili term is used, it will be italicized and immediately defined. After that, it may be used again on its own, especially insofar as certain terms capture qualitative distinctions that are more difficult to communicate in English.

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# Introduction

The research laid out here is, in every sense, a macroscopic study. Though it makes use of local fieldwork, it does so in order to construct a truly global geography of its topic. At the same time, it draws from a broad and, at times, highly theoretical body of literature, but only to synthesize a macro-theory of the myriad phenomenon in question. It is an attempt to understand and explain real world trends via a process of analytic abstraction aimed at grasping their core logic. This is, at its root, driven by the pragmatic process of carving quantifiable and necessarily skeletal frameworks out of inherently contradictory real phenomena—which exceed quantification—allowing models to be crafted from the totality of empirical experience. But these models do not exist in a vacuum. On their own they are like silent machines, their functions only vaguely hinted by their hulking shape. Whether these machines are blunt statistical tools or skillful theoretical constructions crafted from a jointure of nested mechanisms so intricate that they seem at times to emulate living movement, all are models and models must be set in motion and guided by conscious intent. Since their creation and ends are guided by the ultimate goal of explanation, these models can only be animated by the flow of an overall narrative—just as machines sitting silent in a factory are driven to motion only when carried forward by the river of human labor passing through them. As an introduction to this project, I will first detail some of the philosophy behind this approach, then the connection between this general philosophy and geographic practice specifically, before finally explaining the political implications of the project and its reason for focusing on China specifically.

## Geographic Abstraction

This research project, spanning some six years, was both conceived and completed in the fashion of the classic geographic field work that marked the discipline's early years: on long walks through the spaces that would become its focus. Though much of this project is devoted to the dual abstractions of theory and data, since its topic is fundamentally about the territorial character of production and crisis, its heart remains on the ground—as any good geography should. There is also a particular spatial symmetry to the project: conceived on foot in the mature industrial districts of China's tropical, coastal sunbelt in the midst of a wave of outbound factory relocations, the gathering of data for the project was similarly completed on foot in long tours of the adolescent industrial districts of Tanzania's tropical, coastal sunbelt just as a recent tide of inbound factory relocations had begun to recede. Finally, the writing and data analysis was composed in a third industrial corner of the world: in shoddy, flood-prone basement apartments in the logistics suburbs of Seattle along the other edge of the Pacific Rim—supposedly "post-industrial" but in reality built up in successive waves of de- and re-industrialization oriented toward different hubs of global production. This is a project that has in many ways literally spanned the very Eastward movement of capital across the Indo-Pacific that it seeks to describe in theory.

These facts are not merely coincidental. Even though the bulk of this dissertation is devoted to analysis of secondary data sources and detailed explication of theory, it is still at root a work of economic geography, striving to illustrate the territorial character of global production. While abstraction is a necessary part of this illustration, so is the lived reality of such spaces. These spaces are, after all, the instances from which such abstraction is derived. While this abstraction (whether in the form of statistical data or theoretical systems) exceeds the sum of its

myriad individual parts, it does so only by reducing their excess detail in order to derive from them certain core dynamics. This is an invariant component of any scientific modelling and is often the evidence provided in the critique of such methods as "reductive." But equally essential to the process of abstraction, unacknowledged in such critiques, is the continual return to its empirical origins and a persistent attention given to the many real details thus excluded. Those things cannot be captured in the same ways and must, invariably, take on a more artful or literary form, with an emotive depth that matches their intricate, embedded complexity and an open style befitting their boundless character. In other words, explanation in geography must, in fact, be explanatory.

In this sense, it is important to begin with a reminder that, despite its level of abstraction, this project could not have taken shape without the multitudinous and inexhaustible direct experience of the actual spaces involved. But, while I explore this depth in other outlets (see: Neel 2018), this dissertation is an academic project geared toward advancing the social-scientific understanding of the topics in question. This means that it will approach such topics through the particular scientific methods of geography, making extensive use of inherently reductive numerical data, modelling the phenomena being studied through descriptive statistics, maps and other data visualizations, and abstracting from all of these secondary results various theoretical models, which will be compared to the relevant literature on such theories. Ultimately, scientific practice, including the social sciences, is a matter of "artful modelling" (Cartwright 2019). This is particularly true for work that takes inspiration from Marx's own project, since "Marx's insight […] was that the construction of the conceptual abstraction is as central to scientific work as data-production, fundamental even especially in fields where experimentation can play at best a minor role" (Mattick, 2018, p.16). Similarly, the more general communist project to which

Marx retained fidelity has always emphasized the way in which theory is birthed by the churning of history and is thereby shaped at the most fundamental level by social conflict and pragmatic lessons from the experience of struggle, crisis and the everyday conduct of production.

Thus, theory is irreducible to theorists as individuals and must instead be understood through its connection to the political dynamics of history and to the ways in which knowledge as a whole is compartmentalized in the division of labor and encrusted in the technical character of production itself—a feature of capitalism which Marx explored in various capacities and, in one often-cited instance, referred to as the development of "general social knowledge" as a "direct force of production," indicating the extent to which "the conditions of the process of social life itself have come under the control of the general intellect and been transformed in accordance with it" (Marx 1976, p., for the development of the concept, see: Pasquinelli 2019). This notion of "general intellect" is key to understanding the relationship between theoretical production, social struggle and the ways in which both develop in dialectical interdependence with the (geographically, occupationally and conceptually) distributed and (technically, infrastructurally and socially) embedded knowledge represented in the organization of production and the work of collective labor. This can possibly be summarized as a general "labor theory of knowledge" (Pasquinelli 2019), so long as "labor" here is understood in its most expansive capacity and retains its implication of class conflict.

Regardless of this emphasis on the collective, complex and conflictual production of knowledge, the claim that the resultant scientific pursuits are reductive is, of course, true. But this is not necessarily a bad thing, since reduction can also be a process of emergent construction derived from the sublation of the conflictual material composing the "weak nature" (Johnston 2019) that is its topic of study. The controversy is simply whether this reduction through

abstraction also inherently leads to an equivalent intellectual, ethical, or even ontological, reduction, and whether or not even these results are necessarily negative (i.e. there is also a productive dimension even to a seemingly reductive ontological disenchantment). The position that I take here is that of a rigorous philosophical naturalism, which entails the full endorsement of the Enlightenment's nihilistic and eliminative dimensions, as laid out by Ray Brassier:

> The philosophical consummation of the Enlightenment consists in expediting science's demolition of the manifest image [i.e. the folk-psychological intuitive perception of "how the world is"] by kicking away whatever pseudo-transcendental props are being used to shore it up or otherwise inhibit the corrosive potency of science's metaphysical subtractions (2007, p.26).

This philosophical approach is, if anything, even more important for the social sciences than for the natural sciences, since the "manifest image" in the social context has not even undergone the historical "disenchantment" initiated by the scientific revolution for non-human nature. In fact, when society is the object of study we can say that this "manifest image" is more or less identical to "ideology" as a set of spontaneous symbolic rituals which ultimately uphold existing authorities and the apparently natural order of the status quo (Zizek 1989). And ideology remains fully enchanted, so to speak.

This is why any communist intellectual pursuit must inherently attend a paired political project that seeks to perform a similar disenchantment at the level of the human species as such, repeating the scientific revolution within the sphere of society and economy. In this sense, I hope to here uphold the scientific dimension of the "original content of the communist program," as identified by Amadeo Bordiga:

> We maintain that it is possible to investigate the laws of future society insofar as we imbue the science of human society, as much as it is still in its infancy, with the same capability as the science of nature, which was already in full bloom at the beginning of the bourgeois epoch four centuries ago (1958).

Nor were these merely theoretical concerns for Bordiga, who had founded and helmed the Communist Party of Italy in its insurrectionary heyday, fought against Stalin and the forces of reaction internal to the international communist movement and battled against the early rise of Fascism in Italy, for which he spent years in prison and political exile. As for Marx, these advances in knowledge were inseparable from the development of the "general intellect" and manifested most clearly at the mass scale. Individual thinkers (or groups of them) merely help to sharpen the tools forged in the collective struggle of the species, a process that Bordiga referred to as the development of the "social brain" (as described in Bordiga 2020, Chapter 21).

Finally, a few reflections on the specific nature of geographic abstraction will help to frame the project pursued here. As has already been argued above, the science of geography relies on a dialectical movement between the teeming local excess of a space and the abstraction of that space into a numerical, cartographic and/or theoretical model. This abstracted numeric/cartographic/theoretical artifact functions not as a replacement for that space (this is the classic Borgesian labyrinth of the impossible one-to-one map), nor even as its representation in any simple sense. The compounding problems of such supposed symbolic "representation" have been documented well enough that they need not be recited here (for the philosophical core of the critique, see: Baudrillard 1983, Jameson 1991). Meanwhile, to argue that this abstraction is simply an "argument," as is the basis of almost any intro-level GIS course today (Krygier and Wood 2016) is to reduce the process to a purely rhetorical procedure. This enchains geographic

science to the philosophical prison of the "linguistic turn" that has long held the humanities and social sciences hostage and which acts as the shared ground of all contemporary idealist philosophies (Badiou 2011, Eade 2011).[3] It goes without saying that these philosophical systems are fundamentally opposed to a scientific naturalist project at the philosophical level and, within the political realm, compose the basic framework for the liberal administration of the status quo—particularly its methods for the recuperation and suppression of material political conflicts through their rendition into issues of mere mental "awareness" or inadequate institutional "representation."

I maintain here that geography has the same "corrosive potency" (Brassier 2007) as any other science, and that denying this potency in an attempt to focus on what we might call "folk" geography (after the style of "folk psychology" as spoken of in the philosophy of mind—i.e. in Churchland 1981) disproportionately rooted in descriptive ethnography and discourse analysis will ultimately reproduce all the inherent reductivism of any other process of abstraction and theoretical modelling, even while its disavowal of intentional modelling will lead to scattered reductions that fail to be productive of new insights, or, in other words, theories which simply amass in an eclectic fashion without fusing into a greater political, philosophical or scientific project, since theoretical synthesis is *a priori* off limits (seen as "totalizing"). Against this folk-geographic approach, I advocate for a fully scientific geography engaged in intentional modelling, both in the quantitative and theoretical sense. This is not a matter of the "qualitative"

[3] The subsequent "spatial turn," while seemingly an adoption of a certain geographic sentiment by neighboring disciplines, actually represents a dual incapacitation of geography itself: First, the uses of spatial logic beyond the discipline tended to be little more than the rendition of such concepts into mere metaphors, or fashionable nominalizations that sounded geographical, such as "territorialization," but were little more than the newest breed of hollow academic buzzword. Second, the spatial turn just as often expressed a similar and, in fact, more substantial cross-fertilization into the discipline itself, whereby the ideolinguistic predilections of the humanities tended to override geography's own long and fruitful history of grappling with its emergence from a postwar "quantitative turn" half a century prior.

versus the "quantitative," or any other of the suspiciously simple divisions we often impose on the discipline in hindsight. It is instead an argument that, regardless of any differences in methodology, geography is and should be treated as a science. The fundamentally reductive, "corrosive" process of abstraction is the precondition for the productive, narrative process of artful modelling.

**The Unfinished Quantitative Revolution**

The earliest era of modern economic geography was a period in which such an approach was originally theorized and first systematically practiced. Possibly this is because of the early hybridization between what would later come to be known as "physical" geography and "human" geography which, at the time, overlapped in almost every possible sense, providing a productive interface whereby "physical" scientific methodologies and mindsets could be translated into more amorphous "social" topics. Among the earliest modern geographic models was Johan Heinrich von Thünen's abstracted schema of the pre-industrial European city structure laid out in his work *The Isolated State* (in English in available in two parts as Thünen 1960 and 1966), which became the precursor for all later locational theory. In fact, it is even claimed that Thünen's work is "the world's first economic model" (Portugali 1984, p.202) and is certainly "one of the first systematic attempts to formulate a coherent theory of location and of the social organization of space" (Harvey 1981, p.2). The fundamental approach used by Thünen is as follows:

> Instead of taking the whole and breaking it into parts, Thünen has taken a part and has used it as an instrument to understand the whole; instead of taking general economic

> theory and deriving thereof a specific spatial structure remote from reality, Thünen takes a specific reality, derives from it a specific spatial construct (the *Isolated State*), and uses it as an instrument to derive general economic theory, which in turn is designated to act upon society as a whole (Portugali 1984, p.206).

This is a description of the process of scientific abstraction in its classical sense, when the scientific project was not a fragmented mass of specializations but instead an attempt to integrate the specialized observation with the whole body of existing theory—a process which was understood to always have a social and political thrust to it. At the time, these class interests were both explicit and literal, since all forms of science sat well within the practical domain of the rising bourgeoisie. Social science, in this regard, was treated no differently than other sciences, except insofar as its more amorphous subject matter may have been seen to require additional, distinct theoretical and methodological tools.

Thünen's project, moreover, lay within the original body of work on political economy against which Marx posed his own project of inquiry. Although much has been written on the fact that Marx's work is not and should not be seen as a continuation of the political-economic project and how Marx himself is not a "political economist" in any sense of the word (nor an "economist"—the entire idea of "Marxist economics" is a fundamental misreading of the project or else an ill-fated attempt to make it palatable to its enemies), it is important to remember that Marx nonetheless understood himself as working within the *scientific* tradition. It is not a mere coincidence that, throughout his entire corpus, Marx engages with natural scientists as much as political economists (and in that era, the two were not even fully separate specializations), a fact which has been painstakingly reconstructed by those studying Marx's ecological theories in light of the ongoing full publication of his letters, notes and manuscripts (Saito 2017, Foster 2000,

Foster and Burkett 2017). This is because Marx understood his own project to be a further development of this continual species-scale scientific process in which specialist scientific inquiry, pragmatic technical knowledge and philosophical theory were constantly being synthesized and resynthesized via the collective activity of masses of people, from whence sprung particular theorists capable of condensing this social knowledge into critical inquiries, which were then fed back into the alembic of the general intellect as part of the dialectical transformation of history. This process was not, however, characterized by simple synthesis into a holistic totality, but by conflictual synthesis via negations of pre-existing positions, followed by negations of these negations themselves, resulting in a fundamentally mutable "non-all" that was greater than the sum of its parts but not totalizing or overdeterminate (this is the essential Hegelian kernel of Marx's thought, as described by Žižek 2012 and Johnson 2008).

Thus, Thünen's political economic project was the object of Marx's critique, even while his classical scientific approach (and that of Smith, Ricardo, Sismondi, etc.) was mirrored in the style of Marx's own abstraction—even while Marx applied a more thorough logical structuring to his own process of scientific modelling, derived from Hegel (Arthur 2002), and saw the basic nature of his scientific contributions as standing in opposition to that of the then-established scientific disciplines that had attended the political ascent of the bourgeoisie. Thus, we can at least see the early classical economic geographers, epitomized by Thünen, as offering a good representation of the general process of abstraction within geographic science as such, which nonetheless would ultimately benefit from a repetition of the same procedure of negation and overcoming *immanent to the scientific process* that Marx himself performs for the political economists—since early geographers so clearly serve conservative class interests. After Thünen follows the entire suite of classical geographic modelling, the best known products of which are

probably Alfred Weber's least cost model of industrial location (1929) and Walter Christaller's central place theory (1966), both of which are excellent examples of early social-scientific applications of the *ceteris paribus* principle used in such modelling, as described by Nancy Cartwright (1983). Similar methods were then revived in the postwar "quantitative revolution," but this revival took place amidst a general depoliticization and fragmentation of scientific practice.

The work of William Garrison and Edward Ullman, alongside that of their students—including Brian Berry, Waldo Tobler, Richard Morrill and William Bunge—are the prototypical examples here, applying modern statistics to the geographic realm and updating many of the inherited models on the basis of new statistical practices and new computational abilities. But the basic spirit of these attempts was already well captured by in Fred Schaefer's call to reconstruct the scientific character of geography, published posthumously in 1953. For Schaefer:

> The point is, very radically, that there is no such thing as a methodological distinction between pure and applied science. There is only science and science applied. Whatever distinction there is, is practical, a matter of either interest or emphasis. The laws for which the "pure" scientist looks are in no way different from those which he himself or his "applied" colleagues use. Conversely, some of the most important theoretical ideas have been suggested by engineering problems. (1953, p.242)

The echo with Marx's attitude toward the "general intellect" is abundantly clear here, though the political dimensions of such scientific inquiry are obscured. Inspired by Schaefer's call, the subsequent theorists of the quantitative revolution also attempted to scale up the theoretical range of geography beyond the regional focus it had gradually taken on in practice. This included not only attempts to craft general theories of geography as such (Bunge 1966 and Harvey 1969) but

also work to expand geography's mathematical techniques beyond Euclidean geometry (Tobler 1961).

Meanwhile, the regional focus of classical economic geography was advanced in a parallel, though in a much more politically conservative direction, via the foundation of regional science in the same years. This effort grew out of the field of economics, rather than geography, led by the economist Walter Isard, who turned to the work of earlier economic geographers to revitalize the economic discipline's ability to understand questions of urban development, industrial location and regional dynamics. Isard thereby carried on the development of classical location theory (Isard 1956) and laid the basis for a discipline of regional science (Isard 1960) that was somewhat distinct from Geography as such. This offshoot of economics then tended to carry on its parent discipline's conservative political commitments, merely adding a greater ability to shape policy at the local and regional level. In this sense, the grander goals of the quantitative revolution within geography can be contrasted to the more pragmatic and reformist objectives of early regional science, even if the two practices would cross-pollinate in subsequent years.

But, whereas even the most conservative of the classical economic geographers still understood their work as being related to both the collective scientific project and a collective political project—which were integrally connected to one another—this second quantitative revolution in geography was, in its results, hardly revolutionary. The limits of some of these quantitative thinkers would become well-documented in the critiques levelled at them by later Radical Geographers and/or Critical Human Geographers and need not be reiterated here. This is complicated by the fact that many geographers who would later become associated with the Radical or Critical traditions were themselves early contributors to the quantitative revolution or

at least students of these quantitative geographers. The supposed split itself must be interrogated, then, and caution exercised in identifying later appraisals of the division with the actual course of its formation. Over time, however, it is true that this reaction against the first quantitative revolution often led to a gradual rejection of quantitative methodologies as such—a trend which has only recently begun to be reversed (Wyly 2014)—and the ceding of much of the quantitative realm to economists, urban planners, regional scientists and, later, the proponents of GIS as a distinct subdiscipline (as well as a marketized, militarized software infrastructure), all of whom tended to treat their activity in an uncritical, technocratic fashion, ostensibly depoliticized but in reality following the tradition of their political economist forebearers in aiding the interests of the ruling class by either formulating theoretical justifications for the perpetuation of capitalism or at least providing the technical tools needed for it to maintain its rule. Thus, the potentials inherent in the quantitative revolution (its truly revolutionary dimensions) were ultimately ignored by its later critics—who arguably mis-portrayed even the history of this split itself—and, although this critique was somewhat justified in political terms, its ultimate result was the ceding of an enormous disciplinary territory to those with far more conservative politics.[4]

[4] In fact, we might argue that the "radical turn" in geography was not really a product of the 1970s at all, as the conventional story goes. After all, many of the geographers involved in that stage of the debate were themselves well trained in quantitative methods and were at least in part reacting to their own early work (this is taken to a particular extreme in the example of David Harvey). They may have laid some of the foundations for what would later be referred to as a critical or radical turn, but the reality is that this narrative did not truly take hold in its familiar form until much later. In part, this is a case of how the critique translated between generations of geographers. And if we displace the timeline of this split to the generation of students who came *after* that initial cohort of radical thinkers, then a different picture begins to emerge, with the *narrative* of the radical turn itself part and parcel of a larger *reactionary* turn that was occurring across almost all disciplines in American universities that attended the renewed military-industrial project of the late Cold War and the early years of unchallenged American hegemony. This reactionary turn had two divergent trajectories which nonetheless shared a deep-seated anticommunism: in the humanities and social sciences, this turn was clothed in "radical" language but was premised on a rejection of not only Marx but also the severing of any remaining ties between "radical" academics and insurrectionary political activity. In the fields that inherited the advances of geography's postwar quantitative revolution, however, the reactionary aspect was either disguised in a technocratic veneer—even as it was used to shore up military power and privatize all the early advances of the field—apparently depoliticized through

At the same time, even the best of the radical critiques often failed to provide any real account of the larger material limits that induced such theoretical shortcomings. In reality, the problems of depoliticization, positivism, specialization and an uncritical acceptance of faulty presuppositions drawn from mainstream economics were structural factors in the gestation of the quantitative revolution itself. In this period, a general anticommunist drive combined with the increasing specialization institutionalized in the modern university system (with its indissoluble links to the military-industrial complex) to produce a structural limit on the critical abilities of the discipline. Later, the exact same factors would structure the formation of so-called "Radical" and "Critical" Geographies as well, producing our questionable inherited narrative of the "Radical Turn" while also limiting its adherents to an equally debilitating specialization—but now one additionally hamstrung by the frequent rejection of the illuminating quantitative abilities of the scientific method. Over subsequent decades, the result was a widely de-radicalized quantitative current distributed between Geography, Regional Science, Urban Planning, Sociology and Economics departments, and a radical-in-name but powerless-in-practice Critical Geography, which treated politics as an inherently discursive matter of identity, representation and articulation rather than material power. Some of the best work in geography in these years was able to bridge this gap (for instance Storper and Walker 1991 or Sheppard and Barnes 1990), but often only by reducing the radical import of its politics down to a program for mild reforms. Even among supposedly "Marxist" geographers, almost none retained fidelity to the insurrectionary dimension of Marx's own project, which was inseparably linked to the incendiary core of communist inquiry as such.

reduction to "policy proposals" or remained outright reactionary, as in the case of the economists who proudly oversaw the evisceration of the Global South during the Third World Debt Crisis.

The case of William Bunge is a telling example of the quantitative revolution's structural and institutional limits. More than maybe any other scholar associated with the postwar quantitative revolution, Bunge sought to carry on the political-scientific mission of geography with an explicitly communist program of critical research (abundantly evident throughout his life's work, see: Bunge 1966, Bunge 1969, Bunge 1973 and Bunge 1988). Throughout, Bunge would prefigure many of the theses of later critical geographers, but without their rejection of the quantitative realm. Instead, "for Bunge, mathematics and science were interwoven with social-theoretical critique and with action in the field" (Bergmann and Morrill 2018, p.296). His work included both the "global" production of high-level theory, as in his doctoral dissertation, *Theoretical Geography*—later turned into a book (Bunge 1966) that has since been deemed a classic in the field—and the extremely "local" investigation of the myriad intersecting geographic processes defining a single neighborhood in Detroit (Bunge 1969 and 1971), with no contradiction between the two. In fact, they were seen as interdependent components in a broader dialectical structure that defined the practice of the discipline as such, since "the geometries of space are not static but enmeshed in motions" (Bunge 1964) and must therefore be grasped at multiple methodological levels simultaneously.

Why didn't Bunge's work, then, become the basis of a renewed critical, communist geography emergent from the quantitative revolution rather than in opposition to it? The answer is found in precisely those structural and institutional features outlined above. An avowed communist who sought to contribute to the growing, radicalizing political movements of the 1960s and 1970s, and who was based for many of these years in Detroit—which was one of the major insurrectionary epicenters of the black communist movement in the period—Bunge became an open target of anti-communist blacklisting and was thereby induced to leave the

country (Bergmann and Morrill 2018, p.295). He thus became one among many political exiles living the slow extinction of an unprecedented historic defeat, who were at least the best off among a larger group of dissidents that included many murdered or imprisoned by the state for similar activities. In geography, as elsewhere, the effective witch-hunts against avowed communists, accompanied by internecine battles in the New Left and the enduring conservative influence of the Soviet Union (not to mention China's turn to join the US side in the Cold War) all helped to suppress any alternate potentials that might have inhered in the original quantitative revolution. In this sense, the quantitative revolution as such is not best understood as a sealed-off historical moment, nor as an academic milieu associated with a small handful of dead white men, but instead as a stalled political project, incomplete and momentarily broken by the coordinated forces of repression mobilized across society in the years of deadly counterrevolution following the global uprisings of the late 1960s and early 1970s. But, like any real revolution, its potentials never die with it.

**Communist Geography**

The goal of this dissertation is, in part, to reclaim this alternate historical potential and demonstrate the quantitative and theoretical capacities of a renewed communist geography. Here and throughout, I purposely use the term communist geography to describe my work. This emphasizes the integral political elements of the project, which tie its conception of science to a syncretic social project that cannot be shattered into myriad specializations. Instead, any specialized methodology is only made relevant through its ability to universalize into a more general social-scientific practice that incorporates all dimensions of intellect including the manual, giving shape to the development of the "social brain" of the human species. Even while

it is founded on a "corrosive" criticism, this is in every way a positive political project aimed at building new possible futures. Watering it down into a purely negative "critical" geography or an inherently vague "radical" geography simply allows for ambiguity in interpretation to condition a long-term dissolution into prevailing academic institutions. Even "Marxist" geography is susceptible to such a trend, since it's perfectly possible to "draw from" Marx without endorsing the revolutionary overthrow of society or the attempt to build communism. This has been made abundantly clear in recent years, as "radical Marxist" scholars such as David Harvey have now fully endorsed the "no alternative" logic of capitalist realism (Fisher 2009), posing minor reforms in the place of revolutionary aspirations and claiming that capitalism is "too big to fail" (Martín 2020)—and this in an era when capitalism's continuation may well guarantee the extinction of the human species.

Communist geography stands fundamentally opposed to all such political dilution. It is not a practice aimed toward "policymakers" and gives no suggestions for better "governance." The utility of such research is found instead in its ability to contribute to the maximal program of actually-existing political movements and to their general intensification over coming decades. Even more than this: the production of such theory in the first place originates from the material flow of history. It is not an individual intellectual exploration, then, but a (sometimes) individual articulation of class intellect. It is an immanent process, generated by class struggle within capitalism, or a form of "practical reflexivity" (Gunn 1987). In this sense, it cannot be used as a mere tag or brand for some new variation on the old academic practice. If it has no concrete extension to moments of insurrection, it ceases to be a communist geography. But by the same logic: if the new wave of political movements is as decisively defeated as the last, communist geography will cease as well, returning to the same hibernation as the real movement itself. This

is not so much the death of theory or politics as their reduction to a pure, smoldering potential capable of actualizing again in utterly new forms when the forward flow of history again makes them structurally probable.

This means that the production of theory within communist geography is integrally linked to a broader collective revival of communist thought, conditioned by the real movement of history. This revival has in every way been a bottom-up phenomenon, driven by the pursuits of proletarian intellectuals seeking a better understanding of their own class experience. Even if its theoretical expressions have drawn from pre-existing work produced within the academic sphere and if many such worker-intellectuals ultimately entered, to one degree or another and often temporarily, into the academy, the body of theory has largely retained its fidelity to ongoing political movements taking place outside of the university. Beyond this, there has also been a somewhat rigorous attempt to separate work produced for academic consumption from political work produced for a more general audience—and to do so in a way that minimizes the promotion of individuals as intellectual leaders, instead emphasizing the collective production of texts, often to the extent of effacing individual authorship through anonymity. Though this dissertation draws from this broad current, then, its academic context places inherent limits on its political utility. It should, therefore, be treated less as a final work and more as raw material for future repurposing and use beyond the academy.

Why, then, not limit the project to a purely theoretical inquiry into this new body of communist thought—or specifically as an intervention into ongoing Marxological debates, arguing for greater empirical measures or a more thorough use of quantitative methods? The answer is, again, the nature of social-scientific abstraction, which simply cannot be severed from its material and requires continual grounding in its object of inquiry. Without methods for

controlled empirical experimentation, social science must make use of thought experiments and data derived from the past flow of history, then confirm or reject certain hypotheses through careful observation of real events occurring at the macro-social scale. The flow of history, then, becomes both a source for data and intuition in the creation of the theory, as well as the ultimate testing ground for that theory's utility. It is important, then, for a communist geography to seize upon the world as it really is and, moreover, to capture the crux of global economic dynamics as they really exist—which means as they have taken shape in space.

This means that a communist geographic project seeking to understand the state of the global economy and global crisis dynamics must have a grasp on the global distribution of "territorial production complexes" (Storper and Walker 1991, p.138), as well as the conditions that would permit new such complexes to form at the frontiers of accumulation and induce other, older complexes to decay as they bear the brunt of any breakdown in the circuits of capital. It is this territorial awareness that makes communist geography distinct from communist theory as such. While, in broad terms, it would be correct to say that exegetical work on Marx's original writing or intensive investigations seeking to reconstruct a materialist philosophy drawing from the Hegelian system and contemporary philosophy of science is also all communist theory, for instance, this is theory in its rarefied form. Its relation to communist geography is similar to the relationship between the math of purely theoretical physics to, say, mechanical engineering—or even to a science of greater emergent complexity, such as biology. All are science, as Schaefer reminds us. The same core "laws" are all there, but the ethereal logic of pure *ceter paribus* models must now meet the messy complexity of reality. The theory itself must then be open to pragmatic knowledge derived from this reality, even while not being reduced to a purely eclectic empiricism with no abstractive ability.

So communist geography seeks to understand how the "laws of motion" of the capitalist system first identified by Marx take shape in the real world. In part, this involves all sorts of historical political contingencies. But it also follows its own locational logic, which results in emergent properties not easily predicted in the most rarefied models. This means that geography is integral to the system itself rather than just acting as an external friction on otherwise smooth theoretical interactions. This is also why specifically geographic modelling is necessary—and also why it will only be accurate when it is modelling done on the basis of the real economic laws of the system, rather than the various illusions of equilibrium peddled in mainstream economics. Key to this process are models of the production, destruction and reconfiguration of territorial production complexes. Even more central is the general role played by geographic divisions in the greater system, since, in Marx's model, capitalism only advances through a process of self-limitation and self-overcoming. This is key to understanding not only the racial character of capitalism (Robinson 1983) but also its geography.

This is certainly not a new claim in any way and has long been recognized by various radical and/or Marxist geographers. The notion of uneven development (Smith 1984) has long been central to the field, while concepts such as the "spatial fix" (Harvey 1981) are taught in basically every geography department, as are the resulting "spatial divisions of labor" first systematized by Doreen Massey (1984). All of these concepts have become central to mainstream geography textbooks and studies of so-called "globalization" (for instance: Spark 2013). At the same time, the "Marxism" that lies at the base of much "Marxist geography" (but especially among key texts and theorists, such as Harvey) is an archaic variant—as indicated by its very use of the proper name—often entirely divorced from the thriving debates within contemporary studies of Marx, frequently drawing on obsolete work from the postwar period,

which composed a now-outdated "standard interpretation" (as identified in Moseley 2017) and which rejected the importance of Marx's theory of money, seriously misinterpreted the logical structure and level of completion of *Capital* and endorsed long-disproven theories of underconsumption as the source of crisis (such influence can be found in Harvey 1982). These faults combine with the depoliticization of Marxist geography (manifest in its persistent turn to mild reforms and small-scale local activism as the focus of political activity, even in the face of widespread mass rebellions) to justify a rejection of the designation entirely in favor of the more affirmative project of communist geography.

**The China Question**

Even if the ultimate goal is to understand the current state of capitalism and its crises in abstraction and at the global scale, a communist geography begins by taking the actual territorial character of the system into account. It is therefore essential to choose a focal point that illustrates both the local/regional production of such industrial-territorial complexes and the expansive aporia of global crisis. In both respects, the specific role of the Chinese economy within the global capitalist system offers just such an example, simultaneously illuminating the means by which crisis reshapes specific production complexes on the ground and the ways that it reverberates at the largest scales, exerting a macroscopic influence on the baseline conditions of production in general. The Chinese economy is able to serve this analytic function here because of its central position within global value chains, sitting at the heart of the "hidden abode of production" (Marx 1992, p.279) and therefore providing a window into the terrain on which capitalism's central contradictions play out.

The importance of the Chinese economy in both material and analytic terms will be clarified further in the main body below. Here, a quick summary will help to frame the topic: Geographically, China's mega-urban territorial production complexes are currently the heart of global industry, manufacturing numerous baseline products for use elsewhere. These draw on an even larger, more widely distributed constellation of Chinese industrial complexes that either feed unfinished products into these export-oriented territories or produce for domestic consumption. At the same time, the past decade has seen many of the most important of these territorial complexes aging out of their former positions in the global hierarchy of production, leading to factory closures and automation, all in attempts to climb up the value chain (Yang 2016, Zhang 2014). This is the manifestation of that deeper global crisis tendency that acts as the central immanent contradiction of the capitalist system—one dimension of the "moving contradiction" of class struggle (Endnotes 2010 and Marx 1993c).

In this regard, the Chinese economy is positioned as a sort of nexus. It is an open question or, more accurately: it is where all the questions about the future of the global economy itself—including its financial character, its industrial geography, the prospects of continuing profitability, demographic questions of access to labor, technical questions of automation and industrial organization, etc.—all converge with seemingly geopolitical debates about the future of the global political power structure. Below, I will seek to break through the limits imposed by purely geopolitical framings of the US-China conflict, which lead to erroneous conceptions of a "China Model" (Bell 2015) or a "Beijing Consensus" (Ramo 2004) advocating for global "win-win" development (Lin 2017). In contrast to these accounts, I will illustrate the fundamental connection between global economic crisis and the subsequent political conflict between competing hegemons. Though this is similar in kind to some existing work (for instance Arrighi

2014, Tooze 2020, Klein and Pettis 2020 or Davis and Wei 2020), the key distinction is not just that it takes the economic dimensions of the conflict into account but that it does so with a more robust theory of capitalism and crisis capable of linking long-term economic trends to the fundamental laws of motion of the system itself, rather than its political maladjustment or temporary disequilibrium.

The research below will approach the China Question from multiple angles, then. At a more abstract level, the recent history of China's role within global value chains will be reviewed, as will its role within the global financial system. Then the restructuring of these roles will be explored in more detail, both domestically and internationally. On the domestic level, this involves a review of general data on crisis dynamics (with an emphasis on trends in industrial profitability) and on the geography of factory relocations and technological upgrading. On the international level, it involves the aggregation and review of all existing data on Chinese outward investment, as well as a review of other databases that record more expansive data on outbound financial transactions in general. In addition, it is here that my own primary data-gathering will be added to existing secondary sources, recording basic information about Chinese firms operating in the industrial districts of Dar es Salaam, Tanzania. This will be paired with secondary statistics and a number of country-by-country case studies to illuminate the general global geography of Chinese outbound investment, with an emphasis on the manufacturing sector. The result, I hope, will be both a cache of resources for further use in real world political movements and an example of the ways in which a scientific communist geography can contribute to the understanding of our global political and economic impasse.

# PART I

# The "Long Crisis" and its Consequence

## Theories of Accumulation, Breakdown, and Imperial Competition

### Introduction

Placed in comparative perspective, the 2008 Great Recession seems to have a broadly similar character to the Long Depression of the later 19th century. First characterized as a mere "financial" crisis, beginning with the Panic of 1873, the initial breakdown soon gave way to two decades of economic stagnation accompanied by persistent overproduction of core industrial goods, signaling that the roots of the crisis were much deeper than the popping of a mere speculative bubble (Itoh 1990). The period was therefore initially known as the Great Depression, at least until the crisis of the 1930s would steal the title. But, while the 1929 breakdown was sharpest in the severity of its downturn, the crisis of 1873 was far more stark in its effects on political history and the popularization of particular theories of capitalism and capitalist crisis through mass political activity.

The two decades that followed the crisis—capped by another, more sudden and severe collapse in the Panic of 1893—were also a period of maturing class conflict in which the basic groundwork was lain for the mass parties, industrial unions and peasant armies that would shake the world in the early 20th century. The end of the Franco-Prussian war and the crushing of the

Paris Commune set the period's initial political terrain, while the crisis carved out its economic coordinates. Not coincidentally, these were the decades in which what would come to be known as three volumes of Marx's *Capital* were first being compiled and published, translated and slowly propagated through the new mass political movements, culminating in the emergence of a renewed wave of communist organizing by the turn of the century. But this was also a period in which the imperial drive of industrial capitalism was first becoming evident, in both the formation of competitive trade-finance blocs and in bloody overseas conquests—generating a series of fault lines within these growing political movements that would result in the first major splits within the mature global communist movement. These were, finally, the years in which the dominance of the global hegemon, Great Britain, would first begin to be challenged by the rapidly growing economic power of its major challengers: the United States and Germany (Hobsbawm 1989).

It's also worth noting that the canonical texts of Marx's economic critique were themselves fundamentally shaped by these political movements and are, as historical documents, inseparable from this context. The mechanics of this influence were quite literal, since, aside from Volume 1 of *Capital*, the compilation, release and interpretation—and the presentation of Marx's economic critique as a completed theoretical system, rather than an open or incomplete one—were all facilitated by this rising movement, which would peak with the first "Marxist" orthodoxy established by the "second international," centered on the German Social Democratic Party and out of which would grow the second orthodoxy centered on the Russian interpretation. Not only were Volumes II and III of *Capital* compiled by Engels in conversation with these rising movements, but also as part of a political struggle against the residual influence of other theoretical interpretations in these movements, such as that of Ferdinand Lassalle. These texts,

therefore, cannot be treated as if they were hermetically sealed from the oxidizing influence of the mundane world of everyday politics either before or after the death of Marx. In particular, the "completion" of the incomplete critique of political economy cannot be separated from the political passage from the German SPD's Gotha program, explicitly critiqued by Marx, to its Erfurt program, composed by Karl Kautsky, who also edited, compiled, abridged and released the manuscripts that would come to be known as the *Theories of Surplus Value*, often portrayed as "Volume IV" of *Capital* (for an overview of some of the major questions raised by this publication history and comparisons of the various drafts of Capital, see: Heinrich 2013 and Heinrich 2012, Chapter 1). It hardly seems coincidental that similar "returns" to Marx—each of which has involved renewed debates about canonization, reconstruction and the relative importance of particular unpublished manuscripts—accompanied renewed crisis in the 1970s and again today.

The Great Recession mirrors the Long Depression in multiple ways, then. Overall, as will be explored in more detail below, what was initially thought of as merely a "financial crisis" has since proven to be far more persistent than the popping of a simple speculative asset bubble. Instead, the breakdown has resulted in more than a decade of what is now consistently referred to within mainstream economic literature as "secular stagnation" (Summers 2013), triggering or otherwise sculpting a series of second-order political and economic crises (such as the North African insurrections, the European sovereign debt crisis, the South African strike wave, a new sequence of political crises across Latin America and a renewed cycle of unrest in the United States) and threatening the reemergence of full-on trade wars or even the potential future formation of new economic blocs dividing the global economy. Prior to the Great Recession, such events had been increasingly considered to be relics of last century, made obsolete by

globalization and the "end of history" that accompanied the collapse of the Soviet Union. After the Great Recession, a "rebirth of history" (Badiou 2012) seemed to be taking shape, marked by the return of mass political mobilization, renewed trade conflict and the re-popularization of theories that questioned the very basis of the global economic order.

To maybe stretch the historical analogy a bit too far: the Great Recession began to have an effect distantly similar to that of the Long Depression of the 19th century, as those thrown into new mass political movements (or just experiencing the "new normal" of permanent stagnation) began to seek out theories able to explain their predicament. One result was that many people across the globe now seized on theories of capitalism, crisis and communism that had been quietly honed over previous decades, but which had remained sequestered in purely academic realms or limited to the spare few popular struggles that did occur in this period, centered mostly in Latin America. But this now occurred within a much larger ferment of theoretical and political activity, with the two feeding actively into one another. This created an active market for new translations (for instance Heinrich 2012 and Bonefeld 2014) and new writings by established Marxist thinkers and communist philosophers, but also gave a critical edge to the reception of such works, which were now, at least in part, judged according to the measure of real world events. This placed a particular importance on works that were able to bridge the gap between pure theory and real economic history (for instance Brenner 2002 and 2006), with a natural emphasis on accounts of the most recent crisis (for instance Kliman 2011 and Mattick 2011). At the same time, out of this ferment also emerged a wave of new works produced or rediscovered and newly popularized by younger theorists, often enmeshed within emergent political movements. These works thereby took on a certain communal character, often affirmed by their collective, anonymous authorship (see: Endnotes, SIC, Chuang and Kosmoprolet).

Overall, the result was a vibrant new body of research into the political and economic consequences of the Great Recession and, ultimately, the nature of economic crisis and the capitalist system as such. It is from this ferment that my own research project first emerged, and it is here that I will begin—albeit moving "backwards" so to speak, from the most orthodox accounts proximate to the political mainstream to the more heterodox accounts, directed against that mainstream. In Chapter 1, I first give a brief overview of some of the mainstream economic literature on the Great Recession and crises as such (particularly theories of crisis that emerged after 2008), attempting to trace out in detail the limits of any critique that remains immanent to the bounds of economics as a discipline. Following this, I will turn in Chapter 2 to the communist critique of capitalism, crisis and imperial conflict, as informed by Marx but with an emphasis on more recent works within the field. Finally, in Chapter 3 explore the ways in which such theory can be used to explain recent economic history and the questions thereby raised about the role of China within the global economy. Altogether, this will provide a theoretical and historical account of the "Long Crisis" of capitalism, in which a secular tendency toward systemic breakdown builds over time, driving the system forward even while it obstructs the mechanism of continuing accumulation and slowly disintegrates the circuit of capital.

# Chapter 1

## Mainstream Accounts of Crisis

### Secular Stagnation and the Astrology of Supply and Demand

It has now become common sense to point out that the economies of the world's richest countries, and even the global economy more generally, are caught in a period of "secular stagnation" (Summers 2013), constituting a "new normal" (IMF 2010) in which growth remains subdued even as inequality continues to rise, un-and-underemployment remain high, and the continual roll-over and refinancing of debt have become increasingly integral to both personal and international finance. But if an economist in, say, 2006 were to have predicted this imminent outcome, the presumption would have been that this economist belonged to some heterodox school of thought—a "post-Keynesian," maybe, or even a "Marxist." More importantly, that presumption would have been accurate, since thinkers who would have ascribed themselves such appellations were the only economic theorists seriously making such predictions at that time. The economic orthodoxy at the time—often described as "neoclassical," but in reality much more diverse than this and united as much by what they excluded from serious study (such as questions crisis and disequilibrium) as by any shared reference to a single canon of "classical" theory (see: Mattick 2011, Pitts 2020)—had essentially declared that the problem of deep, long-term economic crisis had been solved and that the few moments of disequilibrium caused by

political overreach into the market or the regular business cycle could easily be rectified through textbook monetarist intervention, to be conducted by the central bank.[5]

In fact, the term "secular stagnation" was itself drawn from a seemingly discredited economic theory composed in the 1930s by Alvin Hansen, the Harvard economist who had helped to introduce what would come to be known as "Keynesian" economics to the US and who had formulated key policy responses to the Depression (Backhouse and Boianovsky 2016). It certainly didn't seem to describe economic conditions leading up to 2008 and even those influenced by the work of Hansen, Keynes and other fellow travelers did not use the term to describe economic conditions in these years. Meanwhile, even many of the more mainstream "New Keynesians" who were predicting some sort of potential crisis, such as Paul Krugman, also misidentified its cause in US fiscal deficits—in a series of policy debates that might be characterized in retrospect as the first stirrings of the US-China trade war (Tooze 2018, Chapter 1). But in the years after 2008, the mainstream economics discipline was in shambles as it attempted to explain away its own long-term denial of the crisis that had just occurred. It was in this context that the term "secular stagnation" was revived by none other than Larry Summers, former Treasury secretary under Clinton and head of the National Economic Council under Obama, where he was faced with the reality of the new economic situation firsthand (Summers 2013). This signaled the reopening of mainstream economics to crisis theory, which had long been excluded from the accepted orthodoxy.

The immediate effect has been the general destabilization and opening of the field, which has not yet settled into any new orthodoxy. That said, when the economic orthodoxy is

[5] For a simple play-by-play overview of such failed predictions, see: Krugman 2009; and for an in-depth look at their theoretical roots, see: Colander et al. 2009 or Labini 2016; or for an historical overview, see Mattick 2011.

destabilized by real-world crisis, economists are also faced with the reality of extensive state intervention in order to restore stability—as Robert Lucas, professor of economics at the University of Chicago (the very site wherein the pre-crisis "neoclassical orthodoxy" was systematized) was forced to confess in the midst of the crisis: "everyone's a Keynesian in a foxhole" (Fox 2008). This is a fitting concession, coming from the man famous for declaring, in the early 1980s, that Keynesianism was dead (after which he was awarded a Nobel Prize for his work proving this point). But it also illuminates some of the deeper implications in the term "Keynesianism" itself, which has never been as consistent a body of theory as it is often treated (by both its own adherents and its critics) and yet still returns as a point of reference again and again. Geoff Mann (2017) has produced by far the most systematic appraisal of just what "Keynesianism" designates. In so doing, he expands the concept well beyond the work of Keynes himself. The core of Mann's appraisal is the idea that Keynesianism is an immanent critique of liberalism, advocating for a "revolution without revolutionaries," which retains faith in the centrality of the rational, universal state, emphasizes pragmatic, limited intervention and always exists in a tenuous relationship with the threat of real revolutionary instability: recognizing the legitimate demands of the poor but also fearing the potential for revolutionary terror and the anti-civilizational import of insurrection. But here one element of Mann's appraisal seems particularly relevant. This is the fact that "Keynes's political economy—and all varieties of Keynesianism—are about the production of credible stability in the face of crisis" (Mann 2017, p.15). It is here that we can identify some consistency within the present predicament faced by economic thinkers.

It thus becomes possible to use Mann's characterization to broadly define the past decade's entire range of attempts to reestablish the public credibility of economics as a discipline

(in the face of its failure to predict the crisis and lack of agreement on how to remedy it) and of the policies derived from this theory as "Keynesian," regardless of the extent to which they focus on what might be considered traditional Keynesian policies, such as direct fiscal stimulus through public works. What unites them is the very fact that they have all been forced to evolve into immanent critiques of liberalism to one extent or another, all while recognizing the reality of state intervention and often rallying behind it when a real crisis looms, emphasizing Mann's point that "capitalist modernity, in fact, is and always has been Keynesian on the inside, as it were—the call for the state when disorder looms or revolution threatens has always been an option […]" (ibid, p. xi). Many thinkers engaging in what Mann would, in this sense, characterize as the fundamental Keynesian theoretical procedure would hardly characterize themselves as "Keynesians." That said, the crisis did see the ascent of two loosely grouped schools of thought that drew more explicit influence from Keynes and the history of "Keynesian" macroeconomics and both of which have tended to understand themselves in these terms regardless of the often amorphous and eclectic nature of the groupings: these are the "New Keynesians" and the "Post-Keynesians," as identified by Mann.

Maybe the most notable shift in political appointments since the crisis has been the placement of several prominent New Keynesians into key advisory positions, alongside the ascent of economists like Paul Krugman, Joseph Stiglitz and Robert Shiller to leading positions within the media, where they crafted the most widely publicized explanations of the crisis and justifications for the policies undertaken in its aftermath. As Mann explains, New Keynesians "work on a neoclassical synthesis foundation" and see "Keynes as an inspiration for skepticism" but not "as a sage to be turned to again and again" (ibid, p.60). This proximity to the once prevailing orthodoxy has naturally allowed them to step into key roles within the discipline after

its destabilization by the crisis. At the same time, the post-Keynesians (also referred to as left-Keynesians) are "generally of a social-democratic tendency" and "hew to a close, if not exegetical, reading of Keynes' later work" (ibid). Thomas Piketty and L. Randall Wray are probably the best known thinkers in this tradition today, but the work of post-Keynesians from previous decades (such as Michał Kalecki, Hyman Minsky, Joan Robinson and Roy Harrod) are often equally influential.

At the same time, among either New Keynesians or post-Keynesians, the theory of crisis (if it even exists in any systematic way) is most often a theory of individual crises rather than a general theory of the phenomenon as such. In other words, "crisis" is often understood as a singular or momentary event that can only be explained with reference to its particular and always completely unique precipitating conditions. Even if certain mechanics (such as the deflationary spiral) might be shared between *certain* crises, there is no necessary linkage between any of these individual crises and crisis in general, nor a long-run, secular crisis lying beneath. At best, these economists might also gesture in the general direction of factors such as inequality and insufficient effective demand, pointing to the work of Piketty, for instance, who demonstrates certain secular tendencies toward increasing inequality (and subsequent downward pressure on growth) in a pair of bestselling books (2014 and 2020). But even Piketty's theory, while admitting that capitalism may have certain degenerative tendencies built into its basic structure, offers no real theory of crisis. It therefore provides only middling solutions to the basic structural tendencies that it identifies, such as a global income tax—as impossible in practice as it is questionable in theory. This is, however, a general feature of "Keynesianism" as such, at least as defined by Mann, since it is "always subordinating questions of principle to those of practice" because its general unifying feature is "fear of disorder" paired with a lack of faith in

the self-regulation of society, and thus "pragmatism is essential to all things Keynesian" (Mann 2017, p.51). This means that self-described Keynesian thinkers may not be the best sources for truly "general" theories of crisis and secular stagnation, even if their work has gained influence within the discipline. It remains notable, in other words, that this generation of economists has not yet produced its Schumpeter (maybe the work of Koo, explored below, is one exception), capable of at least formulating both a socially and mathematically coherent *cyclical* model of crisis—likely because the "secular" part of "secular stagnation" seems so salient.

There are a few trends within the destabilized mainstream of economic thinking, however, that do offer a more systematic approach to the question of crisis. These tend to be what might be thought of as a different strain of pragmatist economics that has some overlap with the two Keynesian factions identified above but is hardly co-extensive with it. On the one hand, this sort of work is often formulated by journalists, investors and research consultants who may also have formal economics training or who have worked, at times, as academic economists, but whose more practical experience guarantees that they start with empirical measurement and build theories up from there, rather than deriving them from the rarefied equilibrium equations and rational-actor presumptions of the old orthodoxy. They neither try to return in full fidelity to the original thrust of Keynes himself (as do Piketty and many other post-Keynesians) nor do they try to fully synthesize their theories with the now crumbling neoclassical foundation (as do New Keynesians such as Krugman or Stiglitz). That doesn't mean that economic orthodoxy or Keynesian political pragmatism never come into play, however, since such thinkers' basic conceptual schema still determine the basic language available to describe economic phenomena. The core element of economic orthodoxy is, after all, the idea that "the economy is essentially a vast mechanism for allocating resources to satisfy consumption needs" (Mattick 2011, p. 37) and

that this mechanism is therefore composed of buyers and sellers whose aggregate relationships can be summed by supply and demand. This is still true for the immanent critique of the Keynesians, even if these thinkers at least acknowledge that "neither civilization nor capitalism is natural" (Mann 2017, p.51) and therefore emphasize the "political function of the state as the sole, if flawed, legitimate universal institution," placing their "unshakeable faith in techno-bureaucratic expertise, clear jurisdictional definition, and policy independence from interested 'meddling'" (ibid, pp.53-55). Thus, the myth of the economy as a distributional mechanism and the state as a rational, universal institution are paired illusions. Moreover, neither seem like illusions. They are instead common sense, because this is how both the state and the economy appear to us, especially when every element of our own subsistence must be obtained on the market and, if not there, through appeal to a state that is portrayed as existing above "the economy" and separate from it. Thus, the economy appears as a vast collection of commodities changing hands, with the primary movers being those who buy these commodities and those who sell them. Meanwhile, the state appears to be completely and correctly separate from this sphere, ideally unsullied by it but able to intervene when necessary—if relationships between the prime movers of buying and selling somehow break down.

But the sort of folk science (again, in the sense used by Churchland 1981) that emerges from common sense observation is not science. After all, it is common sense, based on such rudimentary observation of lived experience, that the sun orbits the Earth because it "sets" behind the horizon and that the Earth itself is flat. Faced with new and contrary data (such as the reality of secular stagnation, or persistent crisis) it is perfectly possible to simply twist this folk-scientific common sense into ever-more-complex systems in order to accommodate both the new evidence and maintain the cosmological status quo. To continue the metaphor, at the risk of

repeating a cliché oft-cited by thinkers who draw from the work of Marx: Neither the evidence that led to the Copernican revolution nor Copernicus' own systematization of that evidence into a coherent theory of heliocentrism triggered a sudden shift in astronomical science in favor of this theory. In fact, even though the theory was published just before Copernicus' death in 1543, it was not until around 1700 that it gained any widespread support among astronomers. Instead, its publication stimulated a simultaneous political crisis (including the persecution of its adherents, most famously in the heresy trial of Galileo) and an explosion of ever-more complex theories twisting the geocentric orthodoxy to fit the new observations. In the course of this process, even the defenders of the folk-scientific status quo had to make increasing use of methods and data which hewed ever-closer to the Copernican conclusion. But it was perfectly possible to do so while never conceding to the cosmology implied in that conclusion—which, after all, opposed both the ideologically accepted cosmology of the church and its theoretical basis in Aristotelean physics. The apogee of geocentrist theory was the system of Tycho Brahe, who stretched the theory to its limits by formulating a conceptually geocentric model that incorporated heliocentric elements (the sun and moon orbit the Earth, the other planets orbit the sun) in order to maintain the fundamental cosmological primacy of the Earth and retain the basic presumptions of Aristotelian physics all while accommodating the accumulating evidence challenging both. In fact, Brahe's system made use of even more expansive and accurate data than that of Copernicus and had an equal, if not greater, predictive power—at least before it was superseded by later heliocentric systems.

Thus, it is perfectly possible for even the most skilled, pragmatic economists starting with robust data to build up from that data a model that still maintains the ideological fundamentals of the old economic orthodoxy or its immanent critique, even while conceding more and more

points to critics. Like Tycho Brahe's model of the solar system, such theories can be critical and accurate with regard to the data even while they uncritically uphold the status quo cosmologies through which these data are understood. Today's heterodox economists often offer accounts of a similar character: revisiting all the data on economic phenomena, acknowledging the failures of orthodox economics, rejecting key components of mainstream economic theory in favor of once-heretical alternatives, but nonetheless still believing that the economy is fundamentally a mechanism for meeting human needs—the cosmological status quo, so to speak—driven by an equilibrium physics defined by *distributional* relationships between buyers and sellers, supply and demand. This is equally true of even the most extreme cases of immanent critique, in the form of the most radical Keynesians, in Mann's sense, who draw from the work of Marx and even political economists like Ricardo or Sraffa[6] to formulate what seems to be a critique of "capitalism" and to advocate for extensive state intervention (up to and including extensive nationalization) which they may even refer to as "socialism"—but this "socialism" is nothing more than the name for Keynes' own "revolution without revolutionaries," retaining all the fundamental features of capitalism, holding to the separation between "state" and "economy," and laying out a (fanciful) program whereby power is gradually handed over to the power of an enlightened state bureaucracy, often buoyed by some vaguely-indicated "social movement" giving it popular legitimacy and constituting the mechanism whereby its commands can be distributed across the population via seemingly cooperative and mass-democratic processes which, in terms of actual power, are little more than consultative organs. The real heliocentrism with regard to any variant of such a system would require the overturning of this cosmology and

[6] And here it is important to note that such thinkers, even if they call themselves "Marxists," also tend to refer to their school of thought as "political economy" or "political ecology," neither of which retain much of the revolutionary implications of Marx's original work, which was categorized, after all, as a "critique of political economy."

its attendant physics. It would require asking more basic questions, such as: what if the economy is not primarily concerned with meeting human needs? What if the state is neither rational, universal nor competent? Or: what happens when we focus on production, rather than distribution, or presume inherent imbalance rather than equilibrium? These would be only the most rudimentary beginnings of formulating a new system, not yet necessarily revolutionary questions in and of themselves but containing the germ of a revolutionary insight.

But even if these accounts are ultimately lacking, they will provide useful in framing the basic terms in which crisis is spoken of today, illustrating the current state of the global economy and thereby illuminating the theoretical and empirical terrain on which the contemporary Marxist theory of crisis must unfold. In fact, the closer such theories hew to observable trends in the data, the more they will tend to approximate key elements of the communist theory of crisis, even if they might disavow this fact—just as Brahe's system came closer and closer to that of Copernicus, ultimately allowing every planet to budge except for the Earth. This has become something of a constant theme in recent economic research that attempts to abstract up to the theoretical level from the observed dynamics of the real-world economy, rather than from the false and fanciful microeconomic imaginaries of the neoclassical school. Historically, this process of piecemeal concession performed for the sake of sacrificially preserving the fundamental coordinates of the theoretical system as a whole has defined Keynesianism as a broader immanent critique. Such theories tend to either signal or even directly reproduce certain key conclusions from scholars who have attempted to carry on Marx's critique of capital, since they are based on observations of the exact same long-term trends predicted by Marx himself. This phenomenon whereby economists and economic journalists rediscover the basic tenets of *Capital* (usually in an era of renewed crisis), is therefore similar to the recently popular fad

wherein avid Flat Earthers set out to prove their hypothesis by testing the very same data that demonstrates the Earth to be round. But just as the Flat Earthers, even when confronted with unassailable evidence, will often fail to concede their position—which is, after all, more an expression of faith in a very confused social critique than it is a scientific inquiry—so too will even the most heterodox economists cling to faith in their discipline even when confronted with immense uncertainty or downright disproof of its predictions.

At the most extreme, this amounts to proposals of a "socialism" that, in all its fundamental features, still preserves the capitalist law of value, now tended by the supposedly universal and rational state, conducted for the sake of preserving civilization in the face of growing unrest. Following this logic, we might even argue that Mann's version of Keynesianism, in its most extreme form, is best described not simply as "revolution without revolutionaries" but also as "socialism without the overthrow of capitalism." This trend is evident at a mundane level in the subtle change in the popular meaning of the term "socialism" itself, which is now often mobilized to refer to the restoration of classical liberal policies (often the exact same policies that had been in place in the postwar US, or even weakened versions) or as a description of various Northern European polities. This is, in fact, one of the central themes explored by Marx in *Capital*: why and how does this particular social organization of production so consistently produce fetishistic myths about its own functioning? How, in other words, does capitalist production itself generate economics as an "elite folk science" (Ravetz 1994-1995) that acts primarily to reinforce the ideology of the status quo? Or similarly: how has "socialism" come to mean almost the exact opposite of what it meant to both revolutionaries and counterrevolutionaries a hundred years ago? The result for us today is that, ultimately, even the best non-Marxist theories that are able to slowly, blindly feel out the real laws of motion of the

capitalist system and carefully reconstruct depoliticized variations on *Capital* will still ultimately be unable to provide any precise clarity, since they have disavowed the self-reflexive political and scientific project that underlays communist inquiry—a fundamental observation of which is that economics (or "political economy") is not a coherent science but is instead an ideological emanation of a certain *social* system that then takes itself to be a description of reality as such (as has been emphasized repeatedly, there is therefore no such thing as "Marxist economics," see: Smith 2020).

Below, I will therefore begin by looking at several accounts of recent crises and the reality of secular stagnation that remain immanent to economics as a discipline and retain fidelity to the basic cosmology of capitalism. Though this might at first seem to be a lengthy aside, it is in fact essential to begin any immanent critique of a social system with an exploration of that social system's own self-understanding. Economics acts in just such a capacity here, having built up a complex cosmology from the common sense appearance that commerce and consumption are the drivers of production. This is not an incidental fact, but instead a necessary structural dynamic of the capitalist system as such—since alienation and the commodity fetish facilitate the real social abstraction of money and labor, this cosmology, which communist philosophy refers to as "ideology," is itself a necessary, functional aspect of the material relations composing the system itself. Nor is it an apolitical observation, since such a cosmology always serves to preserve the status quo and to justify the power of those who benefit from it. Just as it was impossible, then, to understand various geocentric models without also understanding the *political* commitment to geocentrism and Aristotelian physics, so too is it impossible, now, to understand various economic models without also understanding their *political* commitment to capitalism and its physics of supply and demand.

But when faced with a crisis of explanation, such systems must adapt to face it and to wrap that crisis into the prevailing cosmology. In doing so, they often reach immanent theoretical limits and begin to point beyond themselves to the real dynamics that they help obscure. At the same time, however, they also provide legitimately useful bodies of evidence that can be taken up by those attempting to upend that cosmology: in attempting to systematically disprove the heliocentric model, Brahe not only felt out the limits of geocentrism by producing its most systematic and accurate variation, but also produced a wealth of new observations and astronomical techniques that would be put to extensive use by later heliocentrists, beginning with his own student, Johannes Kepler. Thus, a true science of astronomy was at least made possible, germinating the potential of a scientific revolution capable of superseding the hybrid astrology that preceded it through an immanent process of overcoming. If the conundrum of recurring crises and creeping secular stagnation currently afflicting the global economy is to be understood, then, a similarly immanent critique must be its basis. We must begin with an exploration of the best possible explanations currently given by economists—not just explanations of singular crises, but systematic accounts of crisis as such, which include both an explanation of long-run trends in the global economy and a compelling account of the phenomenon that they refer to as secular stagnation—as well as those explanations that may be somewhat less systematic but are nonetheless gaining popular purchase as more and more people see the prevailing cosmology of the economists to be not only insufficient, but also actively poised against their interests.[7]

[7] This is obviously a difficult claim to pin down, though it is arguably evident enough in the rise of global unrest (Endnotes 2020) and the rapid growth of avowedly "socialist" organizations such as the DSA in the United States, a stronghold of anticommunism (Godfrey 2020). This is upheld by polling data, where it has been shown to be generational in character, as millennials and members of generation Z have not only tended to have positive reactions to "socialism" but also that the share of these younger age cohorts with positive views of "capitalism" has markedly declined over the past decade, from 66% in 2010 to 51% in 2019 (Saad 2019). At the more general

This chapter will begin in Section 1 with a review of the work of Richard C. Koo and his theory of "balance sheet recessions," as well as the related model of global development mobilized by Koo but derived from W. Arthur Lewis and a theory of trade disequilibrium formulated by Koo and developed into a popular form by Matthew C. Klein and Michael Pettis. Despite these other contributions, Koo's work will be the central reference here because of its ability to synthesize numerous attempts to explain crises, to model long-run cycles in the economy and to account for secular stagnation in a systematic series of interrelated models. Moreover, it does so while retaining a strong fidelity to all the central tenets of economics. Thus, Koo's theory offers a single, expansive system that represents something like a best-possible model within the constraints of the prevailing economic cosmology. While Klein and Pettis develop a similar model of trade equilibrium in a particularly popular underconsumptionist direction, for instance, they are unable to explain crises (such as the Japanese crisis of 1990) which do not meet the standards of their model. Koo, on the other hand, provides a systematic theory that can account for individual crises in all the possible terms allowed by economists' framing of the economy as a mechanism for meeting consumer need: his theory provides room for theories of crisis caused by underconsumption, by overproduction (really, in this framework, more accurately described as underselling) and by simple disequilibrium in trade. Any of these can be induced "naturally" as part of his overall cyclical model of the economy or "artificially" through some incidental (environmental, geopolitical, demographic) constraint.

But, in line with his strong adherence to the basic tenets of economic cosmology, Koo does not give much attention to the technical questions of production. These are, however,

level of cultural production, a similar phenomenon has been documented by scholars such as Annie McClanahan, in her wide-ranging study of the cultural response to the Great Recession and its aftermath (McClanahan 2017).

integral to his ultimate appeal to "innovation" as the salvation of economies stuck in conditions of slow growth and increasing competition from lower-wage competitors. But what might be the limits of "innovation" itself? After reaching the limits of Koo's model, then, I will turn in Section 2 to the work of Robert J. Gordon for an equally compelling account of the cause of secular stagnation derived from very different fundamental presumptions. While Koo's theory takes all the fundamental schematic relationships of economics and extends them to their logical endpoint in an attempt to explain our current global condition, Gordon approaches the question through a more pragmatic methodology. He uses an extensive historical record of technical change within American industry to try to account for a secular trend toward slowing productivity, which leads to slowing growth overall. This means that Gordon, though still very much operating within the realm of conventional economics, tries to answer the question of secular stagnation by returning attention to that fundamental component in the equation of economists like Koo: innovation. The term is not simply taken as a given in Gordon's work, but is instead anatomized in great detail with an eye toward its structural potentials and constraints throughout history. If Koo can be characterized as taking macroeconomics all the way "up" by extending it to its logical limits, Gordon can be understood to be taking macroeconomics all the way "down" to the level of invention, which is understood to be the font of growth. The two theories are, therefore, complementary for my purposes, even if they are exclusive of one another.

Finally, some attention needs to be paid to the way in which theories attempting to account for current economic conditions have been taken up in a popular form. On the one hand, elements of the Koo and Gordon models are already beginning to be incorporated into mainstream discourse, but often in a partial way, as justifications for further austerity or new

waves of protectionism—things that the authors themselves would caution against, even if they seem to advocate for more limited versions of the same thing (more on this below). On the other, Klein and Pettis offer an alternative variation on Koo's same model of global trade imbalance that has proven somewhat more popular, since it roots the model in a Hobsonian theory of underconsumption driven by inequality. Because their solution is ostensibly about reducing inequality, it obviously carries more potential to be taken up at the popular level. But neither the work of Koo, Klein and Pettis, nor Gordon have attained the same celebrity status recently gained by Modern Monetary Theory (MMT). Section 3 therefore addresses MMT, albeit briefly. The context of its recent ascent is explained, the implications of its policies are explored and the relationship between its presumptions and the more general theories offered by theorists like Koo and Gordon are explicated.

The basic tenets of MMT are not new, nor were they explicitly formulated in response to any given crisis (though they've certainly been further developed by the experience of the past decade). The theory is a continuation of the Chartalist tradition and many of its major texts were released decades ago. Its recent rise to prominence has been due to the way that prominent left-liberals have taken up fundamental points from MMT in their attempt to revive expansive fiscal stimulus programs, as in the Green New Deal. The Green New Deal itself had been inspired by MMT, with Rep. Alexandria Ocasio-Cortez a prominent advocate the theory. Similarly, Bernie Sanders hired MMT theorist Stephanie Kelton as Chief Economist for the Democratic Minority Staff of the Senate Budget Committee. Kelton then went on to act as an advisor for the Sanders campaign in 2016, becoming probably the best-known MMT theorist in the process—assisted by her skill in translating the basic tenets of the theory into easily-understood prose, releasing a series of popular articles and a bestselling book. The basic utility of MMT for left-liberal politics

is its proposal of a specific mechanism for expansive fiscal stimulus, mobilized by the Treasury, which does not necessarily require immediate sources of funding in the form of raised taxes or increased debt.

Addressing some of the claims of MMT will be useful, then, to close the chapter, since elements of the theory are already beginning to enter into popular discourse and will likely become a key component of American political debate over the next five to ten years. At the same time, MMT has largely been taken up because of its perceived ability to offer a toolkit for policy makers to respond to crisis and even chart a way out of secular stagnation as such. The experience of the pandemic and its accompanying recession is only strengthening this case. But if Koo offers what might be thought of as a somewhat rarefied, theoretical account of crisis and Gordon offers a technical one, then MMT offers what might be thought of as a technocratic alternative emphasizing political administration above any structural features. In many ways, it is the truest embodiment of Keynesianism in the general sense used by Mann, encapsulating its pragmatism, its faith in the centrality of an enlightened state and its fundamental fear of popular disorder. Equally important for my purposes, however, is the fact that MMT offers no real theory of crisis as such and only vaguely accounts for continued stagnation as a signal of insufficient stimulus. Overall, then, this chapter will move from the more purely economic models of Koo to the technical terrain of Gordon and finally to the popular political technocracy of MMT, all while pointing out the limits of each and their implication of deeper dynamics better accounted for not by economists, but by the supersession of economics as such by a more fundamental science of social production.

# Section 1 – Koo

**The Dao of the Economists**

Richard Koo, former Federal Reserve economist and current chief economist at Japan's Nomura research institute, did not initially formulate his theory of "balance sheet recession" as an attempt to dig down to the fundamental laws of macroeconomics or crisis as such. Instead, the theory developed as a pragmatic effort to understand one specific crisis: the Japanese Recession and the lost decades of stagnant growth that followed. This was a practical choice, since Koo had been employed at Japan's preeminent economic research facility during outbreak of the Japanese crisis. In this capacity he was tasked with understanding what, exactly, was occurring and how the collapse might be stopped or at least slowed by policymakers. Similarly, in this position he had unparalleled access not only to Japan's official business statistics but also to the institute's propriety analytic measures, as well as more confidential information about the state of corporate balance sheets and the attitudes of major investors and executives regarding debt and profitability at the level of the individual firm. Over the subsequent decades, Koo was not only able to formulate a theory of the Japanese crisis, but also came to realize that his findings led to more general insights about the nature of numerous crises, including the Great Depression and, later the Great Recession.

While the initial publication of the theory, in 2003, had focused on the Japanese experience (Koo 2003), it was then expanded into a more expansive claim to have found the "holy grail of macroeconomics" (Koo 2009) in its ability to explain the Great Depression itself. Koo's major work detailing the theory was released in 2008, right as the Great Recession was unfolding. Its main points of reference, in that initial publication, were still the Japanese

Recession and the Great Depression, with one short chapter devoted to the then-unfolding subprime collapse. But, as the depth of the Great Recession became apparent, Koo and his publisher rushed through a revised and expanded edition released in 2009, which included new chapters on the Great Recession and reference to it throughout—it was this version, produced iteratively in the years spanning the Japanese collapse and the Great Recession, which would become the definitive version of the theory. Nonetheless, Koo's interpretation was only popularized in 2011, when his paper, "The world in balance sheet recession: causes, cure and politics" began to go viral online, receiving praise in a New York Times piece discussing the subsequent European Debt Crisis (Thomas 2011). The theory has essentially remained unchanged in the intervening years, with one subsequent publication exploring policy responses and their potential pitfalls (Koo 2014) and another (Koo 2018) turning from specific crises to refine the more general implications within macroeconomics as such, linking his core concepts to a broader schema of developmental stages defined by trends in demography and employment.

While many economists offer only limited explanations for individual crises, Koo's work is unique in its attempt to synthesize mainstream and heterodox explanations into a single systematic model and to link this model to both global dynamics and to historical trends in general economic development. It is not an exaggeration to characterize Koo's work as an attempt to formulate a "general theory" of macroeconomics, similar in spirit to the project pursued by Keynes himself—albeit without nearly as much theoretical depth. This also makes it one of the few attempts (alongside Piketty, perhaps) to account for "secular stagnation" in a systematic fashion that captures long-run trends in economic development. Unlike Piketty, however, Koo attempts to do so without simply returning to well-trod arguments about effective demand and income inequality—in other words, Koo does not attempt a return to Keynes.

Instead, he synthesizes elements from both the neoclassical orthodoxy and the broadly defined "Keynesian" schools into a more pragmatic model capable of explaining, at least in its own terms, why each has seemed correct in different conditions. In the end, Koo's model remains a purely economic one, bound to the ultimate justification of society as it presently exists. But if, in order to understand the real dynamics driving a long-term, systemic breakdown and relating crisis to the production of new territorial complexes, it is first essential to understand the way that these broad social relations generate their own distorted, ideological image—as the folk science of economics—then Koo's work offers one of the strongest possible examples of this folk-scientific understanding today, taking its logic to the necessary extremes when faced with the reality of recurrent crises undergirded by decades of "secular stagnation," and thereby demonstrating its own theoretical limits.

The heart of Koo's system is his model, within an abstracted, isolated domestic economy, of a single macroeconomic cycle that determines all other trends and is itself defined by the aggregate balance sheet position of all private firms across that economy. The microeconomic basis of his macroeconomic model, meanwhile, is not the *homo economicus* rational consumer of most economics textbooks, nor its extrapolation into the individual "entrepreneur" or capitalist, but instead the observed behavior of the capitalist firm in the real world. Such firms are understood to be driven to defend their collective investors' interests, which ideally means profitable expansion but, at minimum, usually amounts to the self-preservation of the firm (especially through hard times) until profitability can be restored or the firm can be profitably liquidated in favor of better ventures. The prospects of the firm, meanwhile, can be directly measured by its balance sheet. Koo links this microeconomic unit to the larger macroeconomic picture by aggregating information from these balance sheet positions into more general data on

private sector liabilities, debt repayment and profitability. Later, he links this domestic model to two somewhat more simple international models, which account for long-term historical trends in uneven development and global dynamics in trade and finance, respectively. Nonetheless, nothing in these international models changes the basic character of his central model, as applied to a single economy—they merely give some idea of the more general historical trend and the possible interactions between many such economies, which are effectively delimited in reality by the separation of monetary systems and the limits of labor mobility. This is an important point however, because one unstated assumption throughout his work is that these real separations (in the form of currency systems and labor markets) are not fundamentally necessary. Were one to imagine a situation in which there was a single global currency, no national restrictions on mobility, and a single global fiscal authority, then Koo's basic model would still apply, only now to the entire global economy directly, despite uneven development. In fact, as will be seen below, Koo often portrays such limits as a hindrance to the potential equilibrium that lies at the heart of his system.

Clothed in language befitting the folk-scientific character of such a project, the core idea of this model is that economies will pass through what he calls *yin* and *yang* cycles, which are defined by debt levels and the subsequent behavior of firms in response to the changing composition of their balance sheets. As Koo explains: "In a *yang* economy, private-sector balance sheets are healthy and companies seek to maximize profits" (Koo 2009, p.161). In other words, a *yang* cycle is when firms tend to follow textbook presumptions, always innovating, expanding and maximizing profit, and when policy toward them should also therefore follow the textbook prescriptions of neoclassical economics: "in this world, the smaller and less intrusive government is, the better it is for the economy" (ibid). This also means that *yang* cycles are

primarily managed through monetary policy—i.e. through the tweaking of interest rates and money supply by the central bank, as has long been the mainstream monetarist consensus for the proper role of policymakers in the economy (for the earliest formulation of the theory, see: Friedman and Schwarz 1963). This is because, in a *yang* period, there will be strong demand for loans from the private sector, so the interest rate will exert its usual effect. At the same time, a vigorous private sector will, in Koo's account, ultimately lead to overconfidence, and this overconfidence is the source of the resulting asset bubble (p.157).

The basic idea so far is uncontroversial: periods of rapid expansion will tend to end in some sort of over-reach, which then leads to the immediate "crash" as asset bubbles pop. In all these terms, Koo has largely just repeated the questionable mainstream account of crises, complete with the non-explanation of private sector "vigor" as the supposed cause of the subsequent crash. This makes sense here, since he is, after all, describing the *yang* cycle, during which presumptions from textbook economics are more or less accurate and textbook policy responses tend to work. But his account gets interesting at precisely this point, because the initial crash is merely the inflection point between the *yang* and *yin* cycles (often triggered by a tightening of monetary policy), and it is the *yin* cycle that characterizes the long crisis which plagued Japan for decades and now defines conditions in United States, Europe, and increasingly the world at large. Koo's cycles are by no means defined by gradual, even transition over time. Though, consistent with the Daoist overtones, elements of the *yin* inhere in the *yang* cycle already (and vice versa), the crash and its immediate aftermath tend to act as relatively strong breaks, during which one rapidly inverts into the other.

Once a *yin* cycle begins, essentially all the terms of the *yang* period are reversed. Rather than maximizing profit, firms are faced with balance sheets stuffed full of deflating assets

purchased on credit during the upswing, leaving them with high liabilities. It is here that mainstream accounts begin to break down and Koo must instead turn to various heterodox explanations, many of which trace back to the birth of modern quantitative economics in the early 20th-century, providing the empirical foundation from which the theories of Schumpeter and Keynes would take shape. Koo's fundamental observation here about the nature of the *yin* cycle is, in essence, a restatement of the "debt deflation" concept first advanced by Irving Fisher during the Great Depression (Fisher 1933) and subsequently revived by both post-Keynesians and mainstream economists such as Ben Bernanke (1995)—but now with the causality reversed (i.e. Fisher argues that debt deflation and associated panic selling causes the crisis, Koo sees it as an outcome). Koo argues that the rising share of liabilities in balance sheets across the private sector then leads to a general condition in which firms are no longer seeking to maximize profits but are instead driven primarily to minimize debt. The entire economy is thereby pushed into a deflationary spiral (Koo 2008, p.93) and traditional monetary policy stops working because demand for loans from the private sector dries up—meaning that no matter how low the interest rate is set it will not help to stimulate further productive expansion (ibid, Chapter 4).

The result is a situation in which mainstream monetarist policies must be rejected in favor of fiscal solutions: the government "must borrow and spend the savings generated by the private sector, so that household savings and corporate debt repayments can be returned to the income stream" (ibid, pp.161-162). Koo recommends direct capital injections into companies, even up to the point of nationalization, alongside things like government-mandated purchases to revitalize cash flow as well as other measures traditionally associated with "Keynesianism" such as "the construction of massive public works" (ibid, p 119). He argues that these factors "arrested the deflationary spiral of the Great Depression, and paved the way for recovery," also noting that

they "were of an entirely different nature" than the "liquidity injections advocated by Friedman and Bernanke," who represent two dimensions of the mainstream orthodoxy (ibid, pp.119-120). Ultimately, though, Koo doesn't see these as permanent solutions, since proper responses to the *yin* cycle regenerate conditions for a *yang* cycle—albeit, he argues, after risk-averse investors who lived through the balance sheet recession either die off or regain their "vigor."

Another inflection point is thereby reached once debt has been paid down and firms, in general, begin again to emphasize profit maximization. One key point of reference here, for Koo, is the successful emergence of the global economy from the Great Depression. Though the fiscal policies of many countries were only halting and incomplete over the course of the 1930s—and Koo advises that, though they could have been far more aggressive, these policies staved off an even worse downturn—he ultimately argues that only wartime mobilization provided the necessary fiscal stimulus needed to diminish liabilities and restore "vigor" to the private sector. His argument, however, makes no mention of the nation-sized wastelands of old fixed capital investment reduced to rubble in the process. But this is a significant fact if one considers the material form taken by the liabilities that weigh so heavily on the balance sheets of debt-laden firms in the midst of a depression. After the war is over, Koo then seems to portray the postwar situation as a period of transition, in which *yang* forces build up again under the shelter of government spending and guaranteed demand. Once the *yang* cycle properly begins, though, the fiscal policies that helped to pull the economy out of the *yin* cycle will start to crowd out private sector investment, threatening a further stalling of growth. Koo argues that there were no major balance sheet recessions between the end of the Great Depression and the beginning of the Japanese crisis in 1990 (ibid, p. 173). Thus, though it is not stated explicitly, we can presume that the crisis of the 1970s was, in Koo's reckoning, not a balance sheet recession but instead a crisis

induced at the opposite inflection point, when *yin* was cycling into *yang* and the Keynesian fiscal policies implemented in years of war and depression began to dampen the profits of the private sector and trigger inflationary overheating in the economy at large. It is at this point, Koo recommends, that fiscal policies must be rolled back, nationalization must be replaced by privatization, and traditional monetarist levers of policy intervention (such as interest rate manipulation) regain their efficacy.

Koo's theory is significant, then, not so much for its novel understanding of crises as for its ability to integrate both heterodox understandings of crisis normally associated with various "Keynesian" or even "socialist" thinkers and mainstream monetarist approaches to the question into a single systematic, cyclical model. In many ways, Koo is offering a more rudimentary version of the Schumpeterian business cycle, with none of Schumpeter's more intricate theorization of numerous, nested cycles driven by different structural dynamics—nor his own conclusion of a secular trend leading toward the gradual (and distinctly non-revolutionary) breakdown of the system over time. Koo does acknowledge the existence of "usual cyclical or inventory-driven business cycles" which persist beneath the larger cycle he identifies, leading to "ordinary recessions" that occur within each period (ibid, p. 162), but he leaves these cycles untheorized, likely endorsing the truncated notion of the business cycle accepted in textbook economics (which is basically a variant of the Kitchin inventory cycle). Nonetheless, it is Koo's work, not Schumpeter's, which has begun to gain influence today, since it directly addresses two of the largest, most recent crises that caught most mainstream economists off guard (and where those who saw a crisis building, such as Krugman, consistently misidentified its source, as documented in Tooze 2018). More importantly, Koo's basic presumptions are a systematized version of numerous other mainstream and heterodox approaches to the question of crisis and act

as a cogent summary of the increasingly popular presumption that, if some level of equilibrium is possible in the capitalist economy, it is not an equilibrium driven by the "natural" movements of the market but instead an equilibrium tended by the technocratic hand of the state, which must use fiscal and financial interventions in turn to maintain the baseline conditions for accumulation.

**Uneven Development**

One of the weakest points in Koo's 2009 book, which laid out the full model of balance sheet theory, was its nearly exclusive focus on national economies. Although he acknowledges the role of foreign investment, global financial markets and some of the international pressures put on fiscal and monetary authorities, his initial theory provides no adequate model for these phenomena. This might be fine if it were a model of the entire global economy, but it very clearly is not. Instead, crises are treated as more or less nationally contained affairs, unproblematically linked to a given pool of sitting capital or potential capital, access to which is determined by their respective lending bodies and national governments (or, in the case of the EU, their respective monetary union). Even if international capital and trade relationships clearly play some role here, Koo leaves them untheorized in the main body of his model, relegating the questions to a subsidiary chapter on globalization which focuses almost exclusively on the question of trade deficits—and mostly in an effort to show that the trade deficit and fiscal deficit (which Koo argues must be expanded) do not have a simple linear relationship. While the placement and relatively narrow focus of such a chapter might seem coincidental, this emphasis on trade deficits is actually an important, albeit underacknowledged, outcome of Koo's model.

The connection between the largely national model of balance sheet theory and Koo's understanding of globalization is clarified in a later book (Koo 2018), where he extends the model into a general schema of global borrowing/investing and lending/saving, while also orienting it within a larger framework of developmental stages. In order to extend the model, he uses his notion of a national aggregate balance sheet position identify four potential combinations of activity between borrowers/investors and lenders/savers within given national economies:

> (1) both lenders and borrowers are present in sufficient numbers, (2) there are borrowers but not enough lenders even at high interest rates, (3) there are lenders but not enough borrowers even at low interest rates, (4) both lenders and borrowers are absent (Koo 2018, p.13).

The first two options are covered by textbook presumptions, corresponding to the *yang* cycle identified in his earlier work. The final two are not considered within textbook economics and correspond to what he refers to as the *yin* cycle in his earlier work.[8] But he goes further than this, equating the four states outlined above to actual national economies, arguing that "the private sectors in virtually all major advanced nations have been increasing savings or paying down debt since 2008 in spite of record low interest rates" (ibid, p.21), placing them firmly in categories (3) and (4) above. Most are in state (3), since central banks are actively pumping liquidity into the system and interest rates hover near zero, but borrowers are largely unwilling to borrow, meaning that investment remains low and all the liquidity pumped into the system "remains

[8] It is worth noting here that, by the release of his 2018 work, Koo had largely dropped the *yin* and *yang* terminology, and refers simply to the conditions of an economy in balance sheet recession as "The other half of macro-economics." But the use of the terms is, in my consideration, indicative of the more general folk-scientific character of the project. Thus, the discursive flourish verging on the cosmological is, in fact, quite fitting.

stuck in the financial sector" (ibid, p.26). But many "Emerging Market" economies, among which China is included, are still within states (1) and (2).

There are a few key differences that arise between Koo's earlier theory, which largely focused on the aggregate position of private sector balance sheets within lone national economies, and his later one, which focuses on the differential position of national economies within the global capitalist system. One key change is the shift from focusing specifically on private sector balance sheets (alongside public sector fiscal and financial policy) to a more general concept of national balance sheets defined by flow of funds data. Such data, often recorded by a country's central bank, measure the balance of assets and liabilities for government, the corporate sector (subdivided into financial and non-financial), households and the foreign-owned segments of a national economy. They are particularly useful for tracking "the growth of debt for each sector; changes in the in the sources of credit […] and the development of new financial instruments for providing credit" (Teplin 2001) and for Koo in particular, the importance lies in the data's ability to "indicate whether a particular sector of an economy is a net supplier or borrower of funds" (Koo 2018, pp. 8-9). It is from these data that he concludes whether the private sector is in financial surplus or deficit, but with one caveat: whereas his previous work focused on the balance sheets of corporations, this later one considers both households and corporations in its broader definition of the "private sector." This, however, opens the door to a return of classic underconsumption theories founded on the decline in effective demand within households, despite his avowed rejection (at least in his 2009 book) of this very starting point—which he attributes to Keynes, and is best exemplified at the popular level today by the work of Piketty. The consequences of this will be explored below. But it remains to be shown what results Koo himself draws from these data.

In addition to this schema of lending and borrowing, Koo also frames global differentiation between national economies in terms of their level of development, defined almost exclusively in terms of a country's relationship to the Lewis Turning Point. This turning point is the crux of the dual-sector model of development formulated by W. Arthur Lewis in the 1950s (for its original version, see: Lewis 1954), where development was treated in terms of a relationship between an agricultural "subsistence" and industrial "capitalist" sector in an economy, with the developmental arc defined by a shift from the former to the latter.[9] Early on, individual economies find themselves[10] with a glut of cheap, low-productivity surplus labor located in the agricultural sphere which is in the process of shifting into the industrial sphere—though this is treated schematically, it of course also entails mass migration, rapid urbanization and all kinds of associated cultural upheaval in the real world. Industry is thereby capable of rapid growth even if its productivity may be relatively low since it has access to a ready pool of cheap labor. But a turning point is eventually reached once enough of this surplus rural labor has been absorbed, draining the pool of reserve labor and driving an increase in wages. Meanwhile, the process has led to general industrialization and urbanization, accompanied by reduced birthrates. This turning point is therefore also the juncture at which industry sees a more rapid shift from labor-intensive to capital-intensive investment, and in which the general population pyramid begins to invert. Unique conditions of demography (such as those created by China's

[9] Lewis' theory was formulated in an era where many countries still had a large rural population dependent on non-market mechanisms for subsistence. But even as literal non-market subsistence declined in its prominence, numerous countries retained the rural surplus labor that had defined the subsistence sphere—albeit now within a realm of informal marketization, as an army of reserve labor. So the key terms here are not "subsistence" and "capitalist" but instead pairs such as low-productivity/high-productivity, rural/urban, surplus/non-surplus or agricultural/industrial. These are the divides that become more important in the theory's later applications, particularly (as we will see) in the Chinese case.

[10] It must be noted here that this notion is itself a dangerous oversimplification of the violent means by which such conditions are originally generated. In particular, it ignores the preliminary requirement that ruralites be effectively dispossessed of common land. Short of this dispossession, there is no guarantee that population will "naturally" move from the agricultural to the industrial sphere.

one-child policy) or agricultural-sector productivity (as in postwar Japan) can enhance or attenuate the trend, but only a general collapse in capital accumulation is capable of reversing it (for an overview of the model's history, see: Gollin 2014).

The Lewis Model is best understood in conjunction with the Three Sector Model, also called the Clark-Fisher Model (originally formulated in Fisher 1935, Clark 1940 and Fourastié 1949), which represents the prevailing orthodoxy on the structural shifts that occur in an economy in the course of development. In this model, the entire process of development is simplified to a series of interwoven waves formed by the changing shares of different sectors in total employment. The simplest form of the model is visualized in Figure 1-1, although service-sector employment is often acknowledged to compose a relatively higher share from the start and/or to move upward more or less alongside the industrial sector. The basic pattern is that an initially high share of labor committed to agricultural subsistence (the primary sector) is overtaken by an ascendant wave of industrial employment (secondary) and then industry by services (tertiary).

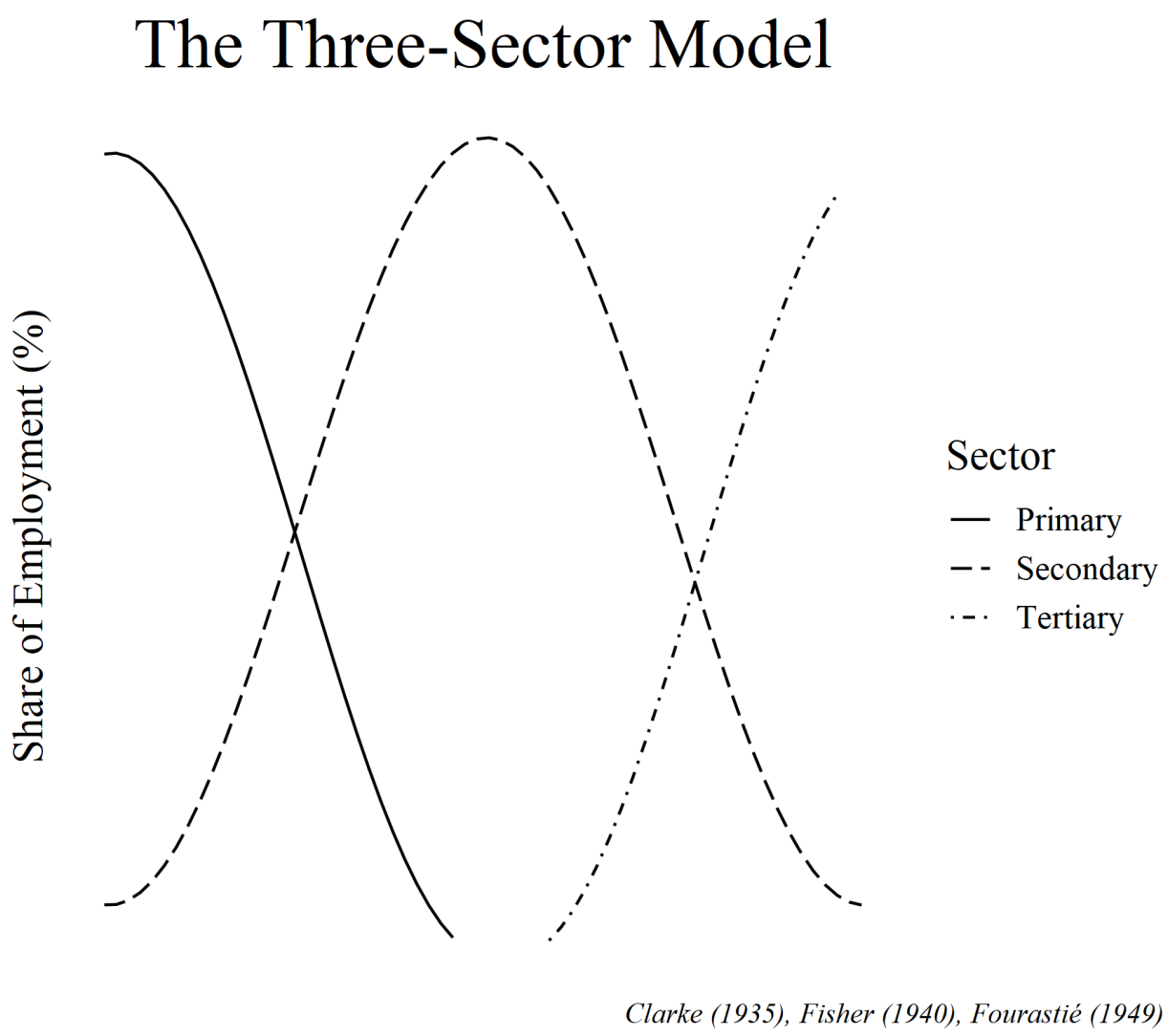



*Figure 1-1*

But these structural shifts imply major social changes. After all, the first is essentially the transition to capitalist production, with the rising wave of industrial employment preceded by a millennia-long plateau in which most people lived in rural subsistence economies. This is the portion that is the focus of the Lewis Model. Similarly, the Three Sector Model often implies a point of developmental "maturity" defined by the final ascent of "post-industrial" employment in services, often portrayed as another plateau. These are the conditions that arise after the Lewis Turning Point is reached, though they aren't captured in the Lewis Model itself. Koo focuses on

the Lewis Model, but is also clearly informed by the orthodox presumptions of the Three Sector Model, defining the rise of service employment as a marker of a country's ascent into "mature" conditions.

Koo simplifies the Lewis model to fit into his schema of borrower and lender availability, identifying three stages in the general industrialization process, each of which is defined by its position relative to the Lewis Turning Point. Every stage also sees different prevailing conditions of borrowing and lending. These three stages are:

> urbanizing economies, which have yet to reach the Lewis Turning Point (LTP), maturing economies, which have already passed the LTP, and pursued economies, where the return on capital is higher abroad than at home (Koo 2018, p.54).

The first stage will see strong investment and weak consumption, leading to slow wage growth and rising inequality; the second will see strong investment and strong consumption simultaneously, allowing for the linked growth of productivity and wages, resulting in a general decline in inequality; and the third will see lowered investment (due to decreasing investment opportunities) and more selective consumption, resulting again in slow wage growth and rising inequality but for entirely different reasons than in stage one (ibid, Figure 3.1, p.55).

The stages are also divided according to how much the international context weighs on them. Koo's first and second stages are essentially modelled entirely on domestic behavior. In the first stage, prior to reaching the Lewis Turning Point, growth tends to be accompanied by increasing income inequality, since the surplus in labor supply allows for rapid investment and increased profits for capitalists, while exerting no upward pressure on wage rates for workers (ibid, p.56). In this stage, investment might originate with foreign interests or it might not—

neither would alter the fundamental dynamics described. The second stage (i.e. "maturing") economies see rising wages, which allows for increased consumption. Rather than squeezing investment, however, "the explosive increase in the purchasing power of ordinary citizens means that most businesses are able to increase profits simply by expanding existing productive capacity" (ibid, p. 57). They are also driven in this period to increase productivity in order to pay higher wages, even if the general skill level of workers might remain unchanged. These are the conditions that define "the *golden era* of economic growth" (ibid, p.58, italics in original), with the postwar US being the prototypical example. But again, Koo gives no mention of international competition or foreign investment here, and in this second stage seems to imply a specifically domestic capitalist class serving a domestic working class.

It is only in the third stage, that of the "pursued" economy, that Koo begins to refer openly to international factors in the national sequence of development. In fact, international factors here become so important that they define the very name of the stage, since "pursued" specifically refers to pursuit by industrial competitor nations. In this stage,

> it becomes far more challenging for businesses to find attractive investment opportunities at home because it often makes more sense for them to buy directly from the 'chaser' or to invest in that country themselves (ibid, p.60)

The decline in investment not only means that businesses have no incentive to expand existing domestic capacity, but also that "productivity gains at home from investment in productivity-enhancing equipment slow significantly" (ibid, p.61). Here Koo cites well-known data from the US Bureau of Labor Statistics showing the general decline in American productivity growth, which averaged 3 percent between 1952 and 1973, then 2.1 percent between 1974 and 2007, declining thereafter to its most recent trough of a mere 1.2 percent following the Great Recession

(averaged from 2008-2015 in ibid, with data derived from: Fischer 2016). For his model, the significance is that lowered domestic productivity and lowered domestic investment (meaning lower growth in general) are induced by lowered rates of return *relative to international industrial competitors*. This is the entry-point for what is essentially an underconsumptionist theory of secular stagnation in the high-income countries (and, in fact, across the globe), driven by international industrial competition.

**Global Imbalance**

When the industrial challenge against a leading economy becomes significant, it takes on the form of a trade war. Koo's frequent example is the case of Japanese competition with the US in the 1970s-1980s, though today Japan itself is a "pursued" economy and China has become the ascendant competitor. This competition is initiated, Koo argues, not by the mere existence of a wage gap between nations (which had always been the case) but instead by the fact that businesses in a competitor nation begin to reach and surpass "the technological and marketing sophistication" of the pursued nation *while also* benefiting from lower wages, thus enabling a higher rate of return (Koo 2018, p.60). Again, Koo's model here gestures in the direction of critical communist theories of crisis (Mattick 2011) and imperialism (Smith 2016, Norfield 2016), all while seemingly unaware of the fact. But this lack of awareness leads to a general depoliticization of the theory—still bound within the prison of "economic" cosmology as an inherently ideological discipline and therefore, in the last instance, obligated to uphold the basic tenets of that elite folk science—and this depoliticization means that, at some point, Koo's model must reach a point in which it distorts the dynamics it seeks to describe.

One result is that small distortions and minor mysticism plague Koo's work throughout. These are most obvious, of course, where he endorses certain uncritical notions inherited from mainstream economics, attributing the build-up of speculative bubbles to such magical factors as the "vigor" and "overconfidence" of private investors—similar, we might presume, to the "animal spirits" of Keynes—or imagining that private sector demand will stay low after the crisis has cleared the market simply because the investors who experienced the bust will remain irrationally debt-averse until their deaths, meaning that an entire generation must turnover before "vigor" is restored. This is all, of course, fanciful nonsense, and it's clear that structural pressures on investors will have far more influence, on average, than individual psychological predilections (or the influence of culture, religion or supposed political ideology, for that matter). Nonetheless, it will be most helpful to set aside these smaller examples of magical thinking for now, since they do not fundamentally affect the mechanics of Koo's model. Instead, the focus should be on the large-scale distortions that become evident when peering back at the model from its outer limits. In other words: what are the fundamental, *unstated*, magical presumptions that define the basic logic of the system?

This more substantial distortion of real economic dynamics becomes most apparent when Koo turns to questions of globalization, trade and industrial competition. Here, Koo pivots from his initial foundation using the aggregate balance sheets of private firms within the economy to a much shakier one utilizing the national accounts of individual countries within the global economy. In so doing, the microeconomic basis of his theory make a subtle shift, moving from the aggregate balance sheets of private corporations to the current accounts of entire countries, which includes their consumer base and thus returns attention to classical Keynesian questions of effective demand on the part of labor. Even if Koo openly denies the classic Keynesian causality

implied here—whereby lack of consumer demand from workers with suppressed wages ultimately squeezes profits and thereby leads to a decline in investment—his international model seems to revive these implications, at least in part. This then allows other theorists to draw more explicitly underconsumptionist conclusions from a similar schema of imbalances in the global structure of trade and finance, as we will see below. More important, however, is the way in which this pivot forces the model away from questions of production (despites its initial proximity to these very questions), placing it almost entirely within the sphere of distribution and consumption. If the folk-scientific character of economic thinking is best defined by its adherence to the core belief that the economy is primarily a distributional mechanism for serving human need, it seems that this marks the point at which Koo confronted the reality that he was approaching heterodox territory. Rather than pursing this line of inquiry further—which would have led him into the "hidden abode of production"—he instead turns back to orthodox concerns of distribution, consumption and models of equilibrium.

This problem is clear as early as Koo's 2009 work, where the few places focusing on global dynamics (Chapter 6 and parts of Chapter 8) place repeated emphasis on the danger of high trade deficits, arguing that "we have reached the point where something must be done about global trade imbalances" (Koo 2009, p.194). No significant changes seem to have been made to the core of his argument in the intervening years, as much of the material from these chapters is reproduced, nearly verbatim, in his 2018 book, where the only real addendum is to link these concepts to his schema of development and, in particular, the conundrum of the "pursued economy." Nowhere does he give a particularly clear view of what, exactly, is to be done about these problems, however, emphasizing instead that the scale of contemporary cross-border capital flows places the global economy in uncharted territory, leading to uncertainty for anyone

trying to prescribe a solution to the imbalance. The problem, however, remains consistent in his interpretation: trade imbalances caused by cross-border capital flows, which only grow worse when countries reach the "pursued" phase of their development. These imbalances exacerbate balance sheet recessions and create structural conditions that all but guarantee slowed growth rates, lowered productivity, stagnant real wages, declining rates of return and rising inequality in a "pursued" economy. Moreover, each of these factors tends to be procyclical with relation to the others.

Throughout, Koo emphasizes the negative effects of "financial capitalism," which is his shorthand for globally deregulated cross-border capital flows and the investors behind them. At first, though, the exact role played by financiers is not entirely clear, since Koo begins not with finance, but with trends in trade and growth. To start, he demonstrates that current account deficits are not, in fact, after-effects of fiscal decisions. This is important precisely because the accepted narrative about such imbalances often focuses precisely on this fiscal aspect, treating the issue as if it were an ancillary outcome of national budgetary policies. Against this presumption, Koo points to research from the Fed that demonstrates the two to be only loosely linked, with "four-fifths of the trade deficit […] determined by factors other than the fiscal deficit" (ibid. p.196). The most important of these "other factors," for Koo, are: "the growth-rate differential between the U.S. and other countries, and the U.S.-dollar exchange rate" (ibid). But of these other factors, not all are equally subject to modulation. While Koo acknowledges that dips in the US growth rate could create a smaller appetite for imports, "inducing a semi-permanent recession solely for the purpose of reducing the trade deficit would be politically unfeasible" (ibid, p. 167) to say the least. Instead, Koo suggests an adjustment of the exchange rate to weaken the dollar and strengthen the currencies of countries with a trade surplus.

For now, we'll put aside the fact that this suggestion is effectively the mirror-image of the one he disavowed with regard to growth, since it would likely trigger a similar "semi-permanent recession" in other countries whose competitiveness would be hurt by such a revaluation.[11] Instead, it is first necessary to understand why the exchange rate is so important in the first place. In the textbook economics narrative, from which Koo draws, the foreign exchange rate should automatically adjust in a countercyclical fashion with regard to trade imbalances—in other words, a country running a trade surplus (exporting more than it imports) will see its currency inflate, causing both the prices of its exports to rise (on the global market) and the return on domestic investment to decline (as priced in local currency *or* global dollars relative to international competitors), which will ultimately lead to less demand for those exports and lower foreign investment, diminishing the current account surplus (as exports decline and imports likely rise) and the paired financial account deficit (since inbound FDI also declines). The reason that this textbook equilibrium does not occur today, Koo argues, is to be found in the role played by cross-border flows of capital—but specifically *portfolio capital* (or, more loosely, "financial capital"), which is focused on the purchase of financial assets in the host country, rather than investment capital, which is associated with the purchase or establishment of a business.[12]

[11] This is a glaring fact, however, given that just such a trade-balancing exchange rate devaluation of the USD via the Plaza Accord in 1985 essentially triggered the speculative sequence that would end in the very crisis that Koo himself built his work around.

[12] The concept of portfolio or financial capital is often a bit difficult to pin down. Often, it actually includes the purchase of shares in companies. One good example of this ambiguity is the rising importance of stock buybacks, wherein companies use their profits to purchase their own shares on the stock market, thereby inflating the stock price, rather than using profits for new investments in R&D, expansions of existing production lines or opening new plants. The same is true of the rising share of mergers and acquisitions in global investment portfolios and the dominance of a relatively small number of major asset managers. Overall, it marks a shift in finance itself away from lending (i.e. providing credit ultimately used for productive activity) and toward extracting financial rents based on asset ownership and management and sustained by bubbling asset prices. The phenomenon is documented in more detail by Gibadullina 2020.

The essential problem, as he explains it, is that "capital flows are producing destabilizing trade flows" and that these movements of capital are "not only amplifying global trade imbalances, but also making it difficult for individual countries to administer monetary policy" (ibid). He demonstrates this with an overview of the procyclical tendencies of the carry-trade, which is (in its simplest form) when a portfolio investor borrows in a low-interest-rate currency (say, yen), converting that sum into a higher-interest-rate currency (say, euros) and then buying higher-return assets in that second currency in order to turn a profit (for a more detailed explanation of the mechanics of these trades and a more detailed version of the same general argument used by Koo, see: Lee et. al. 2019). This behavior is only possible in open capital markets, of course, since it requires the more-or-less frictionless purchase and movement of financial assets between countries. Ultimately, this creates a situation in which "central banks setting low interest rates end up stimulating investment outside their borders" and "those setting higher rates end up attracting a disproportionate share of global savings" (ibid, p.202)—though here we might emphasize that interest rates need to be explicitly contextualized relative to exchange rates and associated asset prices as well, as Koo points out elsewhere, since this provides constraints on the central bank's ability to manage the problem. Ultimately, the relevant point is that these pro-cyclical tendencies further limit individual countries' abilities to use monetary policy to emerge from crises, and highlights the active role played by exchange rates and asset price inflation, which here sit behind modifications in the more general interest rate.

But a more important result of this is that currency inflation is thereby no longer able to act in a countercyclical fashion with respect to trade imbalances. On one level, demand for a country's exports still exerts the same influence it did before, and not all capital flows take on the character of the carry-trade, with Koo arguing that an inward flow of investment capital, instead

of portfolio capital, "will tend to push up the exchange rates of increasingly competitive recipient countries while depressing the exchange rate of the increasingly uncompetitive home countries" (Koo 2018, p.251), thus exerting an equilibrium effect on trade flows. But in the case of portfolio capital, inward investment is not driven by the relative competitiveness of industry. It is instead attracted by relatively higher (or at least rising) currency values and associated high returns on financial assets, which further drives up the price of the country's currency, thus exerting a destabilizing effect on trade flows. Much of this is a relatively recent phenomenon, Koo argues, since capital markets have only been liberalized globally since roughly the 1980s, whereas trade was liberalized much earlier.[13] That fact is confirmed by numerous other commentators writing about "financialization" in a broad sense, but often such claims are made without much measurement.

The quantitative case for the qualitative shift that Koo argues for in this period is, however, also confirmed by Michael J. Howell, the founder of the financial advisory firm CrossBorder Capital, which specializes in the study of, as might be expected, cross-border capital flows. Howell makes use of international balance sheet data to show the increasing mobility and absolute growth in the general pool of global liquidity in the past forty years, which

[13] While the partial capital controls that remain in place in China today have helped to insulate the country from some of the most volatile results of this, it is simply not correct to claim that Chinese capital markets have not been liberalized. The reality is that they have been *domestically* liberalized for decades and this fact is absolutely essential to understanding the rise of various asset bubbles within China. Meanwhile, *de facto* international inflows of capital have often exceeded the official level of liberalization—initially by passing through Hong Kong, which has become less and less necessary over the years (with Shanghai and even Shenzhen ascending in importance as financial hubs). It is impossible to understand the demise of CITIC in the Asian Financial Crisis, for instance, without some recognition that Chinese capital markets did have exposure to international financial volatility. Meanwhile, even in the realm of "state-owned industry," firms have been not only open to international capital but in many ways dependent on it for the past two decades. Almost all the largest central-state-owned enterprises were redesigned into modern corporations with the assistance of Western financial interests and then recapitalized on global stock markets, with massive investments made by these same financial interests. The fact that the state retains majority control through at least 51% share (itself appraised on the market) is not the same as real "closure" with regard to foreign capital inflows (Walter and Howie 2010).

has diverged far above global savings (in absolute terms and measured as a share of global GDP). He also confirms Koo's claim about the outsized importance of portfolio capital within the body of global liquidity, pointing out that "the bulk of cross-border capital movements are speculative portfolio flows and bank financing flows, and not foreign direct investments (FDI)" (Howell 2020, p.34), and also confirming the declining importance of Fed interest rates, which are "an inadequate monetary policy tool" because they are "not the main channel of monetary transmission" (ibid, p.28). The same results are obtained in a different fashion by Gibadullina (2020), who identifies a general decline in the share of loans and a rise in the share of debt securities, corporate equities and mutual fund shares on the balance sheets of the world's largest financial firms (Gibadullina 2020, p.114-115). The result has been that,

> Faced with a climate of declining interest rates and new opportunities to maximize investment returns outside of lending, US finance has largely abandoned its function as a provider of interest-bearing capital, choosing instead to chase profit opportunities in the equity markets. (ibid, p.118)

This is significant because, while Koo's data on global capital flows remains somewhat rudimentary and is largely restricted by his focus on domestic flow-of-funds data on a country-by-country basis, Howell's entire project is based on the measure, study and theorization of global liquidity as such, while Gibadullina's focuses almost exclusively on the investment patterns, balance sheet composition and changing ownership structures of the world's largest financial firms. While Koo, Howell and Gibadullina differ on many important points, here all three identify the same qualitative shift in the character of the global finance and see this shift as the driver of significant structural changes in the economy as such.

In short, there is a fundamental capital glut plaguing the global economy, which takes the form of a ballooning pool of portfolio investment in stocks, bonds, real estate and other liquid financial assets. Behind this glut lies the fact that "the entire world now enjoys excess production and abundant savings" (ibid, p.5), but the immediate relevance, for Koo, is not the source of the glut itself but the way in which this massive, unregulated pool of global liquidity destabilizes trade balances. On one hand, the question is basically geographical: this destabilization arises not because of the capital glut itself nor the unification of this capital into a single global market, but because governments (with their own national currencies) and labor pools remain balkanized even as trade and capital are integrated. In yet another echo of Marx (in this case, on the equalization of the rate of profit), Koo argues that, in such conditions,

> […] market forces work to take the capital away from the low-yielding countries, and place it in high-yielding countries, with the result that the yield in the former is pushed higher and the yield in the latter is pushed lower. Market forces will continue to operate in this way until the ultimate goal of equalizing the return on capital across countries is achieved (Koo 2009, p. 203).

This basic dynamic should average out across national divides, despite the fact that "the equalized rate of return might not be in the interest of any individual country" (ibid). Meanwhile, a similar phenomenon occurs within the global goods trade—also obviously driven by the production glut—with a rough equalization around the price of globally traded commodities leading to previously high-price countries seeing consumer price deflation and previously low-price countries seeing consumer price inflation. Central banks then tend to respond by modifying interest rates in a way that feeds back into the same trend, encouraging further capital movements.

Throughout, though, labor markets and governments' fiscal capacities remain unintegrated, meaning that labor cannot easily move to follow the new global geography of work generated by the shift in capital flows, nor can any agency shift fiscal resources from areas of surplus to areas of deficit. This "spatial fix" will be familiar to geographers, but Koo seems to have reinvented it here incidentally, with little reference to the abundant literature on the phenomenon. Possibly, this is because his real focus is not on labor or even foreign direct investment, but on the structural interrelation between capital flows, exchange rates and the trade balance. Thus, for Koo the more salient fact is not geographic separation in space but instead the simple fact that currency is in use as a means of financial speculation and investment at the global level while simultaneously retaining its capacity as means of payment within the domestic economy. This means that foreign exchange rates can be induced to change by the global financial market in ways that may not be beneficial for the domestic population. In trade surplus countries, this results in an intentionally depressed currency value (often combined with lower original prices for goods), which means that globally-priced trade goods experience rapid consumer price inflation relative to real wages in surplus countries. Deficit countries will see the opposite, with consumer price deflation making goods more affordable. But for these countries, the problem is that their strong currency values makes productive investment expensive (even if it makes financial investment in assets lucrative) due to high wage, land and taxation costs, even while it induces rapid inflation for a particular subset of financialized assets, such as housing. The outcome is that the available pool of jobs and the real wage will tend to stagnate or even decline for most of the population. These conditions are made somewhat livable by the cheapening of consumer goods and easy access to credit (better understood as the financialization of labor itself), but they're also worsened by rapid inflation in heavily

financialized sectors such as real estate, education and insurance. Thus, the disequilibrium leads to a sort of lose-lose scenario for the bulk of the population in all countries even while it generates win-win outcomes for a small minority of each country's elites.

The geographic and class aspects of this are touched on, but only very distantly. For instance, Koo mentions that this divergence of outcomes is both international (some countries do better than others under globalization) as well as intra-national, since the distribution of the limited benefits (even in the countries that do best under the system) is extremely unequal in terms of both population and location: "globalization is splitting societies in all countries" (Koo 2009, p.186). This then leads to further geographic divergences in these countries—and particularly in the "pursued countries," where the benefits of globalization will tend to concentrate in and around limited urban financial centers in the fashion long examined in the literature on global cities (Sassen 1991) and the dual labor market (Piore 1979). In his earlier work, Koo points to the relative health of "urban areas such as Tokyo and Nagoya where the larger corporations are concentrated" even while "local economies dominated by smaller businesses are at the verge of capitulation" (Koo 2009, p.185). By 2018, he then makes the same case for America, using the demographic and geographic divide evident in the protectionist policies promoted by the Trump campaign in 2016 as his evidence (Koo 2018, p.231). These, however, are only brief asides within Koo's larger work. Geography is significant almost exclusively as an abstract divide. More local forms of geographic division, agglomeration and expansion do not enter the model, and are largely mentioned in passing. Overall, geography, like industrial competition, remains a vaguely indicated but largely hidden variable throughout.

**Consumption Wars**

It is on this blurry territory lying in the distant horizon of Koo's model, however, that a revival of a fundamentally underconsumptionist account takes root. Although Koo himself avoids this error, it is nonetheless important to review here because of its rapidly increasing popularity (particularly with regard to the US-China trade war) and, thereby, its slow percolation into both public awareness and the realm of policy. The underconsumptionist narrative—whether the trade imbalance variant explored in this section or its MMT counterpart, examined below—is appealing because it appears to be correct at an intuitive level, since its basic logic is in accord with populist narratives about Chinese factories "stealing jobs" from domestic workers and its thinkers often portray the theory as if it were opposed to the interests of the "financial elite." In actual practice, however, similar narratives have most often been mobilized to serve the interests of those very same elites, using national policy to force through restructurings of the global trade system in the favor of American financial and industrial interests.

To take the most obvious example from the last trade war with Japan: in the 1980s the US pursued an intentional devaluation of the dollar as part of a more general attempt to revive and restructure American manufacturing power and increase its international competitiveness, justifying this in almost identical terms of rebalancing and national revival—in fact, the "Make America Great Again" slogan pointed to by Koo as evidence of rising populism actually originates in Reagan's 1980 campaign, succeeded by his 1984 slogan: "It's Morning Again in America." This effort was largely successful at reviving the competitiveness of American industry—symbolized, in fact, by the mass layoffs that occurred—through a combination of defense-oriented fiscal stimulus, intentional dollar devaluation against the yen (following the 1985 Plaza Accord) and allowing the bankruptcies and/or relocation of less efficient firms. Yet it did not result in any popular redistribution of wealth. Instead, the era saw further financialization

of the global economy and American success in the trade war came at the cost of a severe depression in Japan which had, by the mid-1990s, threatened to destabilize the global economy even further (Brenner 2003). Today, faced with a new trade war, such policies seem to be slowly regaining popularity, with one recent bipartisan bill introduced to Congress to force just such a devaluation against the RMB, explicitly designed to reduce the current account deficit to zero (Baldwin and Hawley 2019).

At the theoretical level, the most developed example of the theory behind such policies can be found in the work of Matthew C. Klein and Michael Pettis, whose book *Trade Wars are Class Wars* (2020) openly argues for a causal connection between domestic underconsumption and the type of global trade imbalance identified by Koo. Klein, a financial journalist, and Pettis, a former wall street trader and current economics professor at Peking University in Beijing, offer an account of global trade imbalance almost identical to that formulated by Koo. They use the same balance of payments data to measure imbalances in the current account and make more or less the same distinction as Koo with regard to portfolio capital versus investment capital, arguing that trade surpluses or deficits driven by "direct equity investment" moving from rich countries to "poorer ones, with lots of potential" are not necessarily unhealthy, so long as they're investing in actual productive ventures. But the problem, they argue, is that in recent decades the flow has moved backwards: "surplus countries have been lenders, rather than shareholders, while deficit countries have often been mature economies that lack useful projects in need of outside funding" (Klein and Pettis 2020, p.88). Identical to Koo, they therefore conclude that "financial imbalances now determine trade imbalances" (ibid, p.39). The role of cross-border portfolio capital is equally essential, then, and these financial flows can be driven by push factors, as surplus capital sitting in, say, Germany, is mobilized to buy up real-estate-backed financial assets

in places like the United States, rather than making productive investments at home; or they can be driven by pull factors, as certain locales attempt to attract any sort of investment by offering financially lucrative terms with little long-term prospects, often driving down the currency value. This is more common in peripheral countries otherwise cut out from major capital circuits, with the authors pointing to the Turkish investment boom and bust in the 2010s, driven by a depressed lira, as one such example (ibid, p.90).

But the key enabling factor here is, as for Koo, a global glut of capital underlain by a global glut in manufactured goods, which Klein and Pettis argue has been made possible by containerization—somewhat exaggerating the role of a single (albeit important) technological advance, but in a fashion consistent with their emphasis throughout on mechanisms of distribution (of trade goods, financial assets, income, etc.) rather than production. So far, the account seems consistent with that of Koo, not adding much beyond a few historical examples and an interesting analysis of offshore tax havens and their distorting effect on trade and investment data (which is corrected for if one uses current account data instead). In almost every respect, Koo makes the same argument in a clearer, more systematic fashion and, throughout, links it to larger debates about the nature of macroeconomic trends. The two accounts seem to mirror each other in all major mechanics, save one: the initial cause of the global glut. Koo, in fact, does not clearly explain this cause. He gestures at the developmental transition from "maturing" to "pursued" status, but gives no account of how, exactly, the industrial competition that defines the pursuit results in a general, global glut in capital. The implication is that it has something to do with trends in productivity and "innovation," but these are features of an economy that remain elusive in his model, presumed to be givens. He does, however, point to the liberalization of global capital markets as a response (in fact, he argues, a more or less correct

one) to the industrial challenge posed by Japan. But he fails to clearly explain whether the growing predominance of portfolio over investment capital in subsequent decades is an incidental or essential result of this opening—in other words: why is the global capital market plagued by a glut in the first place? And, maybe more importantly (as will be addressed in Chapter 2), what is the relationship of this glut to the parallel glut in global manufactured goods?

This is the ambiguity that allows Klein and Pettis to offer an alternate causal basis for what is, essentially, the same model of global trade imbalance. They argue, contra Koo (and necessarily excluding the case of Japan), that inequality is not an effect of these global distortions in trade and finance, but their cause. The authors mobilize a certain vulgarized Keynesianism throughout, pointing to the ways in which a lack of effective demand has been cultivated and maintained across both trade surplus countries such as China and Germany and trade deficit countries like the United States. But their real inspiration comes from an earlier variant of underconsumption theory initially proposed by J.A. Hobson, best known for authoring the first systematic economic theory of imperialism (Hobson 1902), which would later be built on (and ruthlessly critiqued) by almost all the major Marxist thinkers of the early 20$^{th}$ century in formulating their own theories of the phenomenon. Meanwhile, Hobson's variant of underconsumption theory, alongside that of Rosa Luxemburg, would be among the most popular variants on the theme until the work of Keynes, who saw Hobson's model as an important (but ultimately flawed) forerunner of his own.

The basic equation for Klein and Pettis, as for Hobson, is the idea that domestic inequality, forced through by particular factions of elites in each country, concentrates income in the hands of the wealthiest and starves workers of spending power. While workers spend most of their income on goods and services, the wealthy mostly use similar savings for investment. But if

workers in general see their purchasing power suppressed (though lowered wages, high unemployment, high consumer price inflation, or any other means), then there will be less general demand and therefor fewer profitable investment opportunities in that country. Generalize those conditions and the result is that rates of return and rates of growth will diminish across all economies, meaning that investment will tend to be diverted away from productive uses and toward speculation and the purchase of financial assets, with particularly strong demand for "safe" assets like Treasury Bonds or other dollar-denominated assets. Similarly, the glut of manufactured goods can only find a market if excess output is liquidated on markets fed by credit, since widespread inequality guarantees that wages will remain too low to guarantee effective demand. Historically, for Hobson, that requirement was met by the captive markets of the colonies, who "would pay for those goods by borrowing at relatively high interest rates guaranteed by occupying armies and gunboats" (ibid, p.6). In a particularly strange claim, Klein and Pettis extend Hobson's description of high imperialism to modern conditions, pointing out that "from a certain perspective, the United States—and the United Kingdom, Canada, and Australia, all of which play a similar role in the global economy—therefore resembles the imperial colonies of Europe of the late nineteenth century" only now with China and Germany acting in the same capacity (i.e. exporting their excess production) as the imperial nations of the late 19$^{th}$ century (ibid, p.222).

Today, they argue, these same conditions are guaranteed without the use of occupying armies and gunboats because of international alliances between financial capitalists in the deficit countries, with the special role played by the dollar in global finance as their bulwark, and industrial capitalists in the surplus countries: "The world's rich were able to benefit at the expense of the world's workers and retirees because the interests of American financiers were

complementary to the interests of Chinese and German industrialists" (ibid, p.224). Demand, meanwhile, is suppressed in very different ways in trade-surplus China as compared to the trade-deficit US, or even its sister surplus nation, Germany, since slightly different factions of elites are being mobilized in each case. For the US, Klein and Pettis explicitly argue that financiers, in alliance with government officials, have been pushing for policies and trade agreements which would "sacrifice domestic industry" in order to win financial profits (for the financiers) and geopolitical goals (for the politicians), all despite the fact that "Commerce Department officials privately argued [that these agreements] were disadvantageous to American business and workers" (ibid). For China, wages have been suppressed (particularly via the *hukou* system, which bars many workers from social benefits and strips them of legal protections) and the currency has been prevented from appreciating via the mass purchase of dollar-denominated bonds through the Peoples Bank of China (ibid, p.110). But in every case, the result is the same, with domestic inequality leading to a general global condition in which "demand for goods and services has […] become the world's scarcest and most valuable resource," exacerbating competition for larger shares of a global market that is itself subject to slower growth, since the same inequality tends to shrink each of the domestic markets that underly the global one (ibid, p.225).

To rectify these growing imbalances, the authors argue that global flows of portfolio capital must be constrained, currency values renegotiated and domestic inequality reduced. On the deficit side of the equation, typified by America, Klein and Pettis argue that the government needs to absorb much of the initial shock by issuing more Treasury debt to absorb foreign financial inflows in a controlled manner and/or direct those flows into infrastructure investment. Meanwhile, demand for American manufactures must be not only sustained but expanded and

the domestic industrial base preserved by any means necessary, with the authors suggesting that "increasing defense procurement is the easiest approach" (ibid, p.227). Meanwhile, investments in the social safety net and changes to the tax system should be able to ameliorate the unequal distribution of income to a certain extent, but the problem will only really be solved if it is paired with a similar rebalancing elsewhere, signaled by an evening-out of currency values (i.e. the dollar must be forced to depreciate and the yuan forced to appreciate). In surplus countries, such as China and Germany, this means reforming the *hukou* system (in China), allowing wages to rise, rolling out more thorough social safety nets, raising taxes on the highest earners, and, to solve the German problem specifically (which is the largest single source of imbalances today) "the most practical solution is to federalize European fiscal policy as much as possible" (ibid, pp. 230-231). But if changes in only one country are not enough to induce such changes in others, then what is the actual mechanism through which such a wide-ranging global rebalancing could be coordinated? Effectively, the authors defer to the US government, as still-reigning hegemon of the global dollar system to be the prime mover, though they are unclear on why, exactly, other countries should or would follow.

In fact, there are strong reasons to presume the opposite. The most thorough critique of the book so far comes from Aaron Benanav, a historian whose work focuses on long-run global trends in unemployment, growth and automation (Benanav 2020). Like Koo, but unlike Klein and Pettis, Benanav argues that causality actually runs in the opposite direction: Whereas the authors argue that global competitive pressures are simply a political justification to further suppress wages, Benanav points to the primacy of international industrial competition as both the driver of rising inequality and the reason for the growing global glut of overproduction (Benanav 2020, p.144). Both Koo and Benanav point to the essential role played by Japan here, since it

was, after all, "pursuit" by Japanese industry that helped to trigger the opening of the global capital market, which began alongside two bouts of intentional devaluation of the US dollar—one over the course of the 1970s and another after the signing of the Plaza Accord in 1985. But Japan is almost entirely missing from the story told by Klein and Pettis, despite the expectation that a book on "trade wars" might, quite obviously, take the last, largest one as an important point of reference. This absence is not coincidental, however, and it's one of many reasons that Koo, whose work emerged from a detailed study of the Japanese economy, illustrates many of the same facts but sees behind them an entirely opposite causality.

This is because, if the engine of capital exports is really rising inequality and diminished demand, it becomes impossible to explain why it was trade-surplus Japan that was the first major industrial competitor to engage in the unbalanced accumulation of dollar reserves (in the early 1980s) since "income differentials in Japan remained low" throughout this period (Benanav 2020, p.146) and were especially diminished *prior* to it: the gini coefficient for income distribution in Japan, by every measure, was both low in absolute terms (between .35 and .25, similar to the Nordic countries today) and flat in its trend or even declining from 1966 through to the early 1980s when such exports began (Moriguchi and Saez 2008 and Ohtake et. al. 2013). Beginning in the 1980s, the gini coefficient fo rincome increases somewhat, but by most measures quite slowly and mostly *after* (or, at most, alongside) the increase in capital exports (Moriguchi and Saez 2008, Fig. 2, p.716). In fact, the largest increase in income inequality occurs over the 1990s and into the 2000s in the aftermath of the crisis, accompanying rising unemployment rates in what would come to be known as the lost decades (Ohtake et. al. 2013, Fig 3.3, Fig 2.12 pp.13, 23). If inequality were to have *caused* these capital exports, one would expect its increase to have preceded the rise in Japanese purchases of treasury debt. But the

opposite is the case: the inequality increase that does occur is initially mild and almost entirely posterior, which hints at either an inverse causality (i.e. these cross-border capital flows help to cause rising inequality) or no direct relation between the two. Rather than following the Hobsonian logic of Klein and Pettis, with inequality driving down domestic consumption capacity and thereby triggering a wave of outbound investment, the Japanese case seems instead to prove the opposite: even with many of the domestic measures that Klein and Pettis argue for—such as high income taxes for the wealthy, stable employment for workers, an inheritance tax structured in the fashion they recommend for Germany, a recent history of state-funded fiscal projects—the basic dynamic between industrial competition, capital exports and trade wars (all involving pitched battles over the exchange rate) remains completely unchanged.

This absence points not only to deeper problems with the basic mechanics of Klein and Pettis' model, but also to a failure to truly grasp the consequences of the "rebalancing" that they advocate. The problem, again, is that they simply ignore the entire course of the US-Japan trade war, including the way that it was ultimately ended. This is a significant issue, however, since this rebalancing was achieved only through a concerted effort to raise the exchange rate of the yen to the dollar, thereby resulting in the inflation of a massive asset bubble in Japan (funded by speculative capital that could no longer turn a profit by investing in industry) followed by its catastrophic collapse (the basic dynamics are discussed in Brenner 2003, Chapters 3 and 4). Benanav argues that the Japanese crisis was so substantial that, faced with a complete economic breakdown which (because of still-massive Japanese holdings of dollar-denominated assets) would have taken the US economy with it, "Washington agreed to re-engineer a strengthening of the dollar" (Benanav 2020, p.146, also argued in Brenner 2003, Chapter 4). Throughout this period of devaluation, however, the United States successfully restored a degree of

competitiveness to its industry and rebuilt substantial portions of its industrial base, as measured (in Brenner 2003) by its global competitiveness in profit and output *but not as a share of total employment*—Koo points to this basic fact repeatedly, also praising Reaganomics for its fiscal stimulus (in the form of defense funding) and its ruthless focus on cutting back expensive social programs and demolishing obsolete fixed capital, all of which had a necessarily time-staggered effect, creating the conditions for the boom in the 1990s under Clinton (also documented in Brenner 2003, Chapter 2).

Despite this domestic success, however, Benanav's broader conclusion is that the rebalancing itself was a failed venture precisely because it would have induced a global depression from which even the United States would have been unable to escape: "evidently, by the mid-90s, the world economy had borne about as much of a falling dollar as it could take" (Benanav 2020, p.146). The result was another bout of dollar revaluation, leading directly to the tech bubble, the outward flight of American manufacturing capacity (now not to Japan and Germany but to Mexico and China) and the basic structural situation in which "America's willingness to act as buyer of last resort kept the world economy ticking over through a period of worsening stagnation, at the cost of inflating asset bubbles and furthering America's deindustrialization," now enabled (Klein and Pettis are correct on this score) by a very real "alliance among American retailers, American financiers and foreign industrialists" (ibid, pp.146-147). But the reality is now even more grim, as the bursting of these asset bubbles begins to trace out "the limits of US debt-fueled global stimulus" (ibid) while doing nothing to rectify the deeper tendency toward stagnation or the global glut in production, only redistributing these more basic problems. This also means that, even if Klein and Pettis' rebalancing were to occur "it would likely cause a global great depression" in the same way that the US response to

Japanese industrial pursuit risked causing one three decades ago (ibid). This prospect is ignored by the authors, however, since they fail to address the question of the Japanese experience at all. Instead, they disguise the severity of the effects that such a rebalancing would have in places like China and Germany with vague gestures toward the untapped potential of domestic consumption, which can still increase despite falling growth rates. But this says nothing of what, exactly, happens to an economy when its industrial competitiveness dwindles and its corporate profitability withers.

**Koo's Conclusions**

Koo, of course, has a clear answer to this question, derived from the Japanese experience: the inflation of new, even more gargantuan and less sustainable asset bubbles, which then burst and leave a balance sheet recession in their wake. If this recession can't be properly buffered within the global system by something like the turn to dollar revaluation and American debt-fueled stimulus that was pursued in response to the Japanese collapse in the early 1990s (a system which is, Brenner and Benanav argue, on the verge of collapse today), then a Japan-style balance sheet recession begins instead to look more like the Great Depression. In other words, Klein and Pettis aren't necessarily wrong to argue that such policies would, technically, "rebalance" the global economy. But this rebalancing would be an extreme reversion in the opposite direction, with general economic breakdown leading to collapsing global trade and a new fragmentation of the world market into cutthroat trade blocs (as envisioned by Howell 2020) which, albeit "rebalanced" according to the basic accounting measures used by the authors, would nevertheless be locked in a bloodier and bloodier zero-sum competition for influence and control over the remaining global circuits of trade and capital. In the end, this would likely lead to a

degenerative struggle in which decades of severe depression and war will destroy enough of the current overcapacity to enable new rounds of global reintegration, presuming anything of a world remains. When faced with such persistent stagnation for so many years, the conclusion slowly dawns, as it does for anyone whose solutions are fundamentally predicated on the preservation of capitalist social relations above all else: in order to be saved, much of the global economy will likely have to be destroyed and then remade. This prospect is not pessimistic hyperbole, but a matter-of-fact description of how similar periods of depression were overcome throughout the history of capitalism (as argued by Mattick [Jr] 2011 and [Sr] 1981).

If the solutions offered by Klein and Pettis seem invariably to lead to catastrophe, does Koo's more accurate model offer any better outcomes? On the one hand, Koo hints at similar, potentially catastrophic consequences—driven both by the increasing speculative activity of these cross-border capital flows but also by the threat of a sudden dollar collapse triggered by the sell-off of US treasury bonds held by foreign investors or a rapid shift to stronger forms of protectionism (which will be signaled by declining growth in global trade and the regionalization of trade networks). He's clear, and at points even hyperbolic, in laying out these threats, arguing, for instance, that "Europe [is] repeating the mistakes of the 1930s" by forcing austerity and restraining borrowing in the Eurozone precisely when fiscal stimulus is needed, thus again leading to a surge in far-right political parties (Koo 2018, p. 169). In this regard, he seems to agree with Klein and Pettis, even arguing that something like a fiscal union within the EU will likely be a necessity. At the same time, even his non-catastrophic scenarios all seem to imply that the conditions of the "pursued" economies will continue and even generalize as currently pursuing countries are able to join the ranks of the pursued, just as Japan was:

> This pattern of emerging economies taking away investment opportunities from the developed countries will continue until all economies have long passed their Lewis Turning Points and the return on capital has been more or less equalized. (ibid. p.260)

He even gestures toward a moment in the future, which he calls the "Global Labor Market Maturity Point (GLMMP)" in which such an equalization has taken place, and "all slack in other labor markets is eliminated," which will finally again allow for rising real wages since "pursued" countries will, apparently, have no one to pursue them (ibid).

This implies a tectonic shift in global production, premised on the incorporation of all untapped pools of labor into the global economy and complete global urbanization and industrialization (in Koo's terms this would be measured on the national level, presumably by the share of urban population and total industrial output per capita). If there was one central driver of the current trade imbalances within globalization, Koo indirectly points the finger at China, arguing that a key accelerant for the global liberalization of capital markets was the entry of the Chinese labor pool into the global economy, since "China alone added to the global economy a supply of labor equivalent to that of all the industrial nations combined" (Koo 2009, p.185). Yet he offers no hint as to where a similarly large pool of labor might be available today. This will be a persistent question across all theories of crisis reviewed here, and I will address it in some detail in discussing the potentials for industrialization in East Africa. But equally important is the fact that Koo seems to imagine a world in which open trade regimes can continue and competition—paired with a domestic willingness to engage in fiscal stimulus in the face of balance sheet recessions—can exert an ultimately stabilizing effect on the global economy, if only the imbalances wrought by global capital markets can somehow be reined in.

But his own prescriptions for how to do this—and for how the conundrum of secular stagnation in "pursued" economies might be addressed—consistently fall flat. Often he offers no prescription at all, portraying the growth slowdown, for instance, as an unavoidable fact of economic "maturation." Elsewhere, the policies he does proposes simply do not address the problems he has identified at anything like the scale of the problems themselves. It is difficult to see how any of the miniaturized solutions that he offers would be capable of overcoming the gargantuan challenges that he himself identifies.

In the end, it seems that part of this disconnect is due to the fact that Koo's models are, in every instance, indicating a more severe problem and a more grim future than Koo himself seems to recognize. This also means that they require more far-ranging solutions that Koo offers. In both respects, Koo frequently indicates the direction that such catastrophes might take and the immense scale at which it would be necessary to conduct an effective intervention, only to then deny these implications and instead offer mild policy suggestions in their place. These policies almost universally operate at a scope far below that of the problems that he has identified, and he fails to explicate the mechanics whereby such constrained policies might aggregate or at least exert an outsize effect such that they could address these larger issues. All the major problems that Koo recognizes, meanwhile, have to do with the phenomenon of pursuit, which is driven by industrial competition and defined by stagnant growth, undergirded by the global glut emphasized by Benanav—to which Koo offers no solution. The first and most important of the problems that he does address is the question of how to deal with cross-border capital flows, since such flows are so integral to the negative feedback loops pushing deficit countries further into deficit and surplus countries further into surplus. While his frequent invectives against the absurdity of freeing capital markets without integrating currency systems or labor pools might

seem to imply some sort of protectionist conclusion, though, Koo is equally strong in his warnings about the dangers of protectionism, even arguing that while such policies can be a short-term benefit for the working class, "[i]n the long run, history has repeatedly shown that protected industries always fall behind on competitiveness and technological advances" (Koo 2018, p.227). So what, exactly, does he suggest? Ultimately, Koo advocates for a gradual correction of international trade imbalances, driven by renewed capital controls, a wide-ranging but extremely gradual renegotiation of foreign exchange rates aimed at a substantial devaluation (over time) of the US dollar, all accompanied by domestic efforts to stimulate demand in surplus countries (namely China) and new productive investment in deficit countries (namely the US).

Koo rejects outright protectionism because he identifies "free trade" with the early postwar boom under the General Agreement on Tariffs and Trade (GATT) system which preceded the founding of the WTO. The basic argument is that an open global market for trade inherently tends toward equilibrium, even if an open global market for capital inherently tends toward disequilibrium. The evidence he gives is largely historical: he claims that the GATT system marked the "end of imperialism" (seeming to conflate the concept with mere territorial expansion), allowing instead for economic growth to be driven by industrial competition between nations over the global market. He notes that the GATT system, and even the early WTO, functioned relatively well because its early membership was comprised of nations "with similar levels of economic development […] similar wages and productivity levels" (ibid, p.228), all placed under US industrial and geopolitical dominance. But as entrants such as Japan joined the system, they did so with relatively higher initial tariffs and more competitive industry, fed by a larger pool of surplus labor that could be paid lower wages. This began to destabilize the system and led to the beginning of industrial decline and stagnant incomes in many of the

traditionally advanced countries (with Japan and West Germany soon surpassing the UK and France in sheer economic size, for instance). Unlike Klein and Pettis, Koo points to the effects of the US-Japan trade war as a key moment in this sequence, even while he argues that, at this time "exchange rates still responded correctly to trade imbalances" meaning that "excess demand for yen versus the dollar caused the yen to strengthen against the dollar [which], in turn, made Japanese products less competitive in the U.S." and prevented trade frictions from growing worse (ibid, pp.229-230).

Apparently, the claim is that even this imbalance caused by mixing countries at different stages of development into a single global trade market would have balanced out in the end, if only the US had not led the drive to open a global market for capital in those same years, rekindling the trade war and adding to it a financial dimension, as Japanese investors bought up US Treasury securities and thereby affected the exchange rate between the yen and the dollar. On the one hand, Koo seems to treat this decision as if it were merely a misguided political whim driven by the economic faith that any and all forms of further market-opening are inherently beneficial. On the other, he concedes that "financial liberalization in the US started in the early 1980s in response to the high inflation rates of the 1970s" (ibid, p.254) which, in his model, were a result of the fact that even though the US had become a "pursued" country, far past the Lewis Turning Point, it was still implementing policies designed for a "maturing" economy—namely labor protections, wage increases, welfare programs and large fiscal stimulus projects. The real structural factors inducing this change will be explored in more detail below, but Koo has already essentially stated them: increasing competitive pressure began to strain the profitability of older, established industrial firms in all the highest-income nations, which were now placed in direct competition with foreign firms utilizing newer plant and equipment, with access to cheaper

labor—a phenomenon made worse by the overhang of expansive labor protections, high wages and social programs—and producing a general global glut in production which was, due to the narrowing of potential sites for profitable industrial investment, transformed into a glut of increasingly global, increasingly liquid financial capital. Aside from this—and despite Koo's faith in "innovation"—the reality is that there are clear material constraints to rolling-out new technologies within an industrial system that is, after all, embodied in the physical infrastructure composing interlinked territorial production complexes (as documented by Storper and Walker 1991). This is where questions of industrial geography become essential. Meanwhile, there are also deeper constraints when it comes to the rate of return of new technologies in relation to growth, as will also become evident below. For now, the important point is that Koo at least partially elides such factors and treats as incidental the question of why, exactly, the majority of capital holders in such countries gradually chose to, on average, cease investing in production (even in foreign direct investment in places like Japan) and instead become portfolio investors (as documented by Koo himself, and, again, by Howell 2020 and Gibadullina 2020, or, in more play-by-play detail for past few decades, by Tooze 2018).

His vision for a solution to the current conundrum is, therefore, more or less just a suggestion that we slowly make our way back to an international system with a relatively open global market in trade and a relatively closed global market in capital. He is more cautious and pragmatic than Klein and Pettis here, with no real emphasis on reducing inequality, and imagines that this will allow his solutions to avoid the pitfalls of global catastrophe that would result from the more aggressive rebalancing suggested by these other authors. Nonetheless, Koo's solution requires coordinated international action on the part of all the major central banks, which need to implement something like a new Plaza Accord "to realign exchange rates in order to forestall

protectionism and prevent destructive cycles of capital flows and financial crashes" (ibid, p.244). The problem, though, is that the pools of liquid capital that central banks have available for such interventions are today dwarfed by private capital flows (Howell 2020), so the effect may be muted. Koo argues that this is not an insurmountable challenge if all the major central banks coordinate, and if they "side with trade flows [by] buying the currencies of surplus countries and selling the currencies of deficit countries" (Koo 2018, p.245). But then he acknowledges that doing this may risk capital flight. Thus, it would seem to imply that, even if all the major central banks were somehow able to coordinate in this fashion (itself deeply questionable), such coordination would have to be preceded by far more wide-ranging capital controls than he explicitly advocates for, and it is not clear how these controls would be able to adequately constrain cross-border portfolio investment while still enabling productive foreign direct investment. At the most extreme, Koo simply recommends that

> the authorities might want to consider implementing higher risk weights for institutional investors' holdings of assets located in current account deficit countries or denominated in the currencies of such countries (ibid, p.252).

These risk weights will also help to rein in the carry trade and to convert short-term speculative investment into long-term productive investment—but Koo overs no evidence for why this would happen if the rate of return on productive investment itself remains low and poses a clear risk of substantial loss. Overall, it's not at all clear that such an effort would be capable of rolling back global liquidity to the scale that Koo clearly indicates is necessary, or even to the point at which capital flight in the face of exchange rate intervention would be hampered.

Ultimately, it seems that Koo has placed himself in a similar position to others in the broadly post-Keynesian tradition, such as Thomas Piketty, who identifies long-term trends

toward inequality that are driven by global dynamics, but is only able to offer limited suggestions for coordinated national interventions to solve the problem—in Piketty's case, coordinating increased taxes on the wealthy in all major economies—because there exists no global-scale authority capable of rolling out the necessary policies, such as a global capital gains tax. In Koo's case, even if his suggestions were correct (and they are not, since they fail to address the core problem of stagnation in productivity and profitability), they would require something like a global government to implement them, or at the very least a country in as strong a hegemonic position as the US was following the second world war. And in this case, it's not clear why such a government would not simply pursue the other option that Koo gestures toward repeatedly, albeit as an impossibility: actual integration of currency and labor markets, accompanied by a global fiscal union capable of directly transferring funds in cases where these markets are failing to equalize. Of course, such a prospect is either absurdly utopian (were it to be pursued cooperatively) or dangerously imperialist (were it to be pursued competitively). Nonetheless, it is the most natural solution, given his presumptions. Anything short of this will either be extremely improbable to properly coordinate or it will offer only a fatally partial solution, which will result in massive capital flight and even worse imbalances, leading to even greater pressure to protectionism. In this way, the model is ultimately both utopian and dystopian: offering an impossible positive solution, on the one hand (global government and/or unchallenged hegemony forcing through a truly global market plus fiscal union), and an increasingly probable but constantly disavowed negative solution, on the other (deglobalization and the return of protectionist trade and currency blocs).

Meanwhile, the deeper dynamics that he identifies within both global development trends and national balance sheet recession seem to offer equally limited options. With regard to his

developmental schema, Koo appears to simply accept the stagnant-growth, stagnant-wage, high-inequality conditions of the "pursued" economy as a given. It is merely the plateau reached after a country "matures" past the Lewis Turning Point and, although such countries can be overcome by others and therefore lose their status as "advanced," this does not throw them back to previous stages of development since it does nothing to reverse their demographic conditions. Thus, countries that fail to retain their "advanced" position are simply thrown into worse stagnation and forced to scramble to regain their lead in the global hierarchy of industrial pursuit. At this point, he is clear that any redistributionist or pro-labor policies that had been passed in the "maturing" phase, when productivity growth, wage growth and economic growth were all high, will further diminish the competitiveness of a "pursued" economy relative to its pursuers. Meanwhile, general conditions of balance sheet recession will exacerbate these trends, diminishing domestic productive investment further, widening geographic inequalities within and between nations, and thereby triggering ever-more popular calls for protectionism, enabling the revival of far-right politics.

This is precisely where the divergence of Klein and Pettis from Koo is most important. Whereas Klein and Pettis imagine a sort of impossible "win-win" scenario in which global rebalancing can be achieved, industrial competitiveness maintained and inequality reduced, Koo recognizes the inherent constraints of the actual situation. For example, even while his own model of global trade imbalance easily opens the door to a Hobsonian underconsumption theory like that of Klein and Pettis, Koo's conclusion seems to be that rising inequality is an inextricable result of being a "pursued" economy attempting to maintain its advanced status (though this inequality can be dampened somewhat by proper policy, it won't be reduced to "golden era" levels) and that growth rates in such countries will simply remain low regardless of

any redistributionist measures. In fact, the implication of Koo's rebalancing is much the same of that of Klein and Pettis, who are "calling for a global commitment to persistently lower rates of growth" (Benanav 2020, p.148). But Koo is not just advocating for such an outcome as an effect of rebalancing: he is not arguing, in other words, for inducing a global recession by forcing up the value of the yuan and thereby pressing down Chinese growth rates, but instead making the argument that stagnation will be an unavoidable result of generalized conditions of industrial pursuit. China is going to get there regardless. Thus, his most optimistic case seems to be something like: collapse can be avoided, but stagnation will continue, deepen and spread to more and more countries until the GLMMP is reached, at which point some new cycle of industrial competition and pursuit could, conceivably, occur.

What are his recommendations, then, for economies that find themselves stuck in this seemingly necessary extended period of stagnation? The problem in such countries is twofold: how to stave off balance sheet recession and how to do so while remaining an advanced country. His solution, similarly, has two dimensions: effectively-allocated fiscal stimulus to stave off the recession and ensure macroeconomic stability "along with microeconomic policies designed to increase return on capital at home and fend off pursuers," specifically by maximizing "the innovative potential of its people" and "increase[ing] the economy's flexibility" (Koo 2018, p.107). The two are interconnected, since Koo argues that the most effective fiscal stimulus will not necessarily be traditional Keynesian projects such as public works, but instead government-funded efforts to increase "innovation," which largely means the funneling of public money into education, advanced R&D, and subsidies for hi-tech corporations. And such projects must, meanwhile, be accompanied by supply-side reforms that guarantee the continuing flexibility of the economy, which in this case largely means the flexibility of labor.

This, then, is no revival of classical "Keynesianism" in any sense of the word. Even if Koo does point to nationalization or expansive public works projects as potentially lucrative options in certain conditions, his key examples of accompanying supply-side reforms are essentially new versions of the old Reagan-era policies that funneled defense money into education, R&D and hi-tech production while cutting taxes and deregulating markets—this is despite the fact that, under Reagan, this was all accompanied by the opening of the global capital market, with Koo seeming to argue that such opening was an understandable and possibly necessary response to inflation at the time, but now that inflationary pressures have diminished (due to both the general conditions of balance sheet recession and the breaking of labor) this capital opening can be rolled back even while the other supply-side reforms must be deepened. Thus, Koo sees fiscal stimulus geared toward enhancing innovation and supply-side reforms designed to increase the flexibility of advanced economies working hand in hand to revive the competitiveness of "pursued" countries' industry in the same way that the Reagan reforms ultimately laid the groundwork for the brief uptick in US productivity and economic growth in the 1990s (ibid, pp.108-109), correctly recognizing that Reaganomics was in fact undergirded by its own brand of fiscal stimulus in the form of Cold Ware defense projects.

In terms of such stimulus today, Koo emphasizes the necessity of "self-financing" projects and deficit-spending on the part of the Treasury, since he continually advises against raising taxes. This is not a problem, however, because

> the absence of private-sector borrowers [i.e. general conditions of balance sheet recession] means borrowing costs for the government, sometimes the only borrower left, drop to very low levels. Many public works projects therefore become wholly or nearly self-financing (ibid, p. 92).

Not only does fiscal stimulus not "crowd-out" private-sector investment in the "pursued" stage in general due to low domestic investment opportunities for the private sector, but balance sheet recessions also further guarantee such conditions, with "private-sector demand for funds […] now negative in virtually all the pursued economies despite zero interest rates" (ibid). Fiscal stimulus will not produce inflation in such cases precisely because "pursued" economies and those in the midst of balance sheet recession are threatened instead with deflationary spirals—here Koo prefigures the arguments of MMT thinkers, explored below. Nor is increasing public debt a serious problem, since debt will increase regardless. Debt is, after all, "(excluding debt financed by the central bank) […] merely a reflection of the fact that the private sector has continued to save" (ibid, pp.95-96). The apparent multiplication of debt beyond savings is, Koo argues, a financial epiphenomenon caused by re-lending of existing debt, leading to a double-counting in the statistics. Nonetheless, he emphasizes that projects must be "capable of generating returns high enough to pay back both interest and principal" so that they will be "self-financing and will not increase the burden on future taxpayers" (ibid, p.96). This means that low bond yields for government securities (i.e. Treasury bonds) are "a signal to the government to look for public works projects capable of producing a social rate of return in excess of those rates" (ibid, p.97). All the excess savings of the private sector will invariably seek a base of relatively safe assets, and such bonds are traditionally among the safest (Howell 2020 confirms this).

The Treasury should not only be used to purchase risky private assets in the midst of the balance sheet recession, as Koo recommends (in Koo 2009, p.275), but it should also expand the provision of government bonds without fear of the further public debt created, since this debt can be eliminated if it is put into public works projects that, over the long term, have higher yields

than these low-yield bonds. The trick here is that all fiscal projects are not equal in this regard, and the vast majority may even be useless for these purposes, bearing too low a yield or even a negative one. Finding such projects to fund is, according to Koo, "the most important macroeconomic challenge for policymakers in pursued countries" (Koo 2018, p.97). Ideally, this entails setting up an "independent commission" to determine which projects are suitable (ibid). But in reality this takes time and resources while countries in balance sheet recessions instead need immediate stimulus. Thus, such countries should simply "implement whatever projects […] are 'shovel-ready' now, without waiting […]" because "the GDP and jobs so saved by *not* waiting for ideal projects will greatly exceed any savings the latter might have yielded" (ibid, p.100). Koo here returns to a point that he emphasizes throughout his work on balance sheet recessions: both the Great Depression and the Japanese crisis were balance sheet recessions, but the immediate response to each was markedly different. At the outbreak of the Great Depression, the US did not pursue fiscal stimulus until years later, in 1932, and then grew over-eager to roll back that stimulus at the first signs of recovery in the later 1930s, ensuring that it would not emerge from the depression until World War II guaranteed an unprecedented fiscal boost. By contrast, Japan "was able to keep its GDP from falling below the bubble peak because it implemented fiscal stimulus from the outset without waiting for the best projects" (ibid, p.101). Koo generally considers the Japanese response to have ultimately been a success, albeit one marred by some temporary mistakes and ultimately threatened again by the Great Recession two decades later.

This is itself a hint at the curtailed prospects of "pursued" economies in general, however, and Koo gives no suggestions as to how another depression might be avoided in the absence of any country able to play the structural role of the United States relative to Japan in the 1990s,

since it was dollar revaluation and the subsequent debt-financing the US offered to the global economy that pulled it back from the depression threatened by a Japanese collapse. Meanwhile, to characterize Japan's "lost decades" as an example of successful response to the crisis while also celebrating Reagan-era supply-side reforms for increasing the "flexibility" of the US economy demonstrates the deeply pessimistic side of Koo's most optimistic models, where the best possible world seems to be one that simply manages secular stagnation well enough that wages can be kept down, workers will be forced into increasingly "flexible" employment and only minor (or, in Koo's ideal, really no) new redistributive tax schemes are implemented, but somehow all without inducing a widespread turn to protectionism or assisting far right parties in their rise to power. The irony is stark when Koo is able to identify these deep, structural trends contributing to stagnant prospects for the majority of the population and offer essentially no solution save a few tweaks geared toward what is effectively the preservation of the status quo, since things could, after all, be worse.

In fact, he argues for even greater inequality to be cultivated in pursued economies, since many of the fiscal projects he recommends focus on offering better high-tier education and vigorous state-led R&D that will mostly benefit those with the highest education levels. In particularly dire situations, the government can even become "innovator of last resort" (ibid, p.112), completely folding major R&D efforts under its wing to help sustain the pace of technological advances even when the market refuses to fund them. At a more general level, he suggests that schools should identify "students who seem likely to come up with something new" (again, we meet with the mysticism of the economist here, who imagines that "innovation" springs from the brain of the entrepreneur) and then remove these students from the common education system to place them "in a program that encourages them to pursue their creative

passions" (ibid, p. 112). Faced with a secular, global decline in economic prospects that threatens the already fragile stability of the international geopolitical balance, Koo seems unable to muster any solution that exceeds the base politics of a wealthy parent berating a city government for trying to integrate its schools and force their "gifted" child to share a classroom with the dimwitted children of the working class.

**Disequilibrium**

Even if Koo successfully combines post-Keynesian and mainstream currents to identify certain secular trends in global economic development and grasp some of the core dynamics of recent balance sheet recessions, he nonetheless remains well within the bounds of conventional macroeconomics. This is made clear not so much in the exact mechanisms that Koo identifies or the key variables that he integrates into his model, but rather in what he excludes: throughout, he offers no real theory of growth or competition as such (never really accounting for what these categories mean, but instead taking them as given), despite his claims to explain the long-term decline in growth and the phenomenon of industrial pursuit. In this picture there are no real classes, only different categories of saver and consumer. Workers are mentioned mostly in their capacity as spenders-of-the-wage, consistent with the mainstream view, and questions of labor become questions about the constraints on corporate competitiveness on the one hand and consumption demand on the other. All of these omissions derive from the fact that Koo is repeating the most basic ideological procedure of economics (as argued by Mattick 2011 and 2018), as common today as it was in the era of classical political economy: treating the economy as simply an allocation mechanism geared to meet consumption needs, composed of buyers and sellers of various types, rather than as a social system for organizing production.

The presumption, moreover, is that this allocation mechanism tends toward an equilibrium, albeit a strongly cyclical one in Koo's *yin-yang* framing—and one in which the state is an integral part of the mechanism itself. But this means that the long-term trends of slowing growth, rising industrial competition and the increasing frequency of balance sheet recessions today are effectively viewed as *exogenous* causes, derived by Koo from the demographic trends identified in the Lewis Model. In a way, the contemporary economist's deference to demography in the last instance in order to explain clear long-run trends in the system is also itself a feature of classical political economy: this was, after all, the position of Malthus and Ricardo. But the logic here is always circular, because all the key demographic trends—variance in birth rates and mortality rates, migration, urbanization, the changing shares of agricultural versus industrial employment, etc.—are all unexplainable without reference to the very economic trends that they supposedly cause. Yes, growth is enabled by the influx of rural surplus labor into the urban industrial setting. This is a certainly a key part of all accounts of growth, possibly a necessary conditions, but if it were a sufficient one then every country with a large rural labor surplus would automatically generate economic growth. This has not been the case.

In a way, Lewis himself took growth to be the absolute precondition even before he sought to explain the dynamics that could lead to further growth. First, he clearly argued that his "dual-sector" model was, in fact, not something that could be universally applied to all countries (Lewis 1954, pp.140-141) and he certainly never claimed that it would provide a basis for explaining the core dynamics of the capitalist economy as such. It was intended to explain long-run development trends in a particular subset of countries where several essential preconditions were met. The most important of these was taking the "capitalist" or "industrial" sector as a given. The model begins with a condition in which small islands of industrial employment with

demand for labor *already* exist, "surrounded by a sea of peasants" (ibid, p.147). This allows for rapid economic growth without immediate wage increases because of the ready availability of surplus rural labor, of course, but it also presumes that some core of industrial enterprises already exists and that they can successfully utilize that labor for the production of commodities that can then be successfully sold, presumably in circumstances where demand for those products is also growing. If these preconditions are met, then economic growth can be accounted for: "the mechanism being that over time, savings, investment, and capital accumulation produce economic growth in the formal sector of the economy" (Fields 2004, pp.728-729). But where do these funds ultimately come from? There's no ambiguity here: "the source of this growth, for Lewis, was profits" (ibid). Thus, general profitability is a necessary element of the model. Profitability drives growth, enabling not only the demographic trends of urbanization and subsequent declines in birth and mortality rates, but, once these lead to constraints on the labor supply, profitability is also the necessary precondition for investment in more capital-intensive production—both as the sitting pool of investment funds and as the stimulus for investment to take place at all.

Koo's deference to the demographic dimensions of the Lewis Model in accounting for long-term trends, then, ultimately turns in on itself since Lewis himself defers back not to demographic features but instead to profitability in order to account for the baseline conditions necessary to keep the model functioning. Profitability is necessary at every stage. It also explains why certain areas with the proper demographic conditions might still not experience any growth, or why growth might begin and then stall: production in such locations may simply not be profitable. But Koo takes profitability as a given and never inquires into the nature of the concept, even though his focus on the balance sheets of private firms seems to similarly imply its

central importance. Enormous insight is possible precisely because Koo approaches the question in this way, simply taking the firm's own conventional appraisal of profitability (as balance sheet performance) at face value. But this also means that a certain level of theoretical self-reflection is lacking, since this conventional appraisal is not interrogated. Ultimately, based on this presumption he argues that firms prioritize debt repayment rather than seeking profitable expansion during the balance sheet recession, hindering growth. Similarly, he shows that growth slows during "pursuit" by industrial competitors since such pursuit makes it difficult to produce profitably in the pursued country. Both these facts seem to indicate that the generalization of stagnant growth throughout the world economy has something to do with low profitability as well. But the relationship is never made clear.

At every stage, then, Koo's theory seems to ultimately lead to two unexplored questions, only to ultimately treat them as non-issues: The first is the question of profitability. It's not that profit is ignored by Koo—since he mentions it repeatedly—but instead that it's taken to be an unproblematic given that businesses can always figure out some way to make a profit in the end. Koo presumes no explicit structural limits to profitability, even as he seems to implicitly demonstrate them throughout. The second is the question of population. Again, demographic factors are repeatedly mentioned by Koo and they are integral to the Lewis Model from which he draws. But this model essentially presumes that the supply of surplus labor, at the global level, is not a problem—it was formulated, after all, in an era when the bulk of the world's population was still engaged in some form of peasant production. Such conditions no longer hold today. On the one hand, something similar to the rapid integration[14] of the Chinese population into global

[14] It's important to note that this question of integration, which is often referred to as "subsumption" in the literature drawing from Marx, is distinct from Lewis' conception of the divide between rural subsistence and capitalist industry. The two are often conflated via the category of the "peasantry." But the reality is that

production can hardly be repeated at present. Remaining global labor pools are both smaller, more geographically distributed and far less prepared for immediate integration into industrial systems—the widespread literacy and technical training of the Chinese population that prevailed at the time of its gradual incorporation into global productive systems is completely unmatched today (or even historically). But literacy and technical aptitude are even more necessary skills today than they were in the past due to the mechanization of production. On the other hand, as will be explored in more detail below, many poor countries with large surpluses of rural migrant labor today find themselves faced with a seemingly contradictory phenomenon of "premature deindustrialization." This itself hints at deeper contradictions within global production, linked both to the technical composition of industry and the widespread production of a "surplus population" that can't effectively be integrated into production—it's extremely important to note here that this is absolutely not the same thing as "overpopulation" in the Malthusian sense, and prevails *even in countries with rapidly declining birth rates and shrinking populations*. Overall, the point is simply that Koo seems unwilling to acknowledge the potential that deeper questions of profitability and population might be central to some of the dynamics he otherwise so clearly identifies.

The profitability problem is particularly important, since it lies behind the question of the global production glut, which is so integral to Koo (as well as Klein and Pettis) as an explanation for the increasing prominence of global portfolio capital. Aside from a few brief references to the whims of investors and politicians, Koo never systematically explains why, exactly, these cross-border liquidity flows have taken a financial form rather than being funneled into direct

subsumption occurs not just through the destruction of local subsistence economies—it proceeds through the destruction and subsequent integration and adaptation or exaptation of *any* non-capitalist social structures.

investments into new productive facilities. Here a real-world example would have been particularly useful. Since Koo, Klein and Pettis all advocate for a reversal of productive investment flows back into deficit countries (like the US), it will be helpful to ground that claim in a real-world case: Among the best candidates is the recent saga of Foxconn in Wisconsin. The Taiwanese contract manufacturer (the largest single company in the world coordinating electronics production chains) agreed to a massive investment in the small Southeastern Wisconsin town of Mount Pleasant for political reasons (essentially to curry favor with Trump to avoid damage in the trade war) before investigating whether and how the investment would be profitable. This is a fairly common phenomenon, as we'll see later in the case of Chinese investment projects that are announced to great fanfare but which never actually break ground—and Foxconn itself is particularly well-known for this practice. The problem in Wisconsin was that the company had promised a $10 billion investment including construction of a 20-million-square-foot LCD manufacturing center that would ultimately provide some 13,000 jobs, all seemingly without realizing that backing out of such a project would be neither as politically feasible nor as economically inconsequential as doing the same thing in India or Indonesia (Dzieza 2020).

As might be expected, it was not in any way profitable to manufacture LCD screens (an industry with tight margins even in East Asia) in the United States. The result was that the firm had to scramble to reinvent the project, assigning a series of managers to seek out literally *any* profitable use for the land and buildings it had already committed to. These managers attempted just this, desperately trying out ventures ranging from work-share office spaces to warehousing and logistics for dairy exports to manufacturing components for server equipment, at every stage making sure to clothe these promises in obscure and meaningless tech branding about building

an “AI 8K+5G ecosystem” composed of “innovation centers” that, in reality, were nothing more than empty buildings. But despite the consistent praise and support of the state and federal government, including hundreds of millions in subsidies already sunk into the project, the firm consistently fell short of all its investment and employment targets. Now (with a new governor from the opposing party) more scrutiny is being placed on the entire venture, even while the firm appears to have completely failed to find any profitable activity to engage in (ibid). This is one of many practical examples that show why, exactly, investors might tend to prefer portfolio capital to investment capital, especially within in “pursued” countries. This is because it demonstrates a deep, structural difficulty in retaining or attracting investment in such countries, even when everyone involved is enthusiastically supporting the deal. The capital glut, then, has less to do with the whims of investors and more to do with their rational recognition that projects such as this simply aren’t profitable.

The consequences of Koo’s failure to address questions of profitability are wide-ranging, however. For example, even in using the Lewis Model to gesture at the existence of a long-term tendency toward slowing growth, he actually foresees no deeper secular trend toward any sort of breakdown caused by this slowed growth. Instead, Koo seems to earnestly believe that there are no absolute structural limits preventing all countries from ascending to “pursued” status and thereby pushing the global economy into a “Global Labor Market Maturity Point.” Behind this is his unquestioned faith that it’s always possible for business in general to make a profit somehow—usually through some unexplained “innovation” that, in the last instance, might simply need to be cultivated by the state. It’s likely (though not explicitly stated) that his own experience in Japan and his understanding of the Japanese economy’s lost decades as a “successful” response to balance sheet recession might be one source of this view. But the

relative stability experienced by Japan despite slowing growth cannot be severed from its global context. To again emphasize the point raised by Benanav: the Japanese collapse was only prevented from generalizing by American intervention. This intervention then immediately spurred a new sequence of asset bubbles in the US (first the dot-com bubble, then the housing bubble, as explained in Brenner 2003). In the wake of these crises, the limits of the global system fueled by US debt are becoming apparent. Koo's theory hints at all of this and Koo seems genuinely terrified of the rise in far-right populist movements worldwide and the protectionist sentiment they're founded on, but he remains fundamentally unable to even acknowledge the true severity of the problem because he cannot imagine that the economy itself might contain immanent forces that guarantee its eventual breakdown. This is, again, because his basic definition of "the economy" is the same as that of most economists today and of the political economists who preceded them: a machine for serving the needs of consumers.

What this means is that modelling "the economy" requires only an understanding of the flows and subsequent balance or imbalance that arises between buyers and sellers. In this regard, crisis can only be theorized as some sort of disequilibrium caused by underconsumption or "overproduction," and even "overproduction" here would more accurately be conceptualized as underselling. In other words, disequilibrium is derived from some sort of constraint on one side of this equation or the other: either buyers cannot buy enough or sellers cannot sell enough. Production and profit do not come into play except as ancillary variables determined by the core dynamics of purchase and sale (here we return to the old joke that, to train an economist you simply need to teach a parrot the words "supply and demand"). For the national economy, Koo offers a particularly interesting attempt to theorize these possibilities, since he does not focus on underconsumption among workers (a la Keynesian lack of effective demand) but instead among

private businesses. The theory of balance sheet recession is therefore a theory of corporate Keynesianism, where lack of effective demand within firms leads to a deflationary spiral unless the government steps in with fiscal stimulus. It is an interesting theory of underconsumption because it ultimately presses the limits of underconsumption as such through its focus on private businesses, indicating again and again that "consumption" here is not really the problem, since consumption in this case means corporate borrowing to fund investment. The theory gestures in the direction of profitability and production but ultimately disguises these problems as a simple question of corporate borrowing and debt.

Koo offers a symmetrical theory of crisis in the alternate swing of his cycle (the *yang* phase), when corporate borrowing is healthy and firms are seeking to maximize profit. Here there is no similar indication of any underlying problem of profitability, since he seems to imagine instead that crises are rooted in a problem of underselling. Underselling can appear as either an artificial constraint imposed by the government as old fiscal policy now "crowds out" private investment and thereby constrains the amount that the private sector can sell, or as the "overenthusiasm" of firms caught up in the "vigor" of an upward swing, which leads to supply overshooting demand, with the resulting product remaining undersold. Such crises therefore appear as crises of overproduction. But in both cases, the fundamental problem is one of disequilibrium. This same logic is then extended by Koo to the global situation, where it results in a theory of a global imbalance in buying and selling, only rendered now in the form of national accounts. This only further obscures questions of profitability, however, since focusing on national accounts tends to wash away the relative prominence of corporate balance sheets in the data, which now includes the state and household sectors.

Wherever Koo does seem to gesture beyond the standard disequilibrium theory, he ultimately disavows the logical consequences of his own inquiry by deferring to the capacity of countries to "innovate" in order to regain competitiveness or climb up the Lewis Model developmental ladder. Similarly, throughout his several books, Koo never once addresses the real technical or geographical character of the economic trends he attempts to trace, even while he hints that these are important factors in rising inequality and declining productivity. But if "innovation" is to be anything other than a mystical category, it must have a concrete technical dimension and it must contribute in some way to the formation of future territorial production complexes—including their location (does the innovation require labor-intensive production, for instance?) and their shape (does the innovation encourage more geographic agglomeration, does it tend to encourage littoral development, what kind of preceding infrastructural endowments are necessary for it to be realized? etc.). Is it possible, contra Koo, that there are *technical* limits to innovation, either at the micro-scale of invention or the macro-scale of the infrastructural capacity needed to enable production?

Answering these questions will begin to draw us out of the circular reasoning of an economy populated with buyers and sellers and instead toward the "hidden abode of production" (Marx 1976, p.279) where the true dynamics of the social system are to be found. To do this, however, requires moving beyond the work of Koo an instead focusing on another economic model that has become increasingly popular in the years following the Great Recession, due to its attempt to illuminate the long-term trends in declining productivity within the US economy. This is the work of Robert J Gordon, whose 2016 book, *The Rise and Fall of American Growth*, tears through the mystical notion of "innovation" deferred to by Koo and most other mainstream economists in order to argue that technical change does not simply arise from the imagination of

entrepreneurs and may have its own immanent constraints. This means that it is not, therefore, something that can be in potentially infinite supply given the proper institutional cultivation. No matter how good the education system is or how balanced the global trade structure might be, Gordon argues, contra Koo, that none of these things can condition the emergence of new technologies comparable to many historical inventions in their transformative effect on production and society more broadly.

# Section 2 - Gordon

**The Arc of Technology**

While standing in contrast to Koo for all these reasons, Gordon's work also remains well within the greater cosmology of economics. If anything, Gordon himself is a far more prototypical economic thinker since he his career has largely been made within the academy, whereas thinkers like Koo, Klein and Pettis and Howell have all been forced to balance academic economics with the practical realities faced by investors on the ground. Rather than advising hedge funds or working in finance, Gordon studied under Robert Solow at MIT and wrote one of the discipline's most popular macroeconomics textbooks. While he's certainly engaged in the sort of government consulting work common in the discipline, such as when he helped to assess the official US CPI measure (as in Boskin et. al. 1996), the bulk of his contributions have been academic. This means that his work is far more attentive to precise, econometric questions of measurement, is much more aware of major debates and schools of thought within the field and is part of the larger edifice of "New Keynesian" that is now increasingly becoming enshrined in

official orthodoxy (Gordon 1990). At the same time, it also means that his work lies further from the more pragmatic context of authors like Koo and Howell, who take the investment logic of individual firms as their starting point and recognize the practical centrality of the balance sheet—and, therefore, at least distantly address questions of profitability—rather than merely portraying these as incidental features of a larger macroeconomic landscape, as Gordon often seems to do.

Gordon's fidelity to the folk-scientific cosmology of economics is apparent from the first page of the work, where he treats fundamental categories like growth and output as trans-historical, offhandedly gesturing to the conditions of Europe after the fall of Rome as one example of a period in which "there was no economic growth," before proceeding to compare the mild increase in output in the Middle Ages to the extremely rapid growth of America in the early 20$^{th}$ century (Gordon 2016, p. ix). It is of course possible to make such rough comparisons in terms of sheer material to track certain long-term trends in human society over time. But here it hints at a deeper commitment to one of the founding myths of economics: that "the economy" as we understand it today has always existed—which also implies that something like "capitalism" has always existed within human society, even if constrained. Here, Gordon prefigures the classic myth that the rapid increase in output after the end of the Middle Ages is explained as some sort of "freeing" of these inherent capitalistic tendencies through some special cultural or institutional feature of Europe (*a la* Weber). These tendencies are themselves defined in equally vague terms as a certain impulse to truck and barter and to "innovate." Every feature of this story has been systematically disproven by economic historians—either through comparison with Asia (Banaji 2010, 2020 and Liu 2020) or through the anthropological treatment of value and the "myth of barter" (Graeber 2001, 2014)

This may seem like a minor aside, but it is integral to understanding the overall framing of Gordon's project. While his work is founded on extensive research into historical data on the US economy and the effects on growth exerted by particular clusters of inventions, it is analytically committed to the idea that transhistorical comparisons of categories like invention and output are not only possible, but more or less unproblematic. For Gordon (as for economics as a discipline) these are apples-to-apples comparisons. His theory is thus a *general* theory of technological change over time and he frames it in explicitly transhistorical terms, implying that even the most wide-ranging social transformations or historical and anthropological differences between societies result in no essential qualitative difference between such categories. Economic anthropologists and historians (see Banaji 2010, Gudeman 1986, Graeber 2001 and 2014), however, have consistently shown that such apples-to-apples comparisons are illusory, with even simple categories such as "technical change" or "output growth" so fundamentally tied up with other social functions that it is impossible to make such apples-to-apples comparisons. Doing so will tend to disguise qualitative social differences that are themselves integral to understanding trends in those categories over time. Similarly, seemingly quantitative shifts (such as exponential growth rate increases after the middle ages) are usually better explained in qualitative terms, understood in relation to fundamental transformations in the social character of production within the societies in question.

This is an important theoretical distinction, although not one that necessarily hinders Gordon's more practical insights. His real focus is, after all, on technological change within capitalism specifically, and in fact within a single country during a timespan of just over one century. Although his trans-historical understanding of the broad categories in question will ultimately weigh down his broader conclusions, it is not immediately relevant to understanding

his methodology or the basic line of argument that emerges from it: At its core, Gordon's work posits that American "economic growth has varied systematically over time, rising to a peak in the mid-twentieth century and then falling" (Gordon 2016, p. ix) and that this has been caused by underlying trends in the rate of technological innovation. This also implies that the potential for future growth is premised on the emergence of new technologies. But Gordon is explicit in his opposition to the "techno-optimists" who argue that "robots and artificial intelligence" are just such innovations capable of guaranteeing future growth, arguing that "the main benefits of digitalization for productivity growth have already occurred […]" (ibid, p. xi). In contrast, he offers a pessimistic forecast premised on the claim that the bundle of technical advances that fed the growth burst of the 20th century was so unique as to be basically unreproducible, at least on our current technological horizon. Gordon even characterizes the period as "a revolutionary century when, through a set of miracles, economic growth accelerated," but after which "the potential for future inventions having a similar impact on everyday life of necessity was inevitably diminished" (ibid, p. xii). Innovation thus takes on an almost mystical character, as "a set of miracles" that cannot simply be repeated.

While the language might be hyperbolic, Gordon's actual methods are deeply pragmatic. Growth and productivity are measured in multiple dimensions and accompanied by more qualitative inquiries into the technical character of specific inventions, alongside a general historical narrative tracking basic demographic shifts and changes in the composition of the labor force. Throughout, contrasts are made between three roughly 50-year periods: 1870-1920, 1920-1970 and 1970-2014, with some broader data referring back to the earlier century of 1770-1870, which he characterizes as a "transition century" marked by slow growth (ibid, p.2). The real focus, however, is the more general contrast between the "special century" of 1870-1970 and our

current conditions of rapidly slowing growth. In fact, Gordon argues that the period from 1870-1970 is "special" in absolute terms, having been witness to "a singular interval of rapid growth that will not be repeated" (ibid, p.3). While this dating scheme is attuned to the United States, the overall argument is nonetheless making a global claim, since the US has been "the nation which has carved out the technological frontier for all developed nations since the Civil War" (ibid). Meanwhile, using the year 1870 as one of the major dividing lines is both a practical and theoretically significant choice. On the one hand, it was not until 1869 that the first Census of Manufacturing was carried out, providing consistent and in-depth data that could be used for historical comparison. On the other, the end of the Civil War was rapidly followed by the completion of the transcontinental railroad, with the political and geographic unification of the continent thereafter allowing more consistent comparison through time.

**Residuals**

But the question remains as to how, exactly, one might measure productivity and growth. In many ways, the unique character of the inventions that emerged in this period are irreducible. Throughout, Gordon emphasizes that "the flood of inventions that followed the Civil War utterly transformed life" and, moreover, that "these inventions cannot be repeated" (ibid, p.4). This is one reason that his thorough analysis of economic statistics is always accompanied by narrative exposition exploring the concrete changes in peoples' lives made possible by these technologies. At the same time, their effects on long-run economic trends are measurable, with Gordon employing a series of different variables to capture the phenomenon from multiple angles, including output per worker, output per labor-hour, hours worked per worker and the labor versus capital share of investment.

Throughout, the most important measure by far is Total Factor Productivity (TFP), the "best measure of the pace of innovation and technical progress" according to Gordon, since it captures "how quickly output is growing relative to the growth of labor and capital inputs" (ibid, p.2). All of Gordon's measures show similar trends, however. The growth rate of output per labor-hour in 1870-1920, for instance, averaged 1.79 percent per year, while the same figure rose to 2.82 percent per year in 1920-1970 and fell to 1.62 percent per year between 1970 and 2014. The same trend is visible in output per person, while an inverse pattern appears for hours worked per person: work hours rise very slightly in the earlier and later period, but fall by nearly half a percentage point per year on average between 1920 and 1970 (ibid, p.14, Fig. 1-1). So, in short, the years 1920-1970 represent a period of peaking growth and productivity as well as increased leisure. After 1970, all these trends begin to reverse.

But even though these general figures on output and work hours capture the basic shape of the phenomenon, which Gordon characterizes as "one big wave," it is only when output is decomposed into its different components that the importance of TFP becomes evident. In order to properly understand what this means, however, it is first necessary to explain exactly what TFP is and how output is decomposed. The method used by Gordon essentially follows the standard practice in economics, first piloted by his doctoral advisor Robert M. Solow, the economist who essentially invented growth accounting in its current form. Specifically, Gordon mobilizes a variation of the Solow-Swan model of long-run economic growth, incorporates the relative effects of capital accumulation, population growth (or specifically growth in the labor pool) and increases in productivity. In the standard model (Solow 1956), growth can be rooted in increasing capital accumulation or expansion of the labor force, but even if capital and labor input are held constant, a "residual" growth remains. This "Solow residual" is understood to

indicate a certain type of productivity that exceeds the sheer increase in labor or capital inputs. Solow spoke of it in these terms: as a mathematical residual that results when you subtract the growth rates of the wage bill and capital from the growth rate of the entire economy (in his original paper, national income was actually measured as "private non-farm GNP per manhour," the same logic was only applied to economy-wide GDP later).

Ultimately, treating this residual as a measure of "innovation" constitutes a leap of logic that was fundamentally ideological. The impetus for the model was, in part, to identify long-term contrasts between growth in advanced capitalist economies with the investment-driven growth observed in the Soviet Union (Fusfeld 1998). After all, the figure is a residual, which means that it is itself not directly measurable. Solow himself names this third element of his function "technical change" in a purely axiomatic fashion, further clarifying that:

> I am using the phrase "technical change" as a shorthand expression for *any kind of shift* in the production function. Thus, slowdowns, speedups, improvements in the education of the labor force, and all sorts of things will appear as "technical change." (Solow 1957, p. 312)

Later, this residual was re-named "total factor productivity" to better indicate its real meaning. Gordon himself gives a good summary of what is captured in the category, similar to Solow's own characterization above:

> While often treated as a measure of innovation and technical progress, the residual incorporates every aspect not just of major innovation but of incremental tinkering and anything else that improves efficiency […] (Gordon 2016, p.544)

This is important because TFP can be affected by factors such as the developmental shift from lower-productivity agricultural work to higher-productivity work in urban industry as well as broad institutional factors helping to mobilize labor, demolish obsolete capacity and finance new industrial complexes. Thus, "technical innovation" must be understood at both the micro-technical level of invention and at the macro-technical level of building new industrial complexes to roll out these inventions at scale. Meanwhile, it must be accompanied by some notion of "social innovation," indicating the overall efficiency of the way in which labor is mobilized across society and (re)produced, including in both waged and unwaged activity ranging from education to housework.

Throughout, though, the deeper problem is that the logic of the residual is basically circular: in attempting to account for "growth," Solow measures all the contributions of the factors of production (understood as capital and labor), first presumes that they receive back their contribution—or, in other words, that "factors are paid their marginal products" (Solow 1957, p. 313)—and then observes growth in the national income measure (again, in this case "private non-farm GNP per manhour") in excess of the growth in these factors of production, naming that residual "technical change." He then examines the model with the presumption that marginal returns to the factors of production will decline (i.e. adding a new unit of labor or capital will tend to produce less units of output as more labor and capital are added), in which case this excess becomes more important in accounting for long-run GNP growth since, in this scenario, he demonstrates that the residual accounts for a larger share of total growth over time. But all of this just returns the initial question full circle, showing that economic growth as such exists, after all, meaning that it is not simply a linear function of the growth of the labor force and the materials of production. While this might have been a useful demonstration to contrast the long-

run potential of US growth against the investment-driven growth of the Soviet Union (which in the eye of economists such as Solow was essentially based on continual increases in the factors of production, leading to diminishing returns), it does not actually answer the fundamental question about the sources of this growth as such. Instead, it defers that explanation to some sort of vaguely defined and seemingly exogenous cause which itself is not understood to be immanently related to the dynamics of the economy as such. In other words, Solow identified some sort of residual *surplus* in the measurement of national income growth beyond that paid out year to year on labor or spent on plant and machinery. But this surplus remains a mathematical residual which is simply asserted by Solow and the economists who followed him to be a measure of "innovation." Innovation is not, however, demonstrated to bear any relationship to any of the other terms in the model. Moreover, as is made clear by Solow's own explanation and the ways in which TFP has come to be conventionally understood, this residual growth is most strongly associated with either basic institutional factors (like investment in education) or with nearly mystical cultural ones, such as a society's cultivation of entrepreneurial energies. Overall, the important point is that these are treated as exogenous assets. In particular, this surplus is explicitly posed *as if it did not arise from human labor*, since the model presumes from the beginning that labor as a factor of production is repaid according to its marginal utility.

Solow and the economists who followed in his footsteps all seemed to presume, nonetheless, that the murky factors accounting for this surplus, loosely classed together as "innovation" tended to be cultivated—institutionally or culturally—in capitalist societies specifically. Usually, this case would be made with reference back to the philosophical fundamentals of neoclassical economics, the argument being that, under capitalism, the naturally entrepreneurial predisposition of *homo economicus* is cultivated to a greater degree than in any

non-capitalist society. The inherent philosophical presumption is that there is at least a long-run correlation between capitalist social relations and this "innovation." Gordon's work is interesting precisely because it abandons this presumption, and does so on the basis of more or less the exact same evidence as Solow used to construct the model in the first place. Gordon accepts all the same necessary preconditions as Solow, he understands the TFP to be a measure of "the underlying effect of innovation and technological change on economic growth" (Gordon 2016, p.16) and he treats these factors as fundamentally exogenous, in the sense that they are axiomatically unmeasurable with reference to capital and labor input alone. But then, based on the observed decline in TFP after 1970—which led to a similar decline in overall output growth—Gordon essentially concludes that there seems to be no long-run correlation between "innovation" and the continuation (or, in fact, the globalization) of capitalist social relations.

He is not arguing, however, that we are approaching steady-state conditions or a situation of degrowth, since he argues that growth existed at comparatively low levels prior to the 19th and 20th centuries: "Historical research has shown that real output per person in Britain between 1300 and 1700 barely doubled in four centuries" (ibid, p.ix). The implication here is that even pre-capitalist societies saw some low level of "innovation," related to slow but clearly real technical change—and it is extremely important to remember that economists such as Gordon happily apply categories such as "capital" and "productivity" to pre-capitalist conditions, tending to focus on the unique historical status of "industrial" capitalism rather than "capitalism" as a distinct social system with a clear origin in time. This means that whatever baseline of growth might have preceded the "miraculous" boom of the century between 1870 and 1970 (and which may now be superseding it) is not, for Gordon, necessarily correlated to a distinct social system called "capitalism" at all. Instead, it has more to do with industrialization as a technical

phenomenon. As industrialization recedes, Gordon often seems to imply that our fate may be a return to the slow growth status quo that has prevailed for much of human history—again, this doesn't imply reversion to "feudal" conditions, since industrialization has already occurred and growth, though slow, is still positive and applies to the much larger mass of social wealth accrued via global industrialization.

That said, he, like Solow, still understands the growth boom to be an exogenous factor, not accounted for by outlays on capital and labor alone. Here, he elides the large literature on the origins of capitalism (such as Banaji 2010) and even on technical change in history (such as the the "Needham Question," originally proposed in Needham 1969, of why the industrial revolution did not occur in ancient mainland East Asia), even as he recognizes that the industrial revolution was more than simply matter of proper investment or supply of labor. Yet while Solow and other neoclassical economists, in the context of the Cold War, might have gestured at some deeper endogenous connection at the philosophical or anthropological level to account for innovation, Gordon abandons even this, treating it as a truly exogenous effect that he frequently categorizes as miraculous, unprecedented, and unreproducible. The industrial revolution, he seems to declare, has been a bounty for the human race. But it is not something that can be repeated. Because of this, he makes no effort to theorize whether there are immanent mechanics in capitalist social relations that seem to both produce growth via this surplus and that lead to some sort of slowdown in growth production over time, associated with declines not in population or capital but in the residual surplus evident in the production function. If innovation is exogenous and the residual is, in fact, innovation, then growth cannot be understood to be wholly or primarily dependent on the internal mechanics of the economic system.

**Technological Miracles**

Gordon's understanding of TFP diverges in a few important ways from the classical Solow model, however, and these divergences are ultimately important to understanding his basic argument and his conclusions. Since Gordon's focus is specifically on technical inventions, he approaches the question of TFP by attempting to control not just for the contributions of labor and capital in narrow terms (i.e. as the wage bill or price of capital stock) but instead in a broader sense that also controls for some of the additional elements that Solow himself included in the category of "technical change," such as education and the variable capacity of capital utilization and depreciation. In decomposing output, Gordon first breaks it down into a broader category of "Education" that attempts to capture the more "qualitative" aspect of labor, using data derived from a book by Claudia Golden and Lawrence F. Katz (2010) which explores overall investment in wages and education as they relate to technical change. This more expansive "Education" category plays the same role in the production function as Solow's category for Labor.

Gordon then breaks output down further into a category he calls "Capital Deepening," which is similar to Solow's Capital variable but includes a far more intricate measure of depreciation that accounts for sharp changes in the real rate of replacement (see Gordon 2014, pp.659-661). Finally, he receives a residual value, marked as TFP but now conceptually focused even more on the role played by technological invention (and, as will become evident below, general social-institutional efficiencies) since capital restructuring and educational investment are better accounted for. The result is a breakdown of growth in output per hour across the three time periods that not only shows the peak in the years 1920-1970 but also demonstrates that this peak is almost entirely composed of TFP growth. While the growth rate of "Education" and "Capital Deepening" is similar across all three periods, "the margin of superiority of TFP growth

in the 1920-70 interval is stunning, being almost *triple* the growth rate registered in the two other periods" (ibid, p.16, see: Figure 1-2). Thus, factors beyond just investment in labor and capital seem to be far more important to the US growth peak of the 20th century, which is itself often pointed to as the "golden era" of capitalism as such.

From this, Gordon concludes that a unique "set of miracles" created what he calls a "great leap forward" in productivity in the American economy in the early middle of the 20th century (ibid, p.535). In a further breakdown of TFP by decade, it becomes clear that the increase begins in the 1920s, accelerates steadily through the 1930s and then spikes sharply over the 1940s. Interestingly, the "golden era" decades of the 1950s and 1960s only see TFP growth comparable to that of the 1920s or 1930s. Then, by the 1970s TFP plummets and never regains the growth rates seen in the middle of the century (ibid, p.547, figure 16-5). In order to account for these trends, Gordon points to the rapid rollout of technical inventions in these years. Specifically, he cites the work of the economist Alexander Field, from whom he borrows the characterization of the period as a "great leap forward" (Field 2012). Field argues that the 1930s, though marked by the Depression, were also time of vigorous scientific innovation, with many new inventions emerging and others (such as the motor vehicle) seeing rapid improvements. But while such inventions and improvements may have existed in the patent record, this didn't guarantee industry's ability to roll them out at scale. Many, meanwhile, were better categorized as dependent sub-inventions, rather than the emergence of widespread new general purpose technologies such as electricity or the internal combustion engine.

It was only with the New Deal and World War II, Gordon argues, that the numerous inventions that had arisen in subsequent decades could actually be deployed across the economy—and even then often only through outright state investment as "the federal

government financed an entire new part of the manufacturing sector, with newly built plants and newly purchased productive equipment" (Gordon 2016, p.18). These often involved the combination of the myriad new sub-inventions of the 1930s with both general purpose technologies that may have already existed but now, through general electrification, for example, allowed for the widespread adoption of electric machine tools, underground subways, and a whole series of consumer appliances. But the rollout of these technologies depended on large, coordinated investment drives associated with the New Deal and the war. All of this is loosely tracked in patent records and associated technical handbooks, which grow rapidly in these years (ibid, p.552-555). Similarly, it's visible in the fact that "the number of machine tools in the U.S. *doubled* from 1940 to 1945," with institutional factors playing a clear role here, considering that "almost all of these new machine tools were paid for by the government rather than by private firms" (ibid, p.553). Thus, Gordon finds himself making what most would think of as a classical "Keynesian" argument, only viewed through the lens of productivity rather than effective demand.

This impression is reinforced by the emphasis he places on factors like higher wages, shorter working hours and widespread unionization, which he argues helped boost productivity growth: "higher real wages by promoting substitution from labor to capital during 1937-41 and shorter hours by reducing fatigue and improving efficiency" (ibid). While it might seem as if other factors are behind these trends, Gordon systematically disproves other possible causes or shows that they are not primary: urbanization was no faster in 1920-1960 than it was in 1890-1920, for instance. At the same time, he does point to the partial influence of other demographic and institutional factors, such as the drying-up of immigration after the 1920s and the general closure of the US economy through increased tariffs. This made it easier for unions to push for

higher wages while "the high tariff wall allowed American manufacturing to introduce all available innovations into U.S.-based factories without the outsourcing that has become common in the last several decades" (ibid, p.554). Similarly, while the introduction of such technologies began with some government-led facilitation in the 1930s (particularly in large public works projects focusing on key infrastructure), the central role of the state would become immensely clear during the war years, which is precisely where Gordon identifies the most rapid growth in TFP. The war is characterized as a period in which a widespread process of technical learning was engaged in across industry, as "virtually every firm making consumer goods (except for basic food and clothing products) had been forced to make something else" and thereby gained a wide-ranging, practical familiarity with the newest technologies (ibid, p.550). But this was not just a matter of pre-existing private firms meeting government demand. It was also a period in which private investment in general stagnated, replaced by "the creation of new plant and facilities paid for by the government but operated by private firms to produce military equipment and supplies" (ibid, p.552). All of this created lasting productivity gains that didn't subside after the war, because, in Gordon's words and with his emphasis: "*technological change does not regress*" (ibid, p.550).

Overall, he argues that micro-technical innovation in the form of backlogged new inventions—all founded on certain key general purpose technologies like electrification—were combined with macro-technical innovation in the form of new or refurbished industrial complexes and the mass roll-out of new equipment to produce a "miraculous" period of productivity growth. The combination of the micro and macro scales here is, moreover, only enabled through a more general social innovation embodied in the top-down mobilization of the state and the bottom-up mobilization of the workforce—this bottom-up mobilization included

both the mass mobilization (often against the state itself) that birthed the era's unions and the more general patriotic mobilization of the war years. This final point is particularly important, since Gordon treats the bloody class struggles of the Depression era as essentially a non-issue, focusing only on their positive results in the form of higher wages and greater rates of unionization. Ultimately, Gordon offers what might be thought of as a deeply pessimistic Keynesianism.

This pessimism is first evident Gordon's account of the "great leap" made by American industry in the middle of the 20th century, since it becomes clear, by his own evidence, that such miraculous TFP growth would not have been possible without the equally devastating series of bloody class conflicts that marked the Depression, not to mention the brutal destruction of the war itself. Proposing a repetition of such events would, of course, seem preposterous. But it at least seems initially possible—a wartime mobilization without a war or a new deal without a depression, posed instead against the challenge of climate change, as has recently been the argument put forward by supporters of the Green New Deal. The problem here is, first, that all the factors Gordon himself points to actually *required* the bloodshed that led to them. Higher wages did not emerge from the goodwill of employers and the New Deal itself was preceded by mass unrest, epitomized by the Bonus Army's march on Washington in 1932. Meanwhile, the rollout of new industrial facilities in the US during years of war and depression was paired with the destruction of old industrial facilities by those same factors: the rise of the American manufacturing behemoth after the war was made possible, in part, by the fact that the war itself had reduced the industrial territories of all competitor nations to rubble. This will be key to understanding the geography of industrial competition that would then occur within the global economy in later decades (as argued in Mattick 2011). But Gordon is also pessimistic in a more

absolute and explicit sense, arguing that the general purpose technologies that enabled the rapid advance of inventions through these decades aren't things that can easily be repeated. Thus, even if a Green New Deal were possible politically and financially, Gordon argues that there is no technological basis by which it might reproduce the miraculous growth of the postwar period. This becomes clear in his account of the stagnating productivity growth that begin to plague the US economy in the 1970s.

**Stagnation**

In Gordon's account, the postwar "golden era" cannot be understood without reference to the rapid innovation of previous decades. He points out that the 1950s and 1960s were hardly comparable to the 1930s and 1940s in their rate of productivity growth and the rollout of new technologies. Instead, they were simply the period in which the new technologies first implemented in the depression-era public works projects or in the midst of war could now exert a clear effect on the peacetime consumer market. One stark result was the boom in housing and highway construction, which enabled similar transformations in markets for household electric appliances (particularly important was air conditioning) and the possible distribution range of food products (notably, though, industrial transformations in the production and processing of food had made far more significant gains prior to the 1950s). Changes in transportation patterns also grew from the new highway system, accompanied by greater consumer access to motor vehicles, all of which further enabled more sprawling urban development. Meanwhile, a similar transformation emerged from the rising accessibility of air transport, both air and auto transit more or less completely displacing long-distance passenger rail in the postwar period.

While cars, electrified housing and highways all represent the gradual rollout of older inventions across the wider population, air travel is the one case where an invention that was only in its infancy in the 1940s was finished off in subsequent decades. But even in this case, "the ten years between 1926 and 1936 witnessed by far the greatest advances in the history of commercial aircraft," with the only comparable advance after that being "the introduction of jet travel in 1958" (Gordon 2016, p.393). This is also a case that hints at Gordon's deeper argument about a general technological stagnation, since "in the nearly six decades since the first jet flight, there have been no improvements at all in the speed or comfort of air travel" (ibid). If anything, the quality and comfort of air travel have actually undergone a substantial decline since the 1960s (ibid, pp. 405-407). Almost all of the other major advances that occurred in the decades after 1950, ranging from television to space-race aeronautics, exerted comparably less influence on the growth rate of productivity. Productivity growth remained relatively strong in the 1950s and 1960s, but it was still lower than during the years of war and depression. While the rollout of hallmark consumer technologies would see these decades inextricably linked to the "golden era" in the popular imagination, the influence of something like television on general productivity was obviously minimal. This is especially true when compared with the general purpose technologies on which such an invention relied, like electricity. Thus, in Gordon's timeline, the 1950s and 1960s were decades largely buoyed on an earlier boom. As soon as the extension of depression and wartime advances to consumer markets began to run up against its limits, the growth in productivity began to decline. The trough would be reached in the 1970s, after which there would only ever be mild recoveries, quickly reversed.

In arguing that there was never a true recovery from the trough of the 1970s, Gordon's narrative comes into opposition with both common sense and the accepted wisdom of many

economists. While most economists would recognize the turmoil of the 1970s, since it was a decade filled with wide-ranging political and economic crises—though they would account for this in different ways—there is a general assumption that an economic recovery was achieved later and that this recovery was, in fact, driven by technological breakthroughs. The evidence usually given for this claim ranges somewhat, but tends to focus on rapid advances in telecommunications culminating in the emergence of the digital era, innovations in aeronautics allowing the regular launching of satellites during the late cold war, as well as containerization and the revolution in logistics. All of these technologies, taken together, helped to enable the globalization of production that marked subsequent decades. Nonetheless, Gordon's data is unambiguous. More importantly, his specific focus on productivity controls for the effect of speculative bubbles that might give the temporary illusion of more rapid growth rates measured via apparent increases in national income. Not only does TFP growth trough in the 1970s (with annual average growth at less than half a percent over the decade,) it also stays low for the entire stretch from 1970-1994 (at a mere .57 percent average annual growth) and then, when it does increase slightly again between 1994 and 2004, TFP growth in these years is only one third of what it was in the 1940s and half that of the 1930s. Finally, after 2004, TFP growth drops even lower than it had been prior to the brief increase in the late '90s (ibid, p. 547, figure 16-5 and p.575, figure 17-2). Gordon here unearths deeper roots for the secular stagnation of today, linking it not merely to the Great Recession but instead to a more pervasive technological stagnation that first become evident in the 1970s.

In terms of productivity and its effects on growth, even the influence of computerization and the internet ultimately seems minimal. Here Gordon is essentially repeating the wisdom of Solow, who argued as early as the 1980s that "you can see the computer age everywhere but in

the productivity statistics" (quoted in Triplett 1999), in a conundrum that would come to be known as "Solow's productivity paradox" (Triplett 1999). But Gordon doesn't simply demonstrate this through TFP data. Instead, he attempts an equally qualitative comparison between the bundle of inventions that emerged in the late 20th century and those that he had explored earlier, mostly developed around the turn of the century before being perfected and implemented between the 1920s and 1960s. Computers are a case in point. Their cultural prominence and their influence in the daily life of the consumer simply does not match their role in the economy as a whole: "the share of total GDP represented by computers is too small to overcome the great majority of economic activity where the pace of innovation is not accelerating and, indeed, in many aspects is slowing down" (Gordon 2016, p.441). This share, measured in terms of total spending by households and firms on information and communication technology (ICT), "amounted to only 7 percent of the economy in 2014" (ibid). This ultimately minimizes the impact of the rapid rate of improvement observed for many years in the performance of computers, as famously formulated in "Moore's law," which observed a doubling of transistors on computer chips roughly every two years. Even this improvement has recently begun to stagnate, with physical and economic constraints pronouncing the "end of Moore's law," as slowdowns in the rollout of new fabrication facilities now stretch years behind schedule (Rotman 2020, Acemoglu et al 2014). Moreover, the share of spending in both businesses and households devoted to ICT has remained either flat (for consumers) or declined (for businesses) between 2000 and 2014 (Gordon 2016, p.448, figure 13-3). Nor is there any more recent evidence of an increase in the years since 2014.

Throughout, Gordon acknowledges the deep transformations wrought in everyday life by digital technologies, but he is nonetheless consistent in emphasizing that the ability of people to

order goods to their doorstep via their smartphone pales in comparison to the transformation in everyday life created by the electrification of the household or the implementation of centralized heating and running water. At a deeper level, though, even the most stark transformations of everyday life are not really central to the story. They're used as relatable illustrations, but productivity does not (at least not primarily) lie in the household. Far more important is the fact that the earlier suite of inventions completely transformed industry itself, thereby enhancing society's total productive capacity by an enormous measure. Gordon therefore returns, again and again, to the core of his argument: that there are absolute technical limits to the reproduction of the "miraculous" inventions introduced to industry and everyday life between 1920 and 1970. None of the inventions of the last fifty years that have been so lauded by the "techno optimists," from computers to new biotechnologies, have produced a similar transformation in either everyday life or, more importantly, the productive sphere. The biggest burst in productivity in the entire 1970-2014 period held for only a few years, at the end of the 1990s, which was when the few productivity enhancing effects of previous decades' new technologies became noticeable in economy-wide statistics. But the result was short-lived. More significantly, speculative capital piled onto the real boom with speed and force that it helped to inflate growth rates beyond the underlying trends in productivity (and profitability). This oversaturation led to the tech bubble and subsequent bust. Even then, the boom barely registers as such in the TFP data: between 1994 and 2004 TFP growth increased to an average of 1.03 percent, less than a third of what it was in the 1950s (ibid, p.579).

The result has been that the entire period from 1970-2014—in fact, we can round the number off for Gordon here at 2020 now, since no major breakthroughs have occurred in the intervening six years—has been marked by slowing growth in productivity, reversed only briefly

and temporarily between 1994 and 2004. Meanwhile, this slowing growth has been accompanied by a more and more frantic technical optimism promoted in the cultural sphere, predicting everything from immanent full automation to the settlement of Mars, which has consistently seen its predictions utterly dashed. The trend becomes even more prominent when a basic error in the statistics is corrected, since US manufacturing data conflates the production of computers with higher processing speeds with the production of more computers as such. This is particularly important considering that the ITC sector has led what little productivity growth has been seen across the manufacturing sector in the past twenty years (Houseman 2018). Once this is corrected for, manufacturing productivity growth slows even more markedly for the US. Even more tellingly, the pattern is matched in other countries with advanced manufacturing capacity, as Aaron Benanav points out in a series of articles on the question of automation: "In Germany and Japan, manufacturing-productivity growth rates have fallen dramatically since their postwar peaks" (Benanav 2019a). In fact, in all these countries there has not only been stagnant productivity growth but also an inversion in the relationships between output growth, productivity growth and employment growth. Between 1950 and 1973, the US, German and Japanese manufacturing sectors all saw output growth in excess of productivity growth, accompanied by increases in employment. Between 1974 and 2000, this relationship began to flip, with productivity growth beginning to outpace output growth and employment beginning to fall. After the year 2000, these conditions deepened and generalized (ibid, Table I). Benanav calls this phenomenon "output-led deindustrialization" and claims that it is "impossible to explain in purely technological terms" (ibid). Instead, he accounts for it in terms of "worsening overcapacity in world markets for manufactured goods" (ibid). So not only is there stagnation in output, in productivity and in general growth, as identified by Gordon for the US, but this is also

linked to problems of general deindustrialization and manufacturing overcapacity within the *global* economy.

**Malthus of the Machine**

Faced with all of this, what are Gordon's ultimate conclusions? While his emphasis on institutional factors and state mobilization in explaining the "great leap forward" of the 1930s and 1940s might seem to imply his support for similar expansions today, this ignores his more fundamental point about the way that key general purpose technologies underlay that period of rapid productivity growth. In an expansive review of new technologies forecast by the "techno optimists," ranging from AI to nanotech to driverless cars, he is consistent in maintaining that nothing appears capable of living up to its own promises and certainly not of producing any substantial effect on overall productivity. But Gordon does not follow Benanav in identifying the sources of this stagnation in industrial overcapacity and declining profitability. In part, he places some emphasis on the importance of institutional factors. But mostly the cause is to be found in the fact that the past fifty years have, very simply, been short on "miracles." Ultimately, the implication is that this may indicate deeper technical constraints that mark a "maturity" point similar to that identified by Koo. Gordon, then, is offering what might best be understood as a form of technological Malthusianism, identifying an apparent absolute limit to the growth rate of technological progress and ascribing secular stagnation as such to that limit in a way that bears stark similarity to Malthus's attempt to explain similar long-run economic crises to absolute limits in population growth.

It remains important, however, to examine where and how Gordon does offer methods to alleviate the current conundrum of slow growth, stagnant productivity and the dearth of quality employment that follows. In the final two chapters of his book, he pivots from his main inquiry into the limits of technology to focus instead on an array of broader demographic, social and historical factors that act as further "headwinds that have intervened to prevent most Americans from enjoying real income gains equal to the growth of economy-wide output per hour," even if that output might have slowed (Gordon 2016, p.604). The most important of these headwinds is the rise of inequality, since even the modest gains in productivity and output that have been witnessed since the 1970s have tended to be distributed only to those in the upper income brackets. The result has been that "by several measures, including median real wages and real taxable income in the bottom 90 percent of the income distribution, there has been no progress at all" and, in fact, "average real income in the bottom 90 percent was actually lower in 2013 than it was in 1972" (ibid, pp. 605, 610). Downward pressure on wages has been one clear cause here, but Gordon tends to identify this pressure with institutional decisions rather than structural pressures such as those evident in the global production glut identified by Benanav or the declining profitability central to other heterodox accounts (Brenner 2003, Mattick 2011, Smith 2020). Instead, Gordon claims that this downward pressure has been exerted by the bifurcation of the labor market—long observed by theorists like Piore (1979) and Sassen (1991)—between the highest and lowest paid jobs, with the middle (once composed of "routine" manufacturing jobs) hollowing out over time. But this bifurcation is only explained in vague terms. On the one hand the "disappearance" of manufacturing jobs is apparent in its importance, but Gordon offers no real explanation for the phenomenon other than a single paragraph (p.633) pointing to globalization and competition from China. In other words, we again see reference to the

prominent role played by industrial competition, but without theorization. On the other hand, the institutional factors that he does point to, such as changing policies in education and declining rates of unionization are just as much effects as they are causes.

Two other headwinds—education and debt—follow from inequality but are given no more thorough accounting, while a third (demographics) seems minimal in its ultimate effect. The bifurcation of the labor market has been accompanied by a proliferation of college graduates unable to find jobs befitting their degrees (or to find jobs at all), making the debt that they took out ultimately unsustainable. This of course exerts further downward pressure on wages. Curiously, Gordon here returns to the exact same non-explanations and non-solutions as Koo does in explaining the conundrum of the "pursued" economy and how it might retain its advanced status. Both make general cases for some sort of reforms in education, aimed at increasing the competitiveness of American students relative to those in other countries. Gordon thus returns to the PTA politics of Koo, offering a litany of problems in US education with no real program for how they might be rectified. Even worse, he then jumps into even more conservative territory, bemoaning how all of these headwinds have dissolved family structures at the bottom of the income distribution, caused a rise in crime and further exacerbated the rate at which poor students drop out of high school (ibid, pp.630-632). He then offers a series of policy changes that could potentially address these headwinds: implementing a progressive tax system, raising the minimum wage, imposing a more comprehensive earned income tax credit, reducing the rate of incarceration, legalizing illegal drugs, rolling out a public preschool option, funneling more money into secondary and postsecondary education, cutting back regulatory barriers to innovation, giving citizenship to foreign-born graduates of US colleges, eliminating tax deductions and imposing a carbon tax. These are listed in a series of brief paragraphs, with

absolutely no discussion of how they might affect levels of investment, profitability or even productivity itself (ibid, pp.641-645).

The question remains as to why Gordon, who identifies the growth and productivity upswing of the 1930s and 1940s so strongly with expansive state-led mobilization of industry and labor, fails to endorse similarly expansive public works projects or even all-out wartime-style mobilization of the economy to face the challenge of climate change. The answer is simple: "the potential effects of pro-growth policies are inherently limited by the nature of the underlying problem," which Gordon has already identified as the general technical limits to innovation (ibid, p.643). He makes clear that the private sector itself seems to still be producing new innovations at a steady-enough pace and doubts that any state intervention would generate much more. Similarly, he claims that "there is little room for policy to boost investment, since years of easy monetary policy and high profits have provided more investment funds than firms have chosen to use" (ibid). Here, Gordon doesn't even approach the optimism of Koo, who at least considers the state to play a key role in sustaining investment when the balance sheet recession drives down the private sector's demand for capital. Of all the policies that he does suggest, none at all actually address the fundamental problem that he's identified. While it might seem that educational investment should lead to an increase in innovation, Gordon is clear that this is not the case, since the problem is precisely one in which we have a glut of college graduates and a steady state of new innovations emerging from the private sector which is, nonetheless, hardly comparable to the rate of technical advance observed in the past.

Overall, then, Gordon offers purely ameliorative measures. He proposes plenty of policies that seem progressive on paper, such as drug legalization, decreasing incarceration, raising the minimum wage and imposing taxes on the wealthy. But he's equally clear throughout

that, even if these policies were to be implemented, they would merely result in a more equitable redistribution of a pool of total economic resources which is itself growing more and more slowly every year. Nor does he explore the deeper consequences of such policies on international industrial competitiveness or American financial dominance nor does he inquire into their practicality given the prevailing distribution of power in society. While he suggests, for example, that greater unionization would be a good thing since it would increase wages, he sketches no real path to that goal since he fails to discuss the actual social conflicts that led to the birth of widespread unionization in the first place in the 1930s. This is not to mention the fact that there is in all likelihood a connection between the capacity for widespread unionization and wage increases in the period, undergirded by the fact that growth and productivity were increasing. As Jason Smith argues, this was an era in which "a dynamic equilibrium between a rising labor share of income, stimulating consumer demand, and an equally rising profit rate, encouraging capital investment, was targeted" (Smith 2020, p.67). This implies that increasing wages without increases in growth or productivity are going to put pressure on returns to capital. Similarly, Gordon fails to account for what effect the passage of such policies (even if they were politically feasible) would have on employment and investment. This is because Gordon elides the question of profitability. Even though he at least touches on the productive sphere, he does so as if questions of production were more or less engineering problems. The result is a picture that is simultaneously pessimistic, since it views slowed growth as an inescapable condition that will plague every society beyond a certain level of industrialization, and over-optimistic, since it imagines that redistributionist policies can simply be implemented at a political whim, without generating any sort of economic crisis.

# Section 3 – MMT

**Redistribution**

Even if this is the limit-point of Gordon's technological Malthusianism, it's notable that the suite of policies he suggests—alongside many much more expansive fiscal interventions, similar in kind to those advocated by Koo—have since been incorporated into popular calls for wide-ranging government intervention to guarantee jobs and mobilize economic capacity against the threat of global climate change. The most prominent of these calls has been the formulation of the Green New Deal, modelled on the New Deal of the 1930s. This legislation itself, however, remains in the realm of open-ended policy proposals, offering no systematic theories of how such policy would be able to functionally drag the economy out of its conditions of secular stagnation or even level out the maldistribution of income between the highest and lowest earners. But behind the legislation lies a new wave of classical Keynesian arguments that provide just such a theoretical framework for it. Though many "post-Keynesian" (or left-Keynesian) thinkers (such as Thomas Piketty) have risen to prominence for their theories of inequality and arguments for increasing taxation, only the school of Modern Monetary Theory (MMT) has actually attempted to systematically revise the pragmatic basis of Keynesian stimulus in a way that would (according to its theorists) actually enable the sort of spending required for such a wide-ranging program of fiscal intervention.

MMT has a different significance for the question of global secular stagnation than do the theories of Koo or Gordon. The previous authors have attempted to actually account for stagnation as such. Koo identifies its roots in both the cycle of balance sheet recessions and in a

larger global unevenness of development, contributing to an imbalance of trade. Gordon digs even deeper to focus on the absolute technological limits of production as such. MMT offers no such explanation, simply taking secular stagnation to be a fact and, at best, deferring to classical (self-described) Keynesian accounts of crisis as being rooted in a deflationary spiral of diminishing effective demand from labor. But MMT is significant, however, for the way in which it has increasingly become part of the popular political discourse in attempts to formulate policies that would allow for economic stimulus to be delivered directly to the mass of the population, rather than handed out only to the wealthy (in the form of tax cuts, quantitative easing or even direct stimulus delivered to particular banks and industrial firms). Stephanie Kelton, now one of the best-known MMT theorists, has served as an advisor to the Senate Committee on the Budget and worked with the 2016 presidential campaign of Bernie Sanders. Similarly, Alexandria Ocasio-Cortez, the US Representative most vocal in support of the Green New Deal, has repeatedly cited MMT thinkers to justify the practicality of the proposal. Thus, while MMT may be less thorough or elegant in its explanation of long-run economic trends, its rapid popularization makes it a more than worthwhile focus for understanding the limits being reached by mainstream economics when faced with a widespread crisis in their fundamental cosmology. Since it does not address most of the core dynamics at issue here, it will be reviewed only briefly, emphasizing the point at which it overlaps with mainstream theories of crisis.

In essence, MMT is not so much a theory of money as it is a theory of the state as an economic entity. This is itself a revival of the "chartalist" theory of money, first systematically formulated by Georg Friedrich Knapp in his book, *The State Theory of Money*, published in German in 1905. Chartalism then became an essential component of the theories of certain first-generation "Keynesians," such as in Abba Lerner's notion of "functional finance." Though less

emphasized by many "New Keynesians" and even many "post-Keynesians," contemporary MMT thinkers (many of whom might also self-identify as "post-Keynesian") have developed the chartalist core of the theory as much through an engagement with Keynes, Schumpeter and Lerner as through Knapp's own work (Wray 2014 and Roberts 2019a). This is significant because this has also led them to absorb much of these economists' own emphasis on pragmatism and functional economics, developed in the context of a global depression that seemed to systematically disprove the tenets of economics at the time. Kelton's book, which is by far the most influential and widely read text on MMT to date, constantly emphasizes the mechanics of how monetary accounting actually works in the Treasury and Federal Reserve systems, contrasted against the abstract models of "national debt" and inflation used by economists. In this sense, it echoes one of Mann's core conclusions about the nature of "Keynesianism" as such, with its tendency of "always subordinating questions of principle to those of practice. This pragmatism is essential to all things Keynesian" (Mann 2017, p. 51). Thus, MMT also contrasts with the work of Koo and Gordon in the way that it does not attempt to construct any larger "economic" theory at all. Instead, it is a theory of pure pragmatism, based on the fundamental conclusion that administration of the economy is subject to political power.

If there is a larger conceptual apparatus to be found in MMT, it is at the level of the state and is largely taken for granted. Overall, the thinkers affiliated with MMT do not dig much more deeply into the nature of the political power to which they defer. For them, the state has a fundamental influence over the economic system via its issuing of currency and maintenance of the broader monetary system, but it is both separate from and above the affairs of "the economy" as such. They make this case through reference to the actual history of currencies, but without reference to the more detailed theories of economic historians who have offered theories to make

sense of this history (see, for example: Wray 2004). This is a problem, of course, since the state's myth about its own status as a "public"—rather than ruling class—institution is simply accepted without any critical inquiry. Nonetheless, this repeats another key aspect of Keynes' own work, which would become central tenet of "Keynesianism" as such:

> in all cases, Keynesian reason points to the *centrality of centrality*: to the political function of the state as the sole, if flawed, legitimate universal institution, and to the rational, scientific bureaucracy at the core of modern state function. (Mann 2017, p.53)

Again, the literary style of Kelton's work could not be more clear in this regard: she is constantly contrasting the lofty inaccuracies of academic economists to the thankless work done by "scientific bureaucrats" such as herself (as a consultant for the Senate Committee on the Budget) and an entire pantheon of treasury secretaries and central bankers. Thus, despite their sometimes intentional distancing from the Keynesian tradition, MMT thinkers are, in fact, maybe the best personifications of its fundamental spirit today.

With political pragmatism standing in the way of any point by point comparison with the grander theories of Koo and Gordon, the best approach to MMT is instead to simply review its core propositions and explore their potential overlaps. The first point that stands out most clearly is the notion that the severity of our current economic straits are not mechanically produced by any real underlying dynamics in the economy. Instead, they are the product of a completely political delusion, which Kelton dubs the "deficit myth." In reality, this myth is unpacked into several distinct myths, ranging from a false equation between national budgets and household budgets, to generational costs of government debt, to the idea that the national debt creates a dependency on foreign investors. In fact, a core tenet throughout is the idea that "debt" is an entirely incorrect way to understand what the deficit itself is. For Kelton, the reality is exactly the

opposite, since the national debt is not a "debt" in the normal sense of the word. Instead, the debt is simply "a historical record of how many dollars the federal government has added to people's pockets without subtracting (taxing) them away," with this money "saved in the form of US Treasuries" (Kelton, p.79). Similarly, MMT thinkers invert the normal logic that dictates fiscal policy, often abbreviated to "(TAB)S: taxing and borrowing precede spending" (ibid, p.22). They point out that, in reality, "the government spends first and then taxes or borrows" (ibid, p.23). Though larger theoretical implications are explored for the real function of taxation (if it's not to fund spending), Kelton's emphasis is again on the pragmatic, bureaucratic dimension: the simple reality is that spending always precedes borrowing and taxation in the administration of the actual treasury and federal reserve systems.

To prove this point, Kelton offers a simple explanation of the mechanics of government spending, pointing to the 2019 military budget authorized for the fiscal year. The budget is passed by Congress, which effectively authorizes the spending. At that point, "agencies like the Department of Defense are given permission to enter into contracts with companies like Boeing, Lockheed Martin, and so on" (ibid, p.29). But the government doesn't then go out and immediately find sitting tax funds to apportion for this purpose, nor does it have to immediately proceed into a new round of taxation, siphoning money from the bank accounts of citizens and businesses to pay for the new defense contracts. Instead "the US Treasury instructs its bank, the Federal Reserve, to carry out the payment on its behalf" (ibid). Though this invokes the image of Congress withdrawing money from some sort of national account of stagnant funds, the exact opposite is true. In reality, "the Fed does this by marking up the numbers in Lockheed's bank account […] As the checks go out, the Federal Reserve clears the payments by crediting the sellers' account with the appropriate number of digital dollars, known as bank reserves" (ibid).

The money supply has simply grown in proportion to the new digital dollars added to the economy by the Fed, acting as a sort of "scorekeeper." Nor are these simply back-of-the-house matters of accounting: These dollars will then be used to actually pay for labor, materials, and whatever else is required for the production that Congress has contracted these firms to conduct.

There is no actual requirement here, according to MMT theorists, that the government then follow this spending with taxing and borrowing. They offer an alternate theory of why the government taxes the population (which I won't cover in detail here, but basically amounts to the fact that this forces everyone to use the government's currency) and for why the government chooses to "borrow" in the form of issuing Treasury Securities. In the case of borrowing, MMT thinkers portray Treasuries as "just interest-bearing dollars" which are used "to support interest rates, not to fund expenditures" (ibid, p.36-37). Beyond this, the notion that taxing and Treasury issuance are required for government spending is a purely political illusion. Kelton's book debunks this myth from almost all possible angles, at every point emphasizing that these are political decisions that could simply be changed at the whim of legislators. Meanwhile, in contrast to the myth of the deficit crisis, Kelton counterposes the "real crisis" of mass homelessness, growing unemployment, stagnating wages, ecological catastrophe and growing unrest. In so doing, she implies that these are not outcomes of the dynamics of the "real economy" but instead purely *political* outcomes, determined by legislators' adherence to the myth that taxing and borrowing must precede spending and that budgets must be "balanced," in the end.

This does not, however, mean that there are no real limits to spending. And this is the point at which Kelton and other MMT theorists' predominantly technocratic narrative of government spending runs up against the "real limits" and "real spending constraints" of the

economy. This is also where the reality of their overlap with the grander theories of Koo and Gordon begin to become apparent. Rather than emphasizing mythic budget constraints, MMT thinkers perceive themselves as putting forward a redefined form of "fiscally responsible budgeting" that "prioritizes human outcomes while at the same time recognizing and respecting our economy's real resource constraints" (ibid, p.40). In this regard, two key signals from classical economics are particularly important: unemployment and inflation. Against the idea of a "balanced budget," MMT argues that it is the economy itself that is supposed to balance. The deficit can, in this sense, be too large or too small. But such an imbalance is not judged relative to some ill-informed notion of "how the money will be paid back" (it won't be and doesn't need to be, as Kelton demonstrates). On the contrary: "the federal government has historically almost always kept its deficit too small" (ibid, p.43), and this fact is indicated by the fact that unemployment has been too high: for MMT thinkers, "evidence of a deficit that is too small is unemployment" (ibid). By contrast, evidence of overspending is to be found in inflation. These factors compose the "real limits to spending," but MMT thinkers offer "better ways to manage those kinds of inflationary pressures" than the current monetarist practice (wherein the Fed sets the interest rate by setting the price of credit with the goal of diminishing inflation).

In fact, MMT thinkers consistently point out that "many of the world's major countries have been desperately trying to solve the opposite problem—underinflation—for a decade or more" (ibid. p.45). In this sense, they repeat the observations of Koo and Gordon, since these deflationary conditions are deeply related to the trends in economic growth and productivity reviewed above. But for the MMT theorists, this is not a sign of some deeper crisis in the real economy. Instead, it is a political artifact, with its causes to be found in central bank policy and congressional legislation rather than the deep inner workings of development models, global

trade balances and the technical composition of production. Thus, for Kelton, deflationary conditions and high unemployment clearly signal that the government is underspending, even if it is running a deficit. Underspending is defined as "when we run our economy below its productive capacity" (ibid, p.60). Any amount of unused capacity signals underspending and it will manifest in persistently high unemployment and equivalent deflation. Both of these features of contemporary high-income economies signify that, even if they are running deficits, significant slack remains before the "real limit" of inflation kicks in. Thus, Kelton joins many other MMT thinkers in advocating that mandated full employment in the form of a government jobs guarantee might be one of the best tools to spur the economy out of these deflationary conditions. When private sector spending is well below the real limits of the economy (as signaled by inflation and unemployment) then public sector spending must step in to make up the difference, forcing the utilization of otherwise underutilized capacity (ibid, pp.59-63).

**The Critique of MMT**

At first, it seems as if there is little to critique about MMT precisely because so much of its project is posed as merely descriptive. As Kelton emphasizes again and again, this is how government spending *actually already works*. Similarly, she portrays herself and other MMT thinkers as merely advocating that politicians recognize this fact and act on it, rather than continuing to believe in "the deficit myth." But there are a series of underlying presumptions within MMT that indicate deeper material constraints which would quickly rein in any of its policy proposals. Though it might seem obvious to argue that inflation would be primary among these, Kelton and others do a more than sufficient job of demonstrating why this is not the case—at least for any nation that has its own sovereign currency or, more accurately, a high

degree of *de facto* sovereignty over its currency—and it is apparent that underinflation, rather than overinflation, has been the major difficulty for most advanced economies in previous decades. The Japanese case explored by Koo is among the more systematic proofs of this new reality.

At the deeper level, MMT itself is a sort of automatic ideological expression of an already-prevailing financial reality. These basic conditions are laid out most clearly by Jamie Merchant, in one of the best theoretical appraisals of MMT to date:

> for over a decade now the balance sheet of the Federal Reserve has steadily expanded, through all of its various monetary stimulus programs, by seven trillion dollars and counting. In response to the COVID-19 pandemic in early 2020, the Fed intervened in global credit markets with an unprecedented commitment, if necessary, to flood the markets with a deluge of dollars over three times as large, some $23.5 trillion. In the orthodox view, a monetary supernova of this scale should have vaporized the value of the currency, burning it away through hyperinflation, but in fact the dollar has actually appreciated. In effect, MMT provides a formal theory for the current, de facto state policy of unlimited monetary expansion, as the central banks continue to flood private banks and corporations with tidal waves of free money, like some gigantic iron lung pumping oxygen into the comatose body of a braindead patient. (Merchant 2021)

In effect, what Merchant argues is that MMT is addressing one dimension (the role of the state and its central banks) of the rising dominance of portfolio investment and the centrality of global liquidity identified by Koo. But, whereas Koo sees this rise of financial capital as a fundamental threat, MMT argues that a state presiding over a fiat currency (and specifically, the US state) is able to mobilize this very fact to its advantage: making use of the apparent slack in the economy

and subsequent high demand for the safest possible securities (i.e. Treasury Bonds, which Kelton simply refers to as "yellow, interest-bearing" dollars as opposed to "green" dollars that do not bear interest) to fund wide-ranging social programs.

Overall, Merchant's critique focuses more on placing MMT in historical context and pointing out its fundamental theoretical flaws in its uncritical appeal to an ahistorical state unbeholden to class interests and its failure to capture the broader functional distinctions that money takes on (through a changing relationship to production) in the transition to capitalism. Among his most important observations is the identification of MMT as a foreboding historical echo of the period of imperial decline that spawned classical Keynesianism: "If the British aristocrat Keynes was writing at the twilight of the British Empire, now the MMT movement emerges in the darkening glow of another senescent, slowly dying superpower, offering similar remedies to stay the terminal decline of American capitalism" (ibid). He does not, however, explore the potential consequences if MMT policies were to be put into practice. Does the MMT reversal of Koo's narrative signal an alternate path out of the secular stagnation that remains unresolved in the work of Koo and Gordon—not to mention any of the many other explicitly "New Keynesian" and "post-Keynesian" thinkers? While this is certainly the claim made by Kelton and others, the reality is that their solution resembles the more extreme interpretation of Koo's work by Klein and Pettis, who come only barely short of proclaiming that the US must exert its rights of imperial seignorage to "rebalance" the global economy.

One difficulty that plagues any critique of MMT is the fact that its adherents span the political spectrum. Though it has become extremely popular in the last few years largely through its dalliance with democratic socialism and Kelton's personal role in the Bernie Sanders campaigns and on the Congressional Budget Committee, the reality is that MMT is as beloved by

thinkers on the far-right as on the center-left. Merchant points out that the theory has received "enthusiastic reception by a fun assortment of libertarians, plutocrats, war criminals, and corporate-nationalist reactionaries" (ibid), ranging from hedge fund manager Warren Mosler to Donald Rumsfeld, notorious architect of Reaganomics "trickle-down" theory Arthur Laffer, the Trump-supporting economist John Carney at *Breitbart* and even senator Marco Rubio, "who approvingly cites the prominent MMT economist L. Randall Wray in his corporate nationalist manifesto" (ibid). Rather than simply representing the eclecticism of heterodoxy, however, this seemingly contradictory pool of support points to a deeper consistency in the theory: it is, in essence, an argument for the national revival of US industry through the forceful reimposition of American dominance in global monetary and financial regimes.

This is the narrative subtext of Kelton's book, made even clearer in the mobilization of MMT by democratic socialists and right-wing nationalists in their dueling visions of a revitalized America. But narrative flourishes alone aren't proof of the point. At one level, the natural outcome of MMT policies are already evident in their inherent methodological nationalism: by focusing on the state as the enlightened caretaker of the nation's prosperity, the theory begins from an axiomatic position that excludes the reality of the global economy. This is precisely why the "real limits" and "real productive power" of the economy are only vaguely gestured toward by Kelton, who largely limits herself to commentary on legislative policy and the accounting practices of the Fed and Treasury. On the one hand, she acknowledges that there are real limits and that these are indicated by inflation. On the other, she argues that there is a high level of "unused capacity" in the economy, which should be put to work. It is on these two points that we can identify the real implications of the MMT program and the way in which it proposes to solve the problem of secular stagnation.

While "inflation" in the traditional sense—i.e. intra-economy inflation in the prices of consumer and producer goods bought and sold entirely within a national economy—is not a real limit in our present economic conditions, there is a second kind of inflation that remains regardless: the inflation of the exchange value of the dollar against other currencies. It is here that MMT overlaps with the theory of Koo and similar interlocutors such as Klein and Pettis, who all argue in different ways that a rebalancing of the global economy must be pursued, at least in part, via a structured devaluation of the dollar. In this regard, MMT policy could bifurcate in two contradictory directions: 1) the linkage between taxation and spending could be severed while maintaining the link between "borrowing" and spending, since MMT thinkers point out that this is does not incur real "debt" but instead is simply a different form of currency issuance (via the interest-bearing "yellow dollars" of Treasury Bonds). In this case, bond sales would continue and in fact increase rapidly, meeting the immense demand among global investors (identified by Howell and Koo) for the most secure possible assets. 2) spending could proceed purely through the "printing" of money, with no need to issue bonds matching this spending.

In the first scenario, the present global imbalances persist. In fact, the value of the dollar would likely rise since there is so much sitting demand for secure assets like treasury bonds—and the bond market has been integral to sustaining the dollar's valuation for decades. A rapid foreign sell-off of bonds was an enormous fear in the early Obama years (as documented in Tooze 2018, chapter 1), since this would lead to a sudden decline in the exchange value of the dollar and the tanking of the American (and British) financial apparatus, so much of which was built on facilitating what were essentially complex carry-trades simultaneously profiting from rising US asset values and an artificially depressed RMB, which allowed for a cheapening of

consumer goods. In the end, the Obama-era fear was misplaced, since Chinese authorities had more interest in keeping the value of the RMB low—which they did through systematic purchasing of Treasury Bonds. Thus, the pattern in previous decades has been one of an inflated dollar and a deflated consumer price index given many manufactured goods are imported to the American market (this is essentially the pattern described in Klein and Pettis 2020). The first MMT scenario keeps all of this in place. It presumes (correctly) that demand for bonds will remain high. What it doesn't explain is how American industry could be revived on this basis, since its goods will remain uncompetitive and its production unprofitable.

For thinkers like Kelton, this does not actually seem to be a large concern because they argue (incorrectly) that government make-work programs are equivalent to work in private industries, since they both result in the production of goods and services that meet social need. The key difference, of course, is that capitalist production is not production for social need but production for profit—and this is how the system grows through "expanded reproduction" (explained in Marx 1973, chapters 23 through 25). Thus, even if a jobs guarantee focused on "care work" and "green industry" were to be implemented to achieve "full employment," it's not evident that this would lead to more general investment, higher productivity or increased growth rates. In other words: even the most systematic implementation of MMT policies in scenario 1 would not solve secular stagnation. That means that inequality would continue to increase and that *even if full employment could be reached before consumer inflation kicks in*, this would be little more than an expansive welfare system paid for not by taxation by instead by the rest of the world, exerted through American seignorage rights within global capital markets. It is essentially an argument for the classic linkage between imperialism and social democracy (as identified by Bukharin 1929) to be intentionally reinstated through the US financial system. It would work

insofar as it could guarantee continued dependence on the US dollar, which would facilitate a further depression of labor costs in overseas factories and effectively cheapen resources extracted from dependent economies, since FDI and most international trade goods are often denominated in USD, with limited foreign exchange reserves often a bottleneck to industrialization for this very reason (as emphasized repeatedly by Koo, Klein and Pettis). It's not clear, however, that this system *can* continue to work, because the development of China's productive capacity has reached a point where no amount of manipulation can sufficiently depress labor costs in low-end industries, meaning both that these industries must be shed to other locations (which, given the sheer size of the Chinese working population, is a difficult problem, and will be appraised in Chapter 4 below) and that Chinese industry will continue to climb the value chain and come into more open conflict with American, Japanese and German competition (all of these dynamics will be explored in more detail in Part 2 below). Meanwhile, it's not at all clear that full employment could be reached without triggering the "real limit" of inflation within the US economy. Thus, the outcome of scenario 1 would either be, at best, the redistribution of the ill effects of secular stagnation onto other parts of the world economy or, at worst, the triggering of another stagflationary scenario in the US. In either case, scenario 1 completely fails to revive the flagging private sector, restore general economic growth or even reignite TFP growth.

Scenario 2 at first appears as if it could avert these outcomes by severing the link between bond issuance and spending. Thus, spending would be pursued without the sale of bonds. Even if gradual, the reduction in bond sales would have an immediately negative effect on global liquidity (as demonstrated in Howell 2020) since these are the most secure assets. Howell (2020) clearly documents that demand for US treasury bonds has been rising in line with the shift in global finance from lending to asset acquisition and management (Gibadullina 2020) because

bonds are a key piece of financial infrastructure underpinning flows of global liquidity. Tooze (2018) documents the same phenomenon at the more granular level of central bank administration. In both the macro-level picture provided by Howell and Gibadullina and in the more pedestrian account provided by Tooze, there is a clear cause-effect relationship between the drying-up of "secure" assets such as bonds and the outflow of capital into more volatile sectors or even a freezing up of liquidity entirely, as in the crisis in repo market in 2019 (Tilford et al 2019). Thus, a series of new bubbles would be likely, based on whatever assets seem best posed to replace the security of the treasury bill. Whether through the bursting of these bubbles or simply due to the freeze-up of liquidity, wide-ranging defaults would eventually spread throughout the global economy—and would be most heavily concentrated in whatever locations played the largest roles in stepping in to fill the gap left by the retreat of American debt. This would also decisively disrupt the prevailing, imbalanced global order identified by Koo, Klein and Pettis, which is defined by trade relationships evident in the current account but matched by equal and opposite trends in the financial account. The trade wars that Klein and Pettis identify are, in reality, better understood as capital wars (Howell 2020). Strangling the supply to the bond market effectively severs a major artery in the global dollar system and would likely lead to a closing-out of positions that rely on a strong dollar—this would mean massive financial divestment from US assets, paired with the sudden increase in the value of foreign currencies (particularly the RMB). On the one hand, this would then immediately trigger the very consumer-end inflation that Kelton argues MMT can avoid, since a falling USD (especially when paired with a rising RMB) increases the effective cost of Chinese-produced consumer goods. This suggests that the real slack that Kelton and other MMT thinkers have identified with regard to spending and inflation (visible in the fact that the sudden burst of spending mobilized to

confront the coronavirus pandemic did not result in any equal measure of inflation) actually relies in part on prevailing inequalities in the global trade structure, which is subtended by American seignorage. If these trade and currency relationships begin to collapse, the slack between spending and inflation could rapidly grow taut. Even in the American case, superinflation is not entirely improbable.

It is here that the right-wing interest in MMT becomes more evident, however, because scenario 2 effectively forces a delinking of the US and Chinese economy. While it thereby increases consumer goods prices, it also makes US industry more competitive and could result in some expansion of investment and productivity. At the same time, it's not clear that this expansion would be sufficient to make up the losses of the collapsed financial system. It would certainly be profitable for particular subsections of American capitalists (those invested in, say, heavy industry), but only by collapsing an even larger segment of the American economy and potentially triggering a series of debt crises across the world. The last time the US pursued an intentional devaluation strategy (relative to the Japanese *yen*) in the decade after the signing of the Plaza Accord, American industry did see a revival, with the clearing out of old plant and equipment and increasing competitiveness across the global market (Brenner 2003). At the same time, this revival was more limited than might have been hoped and it triggered a severe depression in the Japanese economy. Revaluation of the USD was agreed upon in part to avoid a decline into global depression, with the fear that the crisis afflicting the Japanese economy would continue to spread (Benanav 2020a). Thus, scenario 2 seems to be gamble between, in the worst case, triggering a global economic depression now (possibly centered in the US) and, in the best case, triggering one later (centered elsewhere). While it might temporarily revive investment and productivity within American industry and trigger a brief ascent in the growth rate of the US

economy, it does nothing to alleviate secular stagnation as a *global* condition. In fact, it will trigger even more rapid declines in growth and productivity elsewhere. As Howell argues, "the entire debate surrounding People's QE and MMT is misdirected because our economic problems and low rate of new investment have little to do with the lack money, per se" (Howell 2020, p.274). Nonetheless, in contrast to Koo and Gordon, MMT at least pretends to have a solution. But this solution turns out to be nothing more than a temporary hedge against renewed global depression.

**The Imperial Solution**

MMT is relevant here both for its recent social impact and because it offers at least the illusion of a way out of the seemingly unstoppable stagnation. Ironically, however, it only does so by doubling down on the worst aspects of global inequality. Similar to the forcible rebalancing advocated by Klein and Pettis and the aggressive, defense-funded R&D programs advocated by Koo and Gordon, MMT thinkers like Kelton essentially argue that it's necessary to double-down on American imperial hegemony by making full use of US dollar seignorage to serve national interests. Kelton and others attempt to deflect criticism of such a program's imperial implications by arguing that other countries or groups of countries should simply follow suit, converting to sovereign currencies delinked from the dollar (if this is not already the case), after which their own Treasuries can print money to fund similar full employment programs. Without ever explicitly acknowledging the fact, MMT thinkers seem to be arguing for the intentional reconstruction of the currency blocs that defined the era of classical imperialism. Ironically, the theory therefore acts as a sort of roadmap to Howell's pessimistic scenario, predicting: "a deglobalization and new regionalism, with geopolitics dictating the realm of capital," which

would ideally be accompanied by "the encouragement of currency blocs, with greater exchange rate adjustment allowed between these blocs than within them." (2020, pp.269, 275).

Given the necessity of growth in a capitalist economy, it's hard to see why such blocs would not then become competitive between one another—and now in a context where this economic competition has a much more systematic connection to geopolitical interests. In other words, it's not clear how this potential future is anything other than the revival of "classical" imperialism, complete with the drive for territorial expansion. Moreover, it's clear that pre-existing international divergences in productive capacity already signal the unacknowledged limits to the disingenuous suggestion that any country with a sovereign currency can engage in the same policies. Kelton and other MMT thinkers very clearly acknowledge the fact that

> every economy has its own internal speed limit, regulated by the availability of our *real productive resources*—the state of technology and the quantity and quality of its land, workers, factories, machines and other materials. If the government tries to spend too much into an economy that's already running at full speed, inflation will accelerate. (Kelton, pp.3-4, italics in original)

Apply this same logic to the claim that any sovereign-currency country could pursue MMT policies and you quickly realize that differences in these "real productive resources" means that poor countries will exceed this "speed limit" much sooner than rich ones. Even in the ideal conditions posited by MMT thinkers, then, the theory essentially acknowledges that pre-existing global inequities in wealth make such policies most possible in areas that already have most of that wealth.

While a rich nation—and especially the richest one, at the top of the imperial hierarchy—might conceivably be able to reach full employment before inflation kicks in, this inherent "speed limit" determined by pre-existing wealth all but guarantees that poor countries will fail to do so. If they were to implement the exact same policies as MMT thinkers advocate for the US, such countries would, according to Kelton's own standards, very quickly see inflation accelerate. This is, in fact, exactly what they argue happened in Zimbabwe where Bill Mitchell, another prominent MMT thinker, explains that "hyperinflation was almost inevitable" due to "dysfunctional government" that "mismanage[d] the supply side of [the] economy." In such conditions "the only way to avoid inflation is to severely contract real spending to match the new lower capacity." But, had this solution been pursued, "More people would have starved and died than already have" (Mitchell 2009). The MMT argument, then, is quite explicit that poor countries left with lowered industrial capacity after the experience of colonialism and the subsequent third world debt crisis simply do not have the slack to pursue wide-ranging programs of government spending. In fact, Mitchell essentially points out that the only properly economic policy to be pursued here would have, in fact, been to let more people starve in order to drive down spending—he of course doesn't endorse this, instead more or less arguing that the government was, by that point, in an impossible position. The point Mitchell is trying to make is simply that the case of hyperinflation in Zimbabwe often used as evidence against MMT's basic argument is not actually a disproof of the core concept, since MMT always emphasizes the "real productive resources" of the currency-issuing country. In this regard, Mitchell is completely correct.

Ironically, however, the fact that hyperinflation in Zimbabwe does *not* disprove MMT is all the more damning, since it shows the imperial contours of the basic theory which promises

full employment for the rich and hyperinflation for the poor. At best, hyperinflation might be avoided by poor countries if they pursue "responsible" fiscal policies relative to their minimal productive capacity—and these policies would in all likelihood perpetuate the very relations of dependency that put Zimbabwe in such a position in the first place. Meanwhile, for all those in between, more and more ruthless competition between currency blocs would seem to imply conditions where growth could best be guaranteed by the inclusion of new territories with cheap land and cheap labor into each respective bloc—and this would only be a possibility if these territories were forcibly prevented from establishing their own sovereign currency or aligning with an opposed bloc. In another ironic twist, the fact that Kelton uses Greece as a cautionary tale seems here to turn into its opposite: without monetary sovereignty, Greece and other poor parts of Europe are essentially subject to the will of wealthy EU members like Germany. But even though this relationship has clearly been disastrous for the Greeks, it's also been profitable for the Germans—and not just due to the onerous rents levied on the Greek government, but also because of the widespread access the union has given to cheap pools of eastern European labor for German industrial firms. The case seems to all but prove that forcible inclusion of subordinate nations within such a currency bloc would, in such conditions, not only be feasible but also quite lucrative for those who wield geopolitical power.

Again, the implication seems to gesture toward many of the spatial patterns and economic policies that defined the era of high imperialism—only now without any non-capitalist periphery. Altogether, the mainstream thinkers reviewed throughout this chapter seem to offer only a few paths forward, and each seems more grim than the last: Koo and Gordon suggest that stagnation may simply be permanent, either due to demographic and developmental change in the context of global industrial competition or due to inherent technological limits that prevent further

increases in TFP; Klein and Pettis suggest a “rebalancing” that will in all likelihood lead to global depression and the balkanization of global trade; and, finally, MMT thinkers take these same prospects to their logical extreme, essentially advocating the fragmentation of the global currency system, proposing the immediate renewal of US imperial power through the forceful utilization of dollar seignorage in the service of national interests and triggering a currency arms race guaranteed to lead to disastrous divestment and hyperinflation in some poor countries, renewed dependency in others, and a geopolitical competition between the new currency blocs for the forcible inclusion of territories with cheap land, labor and natural resources which will almost certainly lead to renewed world war. Among these thinkers, only Koo and Gordon (we could also add Howell’s prediction tot his) make a real attempt to earnestly trace out the consequences of their conclusions. Klein, Pettis and the MMT thinkers, on the other hand, seem to actively disavow the international consequences of their own prescriptions just as readily as they deny any inquiry into the real roots of the crisis itself.

# Chapter 2

# Marx and the Communist Theory of Crisis

## The Long Downturn

### Introduction

The accounts above cast an illuminating light on the roles of the balance sheet, technical change and the state's fiscal capacities in shaping the economy and its long-run crisis of stagnating growth. But it is only in the shadows cast by these topics that it becomes possible to feel out the connections between them. The very problems that the above theorists occlude or simply take as a given are, in fact, the more fruitful points of interest for any inquiry into the real cause of crisis in both its cyclical and secular sense. Ironically, of all the mainstream thinkers reviewed above, the one who comes closest to such a theory is Michael Howell, who founded a financial consulting firm that draws from flow-of-funds data to calculate the sum of global credit, savings and other highly liquid financial assets in order to advise hedge funds and other large-scale capitalist interests in their investment decisions. This is because, throughout his work, he emphasizes the integral role played by profitability to account for trends in the changing shape of investment and liquidity. Similarly, he recognizes both the eviscerating effects and ascending importance of simultaneously fiscal and financial interventions on the part of the state under conditions defined by a general dearth of opportunities for profitable investment in productive

industries. In his own words: “falling industrial profitability and the associated structural shortage of safe assets are key factors behind the long downward slide in World interest rates.” (Howell 2020, p.46). This means not only that global liquidity is dominated by portfolio capital rather than foreign direct investment, but also that “modern capitalism has become far more a *refinancing* system than a *new financing* system” (ibid, p.30) since the maintenance of this financial superstructure requires continual tending through the provision of credit. Thus, it’s not coincidental that falling profitability is accompanied by rising credit and a shift away from investment in industry and toward investment in an array of financial assets.

On the one hand, Howell’s proximity to the world of finance and the granular logic dominating real-world investment decisions likely contributes to this recognition. On the other, it seems Howell himself admits that some of his own insights come from well outside of economic orthodoxy. A reference tucked away on the very last page of Howell’s book-length analysis of global liquidity hints at this fact: alongside various books on bond math and the history of interest rates, he cites both Keynes’ *General Theory* and Volume 3 of Marx’s *Capital*, which he characterizes as offering “some of the best granular descriptions of money markets and financial crises anywhere” (ibid, p.288). With this passing reference, it becomes clear why Howell, rather than Koo, Gordon, Klein, Pettis or any of the MMT thinkers, views changes in profitability as the central determinant behind long-run transformations in the character of investment and, thereby, liquidity, and why he so firmly predicts a deglobalized world of competitive regionalism shaped by capital rivalry and “possibly based on digital monies,” which other thinkers pose as one of the worst possible outcomes. In the purely pragmatic pursuit of accurate, real-world investment forecasting, Howell seems to have found use in heterodox theories, which allow him to recognize trends that remain in the shadows of these other accounts.

Throughout all the work reviewed in Chapter 1, the focus on the concrete problem of secular stagnation constantly gestures toward this territory, even if the authors are unwilling to tread beyond the bounds of inherited economic wisdom. This also results in a strange phenomenon in which they are either unaware of, or simply do not engage with, earlier debates on these same topics. Koo, Klein and Pettis lay out theories of disequilibrium first debated at the turn of the last century and, in so doing, they either fail to cite such theories entirely (in the case of Koo) or only cite half the debate (in the case of Klein and Pettis' references to Hobson). In tracing the roots of secular stagnation back to a general technological plateau reached as early as the 1970s, Gordon is unwilling to acknowledge (it seems doubtful that he is simply unaware) that this claim has long been associated with self-described Marxist thinkers who have argued both that the period since the 1970s constitutes a single "great depression" (Itoh 1990), a "long depression" (Roberts 2016) or a "long downturn" (Brenner 2006) and that this is intimately related to long-run technical changes in production (Braverman 1998, Mattick 1969, 1981). This is not coincidental, of course. Instead, it follows the same logic as Brahe's increasingly complex geocentric modelling, which conceded more and more features of the heliocentric model for the sake of retaining its geocentric core, all the while treating heliocentric criticism as unworthy of equal-footed engagement because of its social branding as heresy. Even though Brahe's model was, in many ways, incredibly accurate, his refusal of any heresy against the prevailing social order prevented him from recognizing the more fundamental dynamics of the system he was attempting to describe. In the end, the negative, critical dimension of heresy proved to be its greatest asset, precisely because multiple systems can always be built from the same data—not to mention the fact that the production, organization and description of this data must always also be the subject of equally critical inquiry. The scientific process is therefore driven by a corrosive

negativity that seeks not the best accommodation between the data and social orthodoxy but is instead obligated to always pose new heresies against this orthodoxy—though also without posing these heresies as alternate, occluded orthodoxies. Science is a mobile negativity, or a sort of hyper-heresy, then, as opposed to the Manicheism that marks most forms of heresy, shaping them into a symmetrical and dependent system that is the mere shadow of that which they oppose.

This mobile negativity of the scientific project is key to understanding the original intent of Marx's own critical project, shaped by both an engagement with the scientific thought of the era (see Saito 2017) and an interpretation of Hegel that focused on the radical negativity at the heart of his philosophy (Zizek 2012, Mosely and Smith 2014, Johnston 2019). It is only in this sense that we can say that Marx pursued a "science of value" (Heinrich 1991) and understand exactly how integral this scientific spirit is to the development and intent of the critique of political economy and, thereby, how important tools from philosophy of science are to understanding it (Mattick 2018). Many later thinkers sought to extend, develop, reformat, reread, return to or move beyond this critique and, in so doing, either designated themselves "Marxists" or were designated as such retroactively. Of "Marxism," Marx himself found the use of the term by such a wide array of thinkers somewhat laughable, famously claiming (as recounted by Engels in a letter to Bernstein) of the French "Marxists" Jules Guesde and Paul Lafargue that their use of the term was little more than "revolutionary phrase-mongering" and, if their politics represented Marxism, "what is certain is that I myself am not a Marxist" (MECW 35, p.388). Though this quote is often exaggerated into a claim that Marx himself rejected the very idea of a "Marxism" built on his thought (in fact, he just as often called these thinkers "so-called Marxists," implying the opposite), there is a kernel of truth here insofar as the variegated

development of the workers movement and then the global communist movement over subsequent decades would, in fact, imbue the term "Marxism" with the aura of orthodoxy, even as the term was used to pursue political projects that existed at some distance from Marx's own. More troubling was the way in which many strands of "Marxism" came to take on the very character of pre-Marxist political economic orthodoxy that Marx himself set out to critique.

Beginning in the late 19th century, after Marx's death, "Marxism" also came to be associated with what Michael Heinrich calls a "worldview" defined by its "comprehensive explanation of the world offering an orientation and answers to all questions" (Heinrich 2012, p.24). Heinrich argues that, facilitated by Engels and Kautsky, this "worldview Marxism" rose to a dominant position in the early workers' movement and, from there, shaped even the critical appraisals of the Second International put forward by thinkers like Lenin and Luxemburg, ultimately resulting in the official canonization of "Marxism" as a particular, Soviet-aligned "Marxism-Leninism" that retained this "worldview" foundation even while it offered a new orthodoxy opposed to that of the Second International. It was this Soviet orthodoxy that would go on to define the meaning of "Marxism" for much of the 20th-century after its solidification "into a dogmatic system of philosophy (Dialectical Materialism), history (Historical Materialism), and political economy: Marxism-Leninism" (ibid, p.25), concurrent with ascent of Stalin and assisted by the official moratorium on the publication of the complete works of Marx and Engels.[15] The influence of this orthodoxy was so strong that essentially all alternate interpretations of Marx in these years were defined by a dependent, symmetrical opposition to

[15] The MEGA (Marx-Engels-Gesamtausgabe) project was initiated in the Soviet Union in the 1920s but put to an end with the deposition (in 1931), imprisonment and execution (in 1938) of its editor, David Riazanov. It was only revived after Stalin's death, with work beginning again in the 1960s and publication renewing in the 1970s (Heinrich 2012).

the orthodox positions taken up by all the world's Soviet-aligned Communist Parties. Thus did official "Marxism"—as well as its unofficial, wholly dependent internal heterodoxies of "Trotskyism," "Maoism," and even "Western Marxism"—counterpose itself as offering a constitutive worldview that posed an "alternative" to capitalism. In every one of its forms, this "Marxism" was ultimately a sort of Gnostic, counter-orthodox heresy, linked to its opposing pole through a dependent chain of Manichean oppositions between competing worldviews. It is in this sense that many often classify Marxism as a form of "heterodox economics," alongside an equally aleatory "Keynesianism" (Mann 2017). But, Heinrich claims, this conception of Marxism as a "worldview" or, maybe more accurately, as a heretical counter-ideology to capitalist cosmology, obscures Marx's own method (not to mention the historical process of its production) and thereby clouds the more incisive political import of his project. At root, the critical, scientific core of Marx's own inquiry opposes orthodoxy as such, including that of its "heterodox" twin. In terms of Marx's economic writings, then, while both proponents and detractors came to see *Capital* and subsequent work by other "Marxists" as examples of "Marxist political economy," Heinrich emphasizes that we must treat Marx's inquiry in the terms he set for it himself, as a "critique of political economy" (Heinrich 2012, pp. 32-33).

It was only with the revival of mass movements that lay outside the control of the Marxist orthodoxy in the 1960s and 1970s that the "return" to the critical core of Marx's thought would be attempted. This was accompanied by the renewed publication of the original writings of Marx and Engels, each segment of which would soon be seized on by different factions in the new political movements and used as a justification for various "new readings" of Marx, the most expansive of which was the German "new reading" first represented by the work of Hans-Georg Backhaus and Helmut Reichelt in the 1970s and 1980s, named as such in the 1990s and then

continued over the course of subsequent decades by other German scholars, including Heinrich himself, as well as within the anglophone world by the "Open Marxism" school and thinkers such as Moishe Postone, much of which would later be lumped together, despite substantial differences as "value-form theory" (Elbe 2013, Endnotes 2010). These readings could then be placed alongside similar attempts at a return to Marx among the French ultra-left, drawing from various underground Marxisms that had persisted in the councilist and Bordigist traditions and took political shape in the aftermath of the upheaval in May of 1968 (Endnotes 2008). Despite their differences, what tended to unite many of these new readings was both a commitment to the analysis of Marx's original writings, the refusal of allegiance to any established faction in Cold War politics and a general wariness of portraying "Marxism" as a closed, completed and constitutive system. In some cases, this more or less entailed the rejection of the term "Marxism" as such (as in Heinrich 2012 or Elbe 2013) and was often critiqued for an apparent political quietism. In more recent years, however, the popularity of such readings among participants in new waves of unrest has tended to weaken such claims.

Ultimately, the schema explicitly offered by Heinrich is convenient, but too simple. It is too easily misshapen into a sort of moral drama, where the bulk of twentieth century politics is rendered almost exclusively in terms of contrasting intellectual positions rather than any real study of the material constraints that may have led to certain theories taking the pragmatic shape that they did. Ironically, this is in stark contrast to Heinrich's own historical research on Marx's life and the workers movements of the nineteenth century, where he demonstrates in great detail the ways in which political struggles shaped Marx's work and defined its interpretation in the immediate decades after this death. It is also in contrast to his own emphasis on the political import of Marx's form analysis. Lying where it does in his work (within the introduction to an

introduction), a charitable interpretation is that the phrase was meant to offer the broad outlines of a critique that could not be elaborated on the spot. It would therefore be an overextension to claim that all self-described "Marxists," or those who described their work as "Marxist economics" have committed such a fundamental misinterpretation that their work ceases to be useful, regardless of the fact that many partake of this "worldview" to some degree or another.

It does not help, however, that Heinrich himself has tended to use similar characterizations in more recent debates with other scholars also attempting a "return to Marx," often in a way that fundamentally mischaracterizes their positions and seems to argue that *anyone* attempting to reconstruct a more "complete" system from Marx's texts (especially as regards economic crisis) or even just trying to use any empirical data to demonstrate the insights of this critique is inherently committing the same faults as the "worldview Marxists" of last century.[16] It's possible that this tendency is itself a signal of the deeply dependent character of such "new readings," which were very much shaped by the context of the Cold War even if their exegetical currents only tended to bloom after its end. The various "new readings" all emerged alongside and in relation to the new political currents in Europe in the 1960s and 1970s, almost all of which were defined by their outright rejection of Soviet orthodoxy or their reinterpretation of it (via Maoism). After the collapse of the Soviet Union, the capitalist turn in China and the subsequent disintegration of essentially all the major "Eurocommunist" parties that had defined their politics in relation to this orthodoxy, it can hardly be argued that "worldview Marxism" any longer offers a credible "alternative" in the same sense. The revival of interest in Marx that has taken place in more recent years (accelerating alongside the reemergent global struggles of the

[16] See, for instance, his discussion with Fred Moseley in the Franke Lectures in the Humanities series of 2020, where Heinrich quite bafflingly seems to ascribe to Moseley a reading of Marx that retains Ricardian value theory.

2010s and, now, 2020s) has hardly signaled any such allegiance, even if some of the young thinkers affiliated with this revival may also use the term "Marxist" and periodically refer to historical thinkers who might fairly be subject to such critiques.

Here, I want to emphasize the critical, scientific dimension of Marx's project without, at the same time, being overly pedantic about the use of terms like "Marxism" or throwing out the potential insights of any thinkers from the twentieth century simply because they engaged in the practical politics of their era, even if these politics were ultimately sullied by the "worldview" interpretation. Similarly, it seems that the few remaining adherents of something approximating worldview Marxism—or those who describe themselves as "Marxist political economists" and explicitly elaborate "Marxist" theory on a Ricardian basis—can nonetheless offer useful empirical experiments, if only in counterpoint. Such an approach is particularly necessary for theories of crisis, since such theories have seen far more original development beyond Marx than almost any other feature of the critique of political economy. They have also always had to engage more directly with problems of measurement, the political nature of business statistics and the relation between empirical methods and the abstract theoretical core of Marx's argument. While it is absolutely true that almost all "Marxist" theories of crisis have suffered from "worldview" presumptions and have tended to base themselves on various mis-readings of Marx's inherently incomplete late writings (which signal that *Capital* itself is a fundamentally incomplete project and contains internal contradictions), Heinrich's own responses to those arguing for a theory of crisis (reviewed below) is ultimately misplaced precisely because of his tendency to see everything through this lens. Thus, Heinrich himself seems to misread the basic intent of Marx's theory of crisis and misidentifies the nature of its incompletion.

In this chapter, I will explore the question of crisis (in its multiple senses) in successive stages working back from the immediate appearance of crisis as "secular stagnation," "financial crisis" and "trade imbalance," identified by the authors above, to their deeper causes. While each section will draw from a particular subset of thinkers whose work derives from Marx's own critical project (almost all of whom would describe their own work as "Marxist"), this chapter will, unlike the above, be organized more around topic and less around author. Section 1 returns to the technical question posed by Gordon, now framed in terms of automation and its relationship to long-run tendencies in capitalist society. Section 2 then draws out the immediate consequences of these trends in global industrial competition, of which "trade wars" and the international disequilibria identified by Koo, Klein and Pettis are epiphenomena. Section 3 briefly explores the implications that this has for a more general theory of global production, briefly reviewing various arguments on the nature of imperialism. Section 4 then summarizes the three preceding sections through an illustration of their geographic implications. After these implications are laid out, it finally becomes possible to explore the specific role played by China, in Part 2, which begins with Chapter 3.

# Section 1
# -
# Automation and Surplus Population

**Introduction**

As mentioned in Chapter 1, Gordon sees the roots of "secular stagnation" in intractable technological limits. His argument can be characterized as a sort of technical Malthusianism,

linking the general postwar decline and then flattening of TFP growth to the irreproducible nature of the technological "miracles" that undergirded previous bursts of growth. This stands in contrast to the widely publicized position of those he deems "techno-optimists," which is that an array of new technologies drawn from a grab-bag of clickbait headlines—green energy, space exploration, quantum computing, machine learning and AI, biotechnology, blockchain currency, nuclear fusion, etc.—will soon lead to an unprecedented boom. Gordon returns to the paradox first pointed out by his doctoral advisor, Solow, to show that none of these claims are evident in productivity data, nor do they seem likely judged by the outcomes of similar predictions in the past. At the same time, Gordon simply refuses to engage with others who have made similar arguments to his own, since their work lies outside the bounds of accepted economic orthodoxy. Thus, he never inquires into any of the alternate explanations for the trend he observes in productivity data and its subsequent effect on growth. But it is entirely possible to explain this trend without reference to a Malthusian cause. Instead, it can be argued that, just as the "natural" limit Malthus identified with regard to population proved to be social, so too does the "technical" limit identified by Gordon ultimately trace back to the social organization of production.

The same basic trend of declining productivity and its relationship to declining growth and general business investment over time has long been a point of focus for various thinkers who root their work in Marx's critique of political economy. While it is entirely possible to respond, point by point, to Gordon's argument with Marx's own writings on technical change under capitalism, this rhetorical method tends to be both boring and produces a certain exegetical externality in over-asserting the authority of Marx's original texts, which are always easy to cherry-pick to make a particular case. More importantly, such an approach entirely sidesteps the most important issue for a scientific inquiry: being able to at least roughly judge one's theory

against outcomes in the real world, which requires at least some concrete methods for empirical measurement or broader qualitative appraisal and some sort of (critical) engagement with existing data and the prevailing narratives of what is going on. Therefore, I will focus here on thinkers who have a familiarity with Marx's own method and body of works but who themselves emphasize a direct engagement with recent economic history, the measurement of key variables to identify its core dynamics and the relationship of these dynamics to the deeper logic of capitalist production.

The technical questions raised by Gordon are a particularly good opening point for this inquiry since the issue of "automation" has recently garnered renewed attention among communist thinkers. This attention first took the form of various "accelerationist" texts by several socialist authors, most of which were techno-optimist in their character and predicted the forthcoming collapse of capitalism under the pressure of a coming technological revolution that would effectively reduce the marginal costs of production to zero (Mason 2017, Srnicek and Williams 2016, Bastani 2020).[17] This then stimulated a much more interesting inquiry by a number of communist thinkers exploring why these ideas were ill-founded. Though these texts made reference to the longer history of debates among self-described Marxists on the role of technological change within crisis (for instance: Braverman 1978, Mattick 1981) they were more notable for their ability to narrate the basic dynamic in relation to the same empirical data used by thinkers like Gordon, but without the same techno-Malthusian conclusion. Instead, these thinkers linked the decline in productivity growth to more fundamental dynamics, namely the relationship between declining profitability driven by technical changes in production and the

[17] Since these thinkers are systematically critiqued in the literature I cite below, I will not focus much on their work here. I have written a more general overview of the accelerationist trend and the problems with its presumptions in Neel 2016.

subsequent tendency to produce both surplus population and surplus capital. These relationships are explored in relation to questions of automation in two recent books, one by Jason Smith (2020) and one by Aaron Benanav (2020b), both of whom draw on earlier work by Robert Brenner (2003 and 2006), Paul Mattick Jr. (2011), Paul Mattick Sr. (1969), and from thinkers associated with the "communization" current, epitomized by the output of the anonymous collective Endnotes (particularly Endnotes 2008 and 2010). I will therefore focus here on the work of Smith and Benanav, with periodic references to these background materials, before moving on to a more detailed analysis of the work of Brenner in the next section.

**Overcapacity, Deindustrialization and the Global Glut**

Both Smith and Benanav focus in on the exact same productivity data used by Gordon, with Smith attempting a more granular look at the nature of the "service" economy and Benanav emphasizing the more general context of global deindustrialization and changes in employment. One useful thing about Smith's overview of productivity data is the way that he narrows in on sectoral differences. The starting point is essentially the same as Gordon, though working backwards from the present. Smith points out that, "since 2007, according to the BLS, the productivity of American workers […] has on average risen at an annual rate of only 1.2 percent" (Smith 2020, p.64). This, however, seems to have slowed in more recent years, with a "0.75 percent average annual growth rate over the half-decade beginning in 2011 […] for *all* sectors of the economy other than agriculture," including at least one year of negative growth in 2011 and one year of nearly zero growth, in 2016 (ibid). While it might seem that this low figure is being driven down by the inclusion of "typically low-productivity sectors such as healthcare, education, restaurants, and so on," when one narrows the data further to capture similar trends in

the manufacturing sector one finds "even lower, in fact *negative* rates of productivity growth: -0.2 percent" for the same time period (2011-2017), with 5 out of the 7 years recording negative values (ibid).

Smith then contrasts this with historical data, though, unlike Gordon, he draws from global data rather than just the US. Specifically, he points out that, in the postwar period, "those countries with a less-developed industrial base, like Italy and France, saw the productivity of their workers shoot upward at a rate, in the late 1940s, of over 10 percent annually [and] Spain saw a similar surge in the early 1960s" (ibid, p.65). Thus, the 2.6 percent growth of productivity seen in US manufacturing from 1950 to 1973 was far outstripped by accelerations among the former Axis powers in the same years, with Italy showing growth of 6.1 percent in manufacturing productivity growth, Germany "just a notch below 7" and Japanese industry beating all records by "averaging a full 10 percent annually for a full quarter-century" (ibid). Thus, it seems that the slowing growth evident in narratives of "secular stagnation" is, as Gordon argues, somehow linked to changes in labor productivity and specifically low productivity growth in industry specifically. At the same time, Smith's more detailed breakdown by country demonstrates that the story is not quite as simple as Gordon's US-centric timeline suggests. At this point, however, nothing in the story seems to contrast that wildly with Gordon's narrative, with the presumption being that technologies rolled out early in the US context were introduced later in the countries that lost the war, in the course of their reconstruction.

Benanav deals with essentially the same data as Smith and explores these figures at a similarly global scale, but he also contrasts productivity growth to similar growth rates in output and employment. This helps to account for some of the divergence between the false perception of rapid automation and the reality of persistently slow productivity growth. The most important

observation is that "productivity growth rates have been high relative to output growth rates, but not because productivity has been growing more rapidly than before" (Benanav 2020b, p.20). Instead, "the key to this trend is that output has been growing much more slowly" (ibid). This has been true across the board for all the countries that have composed the core of the global economy since the postwar period. In France, Germany, Japan and the US, the postwar growth booms each saw average output growth exceed productivity growth in manufacturing (for the period 1950-1973), after which this relation began to flatten or invert. Thus, the periods 1974-2000 and 2001-2017 showed manufacturing productivity growth increasing more rapidly than output in France, the US, Germany and Japan—in each case productivity growth was faster than output growth, save for the 2001-2017 period in Germany, when productivity growth of 2.2 percent slightly exceeded an output growth of 2 percent. But across the board, *both* decreased substantially in these years. The pattern is here the same as that observed by Smith above: the Axis countries saw incredibly high growth rates in manufacturing productivity in these years, but they were far exceeded by growth in output. In Germany for the period 1950-1973, average manufacturing output increased 7.6 percent, while productivity increased 5.7 percent. In France, 5.9 and 5.6 percent, respectively. In Japan, 14.9 and 10.1 percent. This compares to an average output growth in American manufacturing of 4.4 percent, and productivity growth of 3.1 percent in the same period (ibid, pp. 20-21, Figure 2.1 and Table 2.1).

But more interesting is the fact that, since productivity is (at least at the macro-economic level suitable to such a growth accounting approach) the ratio of output to employment, this inversion has led to a situation in which all of these countries switched from a positive average employment growth in manufacturing in 1950-1973 to negative average employment growth in manufacturing after 1973 (ibid). This, of course, had stark political consequences, since, as

Smith points out, the rapid growth in employment driven by output growth exceeding productivity growth enabled a "correlation between growth in real wages and labor productivity" ensured by "a constant share of income between capital and labor," which then allowed for "a dynamic equilibrium" to take shape linking rising profit rates to rising wages (Smith 2020, pp.67-68). Since these conditions seemed to prevail for decades and these were decades in which many features of the disciplinary orthodoxy of economics were being set in stone, this also meant that these conditions exerted an outsize influence on "economists' assumptions about market economies," taking this "temporary correlation" between wages and productivity "not as an orchestrated social artifact, but as a norm" and thereby elevating it "to the status of stylized fact" (ibid, p.68). In this way, later observers were led to misdiagnose the transformation that took place after the 1970s by focusing on the divergence between wage growth and productivity growth rather than seeking the causes of the decline in productivity growth and its relationship to output and employment. It is essential to get the causation correct here, however, because theories of this era that emphasize the delinking of wages and productivity due to the political victory of capital over labor (i.e., the theory of "neoliberalism" evident in Shaikh 2016 or Harvey 2007) imply that this link could be restored if the political power of labor were rebuilt (through increased unionization, renewed growth in the public sector, etc.). But Smith points out the basic error with this logic: "if the productivity gains that alone afford the 'material foundation' for workplace demands are missing, even the most combative worker organizations will run up against forbidding material conditions and limits" (ibid, p.70). Productivity gains here refers not to just any increase in productivity, however, but to the specific linkage conditions Smith and Benanav identify wherein output growth exceeds productivity growth, driving growth in employment.

Benanav drives the point home further with his data on output[18] and employment, since it is only in the context of what he calls "output-led deindustrialization" which is not the result of so-called neoliberal reforms (these were, in fact, a response to the phenomenon) and "is impossible to explain in purely technological terms" (Benanav 2020b, p.21). This leads many economists, such as Koo, to "describe this trend as a harmless evolutionary feature of advanced economies" (ibid). But the reality is that what is conventionally referred to as "deindustrialization" (usually measured as the declining share of employment in industry, which does not correlate to a declining share of industry's total output) has tended to occur at very different levels of GDP per capita. In fact, one of the key problems that Benanav points to—and which will be absolutely essential to understanding the conditions of industrial development in both China and Tanzania later on—is the fact that, although this output-led deindustrialization began in all the core, developed economies in the 1970s, it then started to outpace conventional measures of "development"[19] as such, reaching southern Europe in the late 1970s, "much of Latin America, parts of East and Southeast Asia, and Southern Africa [then] followed in the 1980s and '90s." This is often called "premature de-industrialization" (Rodrik 2015), and Benanav points out that "peak industrialization levels in many poorer countries were so low that it may be more accurate to say that they never industrialized in the first place" (Benanav 2020b,

[18] If not already apparent, output here is not some material accounting measure. Instead, it is simply value-added (usually of manufacturing, in Benanav's use) and is commensurate with GDP, which itself is often used in these growth accounting procedures. Specifically, Benanav draws from the Conference Board's International Labor Comparisons (ILC) series for most of his numbers, which explicitly measures manufacturing output as manufacturing real value-added and calculates labor productivity, unit labor cost and other derivatives from this measure. More specifically, many ILC values are index values, with a certain year's value set to 100 (in the most recent data, this year is 2010), making the data even more easy to use for growth accounting purposes. Unit labor costs, by contrast, are not index values. The ILC data also includes both Real and Nominal Value-Added estimates as raw numbers as well, which I will utilize in subsequent chapters.

[19] This hints at the poverty of such measures, certainly, but it also captures a real disjunction between the process of industrialization and the general features of "development" that it was supposed to be correlated with—such as rising wages, formalization of labor, urban development, increasing gender equity, better performance on global health metrics, etc.

p.21). Even in cases such as South Korea, South Africa and China, where manufacturing employment has seen an increase in the post-1970 period, it has always then been followed by a decline. In other countries such as Mexico or India, it has failed to manifest at all (ibid, pp.22-23, Figures 2.2 and 2.3). The result is that "rates of output growth in manufacturing have tended to decline, not only in this or that country, but *worldwide*" (ibid, p.23, italics in original).

Though it seems like this would then lead to a situation in which mass unemployment should follow, this is not exactly the case, especially in wealthy countries. Instead, the result is "that this underdemand for labor has tended to manifest not as mass unemployment but rather as persistent *underemployment*" (ibid, p.11). The employment lost in the manufacturing sector has been spread out across services, where it has tended to be accompanied by "a decline in the share of all income earned in a given year that is distributed as wages rather than profits," since these jobs are, on average, lower-paid and have less consistent hours (ibid, p.9). This is despite "massive accumulations of so-called human capital, in the form of rising educational attainments and healthier lives" (ibid). This is why the role of services are so central to the story, as recounted by Smith, because as various technical and geographic changes (see below) result in a cheapening of manufactured goods, the total consumer demand directed toward services increases[20] and these become the main sectors that see growing employment. At the same time, the sheer surplus of labor leads to a situation in which there is still fierce competition for even the lowest-skill, lowest-productivity jobs, which drives down wages. Ironically, this also tends to

[20] This is not the same as arguing that the share of total demand in the economy sees a similar shift. In fact, Manufacturing Value-Added essentially grows at the same rate in most G7 countries between 1973 and 2000, meaning that it composes the same share of total output and "there was no significant shift in demand from industry to services" at the economy-wide level (Benanav 2020b, p.31).

prevent the further mechanization of many of these jobs, since wage costs remain so cheap by comparison (Smith 2020, pp. 119-120).

This pattern is held up in the data and became particularly notable in the "recovery" from the Great Recession, after which "the largest job losses were in mid-level clerical work" such as office and administrative jobs, while "three out of four jobs added [in the period from 2008 to 2017] earned below the median wage" (ibid, p.115). The result was that the unemployment rate did, in fact, decline, but only because more and more "workers pool in low-productivity jobs" (Benanav 2020b, p.35) that are marked by inconsistent hours and low pay in what Smith, quoting Marx, calls a new "servant economy." This is, again, not just true in advanced economies but worldwide. In fact, the problem is far worse in poorer countries precisely because the gross consumer demand for such services is so much lower and is therefore able to sustain less employment and only at an extremely low wage (in fact the "wage" here tends to break down entirely into much more informal systems of piecework payment). Citing data from the International Labour Organization, Benanav notes that "only 26 percent of the global workforce had permanent employment of any kind in 2015 […] leaving 74 percent to work either for employers on temporary contracts or else informally" (ibid, p.55). Rather than the normal designation of such employment as "nonstandard," then, the reality is that informal, temporary employment without a contract (and in many places far below minimum wage) is, in fact, the normal condition of the global workforce.

More fundamentally, neither Benanav nor Smith identify this problem as primarily an issue of the drying-up of technological "miracles." In a clear jab at Gordon, Benanav explains that:

> Some commentators describe the present period of economic stagnation in terms of an exhaustion of the frontiers of technological advance—as if there were nothing left to invent—but it is more likely that low rates of industrial productivity growth are the result of a slower pace of industrial expansion rather than the reverse. (Benanav 2020b, p.34)

After all, if war-torn Italy, France, Germany and Japan were able to stoke industrial booms by deploying technologies that had already been implemented in more advanced countries, it's unclear why, exactly, the same would not be possible in the many parts of sub-Saharan Africa that still lack electrification or running water.[21] There is no "technical" limit here. In fact, the inventions all already exist. Instead, there is some hidden limit to investment.

To narrow down what this limit might be, it is first important to note that manufacturing plays a special role within the economy at large. GDP growth in general across all the world's leading economies tends to track growth in those countries' manufacturing output (most often calculated as manufacturing value-added, or MVA). Between 1950 and 2017, MVA growth moved in close correlation with GDP growth (ibid, p.31, Table 3.1) and a similar correlation is evident between the growth rate of capital stock and that of productivity (ibid, p. 32, Table 3.2). This, rather than some supposed technological limit, is the immediate root of the decline in productivity:

> As the average firm has invested less in expanding its fixed capital base, average labor productivity growth rates have declined, as well. That is because labor-saving

[21] Or, in fact, why the same would not be possible in the many parts of even wealthy countries such as the United States that lack similar infrastructure—as in the borderland *colonias*, parts of the Mississippi river delta or on numerous reservations.

> innovations tend to be embodied in capital goods, or else typically require complementary investments in capital goods to be realized. (ibid, pp.31-32)

In other words, causation is equally likely to run in the exact opposite direction identified by Gordon, with investment determining productivity growth and not the other way around. The impact of this is amplified by the fact that "manufacturing was a unique engine of economic growth" that, measured in terms of gross output rather than value-added (and thereby including the costs of intermediate outputs) composes an enormous share of GDP. Even in trade deficit countries like the United States, "manufacturing gross output accounted for 42 percent of total GDP in 2000," falling "to 30 percent over the course of the 2010s," while "in Japan, manufacturing gross output's share of GDP was still higher: 59 percent in 2017" (ibid, pp. 34-35).[22] The problem, then, seems neither to be that there are no longer sectors that could be mechanized or countries that could be industrialized, nor that manufacturing is incapable of producing enough in absolute terms, but instead that the incentives to further mechanization and renewed investment have somehow been reduced.

Benanav follows his doctoral advisor Robert Brenner here in arguing "that global waves of deindustrialization find their origins not in runaway technical change, but first and foremost in a worsening overcapacity in world markets for manufactured goods" (ibid, p.24). Overcapacity is marked by the "global redundancy of productive and technological capacities" which creates "more crowded international markets" which make further industrial expansion difficult (ibid, p.26). It is signaled by "depressed prices in global markets for manufactured goods," which act

[22] The reason gross output is useful here is to get a sense of *total* economic impact. Though it's less clear when taken as a stand-alone quote, calculating gross output as a share of GDP is not meant to imply that gross output is a component part of GDP, a value-added measure. Elsewhere, Benanav uses manufacturing value-added, and in the text itself this distinction is more clear than in this single line.

as the "mechanism transmitting this problem across the world" (ibid) because they force competitors everywhere to meet the same global price, at least when controlling for transportation and various other transaction costs (this is also, it should be noted, one of Koo's points). The result for investors is that these falling prices then lead "to falling income-per-unit capital ratios (falling capital productivity), then to falling rates of profit, then to lower rates of investment, and finally to lower output growth rates" (ibid). Because of manufacturing's centrality within production more generally, this industrial glut is then generalized across the economy, leading to lower overall growth in output and productivity.

It is this process, not some dearth of technological "miracles," that is both the source of the stagnant productivity growth identified by Gordon and of the decline in productive investment identified by Koo, Klein and Pettis. Investors prefer portfolio investments not simply because they are easier (as Koo implies), but because investment in an over-capitalized industrial sphere that is already suffering severe overcapacity is very simply a bad investment.[23] And, as Howell repeatedly emphasizes, it is a bad investment because it isn't profitable. In fact, it is so unprofitable that dumping money into "safe" low-yield securities like Treasury bonds or pumping financial markets to constantly refinance portfolio investments is clearly preferable to funding new production. As Smith points out, "corporations spent $3 out of every $5 of their net profit on stock repurchases between 2015 and 2017" with it widely recognized that "the net

[23] This also reiterates the point that "underconsumption" cannot possibly be the cause of this slowdown, since a large portion of the industrial sector is composed not of lines dedicated to consumer goods but instead those devoted to producers' goods, with expansion periods driven as much by industrial demand as consumer demand. Though this is only one of the faults of underconsumptionist theories of crisis, there is no need nor space to reiterate the larger problems with such a theory here. The critiques of underconsumption are so numerous in the literature, so widely agreed upon between otherwise competing schools of thought and so damning as to need no repetition. For the basic argument, see: Mattick (Sr) 1969 and 1981, Mattick (Jr) 2011, Shaikh 1978 and 2016, Roberts 2016, Carchedi 2013, Kliman 2006 and 2011, and Clarke 1994. But even Koo's works offer a functional critique of the Keynesian underconsumption approach.

effect of such buybacks is […] to 'starve' investment" and further drive up asset prices (Smith 2020, p.49). Even among traditional manufacturing firms, such stock buybacks, asset investments and various manipulations of the carry trade have consistently provided more reliable returns than traditional productive investments (as documented by Howell 2020, Tooze 2018 and Lee et. al. 2019). This has more or less mirrored, at a global scale, the conditions of the bubble in Japanese industry that built up over the 1980s (as documented in Itoh 1990, Brenner 2003 and Koo 2012). Nor is this simply a problem for older manufacturers burdened with masses of obsolete fixed capital. Supposedly cutting-edge firms in new technology sectors such as Electric Vehicles have been equally or even more prone to this condition. For firms such as Tesla, actual productive activity (making electric cars, their affiliated software, solar panels and various battery systems) is almost entirely dependent on federal subsidies rather than sales on the market (Isidore 2021). Similarly, the firm has made more profit through its stock-pumping and speculative cryptocurrency purchases than on vehicle sales (Kharpal 2021). Meanwhile, the vast bulk of revenue for major tech conglomerates such as Alphabet and Facebook comes from advertising (Smith 2020, p.43) and many are entirely unprofitable, being sustained for years on nothing more than venture capital injections (Holmes 2019). Such "Big Tech" firms now compose some two-thirds of the top ten publicly traded companies by market capitalization (Smith 2020, pp.48-49). Thus, even many seemingly traditional manufacturing companies that lie at the forefront of technological advances within hi-tech industry are, in reality, largely subsisting off of some combination of federal, financial or commercial rents.

**Production and Profitability**

It is in this sense that changes in profitability, as the necessary precursor for investment, underly more general trends in output and productivity. Moreover, it is easy to see how this correlation is linked to a clear causative mechanism—which remains aleatory in the work of Koo and Gordon—since low returns on investment in a particular sector will tend to discourage capital from flowing to that sector. As Smith explains,

> the rate of profit can […] be understood as having a crucial regulating role in the performance of capitalist economies, since it determines—that is, puts limits on—the rate of investment and, in turn […] unemployment, productivity, and worker compensation (Smith 2020, p.95)

The reality of persistently low rates of return within industry is so widely recognized among actual investors, that it is simply treated as general knowledge (Howell 2020). As already noted above, Benanav points out that the centrality of manufacturing to economic growth means that trends in this sector, even as it has come to employ a smaller and smaller share of the total workforce through a general deindustrialization, nonetheless exert a substantial influence on the overall growth rates of output and productivity across the global economy.[24] In order to understand why, exactly, this is, we must return to the question of technical change, but now

[24] This is something of an oversimplification, since part of the decline in manufacturing workforce share is due to a general firm restructuring wherein many service tasks, such as clerical work, for instance, once performed within the firm and therefore categorized as "manufacturing" employment are spun-off into separate companies, where their categorization changes. Smith is more attentive to this factor. But the reality is that this alone is not the cause of the declining share of manufacturing labor and seems not to be the main contributor in any real sense—recategorizing the occupation of some office workers does not tend to leave behind masses of laid-off workers and toxic abandoned industrial sites. In other words, the reality of the rust belt cannot be explained by mere changes in accounting practices. At the same time, for a more accurate, rigorous picture of the overall phenomenon, these changes should of course be mentioned as playing some role. They, at minimum, aggravate the trend within the available statistics.

with a more granular focus on the nature of concepts such as "services," "automation" and "productive" vs. "unproductive" labor.

Both Benanav and Smith turn to the postwar work of Princeton economist William Baumol in order to define exactly what constitutes a "service" and how the category is conceptually related to questions of "automation," "routinization" and "mechanization" (Benanav 2020b, pp.56-64, Smith 2020 pp.72-91). Baumol's work on the topic is interesting both because he was among the earliest economists to attempt to systematically grapple with the question of the "service economy" and because he did so by emphasizing the differentials in labor productivity between service and non-service occupations, rather than attempting some qualitative definition based on the character of the consumable being produced.[25] In fact, Baumol's argument attempts to account for the exact same phenomenon observed by Gordon: "why advanced industrial economies tend to exhibit, at a late point in their development, ever-lower productivity growth rates" (Smith 2020, p.72). One important point made in Benanav's review of Baumol is the way in which service output growth tends to be "generated by expanding employment," which leads Baumol to refer to services as "a relatively 'stagnant' economic sector" (Benanav 2020b, p.57). Thus, even while the period after the 1970s (here measured as 1980-2010) saw the growth rate of output in services remain higher than that in manufacturing worldwide, productivity didn't follow. In fact, many countries saw negative productivity growth in services in this same period. By contrast, growth in employment in services tended to track growth in output, confirming Baumol's original observation that service

[25] Baumol is by no means the first economist to try and define "services" or to grapple with the term's meaning. The term had long been used, more or less uncritically, all the way back to the era of classical political economy, as documented in Smith 2020, pp.78-80.

output growth is technologically "stagnant" and therefore dependent on the expansion of employment (ibid, p.58, Figures 4.3 and 4.4).

In this period, such an expansion is possible precisely because deindustrialization leads to declining shares of employment in manufacturing and global industrial overcapacity helps to drive down the cost of manufactured goods. These trends are driven by what Baumol calls the technologically "progressive" sector, helmed by manufacturing, which comes to include any type of work in which labor and production are more susceptible to "innovations, capital accumulation, and economies of large-scale" (Baumol, qtd. in Smith 2020, p.72). Since productivity is able to increase in this "progressive" sector it becomes the dynamic core of economic growth. But this very dynamism paradoxically "causes the aggregate productivity growth rate of […] economies to decline over time" (Smith 2020, p.72) because high sectoral productivity ultimately leads to the production of more output with fewer workers and, therefore, the relative shedding of workers (per unit of production). Unless output can expand faster than productivity, these workers will have no choice but to either remain unemployed or seek work in the "stagnant" sector, which is resistant to similar productivity-enhancing transformations. While it's true that output can, at times, increase faster than productivity and therefore result in growing employment despite the shedding of labor per unit of output (as in the postwar period), the very "progressive" nature of the productive sector tends to ensure that, over time, productivity growth outpaces output growth. As we'll see below, this is particularly true given the nature of competition and the reality that productivity-enhancing techniques tend to be embodied in large-scale fixed capital investments that require long-run returns, fixing certain technical regimes in place and ensuring an uneven terrain of technological obsolescence.

On the surface, this produces the now-familiar situation in which "the vast majority of the world's underemployed workers […are] employed in the heterogenous service sector, which accounts for between 70 and 80 percent of total employment in high-income countries" (Benanav 2020b, p.56). In such conditions, the very concept of "services" becomes reified, taking on a life of its own and often acting as the starting point for analysis. In other words, the very concept of a "service" is taken as a given and treated as if it has positive content—as if there is an almost immutable, ontological difference between a "good" and a "service" that extends back into the mists of prehistory. The importance of Baumol's work, however, is precisely that it demonstrates how this concept is produced in the first place, through a material divergence in the technical character of different activities. Essentially, "services" marks a vast "other" into which any activity that resists mechanization will be placed, and it is thus "heterogenous" by definition. But there's nothing qualitatively inherent in the distinction. As soon as something can be rendered via an industrial process, it ceases to be a "service" and instead becomes a "good." This happens with regularity, as new bundles of technology enable sectors previously resistant to "the incremental process innovations that generate rapid rates of productivity growth" to now be increasingly mechanized or even automated outright, at which point they become "amenable to dynamic efficiency gains" (ibid, pp.57, 59). But as soon as these services become goods, they are then subject to the exact same pressures as other forms of production in the "progressive" sector. Even worse, they enter the productive sphere at a late stage, during which it is already suffering from the general redundancy of productive capacity worldwide. The result is that goods in "newer industries" introduced into the growing glut since the 1970s "have suffered from worse overcapacity than older ones" (ibid, p.59).

Meanwhile, since the remaining service industries can only grow via expanding employment—because prices cannot be lowered and demand thereby raised through increased productivity—they become increasingly subject to what Baumol called the "cost disease," where "sluggish rates of productivity growth mean that services become ever more expensive relative to goods" (ibid). On the one hand, this occurs because many services become more complex without becoming more productive, as Baumol explains in the cases of healthcare and education (Baumol 2013). On the other, this means that the only way to lower the costs of services is to somehow lower labor costs. Today, this is largely done through improvements in "flexibility," often advertised as cases of "automation" or as productivity-enhancing advances in "e-commerce" (as in the case of digital car and hotel services, food delivery, or just the general increase in same-day delivery for all kinds of online orders), when really they are nothing more than methods for driving down the cost of labor. This "super-exploitation" is particularly common in service industries precisely because the wage bill makes up such a large share of the firm's total costs. Then, in economies where services make up the vast majority of employment, this naturally leads to a general decline in the labor share of income (Benanav 2020b, pp.59-60).

In exploring the deeper dynamics behind these trends, the case of Baumol, like Howell, leads again to heretical influences. Benanav notes the way that, throughout his work, Baumol seems to not only reproduce key elements of Marx's own understanding of technical change within capitalist societies, but even to reproduce homologous terminology to describe these sectors. It seems hardly coincidental, for example, that the "stagnant" service sector leads to general conditions of general labor surplus which Marx describes in almost identical terms (ibid, p. 57). This is not merely accidental. Despite the fact that Baumol was a liberal who would go on to help constitute the postwar orthodoxy in the discipline of economics, his own early career

actually strayed well outside this orthodoxy. Nor was this merely a matter of youthful political idealism leading him to some general familiarity with the works of Marx. Instead, Baumol was a deeply knowledgeable, if ultimately critical, reader of Marx and, in fact, stepped in to publicly defend the validity and purpose of Marx's value theory against the widely publicized criticisms levelled against it by the New Keynesian Paul Samuelson, who was writing in response to the rising interest in Marx in the student movements of the era (Baumol 1974, recounted in Roberts 2017). Not only did Baumol prove that Samuelson (like others before him) had fundamentally misinterpreted the purpose and character of Marx's value theory, but his defense also provided a foundation for many self-described Marxists who would make the same case over subsequent decades (such as Carchedi 1984, Kliman 2007, Moseley 2018). Thus, it seems hardly coincidental that Baumol here explains the dynamics of technical change in a fashion fundamentally similar to that of Marx.

But one key difference is that Baumol seems to retain, throughout his work, the orthodox presumption that any activity that can find a buyer is ultimately productive of value, in some sense, even if its productivity fails to grow over time. Smith offers an important corrective here, drawn more explicitly from Marx. He begins by noting that Baumol's divide between a technologically "progressive" and technologically "stagnant" sector is more useful than the reification of the concept of services, precisely because "the notion of a service sector, like the category of 'services' more generally, conceals as much as it clarifies" (Smith 2020, p.76). Not only is it deeply heterogenous, but conventional business statistics don't even attempt to measure the category according to occupation. Instead, data collectors deem "all employees of a business whose primary product is, for example, manufactured goods as manufacturing employment, even if they are janitors, accountants, legal staff, or computer-repair personnel" (ibid), which also

means that the technologically "stagnant" sector also tends to be incorporated into even the "progressive" manufacturing industries in a way that remains un(der)accounted for. This leads to particularly strange patterns in the contemporary global economy, since contract manufacturing and the international division of labor create "curious anomalies" such as the case of Apple, which "is a manufacturing company that owns no factories" (ibid) and yet sees the output of the other firms it contracts to make its goods listed as its own in official statistics (as described in Norfield 2017, in a phenomenon that will be explored in more detail below). The implication is that technological stagnation is more widespread than it initially appears, but also that it is not purely technical.

Here we return to the basic observation offered by Benanav, that the (proximate) driving force is precisely overproduction in industry. While the mechanics linking the growth of the technologically stagnant sector to more general economic stagnation in output and employment are clear, Baumol's account doesn't give a clear picture of why, exactly, industrial output cannot simply continue to increase. After all, there are certainly periods in which it does so (as in the postwar decades pointed to by Benanav, Smith and Gordon). While Benanav tends to emphasize the reality of the global glut in manufactures and its immediate source in intensifying industrial competition, this doesn't quite go all the way to the bottom of the phenomenon. It offers a proximate cause, but not an ultimate one. Someone like Koo would immediately point, again, to the postwar period, where trade was liberalized even while capital controls were in place, allowing for the same competition to seemingly generate output growth in excess of productivity. Why, in other words, did this period not suffer from overcapacity, while later decades did? Koo, of course, points to the liberalization of global capital markets. But, as has already been pointed out (and will be emphasized in more detail below, in the review of Brenner), this was a result of

increasing competition in the context of the trade war with Japan. It cannot be the cause, since it did not precede this competition. Where, then, is the point at which seemingly "healthy" competition turns into an eviscerating zero-sum race to the bottom?

Very simply: there is none. The postwar period only takes on the special character in which output growth exceeds productivity because it followed decades of war and depression which wiped out masses of obsolete fixed capital, killed millions of proletarians who had been made "surplus" to the needs of the economy, rejuvenated the coffers of the victorious faction of the global capitalist class with the plunder of war and forced that class to unify around a systematic program of military-led technological rollout followed by joint military-industrial reconstruction schemes in Europe and Japan (Mattick 1969). Essential to this revival was the rising role of government spending as a share of GDP, which continued well beyond the collapse of the 1970s and remains an integral feature of essentially all major economies today (Mattick 2011, Chapter 4). In a way, the pattern here is similar to the "*yin-yang*" cycles identified by Koo. In periods of crisis and depression, investment declines (particularly in manufacturing), output growth slows and productivity growth surpasses it, leading to declines in employment growth. But rather than just being due to the necessity of paying down debt (as Koo argues) this is also driven by the fact that general profitability has fallen across the economy. As Howell argues, there simply aren't profitable investments available in productive sectors. Importantly, this fall in profitability *precedes* the rise in debt identified by Koo. It is the root source of the shift to speculative portfolio investments, which lead to asset bubbles, which then pop, producing the debt that then weights down firms in the balance sheet recession.

But, as is clear in the case of the postwar boom, this very crisis then helps to clear out uncompetitive capital, remove obsolete plant and equipment, reduce the overcapacity identified

by Benanav and often literally create new sites for investment and new sources of cheap labor through even more destructive means such as war. This alternation between boom and bust is conventionally thought of as a business cycle and is, therefore, portrayed by Koo and others as a form of oscillation around some point of equilibrium, or even around the upward-trending line of general growth. At the same time, Baumol's basic observation is that there is nonetheless a secular tendency that lies beneath these cycles: across the economy and especially within its productive core, labor will tend, over time, to be replaced by machines *even while full automation can never be reached*, as Smith and Benanav so clearly demonstrate. But Baumol's point here is not really his own. It is a repetition of one of Marx's clearest and most consistent predictive observations about the long-run tendencies of capitalism. Labor will tend to be replaced by machines and, because of this, a "surplus population" will take shape (this latter point will be explored in the section immediately below). Similarly, the average size of firms will tend to grow and their social scale will tend to increase, implying that the state will become more and more intricately involved in economic management over time (Mattick 2011, pp.48-51). All of these trends are driven by the problematic nature of retaining profitability in the midst of increasing competition. Declining profitability leads to crisis and depression, which are "the cure for insufficient profits," but the new period of prosperity produced "will in turn generate the conditions for a new depression" (ibid, p. 50). More importantly, the various countervailing factors that can be mobilized against declining profitability only ultimately make the problem worse, threatening larger, longer and more permanent depression when they are no longer able to help clear the global glut or sop up the excess money and labor being pumped out of production.

Among the most important of these long-term tendencies is the rise of not just technologically "stagnant" activity across the economy but of more fundamentally

"unproductive" forms of industry and employment. One of the main ways that productive versus unproductive labor can be understood is, again, through its relation to profit.[26] It is here that the account of Smith diverges strongly from that of Baumol, since Smith argues that productivity differentials between the "stagnant" and the "progressive" sectors aren't enough to explain more general stagnation in the economy at large because the "stagnant" sector itself includes many heterogenous activities, among which only some directly contribute to profit. This fact is observable in many of the sectors that Baumol argues suffer from the worst effects of the "cost disease," such as education and healthcare, since these are essentially public services. The lack of marketed product within these sectors leads to fundamental difficulties in measuring their productivity which is why "economists must therefore assign them an 'imputed' output, an estimate of the value of these activities had they been sold on the market by private businesses" (Smith 2020, p.89). This calculation is problem is itself a hint that there is a deeper issue here about the nature of productive, versus unproductive labor.

This can be seen most clearly in the "countervailing" factors that push against the tendency for profitability to fall. While most theorists, including the vast majority of self-described "Marxists," portray the tendential fall in the rate of profit as the Marx's most important law defining the motion of the capitalist system, Marx himself was more tentative and, in fact,

[26] This is actually an oversimplification, but it's a functional one. Technically, it is here that the importance of what I periodically refer to as the "deep structure" of value is revealed, because the critique of value recognizes that there are certain unacknowledged but axiomatic principles dictating the logic behind terms such as productivity, and that this logic often leads to seemingly strange conclusions, such as the idea that human labor is productive of value while that of animals or machines is not. There is no space to get into the distinction here and I generally avoid diving too deeply into this "deep structure" as theorized by Marx himself and picked apart by later "Marxological" investigations. But it should at least be noted that the question of "productive " versus "unproductive" is rooted in an understanding that value is fundamentally an *anthropological axiom* that is created and reified through human activity but is ultimately *inhuman* insofar as it exceeds conscious control of any individual or constitutive collective—which is precisely why the value-annihilating force of revolution, understood as proletarian self-abolition founded on reflexive negativity, is the basis of the communist political program.

clearly located the central "law of accumulation" elsewhere (which we will see below). On the one hand, many have pointed out that Marx's own theory of crisis remains incomplete (Heinrich 2013, Clarke 1991). On the other, many thinkers have offered reasonable, consistent methods to "complete" or "reconstruct" this theory on the basis of the "law of the tendency for the profit rate to fall" (Moseley 1997, Kliman 2007, Carchedi 2013). But both advocates and detractors often fail to consider the strange paradox embedded in the very idea of a "law" that is a law of a "tendency." Rather than the relatively simple mechanics implied in the laws of classical, mechanical physics, Marx seems to repeat a similar ambiguity with regard to this law as he does in his (much more complete) exploration of "value" as such, which he treats as simultaneously acting like a substance and a field (a point first made by Mirowski 1989 and more recently affirmed in Pitts 2020). Falling profitability is a "law" in the substantial sense, because its mechanics (even if involving multiple "forces") are clearly visible and influence crisis—as Mattick points out: "profits stagnated or even began to decline several quarters before each of the three recessions, starting respectively in 1990, 2001 and 2007 [… and data] shows that something similar occurred in each of the recessions that the US economy has gone through […since] the last decades of the nineteenth century" (2011, p.39)—but it is a law of a "tendency" that is clearly field-determined by the relative influence of countertendencies. The probabilistic dimension of the "tendency" thus exists in uneasy truce with the deterministic dimension of the "law." It might be possible to roughly divide out these dimensions as follows: the law of decline exists in substance as a more or less measurable force, but the exact coordinates of the decline are determined more precisely *within a probability space* set by its countertendencies than by a simple, deterministic trajectory that one can project from its core cause (the replacement of labor with machines). In this sense, what most theorists miss is the fact that it is not the (relatively

simple) tendency that is the real protagonist in this story. Instead, it is the countertendencies that drive the plot just as, to return to the original metaphor, it is the underlying probability functions drive the event paths across a probability space without necessarily determining them in the last instance.

Among the most important of these countertendencies is the need to suppress wage costs and exert discipline in the workplace. When flagging profits prevent firms from being able to invest in newer plant and equipment to raise productivity, the only means by which to raise productivity (even marginally) and retain competitiveness is through labor discipline. Ironically, this often entails expanding various "service" occupations even within the productive sphere and "we can even expect—paradoxically—the ratio of managerial staff to non-supervisory employees to rise during turbulent periods" (Smith, p.99). The rise of labor-disciplining supervisory work is accompanied by the rise of similarly marginal occupations in commerce, legal services, accounting and financial activities that help ensure the final sale of goods and sustain the liquidity of capital. The supervisory category itself can also be extended to include numerous activities that preserve social reproduction more generally and which are more clearly net costs, even if they sometimes act as onerous methods for extracting rents through simple dispossession. Nor is there always a strong divide between such activities, as financial services such as payday loans may be more of a supervisory or rent-seeking task serving to discipline labor than it is a necessary commercial activity. These general supervisory tasks linked to the disciplinary functions of social reproduction are, however, more evident in vast carceral and police apparatus (Gilmore 2007, Neocleus 2021).[27]

[27] While Smith tends to split these into two broad categories encompassing "circulation" work and "supervisory" work, he is borrowing these terms from an earlier argument made by Fred Moseley (1997). Today, however, the terms sound somewhat deceptive. This is because what Moseley calls "circulation" activities are better described

Some of these activities are more clearly able to stimulate indirect increases in productivity, as in the case of circulation workers' role in accelerating the turnover rate of the production circuit or managers forcing more work out of the workforce through systems of monitoring, rationalization and punishment, while others seem only distantly related to any sort of increase in production at all, as in the case of public services that tend different aspects of social reproduction. But all are "a necessary but 'incidental' cost of carrying out capitalist operations that must be paid for out of the profits earned elsewhere in a given firm or in the economy as a whole" (ibid, p.100). Such activities are not "unproductive" because they offer no good or service, or because they are not renumerated. In fact, they are unproductive *precisely because* they are renumerated, since the payment of wages and the various other expenses of these occupations requires the funneling of profits from core productive activities out to sustain all these indirect but necessary activities. The same is true at the more general level of completely unwaged but completely necessary work of social reproduction—i.e., work that defines the gender divide—such as housework, childcare, emotional and sexual labor and numerous other activities (Endnotes 2013, "Logic of Gender"). Ultimately, "whether a given

as commercial and financial activities and don't include the type of "circulation" work that has become more familiar in recent "circulation struggles" (Clover 2018) across the logistics sector. For Marx, commercial and financial activities are "circulation" activities, but so are transportation and warehousing. However, the latter activities are often considered productive despite being circulatory in nature. Moseley refers exclusively to the various commercial and financial aspects of "circulation" in his own description: "Circulation labor is labor related to the exchange of commodities and money, including such functions as buying and selling, accounting, check processing, advertising, debt-credit relations, insurance, legal counsel, and securities exchange" (Moseley 1997). Smith later argues that circulation activities such as those in the logistics sector should also be understood as unproductive today. While he acknowledges in a footnote that this is a contested claim, he nonetheless classes the shipping and storing of goods alongside purely commercial or supervisory activities as unproductive labor. While I disagree with Smith's categorization of these activities as equally unproductive—and would argue that this is not a particularly ambiguous point in Marx's writing, though it has been contested, and is instead ambiguous in a reality wherein its often hard to perfectly separate out production, processing, packaging and piecemeal transit within and between firms—the distinction is not important to the overall conclusion.

type of labor is productive or not therefore depends instead on what role it plays in the total circuit of capital" (Smith 2020, p.103).

One long-term trend that follows from those identified by Marx, then, is that "the rising proportion of the labor force working in circulation and supervision represents an increasing cost to the system as a whole" (ibid, p.101). This proportion tends to rise over time as the active expression of the countertendencies pushing against the tendency for profit to decline. It also moves in step with linked trends in the increasing concentration of capital, increasing mechanization leading to a growing total mass of capital and a cheapening of commodities and an increasing trend to drive down wage costs—all, again, countertendencies animated by the problem posed by profitability. In general, this can be characterized as leading to a general increase in unproductive labor across the economic system and often within individual firms. While this unproductive activity is initially justified because of its ability to counteract otherwise declining profits (or the social effects of falling wages and rising surplus population) and to make individual firms more competitive, it ultimately becomes a further drag on profitability at the economy-wide scale. While these activities can frequently be targeted in later labor-shedding efforts and may be eliminated in the midst of crisis (as in the culling of mid-level clerical work in the course of the Great Recession), this doesn't solve the fundamental problem and is usually more a case of squeezing labor by redistributing such work onto lower-paid employees. Every countertendency has the effect of worsening the tendency itself, in the long run. Thus, rather than a divide between "goods" and "services" or even Baumol's more granular attention to technologically "progressive" versus "stagnant" sectors, Smith argues that the real core dynamic

here is "the growing disparity in labor productivity growth between two 'sectors' of the economy, one productive of value and surplus value, the other not" (ibid, p.106).[28]

One important dimension of this not explored thoroughly by Smith—but certainly relevant to the Chinese case—is that state activities tend to multiply, often to such an extent that clearly dividing between "public" and "private" spheres is no longer a simple matter due to intricate chains of interdependency. Paul Mattick Jr. (2011) documents this over the long run, expanding an argument originally made by his father, Paul Mattick Sr. (1969) in regard to the newly emergent "Keynesian" consensus in the postwar West. The rising and continuing role of government spending in almost all major economies in this period, including the United States under the prototypically "neoliberal" Reagan administration, clearly demonstrates the more fundamental point that "economic policy […] was not under ideological control" (Mattick 2011, p.73). Instead, policy has tended to serve economic imperatives. This means that the growth of unproductive labor more generally is also visible in the growing direct involvement of the state in buoying output. On the one hand, this is visible in the federal debt, which "had reached 37.9 per cent [of GDP] by 1970" and, by 2004 "was 63.9 per cent" (ibid, p.75). But it's also more directly evident in the deep interdependency between output and government involvement. By the early 2010s, Mattick points out that "the American government (federal, state and local) [was] already responsible for about 35 per cent of GDP" (ibid, p.78). He continually points out, however, that "government-financed production does not produce profit" in the true sense (ibid, p.81). Instead, much of the profit that appears on the books at firms dependent on government

[28] Again, we return to the necessary caveat that Smith, Mattick, myself and others are often technically speaking of productivity with reference to the "deep structure" of value, not simply in conventional terms. There is overlap between the two but it does require some attentiveness (which I've tried to make clear throughout) between whether we're speaking of "labor productivity" in the sense used in conventional business statistics or "productive of value" in the sense derived from Marx and value-critique more generally.

financing—he points to Boeing meeting defense contracts and receiving general tax subsidies—is effectively just profit siphoned from elsewhere in the domestic economy (through taxes) or the global economy (through bond purchases). In this sense, it is purely unproductive. That also means that, when it surpasses a certain share, as in World War II, "the growth of private capital [comes] more or less to a halt" (Mattick, p.79).

More importantly, expansive programs of state spending and nationalization, such as those advocated by MMT thinkers, have tended to be met with a similar freezing of general investment, culminating in capital flight. Mattick here points to the case of the Mitterand government in France, which raised the minimum wage, shortened the work week, imposed taxes on the wealthy, invested in public works and nationalized private companies. The ultimate effect was that the global financial market imposed an effective quarantine on France and French capital fled abroad. Meanwhile, the freezing of private investment then meant that "unemployment continued to grow and the franc had to be devalued three times," (ibid, p.73) with stagflationary pressure essentially forcing this avowedly "socialist" government to impose almost identical "neoliberal" measures as those seen in the UK and US—not necessarily by minimizing government spending but by redirecting state involvement away from pro-labor policies and toward various measures designed to restore profitability and further discipline labor (ibid). In fact, in the European case, the rising role of the government in this process was clearly observable in the state-building effort that would result in the formation of the European Union. This political project was undergirded by the growing influence of so-called "ordoliberal" theories that emphasized the integral role of the state in maintaining the baseline conditions ensuring further accumulation (Bonefeld 2017) and, as documented by Koo, Klein and Pettis, the

administrative structure of the EU has itself been integral to the relative success of German (and, to a lesser extent, French) capital, at the expense of Southern and Eastern Europe.

The rising involvement of the state as a symptom of the general increase in unproductive labor and the long-term effect this has on depressing the profit rate and, thereby, reducing business investment, provides the general coordinates through which we can understand the basic appeal and the basic error of MMT. On the one hand, MMT is appealing precisely because it points to the fact that the state is already deeply involved in the economy and already has the practical means, through its own accounting mechanisms, to engage in widespread public spending. MMT thinkers build on the popular recognition that this spending currently serves the needs of capital and put forward what seems to be a fairly straightforward argument that many of the exact same mechanisms ought to simply be redirected to serve the population at large. This is often branded in terms that make this logic clear, such as in the call for a "People's QE," where established quantitative easing measures would be used to stimulate public spending rather than pumped into the capital accounts of the financial sector. But the fundamental point made by Smith and Mattick is that this state involvement is both necessary *and* unproductive. It is necessary precisely because it serves the interests of capital, helping to bolster returns in the face of diminishing profitability even while this very activity then further depresses profitability in the long run by ensuring that productive investment will be even less appealing, given the easy access to credit through which a seemingly infinite bubble of financial asset purchases, stock buybacks and continual re-financing that is enabled by this ongoing state intervention. It must be remembered here that QE and these other forms of central bank intervention are performed precisely in the name of preventing a depression and stalling the deflationary spiral identified by Koo. Redirecting such funds to public works and full employment would immediately starve

capital of such funds and trigger a bursting of this chain of re-financing bubbles. Similarly, continuing current policies and simply spending more on top of that in order to fund public works will result in the scenarios outlined above with regard to the global impact of MMT policies.

But even ignoring this, the reality is that expansive public spending, increases in the minimum wage and even the effective nationalization of certain industries will ultimately lead to a general freeze in investment (really, just an acceleration of the decline already underway), as seen in World War II or in the capital flight witnessed in Mitterand's France. Even if we set aside all other potential problems, there is simply no clear path by which MMT spending would be able to revive the core growth engine of production since its policies are, by definition, simply adding more capacity to a global economy already suffering from severe overcapacity. At the very best, MMT seems to gesture toward the classic program of Eurocommunism, itself recapitulating the reformism of the German Social Democratic Party under Kautsky and Bernstein in the early 20$^{th}$ century—wherein it was imagined that the slow, peaceful building of electoral power could result in a series of escalating nationalizations, ultimately amounting to a bloodless transition between capitalism and socialism as productive activity came more and more under the banner of a state that the workers had won at the ballot box. In Weimar Germany, the results of this strategy did not follow the forecast of Kautsky and Bernstein, to say the least. In the case of the postwar Eurocommunist parties, which were the necessary buoying force behind the Mitterand government, the reality of class warfare immediately foreclosed any pleasant forecast and forced the "socialist" administration to enact the very austerity policies it had sought to reverse. If the most extreme instance of MMT theory were to be implemented today, seeing more and more productive activity in leading sectors (such as alternative energy) increasing

nationalized, the result would be the same: class warfare, capital flight, the threat of civil war. Even in the milder case, where MMT-style spending was limited to a few sectors and fell short of full nationalization, any new capacity generated through such programs would essentially all be unproductive, since it will be completely dependent on constant injections of money from the state to sustain itself. This will only serve to further depress the incentive for investment. At the very best, this leads down a negative feedback loop wherein increasing disinvestment on the part of the private sector must be constantly made up for by growing investment on the part of the state. At some point, the inflation limit that marks the "real productive capacity" of the economy will be reached. Even if it isn't, it's hardly reasonable to assume that all the world's wealthiest people will happily sit back and see their property gradually nationalized. If the case of Mitterand's France is any example, it's clear that the immediate response will be capital flight and a global financial quarantine. Similarly, if the case of early 20$^{th}$ century Germany is any example—likely a better reference point for a nation such as the US, where the murderous, heavily armed police force outnumbers most national militaries—the result could be far more grim.

**Surplus Population and the General Law**

The core of this disproportionate growth between productive and unproductive sectors, which initially appears as a divide between goods-producing sectors and services can only be identified by digging below the initial appearance to identify the technical dynamics that produces the disproportion in the first place. Baumol's identification of these sectors as technologically "progressive" and "stagnant" hints at the deeper source, which is, precisely, a technological limit, but not in the sense used by Gordon. Instead, the technological limit here is the fact that

labor is, over time, replaced in the core productive sector by machinery. This expands the mass of capital in absolute terms, while also expanding the mass of surplus value and, since labor is itself still necessary to the process, the capitalist "law of population" (Itoh 2020) guarantees that commodified labor power is subject to similar market pressures as other commodities even while it remains "a difficult commodity for capitalist society to treat harmoniously in relation to fluctuations in demand in a market" (ibid, p.256). This is, for Marx, "a fundamental contradictory problem for the capitalist economy" because it must "utilise human labour power as a commodity without being able to produce it and to adjust its supply to fluctuations of demand in the process of capital accumulation unlike in the case of most other commodity products" (ibid). The result is the incorporation of ever-more workers into the capitalist economy as firms seek cheaper sources of labor, even as less labor is required to produce a given unit of output, resulting in the relative growth of a "surplus population" in excess of the needs of production. As elsewhere, this is not simply a mechanical result of the tendency for the profit rate to fall but is instead a process driven by the fundamental *problem* of profitability in general, shaped more by the countervailing factors pushing back against the threat of falling profit than by the fall in profit itself. In fact, it is this process of technical change and the production of a surplus population, and not the law of the tendency for the rate of profit to fall, that Marx designates as the "general law of capitalist accumulation" (the title of Chapter 25 of *Capital*, Volume 1).

This concept is extremely important, both for framing the precise dynamics through which we can understand related trends in the concentration of capital and falling profitability and for its central importance in contemporary accounts of capitalism based on Marx's original critique, including attempts to translate between the purely abstract realm of critique and the

practical realm of concrete political strategy. Clarke (1994) reviews in detail Marx's own treatment of the question of crisis in the sequential drafting of his critique of political economy and also reviews the longer history of "Marxist" crisis theory after Marx, wherein different aspects of Marx's fundamentally incomplete crisis theory were emphasized at different points in time, producing a proliferation of seemingly non-commensurate "Marxist" theories of crisis. Clarke demonstrates the way in which each of these attempts was shaped by pragmatic, political demands of each era and how each was, more importantly, limited by the fact that most of Marx's writings on the topic were not widely available, even in German, until the latter half of the twentieth century (1994, Chapter 2). Meanwhile, the trend was aggravated by the slow transformation of Marx's *critique* of political economy into "Marxist political economy" or "Marxist economics." In other words, many "Marxist" thinkers attempted to place crisis theory on the same foundations as economic orthodoxy, "in particular the foundations of general equilibrium theory, neglecting the critical dimensions of Marx's theory" (ibid, p.10). This further contributed to the fragmentation of crisis theory, since, on the basis of orthodox economic assumptions, it is impossible to see the interconnection between the various strands of the critique of political economy that come together in the "general law of capitalist accumulation."

As Smith and Benanav note, this general law is driven by mechanization within production, which is associated with a general growth in both the mass and relative share of capital laid out for plant and equipment relative to that laid out for labor. This also produces an excess of employment in the unproductive sectors. In this regard, Smith's emphasis on productive vs. unproductive labor mirror's Marx's own explanation of the phenomenon:

> […] the extraordinary increase in the productivity of large-scale industry, accompanied as it is by both a more intensive and a more extensive exploitation of labour-power in all

> other spheres of production, permits a larger and larger part of the working class to be employed unproductively. (Marx 1973, p.574)

Here, "intensive" and "extensive" have important connotations, since they imply both the intensification of productivity within industry as well as the general extension of capitalist relations (namely, dependence on the wage) to more of the population at large. On the one hand, this means that the systemic requirement of ever-greater accumulation as well as the constant struggle to retain profitability in the face of competition leads firms to "incorporate the labor-forces of areas where the reproductive cost of labor is lower, and where necessary labor is thus less relative to surplus-labor time" (Rubner 2015). On the other, it entails the acceleration of mechanization alongside increasing wages elsewhere." Altogether,

> The expanded reproduction of capital is thus both the expanded reproduction of the employed and unemployed populations, positing an ever greater relative surplus, a "disposable reserve army" bred by the capitalist mode of production. (ibid)

At its base, this process is therefore driven by two fundamental technical changes: mechanization in production and the construction of entirely new productive territories in different regions, constantly shaped and reshaped by new geographies of production.

This geographic implication will be explored in more detail below. For now, the final point that must be examined is exactly what character this "surplus" takes. Based on the language used by socialist movements in Europe in previous centuries, it is often assumed that Marx predicts a general growth of the "working class" in the sense of expanding industrial employment as a necessary precondition for the general polarization of society into two classes as identified in *The Communist Manifesto*. In part, this misunderstanding is based in a kernel of

truth, insofar as Marx does predict that industrial production will grow *in absolute terms* and extend across the globe. Moreover, there are numerous places in Marx's own work where the observable trend of increasing industrial employment in the nineteenth century is commented upon, since the initial proletarianization of the population and early extension of industry to a region will always see a larger share of the population compelled into industrial labor as the entirety of the population is forced into dependence on the wage. But the association of proletarianization with a growing industrial working class is, at root, a bald-faced misrepresentation of Marx's argument.[29] For Marx, "the proletariat is not identical to the industrial working class" (Endnotes 2010, p.33). Instead, "the proletariat is rather a working class in transition, a working class tending to become a class excluded from work" (ibid), meaning that *Capital* is in many ways best described as "a book about unemployment rather than about exploitation" (Rubner 2015). This has important consequences, because it means that the general proletarianization of the population entails the production of a surplus population which is nonetheless dependent on the wage for survival. The capitalist "law of population" thus poses a fundamental problem for the reproduction of the system as a whole.

As has already been argued above, the result is not simply a general growth of unemployment in absolute terms but instead the geographically and demographically uneven distribution of unemployment. In the most extreme cases (which shape the material process of racialization) particular subsets of the population are exposed to higher shares of absolute unemployment and are often systematically prohibited steady access to the wage. Historically,

[29] Nor is this a matter, as in the case of crisis theory, of a misunderstanding at least partially rooted in the "incomplete" nature of Capital and the inaccessibility of most of Marx's economic writings. The "general law" is laid out in full in Capital Volume 1, as published in its intended form, with a few edits over subsequent editions, all overseen by Marx himself. The misreading here is more a case of pragmatic, political needs within the early workers movement—which was, in fact, developing in the context of growing industrial employment—slowly overriding the theoretical conclusions laid out in Marx's own work.

this was often accommodated by expulsions of population back out into land-based subsistence, where they could be retained as a reserve army of labor even while their reproduction costs were externalized. But the bloody closure of the colonial frontier and the general subsumption of remaining agricultural zones into commodity production slowly eliminated this method of dealing with the surplus population. Thus, this uneven exposure to unemployment increasingly began to take on the character of widespread criminalization of the remaining subsistence activities—since these activities now tended to come into more and more explicit conflict with the baseline conditions for the reproduction of commodity society—and therefore the management of the surplus population through ever-larger prison and deportation infrastructure (Gilmore 2007, Endnotes 2013). Meanwhile, some of the worst effects of general global deindustrialization are felt in the poorest countries, where the trend manifests as "premature deindustrialization," meaning that increasing dependence on the wage via continuing proletarianization (almost always accompanied by dispossession from the land) is only accompanied by minimal growth in industrial employment, concentrated in fewer and fewer locations.

In wealthier locations and among populations less subject to racialized exclusion from the wage, the general conditions of "surplus" labor are visible both in a general decline in the share of working-age population in the labor force and in the general expansion of precarious, temporary and informal labor, loosely lumped together as "underemployment," as described by both Smith and Benanav. But Smith makes the point of connecting this trend to Marx's own characterization of the process as the rise of a "servant" economy driven by simultaneous expulsion of workers form productive labor and their need for a wage in order to survive. Nor is this a simple extrapolation from Marx's own "general law" now made with the benefit of

hindsight. It is, instead, the literal language used by Marx himself in describing this law. After explaining the basic technical trend in which machines replace labor and a "larger and larger part of the working class [is] employed unproductively," it becomes "possible to reproduce the ancient domestic slaves, on a constantly extending scale, under the name of a servant class" (Marx 1973, p.574). Moreover, this is related to the fact that "growing wealth, and the relatively diminished number of workers required to produce the means of subsistence, begets both new luxury requirements and the means of satisfying them" (ibid, p.573). This statement is then followed by a review of contemporary statistics demonstrating that "the number of English workers employed as servants […] in the houses of the middle and upper classes exceeded the number of workers employed in the textile and mining industries […] combined" (Smith 2020 , p.135). Smith connects this to more recent, similarly observable trends in employment in the postwar period and, more specifically, in the sharp rise in low-end service occupation that has dominated the "recovery" from the Great Recession.

Thus, the long-term and continual tendency of capitalist development is the production of a surplus population alongside surplus capital as well as the further differentiation of this population such that increasing absolute immiseration is accompanied by a rising "servant class" as well as ongoing incorporation of new populations into the remaining core of industrial production which still grows in absolute terms. Throughout, the "law of population" in capitalism continues to be a fundamental problem. In fact, it becomes more of a problem over time, since previously functional means by which the expanded reproduction of the system could be guaranteed are increasingly circumvented by the system's very expansion as geographic and social limits are reached and overcome in turn. In this way, we can identify a certain long-run tendency in capitalism which takes the character of a crisis but is better described as a systemic

propensity for breakdown, driven by immanent contradictions. This tendency is secular and observable in long-run data, but it is not mechanical in the teleological sense. It creates general conditions of instability that are politically important, insofar as they create a greater probability for intentional political activity to be able to overthrow the system. As Henryk Grossman, the author to whom such a deterministic "breakdown" theory is most often (falsely) attributed argues: "I am far from thinking that capitalism must break down 'of itself' or 'automatically' […]" (Grossman 2020, p.230). Instead, his breakdown theory "rather aims to show when and under what conditions such an objectively revolutionary situation can and does arise" (ibid).

This ongoing tendency toward breakdown is, therefore the "moving contradiction" (Endnotes 2010) of capitalist production, which defines its long-run motion and implies that individual crises will, over time, grow larger, longer and more dangerous, especially insofar as various mechanisms are deployed to diminish their frequency or mute their effects. It doesn't imply an apocalyptic collapse; it only makes such a collapse possible *if* it is taken as the intentional political project of an organized communist movement. Otherwise, the breakdown itself will grow ever-worse even while it periodically resuscitates the system through crisis and ultimately the rise of war and the fascist politics that is the most extreme expression and final result of liberalism itself (for this latter point, see an overview of the debates on the topic in Platkin and O'Connell 1981/1982). But the deeper theoretical problem here is that, even while the release of Marx's complete economic writings has now enabled a more critical and comprehensive understanding of this "general law" and its connection to long-run tendencies toward breakdown, which is related to periodic crisis, this is not quite the same as an explanation for *individual* crises. Understanding crises at the more granular scale requires more attention to immediate questions of geography and history.

# Section 2 – Geographies of Competition

**Introduction**

Even if there is no single "cause" of all individual crises in a simple, mechanical sense, it remains true that the "moving contradiction" of capitalism—its reproduction trending always toward long-run breakdown, plagued by the inherent problems of the "law of population" and the "law of tendency for the rate of profit to fall"—is "causative" in this broader sense, as the process principle underlying the movement of the system. The more fundamental problem with the debate on the question of the falling rate of profit and its exact relation to Marx's own theory of crisis is not, in fact, a matter that can be cleared up merely by additional exegesis of the original texts. It also requires a more systematic theorization of basic questions of concern to the social sciences, and therefore a more integrated engagement with philosophy of science as such. In this case, one useful area of further research would be to connect the dialectic logic used by Marx in attempting to elucidate more complex and inherently reflexive chains of cause and effect to similar debates in other studies of complex systems, such as biology.

I do not have time to pursue this line of inquiry here, but it does remain important to note that, in layout of the "cause" of crises below, there is some necessary distinction that must be made between the "breakdown" or "long run" tendency toward crisis and the immediate cause of individual cries, even while the two often draw on the exact same measures for their demonstration. For the sake of convenience, one could ascribe a similar divide here as in the distinction between "proximate" and "ultimate" causes that formed the basis of the biology

discipline's Modern Synthesis, as first proposed by Ernst Mayr (1961). In this scheme, a proximate cause is an "immediate, mechanical influence on a trait" whereas "ultimate causes are historical explanations" (Laland et. al. 2011). The two are sometimes (though it is not quite correct to say this) ascribed to "how" vs. "why" questions, respectively. Different sub-disciplines are separated by their focus on different types of causes—physiologists focus on proximate causes, evolutionary biologists on ultimate causes—but the two are quite obviously connected and the proximate cause most often exists in what is basically a dependent relationship with the ultimate cause. Explaining "how," exactly, an individual bird species engages in flight identifies a proximate physiological cause that is ultimately dependent to the evolutionary cause, derived from the question of "why" flight exists in the first place. Without the idea of flight as an evolutionary adaptation, the explanation of its physiology may be possible, but lacking. Causation in this model is, moreover, inherently linear.

It is tempting to use this (admittedly simplified) divide between proximate and ultimate causes here to explain the problem of crisis in the critique of political economy. In a very general sense, the similarity exists: the explanation of long-run breakdown is concerned with ultimate causes, while the explanation of individual crises is concerned with proximate causes. But to do so fails to capture the fundamentally reciprocal nature of both Marx's own theoretical categories as well as the reality that social systems are even more complex and reflexive than biological ones. In recent years, the simple divide between proximate and ultimate causation has become a topic of criticism in biology itself. Tellingly, early instances of this criticism originated from "radical" biologists who had studied Marx's work and extrapolated from it a certain understanding of the "dialectical" method that could then be usefully applied to the inherently reflexive nature of evolutionary biology, especially insofar as it is linked to basic questions of

development and environment (Gould 2002, Levins and Lewontin 1985, Lewontin 2002). This work then went on to influence newly emergent sub-disciplines such as evolutionary developmental biology, where the deeply reciprocal interactions being studied required an equally reflexive notion of causality, wherein ultimate causality could itself be portrayed as mutable, its processual trajectory shaped by the very proximate causes that it itself determines. This is essentially the only way to make sense of the fact that "plastic responses to novel environments can influence evolution by directing the expression of heritable phenotypic variation along particular trajectories" (Laland et. al. 2011, p.1513), a point already emphasized by Lewontin (2002) in his concept of the "triple helix" interaction between gene, organism and environment. Thus, linear causality needed to be abandoned in favor of a dialectical model.

It is therefore essential to understand the notion of long-run tendencies in capitalism governed by a tendency toward breakdown as a "moving contradiction" in precisely this reflexive, processual understanding of ultimate causality. While the real work of exploring these new notions of causality within fields such as evolutionary developmental biology and illustrating their potential utility for framing and modelling social phenomena remains an open project, I here want to merely emphasize this necessarily reflexive notion of causality before proceeding to explore the character of individual crises. This is essential because, without the link to the ultimate cause of capitalism's long-run historical dynamics, individual crises can too easily be explained in a purely eclectic manner, seeing no inherent connections between them. At worst, they can be portrayed as if they were unpredictable "black swan" events, or as phenomena linked to fundamentally exogenous causes. While it is essential to understand that each of the proximate causes of individual crises ultimately shape the processual trajectory of the ultimate cause—in the form of the system's long-run tendency toward breakdown—they are nonetheless

dependent on it, even if we might quibble as to whether it is accurate to say that they themselves as "caused by" this tendency.

The section above focuses on ultimate causes insofar as it looks at large-scale, long-run tendencies in the capitalist system as a whole, driven by the persistent problems of securing sufficient profit and acceptably cheap, skilled and disposable population. To review, the most important of these are: the growth of the mass of capital itself, driven by the mechanization of production and therefore accompanied by a growing mass of surplus labor; the growing scale of this capital and the subsequent necessity of its maintenance, in the form of increasing numbers of supervisory and circulatory occupations as well as in the growth of more expansive systems to ensure general social reproduction, all of which entails the increasing intervention of the state; and the constant tendency for the rate of profit to fall even as labor becomes superfluous, ensuring the constant presence of "countervailing" tendencies that drive the perpetual restructuring of capital in its technical, social and geographical dimensions. These countervailing tendencies are, in many ways, the real protagonists of the story of crisis. Even if the fundamental "problem" in the plot is caused by the antagonistic ultimate cause of growing surplus population and falling profitability, any attempt to explain the way that this problem is addressed finds the real narrative arc in the attempt of the protagonists to overcome it. Thus, explaining individual crises requires substantially less focus on the seemingly ever-present, ever-threatening antagonist of the "general law" and substantially more focus on the constant struggle of the countervailing tendencies and the drama of their battle to overcome the challenge set out for them. This drama is, however, an inherently serialized one, since each success for countervailing tendencies only sets the stage for an even more difficult challenge to come.

**Harvey and Keynes**

By far the most well-known appraisal of how the logic of capital takes shape territorially is the notion of the "spatio-temporal fix" as originally proposed by David Harvey (in papers ultimately compiled into Harvey 2001), building on Neil Smith's notion of uneven development (Smith 2008). This account was later expanded in Harvey's account of a "New Imperialism" (Harvey 2003) and has been particularly important, alongside other works by Harvey, in establishing much broader academic discourse around "neoliberalism." In more recent years, it has also been the justification that Harvey himself has given for treating increasingly large, increasingly popular and increasingly violent uprisings with circumspection. It seems that many of the implications of Harvey's earlier work[30] have now taken decisive political shape in an explicit position that can no longer even be accurately characterized as anti-capitalist. Instead, Harvey offers one of the most theoretically substantial variants of "Keynesianism" in the sense used by Mann. In other words, he provides a formula for "revolution without revolutionaries" and proposes a program of state-led reforms more or less identical to those proposed by MMT thinkers. If anything, left-leaning MMT theorists today propose far more expansive programs than Harvey himself. Meanwhile, throughout his work he understands such reforms as standing in opposition to "neoliberalism" rather than capitalism. They are designed not to overthrow the system but to make it more humane. At best, Harvey gestures at the possibility that, over many generations, capitalism might simply wither away.

In previous decades, levelling such a critique at Harvey proved difficult because of the ambiguous, eclectic nature of his writing, which frequently makes use of anti-capitalist

[30] Here I am referring to his earlier work in "radical" geography, epitomized by his 1982 *Limits of Capital* and extending through the early 2000s, but not including publications prior to his own personal "radical turn," such as the (in fact quite good) *Explanation in Geography* first published in 1969.

phraseology and has long couched itself in a “radical” veneer. More concerning is the way that Harvey’s work has taken on a life of its own as a rote source of citation in the field, regardless of the strength of its theoretical underpinnings. After the past decade of uprisings, however, there is no longer any ambiguity about the political conclusions which follow from Harvey’s theory and no need to extrapolate such conclusions from mere implications glimpsed briefly in his many texts. These positions have since been made explicit, in his own words:

> Capital in general is too big to fail. It is too dominant, and it is too necessary to us that we cannot allow it to fail. We have to actually spend some time propping it up, trying to reorganize it, and maybe shift it around very slowly and over time to a different configuration. But a revolutionary overthrow of this capitalist economic system is not anything that's conceivable at the present time. It will not happen, and it cannot happen, and we have to make sure that it does not happen. (Harvey 2019)

This is immediately followed by the caveat that, “capital is too big, too monstrous, too huge to survive, that it cannot survive in its current form” making for a “suicidal path” (ibid). The implication, however, is simply that its “current form” can (in fact “must”) be changed and that those who might once have retained fidelity to a communist overcoming of capitalism must instead settle for a program of policy-based reforms aimed at domesticating capitalism, possibly in the expectation that, after several generations, it might ultimately be eroded into nothingness.

This sort of sentiment should give any communist geographer pause, hinting that whatever muddy logic lies in the depths of Harvey’s theory is likely unstable ground on which to found an inquiry into the dynamics of capitalism today. If this is the natural political conclusion of such a theory, it seems that the theory remains fairly distant from both the work of Marx himself—who always, in the end, retained fidelity to the notion of overcoming capitalism

through revolutionary means—and stands opposed to a communist political project today, even to the extent of portraying such a project as more of a danger than the everyday violence of capitalism itself. More tellingly, it connects an implicit strain of "Keynesianism," understood in the broad sense used by Mann, to the finally explicit expression of the underlying Keynesianism at the heart of Harvey's reading of Marx. Like Keynes, Harvey understands that the capitalist system is inherently "suicidal," but, again like Keynes and Hegel before him, the political conclusion he draws from this fact is shaped by an inherently elitist fear of the "rabble." Though realizing that an insurrectionary populace is itself the product of the breakdown of the system and that the demands of this rabble are fundamentally just, in abstract ethical terms, the Keynesian attitude is nonetheless that this demand for revolutionary justice turns too easily into an unbound revolutionary bloodletting. Keynesianism is fundamentally concerned with preserving civilization, which is portrayed as a fragile victory that runs against the grain of much of human history. Saving capitalism from itself is not really the goal, for Keynes. Capitalism's momentary preservation is simply necessary to the greater endeavor of warding off civilizational collapse. Since revolution risks the destruction of civilization, it is seen as a greater danger than any injustice perpetrated by civilization itself, including the worst dimensions of capitalism. Mann therefore traces what he calls the Keynesian position well beyond Keynes himself and back to Hegel, since both thinkers offered a similar response when faced with revolutions that had mobilized the rabble and destabilized the very basis of civilization. Ultimately, the conclusion is that there must be some sort of "revolution without revolutionaries," which requires a conscious, enlightened agency that can only be found in the realm of the state as the culmination of universalist bourgeois civil society (Mann 2018, pp.50-57).

Harvey is, in this sense, a Keynesian all the way down. His original formulation of the "spatial fix" even draws on the very same arguments made by Hegel in the *Philosophy of Right* that Mann himself identifies as the first manifestation of this "Keynesianism," in reaction against the French revolution (Harvey 2001, pp. 285-288). Harvey's ultimate fear is revolution, which he portrays as a suicidal mission today, even if it may have been a justified measure in the past. In its place, he offers a process of merely "shifting" capital "around very slowly," attempting to reorganize it and even "prop it up." The echo with Mann's anatomy of Keynesianism is stark:

> In contrast to the rather dramatic means of social overhaul on the antiliberal menu, Keynesian reason demands—in the interest of ongoing social stability—a more gradual collection of institutional, political, economic and ultimately sociocultural tweaks, or 'fine-tuning' (Mann 2018, p.53).

This is precisely what Harvey suggests, albeit with the elitist implications always mildly "democratized" by the suggestion of some ill-defined mass influence through implicitly non-violent "social movements" that are somehow able to shape policy. While at first glance it might seem that such a conclusion is somewhat divorced from Harvey's more radical critique of capitalism and that this critique itself could be put to use for different political ends, it simply is not possible to divorce politics from theory so decisively. Though it is often reasonable to separate the individual political activity or moral failings of a particular theorist from the insights of their theory to a certain extent, it is much less feasible to divide a body of theory from the explicit political strategy that the theory's author has developed over decades based on that theoretical work itself. At best, the fragmentary nature of Harvey's work, which has never sought much systematization, means that certain minor elements might still be useful in a very general sense.

Harvey's recent conservative turn is not an accidental or incidental fact but is instead the natural outcome of his reading of Marx, tracing all the way back to his 1982 *Limits of Capital*. This reading has never been particularly attentive to the deeper structures outlined in Marx's economic writings—namely, value theory—and has instead emphasized the superficial features associated with a vaguely defined interpretation of "overaccumulation." *Limits of Capital* develops this theory more systematically than any other text, but it remains largely unconcerned with either internal consistency or even a close reading of Marx. Often, the chapters that shape this argument are little more than musings on one or two passages on circulation cherrypicked from the drafts that ultimately became Volume II of *Capital* or extensive extrapolations driven by a pervasive "misunderstanding of the value theory and confusion between different levels of abstraction" (Mattick 2008, p.218).[31] Overall, Harvey's emphasis on overaccumulation and problems posed by circulation are not entirely without their insights, however fragmentary. Treating the problem of overaccumulation and its associated disproportionalities as central allows him to extrapolate geographic dynamics of the system that were only implicit in a text like *Capital*, which focuses, after all, on elucidating the deep structure of value and the "laws of motion" of the system as a whole.

But the exclusive focus on these superficial features ensures that Harvey repeats the error of the earliest "Marxists" such as Engels and Kautsky, who similarly focused on general and ill-defined problems of overaccumulation and disproportionality which were explained, at their deepest level, merely by overproduction induced by the "anarchy of the market" and were,

[31] There is no space here to offer a suitably thorough critique of Harvey's work. In fact, such a task is difficult precisely because of the eclectic structure of his argumentation and the evasive rhetorical techniques deployed to hedge each claim against the possibility of being proven wrong. For one attempt at a more sustained critique of the book, see Mattick 2008.

therefore, not rigorously linked to Marx's value theory. These positions were ultimately developed into their logical conclusion by Bernstein, who argued that the vicissitudes of capitalism could ultimately be reined in through reform and that, by contrast, the risk of revolutionary measures outweighed their reward, since a gradualist path was possible. At the time, this gradualist path was justified by the claim that enlightened state intervention—led by the German Social Democratic Party (SPD), with its enormous mass base among the working class—could successfully tame the "anarchy of the market" and thereby domesticate capitalism itself (see Clarke 1994, Chapters 2 and 3). Harvey makes the same error and puts forward the same claim, but, bafflingly, does so in a period where the position makes even less sense, given the absence of a workers movement and the non-existence of any mass organization even approximating something like the SPD.

This error is ultimately founded on Harvey's inattentive treatment of the deep logic of Marx's inquiry, best expressed in the value theory laid out in Volume 1 of *Capital*. There is neither need nor space here to delve into these many errors of interpretation in detail, which have been thoroughly documented by others elsewhere (namely Mattick 2008, with more listed below). What is notable is the fact that these critiques of Harvey's interpretation don't originate from just one of many "Marxological" schools of interpretation and therefore do not represent a veiled dispute over strategy or historiography or any other specific topic. Instead, they are a relatively stable point of unity across otherwise divergent readings and relate to Harvey's reading of almost every major topic in Marx, from abstract labor and the form of value (Mattick 2008) to crisis theory (M. Roberts 2020), the nature of imperialism (Smith 2017), the relative weight of spatial dynamics (Das 2017) and even the more basic literary structure and historiographical status of *Capital* itself (W.C. Roberts 2017). The range of the divergence is visible in the very

clear differences between Harvey's widely distributed introductory lectures on *Capital*, with their accompanying book (Harvey 2010), and similar introductory volumes by Michael Heinrich (2012 and 2021). Harvey's presentation of almost all the fundamental topics addressed by Marx differs from that of Heinrich, often quite substantially. Heinrich, however, grounds his interpretation in an extremely systematic reading of the original text in all its forms, including notes and manuscripts, backed by an intricate historiographical study of its production. By contrast, Harvey approaches the text in a chaotic and eclectic manner with very little attention to consistency, a habit of treating passages that do not fit his reading as errors on Marx's part, and a pattern of either misunderstanding or simply misportraying the intent and structure of *Capital*, including its history of drafting and publication (as argued by W.C. Roberts 2017). Underlying this inconsistent theoretical approach, however, is a certain Keynesian (in Mann's sense) political consistency that has persisted more or less unaltered from the 1982 publication of *Limits of Capital* all the way up through Harvey's embarrassing condemnation of insurrection in 2019, on the eve of the largest mass uprising in a generation.

Overall, then, Harvey's more useful conclusions lie mostly at the superficial level and are largely used to try and explain observable trends in, for instance, the changing geography of investment and production. While this level of analysis does allow one to draw certain workable conclusions in hindsight, it has proved remarkably bad at any sort of forecasting or prediction and has offered only the loosest explanations of actual crises that have occurred. The abject failure of the Harvey's (2007) prediction of further "neoliberalization" in China is a case in point, since the paradigm of "neoliberalism" itself tends to reduce contingent and complex historical conjunctures to overused and overly simple discursive schema in a way that ignores both local institutional features and actual long-run secular tendencies in favor of an emphasis on

an overly linear "path dependence" extrapolated from a minority of cases (Buckingham 2017). In contrast to Harvey's predictions, the state has only grown more central to production over the past decade (Lardy 2019, Batson 2020). In fact, the work of thinkers such as Koo and Gordon are often more attentive to internal consistency and therefore offer more systematic accounts of crisis than Harvey. The more fundamental issue here is that Harvey explicitly links these superficial observations to a frustratingly eclectic misreading of Marx that dissolves the critical thrust of Marx's method and continually follows its misinterpretations with "corrections" to Marx's supposed errors. These corrections ultimately come to compose an alternate immanent critique of capitalism that is much closer to Keynesianism in Mann's sense. This theory cherry-picks quotations from Marx to justify an essentially underconsumptionist conclusion that the root of crisis "lies in the diminished purchasing power of the working classes" (Harvey 2018), more or less emphasizing the classical Keynesian centrality of effective demand, while also tending to target various rent-taking forms of "accumulation by dispossession" (Harvey 2005), since this underconsumptionist crisis theory implies that capitalism runs out of ways to secure the expanded reproduction of the system without plundering non-capitalist spheres beyond it (Roberts 2020). Along the way, Harvey deeply misinterprets the notion of "primitive" or "original" accumulation by portraying it as necessary for the accumulation of the surplus needed for expanded accumulation (for why this is wrong, see Bonefeld 2014, pp.82-97).

Harvey therefore tends to reproduce something similar to the eclectic and amorphous "Marxist" crisis theories that predominated in the late nineteenth century and which focused on "overproduction" and disproportionalities, but also left these terms ill-defined (see Clarke 1994, Chapters 2 and 3). At the same time, he attempts to knit this together into a general theory of "overaccumulation" that, sounds somewhat similar to the more intricate understanding of the

"general law of capitalist accumulation" outlined above (and presented in Clarke 1994, Endnotes 2010, Mattick 2018 or Moseley 2018) but which ultimately bears the most similarity to the more explicitly underconsumptionist theory of crisis originally proposed by Rosa Luxemburg. This link is evident in the way that Harvey sees "accumulation by dispossession" to be a necessary feature of the lack of effective demand, leading to both to classical imperialism and a "new imperialism" (Harvey 2005), both of which are necessitated by the inability of the system to secure its own expanded reproduction without plundering a non-capitalist surplus from elsewhere. However, in contrast to Luxemburg—who was a dedicated revolutionary ultimately murdered by the state for her insurrectionary activity—Harvey argues that these disequilibria can be evened out by state action and overaccumulation muted by systematic reforms. He has, therefore, repeatedly advocated for left-Keynesian policies of state intervention modelled on the New Deal (Harvey 2005, Mattick 2008). Harvey's position here is not only wrong, but it is confused and eclectic enough that any attempt to systematically disprove it immediately becomes bogged down in tiresome case-by-case demonstrations of error, precisely because it is itself completely unsystematic and has no real relation to the deep structures of Marx's own critique.

There is not space here to dig down into what I call the "deep structure" of value theory in Marx through an analysis of subsequent debates on the topic, even though this is the framework that informs the methods used here and reference is made throughout to thinkers who pursue this line of inquiry. Similarly, much of the material presented in subsequent chapters largely engages with precisely the "superficial" level of empirical measurement and the description of recent economic history. But, precisely for this reason, I will tend to draw less on Harvey's explanation of the geographic dynamics of capitalism and more on the work of scholars (such as Itoh) who have either offered more systematic linkages between the deep structure of

value and the superficial movements of industrial location—even if their positions on value theory might be in error—and work by other thinkers (such as Brenner) who more or less explicitly limit themselves to this superficial level of historical explanation without attempting to rigorously connect it to Marx's theory of value. This enables me to explain the "spatial fix" phenomenon in a way that hints at a more correct linkage to the deeper dynamics of value in capitalism even I do not explore this connection here. More importantly, the phenomenon can thereby be described in similar terms as those used by Harvey, but without fundamentally mis-portraying its cause and offering false or impossible palliatives as a remedy. In what follows, I will therefore explore the concrete geographic and historical dimensions of the tendencies that countervail against falling profit rates and review the ways that they have been integral to individual crises and their aftermath.

**Stagflation**

One of the earliest theoretical formulations of what is now known as "secular stagnation" originated not from the New Keynesians who have popularized the term since the Great Recession but instead some thirty years earlier, in the failure of "Keynesianism" as the postwar policy orthodoxy evident in the stagflationary crisis of the 1970s. While economists pointed to all sorts of exogenous causes to explain the crisis—particularly the oil price shock—and, later, gestured toward the "crowding-out" effect of government spending, neither of these accounts stand up to scrutiny, since signals of the crisis preceded changes in oil prices and the ultimate containment of inflation was not associated with a decrease of state spending or declining state involvement in the economy, despite "neoliberal" language to the contrary (Itoh 2020, p. 210-215, Mattick 2011, pp. 71-74). Meanwhile the gradual incorporation of orthodox Keynesian

presumptions into influential "Marxist" thought in this period, epitomized by the "monopoly capitalism" theory of Paul Baran and Paul Sweezy (1966), had also left many self-described Marxists without a coherent explanation of the crisis. On the one hand, then, the stagflationary crisis stimulated the invention of a new economic orthodoxy that gradually abandoned whatever remained of the social mission of postwar Keynesianism in favor of a neoclassical synthesis which portrayed itself in "neoliberal" terms of privatization and a revival of marginalist microeconomics even as it sustained itself, in reality, on expansive defense spending, interventionist monetary policy and a series of bubbles which, over time, came to compose a sort of "asset price Keynesianism" (Brenner 2006). On the other, the crisis induced a similar process of revival and reinvention among self-described Marxists, since the field's prevailing underconsumptionist theories of crisis could not explain why no decline in consumption or rise in unemployment had occurred prior to the crisis, even while it had been preceded by a fall in the rate of profit and declining investment.

Initially, this led to divergent attempts to explain the crisis in terms of either a perfunctory theory of rising productivity leading to the falling rate of profit or an equally simple model in which rising wages "squeeze" profit margins (Clarke 1994, pp.61-72). For both sides, the revival of "Marxist" crisis theory was distorted into a mechanical model that simply opposed underconsumption theories by emphasizing opposite and equally partial elements of Marx's own writings on crisis and the inherent contradictions of the capitalist economy. This emphasis was conditioned, in part, by the fact that the established theoretical expectations had unexpectedly collapsed at precisely the moment in which political activity had revived. Thus, the attempt to explain stagflationary conditions in the US and UK was connected to a need to immediately translate this understanding into a useful paradigm through which to advance the political

movements of the period. It was only after the systematic and definitive defeat of these political struggles that scholars began to return to the question of crisis and breakdown to review and more systematically develop these earlier "returns" to the theory of crisis (as in Clarke 1994, Moseley 1991 or Carchedi 2012).

Given this context, it is not coincidental that one of the most fully formed explanations of the crisis that emerged in this period did not originate from anglophone "Marxist" circles at all but instead in Japan, where it was developed in relation to the same crisis but from the opposite perspective of the recently ascendant industrial competitor. Japanese Marxism itself had seen rapid development in the early twentieth century with the works of Marx and his most well-known interlocutors forming the foundation of essentially all the social sciences and addressing questions of colonialism, crisis, the logical structure of capital and the process of its drafting well before these debates returned to the fore of discussion in the west. Accelerating crackdowns on the left devastated these early Japanese Marxist circles and some major figures even joined the militarist right over the course of the 1930s. After the war, however, self-described Marxist thought became more influential than ever (Hoston 1984, Itoh 2021, Walker 2020). In these years, many of the earlier debates within Japanese Marxism were synthesized by Uno Kozo into a single, widely influential "Uno School," based on Uno's project of rewriting *Capital* and guided by his separation of inquiry into three "levels of analysis" that descended from the abstract to the concrete: the first concerned with identifying the most abstract "laws of motion" of the system, the second with the identification of stages of development, and the third with empirical analysis of particular cases (Itoh 2021, pp.50-52).[32] On its own, Uno's theory had wide

[32] Uno's positions were by no means universal and these years were marked by renewed debates that again prefigured many of the terms that Western Marxists would concern themselves with decades later. It was the

influence in Japan but, initially, limited influence internationally. Beginning in the 1970s, several of his students attempted to popularize the theory in the English-speaking world. The most well-known of these interpreters was Makkoto Itoh. While Itoh's theoretical basis is in the Uno School and he often presented his work as an affirmation of Uno's basic theory, his works often developed this theory in new and distinct directions. One reason for this is that he was confronted with the need to more systematically explain the conditions of the global economy during and after the stagflationary crisis.

For Itoh, the apparent success of Keynesianism in the US and Europe in the postwar years could not be understood without reference to global military interventions and their associated dollar spending, undergirded by the central role of the dollar in international trading and financial systems (ibid, pp. 212-213). The same point is made more systematically by Glassman (2019), who rigorously documents the centrality of US military and dollar hegemony to the development of East Asia in the postwar decades. Itoh connects this to trends in credit markets, demographic growth and the profit rate to offer a more synthetic theory of the crisis than other Marxist thinkers of the time. As Clarke explains: "Itoh's development of the theory is a complex synthesis of neo-Ricardian, falling rate of profit, and disproportionality theories, each pertaining to a different phase of the cycle" (1994, p. 71) but ultimately linked back to an emphasis on the "capitalist law of population" (Itoh 2020). This is often portrayed as a simple "profit-squeeze" theory, but in reality is a more complex attempt to grapple with the problem of social reproduction and surplus population in historical and geographic conditions very different from those observed today. Itoh was, after all, formulating much of his work in a period where

work of Uno and his students, however, that came to dominate the discourse between Japanese and Western Marxists.

rapid growth in the industrial labor force had only recently begun to plateau and the Japanese crisis was beginning to build in speculative asset bubbles but had not yet burst. Instead, Japan was proving itself to be a more than capable competitor with established industrial powers such as the United States. Thus, the "problem" posed by population in that place and time was almost the exact opposite of that which prevails in Japan (or the US, or Europe) today. The conditions are similar, however, to those of contemporary China, and this point will be returned to below.

Itoh's basic emphasis, then, is to be found in a theory of uneven development and overaccumulation distinct from that of Harvey because of its more systematic connection to the work of Marx, with Itoh attempting to develop his understanding of the superficialities of global trade and investment patterns on the foundation of value theory. Even if this understanding was somewhat misshapen by the fact that Itoh was originally formulating this theory in a period where many of Marx's texts were not yet widely available and within what Heinrich would call a "worldview Marxist" perspective, his work nonetheless offers a more coherent model of spatial inequalities and the centrality of international industrial competition to the formation of new territorial production complexes. In his theory, the 1970s marks the outbreak of a "Great Depression" marked by persistent stagnation. It is triggered by an array of causes, including the maturity of many postwar technological innovations which "tendentially eroded US hegemony in the world market" because US industry found itself saddled with increasingly obsolete plant and equipment installed during the wartime industrial expansion, as compared to new competitors in West Germany and Japan who were able to both invest in the most modern machinery and exploit relatively cheap and elastic pools of rural labor (Itoh 1990, p.45). Ultimately, this undercut the stability of the Bretton Woods monetary system and led to a series of dollar crises,

all while falling profitability constrained new investment in the US, rural surplus labor remained low and the cost of raw materials from overseas began to rise (ibid, pp.46-48).

While Itoh certainly emphasizes the role of population and wages in squeezing profit rates here, he also emphasizes the central role of technical change and international competition. Robert Brenner (2003, 2006) offers a similar account that corrects against Itoh's periodic deference to profit-squeeze causes, but without digging into the deeper structure of value theory. What is particularly valuable about Itoh's work, however, is precisely the fact that, even if he is not quite getting there, he is at least gesturing at a more inclusive theory of crisis in the "long" sense of secular breakdown, based around the "general law of capitalist accumulation," as described by Clarke (1994) and Endnotes (2010). Itoh identifies the period as a "Great Depression" similar to that of 1873, marked by general stagnation and increasing speculative activity. Brenner categorizes it instead as a "Long Downturn" (2006) and Roberts (2017) as a "Long Depression," but the idea is fundamentally the same. The outbreak of the crisis throughout the late 1960s and early 1970s leads to the gradual erosion of the United States' dominance in global industry as Japan and German competitors outcompete US manufacturing on the global market. At the same time, declining industrial profits lead to increasing speculation which then conditions the need to open international capital markets further. The first burst of these speculative bubbles comes in the Third World Debt crisis, triggered by western capital but borne by the world's poor. Another consequence the increasing intensity and increasingly financial and monetary dimension of the trade war between the US and Japan.

Throughout the 1970s, despite a decline in its profit rate, a gradual slowing of investment, rising wages and the beginning of a wave of layoffs in manufacturing, Japanese (and West German) industry was still gaining an increasing market share against the US (Brenner

2003, pp.28-32). Grappling in the trade and financial markets went back and forth in the 1970s and into the early 1980s, with Japan gradually gaining the upper hand. These battles over the relative value of the dollar and the yen were only settled when the US mobilized its *de facto* imperial sovereignty in the global political and financial system to push through the Plaza Accord and a series of other trade agreements in the mid-1980s, which devalued the dollar against the yen and established a series of tariffs and producers agreements in key sectors, such as semi-conductor manufacturing, all in order to protect US industry against competition. The result was an "American economic revival" in the later 1980s and into the 1990s, which saw the competitiveness of US industry briefly revived and buoyed the manufacturing sectors of other newly-industrializing countries with dollar-pegged currencies. This thereby generated a boom across the Pacific Rim that, even if it entailed a political defeat for the Japanese, initially appeared to guarantee a win-win outcome for everyone involved (Brenner 2003, pp.59-74, Itoh 1990, pp.83-97).

But if American industry in this period was finally able to clear out obsolete manufacturing capacity and revive industrial output—though employment would not follow in equal measure, due to further mechanization—in Japan the result was that money funneled out of industrial investment even faster and poured into speculative financial ventures. Asset prices skyrocketed, with slices of downtown Tokyo becoming some of the most valuable square-footage in the world. Meanwhile, Japanese outbound FDI increased rapidly. As the profit rate of domestic industry fell beginning in the 1970s, Japan also became a major source of FDI globally, and was particularly important to the industrial expansions across the Pacific Rim. By the 1980s, its prominence was even beginning to surpass the United States: "In 1984, the Japanese share of the world total of annual direct investment became 17.8 percent, which was beyond the US share

of 13.6 percent" (Itoh 1990, p.226). After the currency revaluation, this trend only accelerated, with Japanese-origin FDI tripling in absolute value between 1984 and 1987 (ibid). None of this could successfully restore the competitiveness of Japanese industry, however, with Japan's share of total global exports dropping "from a peak of 10.3 percent in 1986 to 8.5 percent in 1990" (Brenner 2003, p.110).

The result was the bursting of the asset bubble in 1990, risking a severe global crisis and ultimately generating the lost decades of stagnation in Japan, as documented by Koo. The crisis only worsened the position of Japanese manufacturers, who decisively shifted all but the most high-tech production offshore, where they could exploit cheaper labor pools elsewhere in Asia while also getting around US tariff barriers. At the same time, Japanese financial activity turned outward as well, pouring money into the global expansion of industry and into financial assets in the US. But, by 1995, this had not only not solved the crisis in Japan, but also threatened to spread the same overcapacity across the new industrial centers in East Asia, which were now dependent on Japanese capital (Brenner 2003, pp. 116-118). Conditions worsened to such an extent that a "reverse plaza accord" was forced through, with the Mexican peso crisis seeming to forecast the imminent possibility of another collapse in Japan, which, due to its centrality within the new Pacific Rim industrial complex, risked pulling the entire world economy into a severe depression. Thus, Treasury Secretary Robert Rubin pushed through an effort to devalue the yen and the mark against the dollar (ibid, pp.130-132). While this likely prevented the global depression that would have been triggered by another collapse of the Japanese economy, it did so only by displacing the bubble onto the US and new exporters that had pegged their currencies to the dollar.

**Geographic Contours**

For both Itoh and Brenner, geographic divides are essential to understanding the dynamics of the global market. Specifically, divisions in labor markets, capital markets and currency markets tend to throw together groups of capitalists into loose national blocs who share similar competitive interests and will tend to exist at roughly similar levels of technical development. A level of complexity is added both by the fact that these blocs then go on to incorporate subordinate blocs of dependent firms in other countries, linked through financial ties and key trade links; and by the fact that, in the context of global competition, even opposed blocs are interdependent with one another. While this interdependency can briefly generate seemingly "win-win" conditions, the inherent unevenness of development will ultimately lead to increasing industrial competition, culminating in trade wars which then invoke the threat of protectionist policy, financial maneuvering and, ultimately, political intervention into the global monetary system. For Itoh, geography also plays a role in delineating internal labor markets, especially insofar as rapid Japanese industrial growth was only possible because of the existence of a sharp urban-rural divide wherein rural areas were already saturated with surplus labor.

But there is another important dimension in which Itoh and Brenner focus on geography even while they don't make this emphasis particularly explicit. This is, again, the question of technical development, which, understood at the larger scale, entails the development of territorial industrial complexes. This topic will be returned to below, but here it is important to point out the ways in which both Itoh and Brenner emphasize both sides of the "general law of capitalist accumulation" in driving the increasingly zero-sum competition at the heart of the global industrial system: on the one hand, growing surplus labor; and on the other, a growing mass of capital that takes both monetary and physical form. Here, it is tempting to

overemphasize the role of fixed capital as a fundamental explanation of the business cycle. This was, after all, an explanation endorsed by the classical political economists and is one of the foundations of Schumpeter's theory of business cycles. Marx himself even speculated on this cause—which he borrowed from Charles Babbage—before ultimately throwing it out (Clarke 1994, p.261-267). Harvey returns to it, but in an amorphous and eclectic way that doesn't clarify much about its precise function (Harvey 2006, pp.300-307).

But for Itoh and Brenner, investment in plant and equipment plays an essential role because such investment tends to come in peaks that can be easily traced in conventional business statistics. More importantly, these peaks are geographically clustered. While this has a regional dimension, which will be explored in more detail below, the important point here is that each high tide of manufacturing investment within a country will tend to leave behind an encrustation of plant and equipment that, on average, dates back to the general level of mechanization that prevailed during that peak period. This then implies that these firms are, at least at the technical level, more or less stuck with a given ratio of workers to machines that will, due to the nature of ongoing technical changes, always lie behind the curve and tend to fall further behind as time goes on. Since plant and equipment compose the largest, longest-maturity investments, the increasing obsolescence of fixed capital cannot be overcome through incremental additions of capital. It makes no sense to buy a new machine when one bought five years prior has not yet paid itself off. Thus, this general technical lag can only be corrected for by intensifying work, reducing wages, or finding cheaper inputs. At the same time, increasing competition from countries that saw their investment peak later—and therefore have manufacturing sectors operating with more modern, more heavily mechanized facilities—will ultimately lead manufacturers in both countries into aggressive attempts to level the market

through political means, imposing protectionist policies, coordinating financial activity to manipulate the value of the currency, or securing cheaper inputs by leveraging political power to plunder the poorest countries at the bottom of the international hierarchy.

This is also where the superficial, empirical dimension of this work connects most clearly to the deeper structure of value theory, since this process of technical change and its consequences for the value composition of production overall are integral to Marx's "general law." The rollout of new technological innovations will tend to increase the profitability of the firms that adopt them first, allowing them to produce more output per unit of labor while selling that output for similar prices and/or undercutting competitors with lower prices to guarantee larger sales revenue overall. As other firms adopt the same innovations, however, the competitive field is levelled through a systematic cheapening of the final product. More importantly, in value terms, as theorized by Marx, this process dampens the growth of surplus value extraction in these firms because it replaces workers (who are integral to the value circuit) with machines (which are not productive of value) and thereby tends to increase the ratio between surplus value and the capital spent on plant and equipment across the economy once these technical changes are distributed through competition and the chaotic process of equalization around new price levels of key manufactures and associated rates of profit.

This is where the countervailing tendencies take on their protagonist role, since it remains possible to sustain profitability if firms increase the amount of surplus value extracted from workers relative to the amount those workers need to be paid to sustain themselves (the "rate of exploitation"). On the one hand, this can be done through increasing discipline, rationalization and supervision, as argued by Smith. On the other, it can also be achieved by further cheapening the consumer goods against which the wage is judged. This can mean even more rapid

mechanization and/or the exploitation of cheaper labor pools via global labor arbitrage. This latter dynamic will be the focus of the next section. Here, the importance of mechanization is more central, since it becomes locked in a positive feedback loop wherein the pressure on profitability created by the replacement of workers with machines tends to encourage further mechanization in other lines of production, with the entire process conditioned through constant and constantly increasing competitive pressure. In such a context, fixed capital investments take on a particularly troublesome character since they will tend to lock firms—usually together with others in a given country or region—into a particular technological level for years on end. This is the feature of mechanization most central to the accounts of Itoh and Brenner.

Itoh points to the way in which this process generates not only further mechanization of new lines of production and the founding of new industrial zones in areas with cheaper pools of labor, but also how, in the increasingly obsolete but nonetheless more "advanced" economies

> the existing fixed capitals tend then to be carried over as excess capacity which provokes cut-throat competition in various major industries. As the depressed rate of economic growth affects most severely the real growth of demand for capital goods, the important industries, such as steelmaking, heavy petro-chemicals, and shipbuilding, have been suffering from large-scale excess capacity. (Itoh 1990, pp.118-119)

A similar phenomenon follows in the consumer goods industries, which can give the impression of an underconsumptionist crisis caused by a shortfall in effective demand, and which then goes on to aggravate the crisis or even initiate a second wave centered on the consumption goods industries and consumer credit, often facilitated through increasing financial investment in overlapping assets such as housing. But these financial crises are only the form of appearance of a more fundamental problem: "the basic reason why such a structural difficulty cannot easily be

resolved rests in the immobility of the huge amounts of excessive fixed capitals still not yet fully depreciated" (ibid, p.119). Itoh compares the depression that began in the 1970s to the first "Great Depression" in 1873-1896, since in both cases "excessive fixed capitals were not easily left idle or abandoned, but forced a lower profit margin on capital through persistently severe competition which generally restricted and lowered the prices of products" (ibid, p.121). This is in contrast to the Great Depression of the 1930s, where the catastrophic character of the collapse and the severity of immiseration over the decade, capped by war, all contributed to a systematic devaluation (and often, the literal destruction) of this overcapacity, embodied in masses of obsolete plant and equipment. This then cleared the ground for the postwar growth boom.

Since no similar catastrophic devaluation has occurred, new booms have only come in short, uneven and partially speculative bursts, each of which has left an ever-larger bubble in its wake, as documented by Brenner. The late-developer growth spurt of Japan left behind the asset bubble that would mature into the Japanese Crisis of 1990. The revival of American industry that accompanied the decline of Japanese competition was itself capped by a series of burst bubbles in the 1990s and into the 2000s, culminating in the Great Recession. Similarly, the rush of growth centered on the newly-industrializing economies of the Pacific Rim that had been cultivated by dollar-pegged currencies and fed on excess Japanese capital which could no longer be invested domestically in either production or speculation, ultimately faced a similar bust in the Asian Financial Crisis of 1998 which threw many once-ascendant economies of the region into a deep stagnation. It was only in this context that Chinese manufacturers were suddenly able to outcompete Thai, Malaysian and South Korean competitors, often supplying or contracting directly with Japanese firms (Chuang 2019). More importantly, the basic characteristics of such a depression, as identified by Itoh, will tend to persist so long as catastrophic crises are averted.

Thus, the very measures undertaken to prevent a more widespread breakdown will themselves ensure long-run stagnation. Over time, this stagnation will grow more and more noticeable, even as industry is technically restructured and geographically relocated as the countervailing tendencies proceed in their battle against the persistent problem of profitability in the face of the global glut. The juggling of smaller crises between poles in the global economy ultimately does little to clear overcapacity at the larger scale. Instead, new centers of production intensify it.

**The Law of Population**

Geographic differentials in both wage rates and the relative foreign exchange position of the currency are also essential to the story. The role of the monetary system will be discussed more in the next section. Here, however, I will conclude with an overview of the importance of the capitalist "law of population" in international industrial competition and the general global manufacturing glut. Itoh explains how postwar population dynamics put Japan in a unique position, from which it could maximize domestic development and its competitiveness in international markets:

> An enormous relative surplus population was created in Japan with the end of the Second World War. 7.6 million demobilized soldiers, 4.0 million demobilized workers from military production, and 1.5 million repatriates from abroad immediately made up an aggregate of 13.1 million relatively surplus population seeking new jobs. Its size was quite considerable in comparison with the total number of 34.8 million employed in 1948, out of a total Japanese population of about 72 million. (Itoh 1990, p.145)

While unemployment as a whole did not increase substantially, this was because underemployment and absolute exclusion from the labor force (and therefore the unemployment counts) was the norm. In this regard, the fact that Japan still retained a relatively large semi-subsistence sphere was essential, since the surplus population was composed of "a great number of imperfectly employed, or persons who were forced to live at a very low level of income in rural peasant families and small urban businesses" (ibid). This ensured that wages would remain low due to competitive pressure in the labor market and simultaneously allowed for extreme flexibility with regard to labor supply, since the informal and semi-subsistence spheres were capable of absorbing laid-off workers:

> By making use of both family ties, and the reality that there were home villages for the majority of wage-workers to go back to when necessary, Japanese capitalism could dispense with many of the costs of social expenditure. (ibid, p.154)

Similarly, the postwar crackdown (in 1945-47) on resurgent, communist-aligned labor unions—encouraged and overseen by the US military powers in control of the country—ensured that labor conflicts would remain subdued (ibid, p.147). The similarities with conditions in China forty years later are stark, as will be explored below.

Even though initial developmental cycles follow the general pattern Koo identifies, via Lewis, whereby the share of workers in agricultural occupations declines as a share of total employment and the share in industry increases—indicating a general shift from "subsistence" spheres into dependence on the wage—this doesn't simply entail absolute growth in the manufacturing workforce. Instead, the technical trends described above guarantee that

> the expanded reproduction of capital is thus both the expanded reproduction of the employed and unemployed populations, positing an ever greater relative surplus, a "disposable reserve army" bred by the capitalist mode of production. (Rubner 2015)

For Itoh, this is inextricably related to fundamental demographic trends and structures the geography of capital exports once profitability begins to fall domestically. This is an extension of much older concerns within Japanese Marxist circles, where debates on the nature of the rural economy and the linkage between demand for cheap labor and imperial conquest had been central topics since at least the 1920s (Hoston 1986). It's important to note, however, that the growth in the surplus population and its *relative* immiseration is not the same as the growth in *absolute* immiseration over time. Marx himself is extremely clear on this, indicating that even the long-run trend is cyclical and that immiseration is understood relative to society's productive capacity and the "moral" aspect of the wage. As has already been argued, it is more often the case that "surplus population" takes the form of informal underemployment, cheapened wages for those in the "servant economy" and more frequent job changes even among those who find full employment, rather than the simple increase in absolute unemployment and its associated miseries—though this, too, accompanies the trend and is increasingly visible, even if unevenly distributed geographically.

The most important aspect of what Itoh calls the "law of population" is that, like falling profitability, population poses a constant problem for accumulation. While Itoh, following Uno, sometimes frames this problem primarily in terms of rising wages and argues for something like a "profit squeeze" theory of crisis, this is at least partially because he was drawing from recent Japanese history where rapidly rising wages had posed a particular problem for domestic industry. This was solved, in part, by keeping the currency itself depressed through purchases of

US treasury bonds which could at least secure superior export terms with the US and other countries with dollar-pegged currencies. Here, we return to the central concern of Koo, Klein and Pettis, but with Japan today replaced in this role by China. When the currency suppression failed, Japanese investment accelerated its outward expansion, concentrating in the Pacific Rim economies that had dollar-pegged currencies and thereby benefited from the new devaluation of the USD after the Plaza Accord. Thus, "population" must be understood not only in purely demographic terms or even in terms of nominal local wage costs, but specifically in terms of relative production costs given foreign exchange position.

The entire restructuring of investment patterns across the Pacific Rim in these decades was linked to high-level geopolitical interventions in the global monetary and financial systems, which had a military dimension, all justified through Cold War anticommunism. It is not coincidental that Japan's outward expansion under US patronage so readily recomposed the general spatial pattern of the imperial "East Asian Co-Prosperity Sphere," nor that the "flying geese" model of development re-popularized in these years had its origins in the Imperial era, when it was used not as a model of laissez-faire marketization but as a justification for colonial conquest. In a more literal sense, wartime procurement contracts from the US military in the Korean and Vietnamese wars helped to lay the groundwork for the early Japanese and Korean industrial conglomerates that would later go on to compete against US manufacturers. Similarly, the Japanese industrial firms that poured money into Hong Kong, Taiwan, Singapore and South Korea made use of contacts among the domestic bourgeoisie in each country originally cultivated in periods of occupation and colonization (as documented by Itoh 1990 Chapter 10, Glassman 2019, Chuang 2019). In this way, the combination of surplus labor, currency differentials, geographic proximity to seaborne trade routes (increasingly important as

containerization proceeded) and military linkages through both new US networks and earlier Japanese colonial holdings ensured that development would take a particular regional character across the Pacific Rim.

# Section 3 – Imperialism

**Introduction**

Global production can only be understood with reference to global divisions of labor, which are conditioned by the reality of uneven development and the relative cheapness of some pools of labor over others. But this does not mean that production simply moves to any location with cheap labor. As discussed above, numerous geopolitical interventions help to structure this process and, as will be seen in Section 4, more local questions of industrial location, proximity and infrastructural development play an equally important role. Overall, however, the key fact here is that the increasingly cutthroat competition that emerges between global industrial blocs, united (at least initially) by dependence on a shared currency and access to a shared—though often subdivided—labor pool, does not occur in a smooth space of market equilibrium. Instead, as is evident in the US victory in the trade war with Japan, this competition occurs within a simultaneously political and economic hierarchy. The name for this hierarchy is imperialism.

While the term imperialism is often conflated with colonialism and understood primarily in relation to the late nineteenth century era of "high imperialism" associated with the peak of the British Empire, the core dynamics of imperialism are not reducible to the superficial features of this era. Though colonialism became an essential element of high imperialism as an actual

historic process, it's important to remember that Marx, in his writings about Ireland and India "did not argue that capitalism in Britain had to have colonies, but that once history had taken that turn, there was no going back. India become necessary, not to capitalism in general, but to capitalism as it had in fact developed" (Brewer 1990, p.51). This is distinct from the later argument of Luxemburg, today revived by Harvey, that colonial conquest was necessary to secure the surplus required to expand the system. In fact, this was based on a now widely-acknowledged (at least in "Marxological" circles) misinterpretation of Marx's basic argument and the nature of his use of "reproduction schemas" in Volume II of *Capital*, compiled from his notes by Engels (see Brewer 1990, Ch. 3 and Clarke 1994, pp.55-57). Harvey perpetuates the same misunderstanding in his theory of "accumulation by dispossession," framing it alternately as a problem of overaccumulation and underconsumption, making only loose reference to Luxemburg but justifying the idea in almost identical terms of securing the surplus necessary for expansion (Harvey 2005). Though not a "Marxist" in any possible sense of the word, Hobson was an equally important precursor here as well, since his theory of the link between the lack of effective demand on the part of consumers and colonial expansion in the era of high imperialism became the counterpoint against which the polemical formulation of Lenin and Bukharin's theory of imperialism took shape. Meanwhile, Hobsonian theory has today returned in the form of the "trade imbalance" narrative of Klein and Pettis, reviewed above, whose book opens with a quote from Hobson.

Against these various formulations of an underconsumptionist theory of imperialism and crisis, where lack of effective demand in the home market leads industrialists and financiers to turn outward, Bukharin composed what would become the "classical Marxist" account, ultimately popularized by Lenin. Ironically, the basis of this theory was ultimately rejected in the

Soviet Union's formulation of an underconsumptionist orthodoxy based on the work of Luxemburg and Varga, even as Lenin's text was canonized (Clarke 1994, Ch. 2). In Bukharin's account, the problem of securing competitive advantage and retaining profitability in the face of declining returns led to both intensifying internationalization of production and the formation of national industrial blocs. Thus, internationalization coexists with intensifying nationalism, all undergirded by the increasing centralization of production at larger and larger scales. This involves the increasingly politicized use of tariffs and other measures in various trade wars, which then spill over into competitions between blocs attempting to secure markets not for their consumer goods but for their capital exports, seeking new sites of cheap production and raw materials (Bukharin 1929). The canonization of elements of this theory via Lenin largely occurred because it seemed to accurately capture the way in which imperial rivalry could lead to worldwide war and the new methods whereby the spoils of imperial conquest might be shared with domestic workers organized into a "labor aristocracy" that benefited from its imperial position (Brewer, pp.111-127).

While the most frequently cited variants of "new" theories of imperialism come from world systems theory, dependency theory or Harvey, none of these theories have been able to account for the rapid development of the Pacific Rim in adequate terms, nor connect these dynamics to the deeper structural logic of capitalist accumulation. It's not simply that such theories don't address or predict the phenomenon but instead that their early formulations seemed to actively *preclude* the type of development that did, in fact, occur. It's worth remembering, after all, that China, Southeast Asia, Hong Kong, Singapore and even Korea are all classed alongside Latin America and Sub-Saharan Africa in the consideration of early dependency theorists (such as Frank 1966 and Rodney 1972). Similarly, Harvey's emphasis on

pigeon-holing China into the "neoliberal" catch-all (in Harvey 2007) quite clearly suggested that subsequent decades should see further privatizations and continuing retreat of the state, at least within the economic realm. In fact, with regard to China he gives two options, both of which result in a neoliberal order: "further neoliberalization" or "the ascendance of the same sort of technocratic elite that led the Mexican PRI towards neoliberalization," even while he hedges this forecast (as always) by suggesting that "the masses" might seek "a restoration of their own unique form of class power" (ibid, pp.150-151). Of course, the reality has been the exact opposite, as the state has consolidated its economic power (Lardy 2019), protests (which didn't conform to the pattern Harvey predicted, either) have been quelled and development has been fueled by state-backed debt, as accurately forecast by others more attentive to value theory long ago (Mattick 1969) and reemphasized by others (Brenner 2003, Mattick 2011) around the same time as Harvey's own predictions (the failure of the "neoliberal" model with regard to China is well documented by Buckingham 2017).

In contrast to what might be thought of as the "new imperialism" school spanning both the early dependency theorists and the later Harvey, recent years have seen the rise of fundamentally different approaches that attempt to return to fundamentals in order to both account for the reality of rapid development in East Asia and link observed capital accumulation at the superficial, empirical level to the deeper structure of value theory and the "laws of motion" identified by Marx. While certain elements of the "classical" account, as well as the insights of earlier[33] dependency theorists, are integral to these contemporary theories of imperialism, the

[33] To make the picture a bit more complicated, many adherents of the dependency school have contributed to this process. In so doing, they have often abandoned earlier elements of dependency theory without entirely acknowledging the fact. At the more concrete level, these more recent theories of imperialism have tended to be published in the same venues associated with the older dependency theorists (namely, in outlets associated with the "Monthly Review" school) and are based on similar, but not identical, interpretations of Marx. Thus, as elsewhere, the actual history is knottier than any simple schema of intellectual genealogies can capture. It is

central focus tends to be on the process of international labor arbitrage enabling "super-exploitation" wherein the price of labor is forced below its value, the construction of intricate global value chains that create the same contradictory tension between international interdependence and national competition between industrial blocs first clearly theorized by Bukharin, and the mobilization of geopolitical power in international financial markets and the global currency systems to enforce the global hierarchy of production. These are the essential elements of imperialism, none of which fundamentally require explicit colonization. Meanwhile, the entire process is immanent to capitalist reproduction, rather than representing the plundering of non-capitalist spheres in order to obtain surplus resources or liquidate surplus product beyond the bounds of the system.

**Value Chains**

The basic shape taken by this global hierarchy of production is that of the global division of labor, which comes to compose what Intan Suwandi (2019) calls a value chain, wherein different levels of production are geographically distributed in links of interdependency between different national blocs of capitalists, all overseen by more hegemonic owners who are able to secure larger shares of the final value. Though Suwandi and other theorists of imperialism tend to emphasize the national character of such links and divisions, it will be shown below that the same relationships prevail at multiple geographic scales—and in the Chinese case in particular, it is impossible to understand coastal development independent of geographic inequity with the interior. Regardless of the scale, however, at every level this seemingly cooperative chain is

maybe best categorized as an evolution of traditional dependency theory, and the Monthly Review school remains one of the few places where the former orthodoxy of "worldview Marxism" has been retained.

shaped by intense competition and by inequities of power. At the more local scale, component producers can easily be switched out and contracts awarded to an opposing bloc if that bloc can guarantee cheaper production. In this way, many domestic capitalists in poor countries or regions are made effectively subordinate to those in richer ones, even if they themselves may act in a similar capacity with regard to capitalists in even poorer countries/regions further down the value chain. At the larger scale, this all occurs on intensively competitive global markets for final manufactures that are suffering under general overcapacity, which has driven prices to all-time lows across many industries. Even the largest imperial powers and their affiliated capitalists, whose firms are held up as the prototypical "multinationals" are subject to this competition and have a particular, material interest in maintaining certain capital flows, relative currency valuations and all kinds of institutional arrangements. In this sense, the persistent national character of internationalization first pointed out by Bukharin is still evident. Suwandi notes that "multinational corporations are still pretty much national in their governance structure" (ibid, p.29) and that this is particularly true in terms of their management, R&D and legal ownership of intellectual property.

At the same time, it is clear that that the existence of such value chains is not as easily divided into the narrative of Global North and Global South that Suwandi frequently makes use of, despite all the baggage they carry from earlier incarnations of dependency theory. It is particularly jarring because Suwandi offers a very different picture of global production than these early accounts. In the classic dependency theory context, capitalist development in the wealthy countries—which are portrayed in relative fixity, as the old colonial powers in the "Global North," including Japan—is accompanied by an equivalent enforcement of underdevelopment everywhere else. Among the earliest formulations of the thesis were works by

Frank (1966, 1967) and Rodney (1972), focusing almost exclusively on Africa and Latin America but tending to class much of Asia in the same categories. Dependency theorists ultimately proved unable to predict or account for the substantial developmental boom across Asia in subsequent decades and either abandoned their "Marxist" underpinnings for the classical, Ricardian political economy of the World Systems Theorists who, by the 1980s, were predicting that Japan would become the next global hegemon, or they explained these developmental bursts as exceptional phenomena produced by the purely exogenous influence of the US military (on the history of dependency theory and its relationship to world systems theory, see Brewer Ch. 8-10). This argument itself become more and more untenable, however, when the same phenomenon occurred in China.

Today, dependency theory has largely been abandoned and its remaining theorists have had to modify its core claims so substantially that it has come more or less in line with the "value chains" theory of imperialism put forward by thinkers like Suwandi (Amin 2018). In this variation, language is borrowed from dependency theory and the political implications are framed in the "anti-imperialist" language popular to the decolonization struggles of the later twentieth century, but the notion of value chains ultimately defers to very different theories about the relationship between imperialism and value theory. Contra classic dependency theory, this understanding of imperialism does not simply divide the world between a global north and global south, which are core and periphery, dominated by development and underdevelopment, respectively. Instead, renewed emphasis is placed on the very features once associated with the classical theory of imperialism put forward by Bukharin: imperialism as the name for a global division of labor, the distribution of gradations of exploitation across this uneven geography, and as the developmental engine whereby capitalist relations extend across the world.

Furthermore, in an era where this extension is complete and there remains only shrinking, encircled fragments of a "periphery" in the sense of an area at the edge of the capitalist system where subsistence predominates, imperialism is a method whereby capitalist relations are not only extended, but unevenly deepened, as different national or even regional factions of industry compete to source ever-cheaper inputs. Today, it is better to conceive of the spaces that exist beyond the cores of administration and management of capital as a hinterland rather than a periphery, since they are often not peripheral at all but are instead integral to global production and fully subsumed into global capitalism even while they bear a distinct geographic relation to it (Neel 2018). This is also significant in its relation to the problem of population, as posed by Itoh, since global labor arbitrage "is a strategy for both reducing socially necessary labor costs and maximizing the appropriation of surplus value" and is, therefore, dependent on the global reserve army of labor embodied in the surplus population (Suwandi 2019, p.54). Here, the two most important aspects of the growth in the global reserve army have been the incorporation of formerly "socialist" countries—namely China—into global capitalism and "the *depeasantization* of a large portion of the global periphery through the spread of agribusiness […which] has resulted in the growth of urban slum populations" (ibid). We will explore each of these in more detail with respect to China and Africa below. For now, it is simply important to emphasize that this growth in the global surplus population, which accompanied the general proletarianization of the remainder of the human population, is an essential prerequisite for the global division of labor to function.

In value chains, the notion that global labor arbitrage enables a form of unequal exchange that maximizes valorization for leading industrial capitals is central, but Suwandi doesn't systematically connect the idea to a more rigorous conception of how this works in the deeper

dynamics of value theory, gesturing instead to various thinkers more influenced by archaic conceptions inherited from dependency theory or the "monopoly capitalism" of Baran and Sweezy. A much more systematic explanation is instead offered by John Smith (2016), who focuses on more fundamental issues of production and value in these global commodity chains, and Tony Norfield (2017) who focuses on their high-level political and financial dimensions. Suwandi notes, for instance, that global labor arbitrage basically operates through the appraisal of unit labor costs, which is a measure that combines

> data on labor productivity and compensation to assess the price competitiveness of a given set of countries [… and] is typically presented as the average cost of labor per unit of real output, or the ratio of total hourly compensation to output per hour worked. (Suwandi 2018, p.56)

This is true, but it remains a superficial understanding since the dynamics are here explained purely in terms of empirical measures of price rather than value, with the connection between the two, in Suwandi's case, never made entirely clear. Although not necessarily a problem, a deeper understanding of these dynamics at the level of value production will help to illuminate the real mechanics at play.

John Smith explains the same phenomenon as Suwandi but attempts to do so by integrating the account with Marx's understanding of value, emphasizing that value can only be understood at the systemic scale. There is no way to isolate a single firm and determine how much value its individual workers produce relative to the surplus value extracted in the form of profit, because profitability can only be understood relative to other firms. Profit is only reckoned through competition and, for individual firms, it is defined by value "capture" rather than local value production. This is important precisely because Marx himself argues that the fall

in the rate of profit can be counteracted if firms in a more advanced country are able to sell goods above their value through international trade. He compares this to an individual firm adopting a technological innovation before others and is thereby being able to sell its goods above their value since the firm is able to produce the good more cheaply through substantially increased productivity even while the market is still appraising the commodity's price according to the prevailing technical methods of its production across all other, less productive firms. Once the technology generalizes, the price advantage is lost because productivity also generalizes across the industry. While this is an example within a single industry and won't apply in the same way between lines of production (see below), the key point, as noted by Smith, is that: "the more productive capitalists' extra profits derive not from their own more productive workers but from surplus labor extracted from workers employed by technologically deficient capitals" (Smith 2016, p.241).

This idea of value transfer is the whole basis of Marx's attempt to extend the value theory formulated in *Capital*, Volume I in terms of total social production (and in terms of circulation in the drafts that would become Volume II), to an ultimately more granular model that incorporates competition between individual firms (which was the focus of the far more fragmentary drafts compiled into Volume III). This overall movement from the completed Volume I through the uncompleted drafts is important, despite its incompletion, because value cannot be understood solely at the point of production, but only in relation to the overall circuit of capital, including exchange, competition and credit (as argued by Heinrich 2012 and Moseley 2018). Smith notes, like Heinrich and Clarke, that what came to be "Volume III" was little more than fragmentary notes at the time of Marx's death, all of which were written prior to the completed version of Volume I. Thus, Smith is explicitly attempting not so much an exegetical analysis as an

extension of the logic based on these notes and in accord with Marx's later writings (which are, to add to the confusion, posed as "earlier" volumes). In this regard, it is important to note that even while the fragmentary nature of Volume III has led to a consistent misreading of Marx's crisis theory (per Clarke 1994), the more basic methodological shift (between the total social capital as unit of analysis versus a more granular model focusing on competition between firms, or "individual" capitals) that occurs in Volume III is less controversial (it is noted by Mattick 2018, Mosely 2018 and, originally, by Henryk Grossman—even Heinrich's disavowal of the centrality of the "law of the tendency of the rate of profit to fall" includes the recognition that Marx moves between these different registers of abstraction, in Heinrich 2013). What is most important here, however, is the fact that, by touching on the role of value capture in competition, Smith marks out a key distinction between international trade within lines of production and trade between them.

In so doing, Smith distinguishes between the counteracting effect that international trade *in similar branches of production* can exert against falling profitability in those branches and a fundamentally different counteracting tendency based on "super-exploitation," enabled by the selling of goods above their values *in dissimilar branches of production*. Both involve the selling of goods "above their values." But it is this latter form that defines the process of global labor arbitrage, according to Smith, and which has made the outsourcing of production capable of acting as a countertendency against declining profit rates in general rather than for lone producers. There are two ways to account for this, one of which is more epiphenomenal and involves only price movements, more or less measurable in conventional statistics. The other is related to the deeper machinations of value capture hinted at above. Smith begins with the epiphenomena: Understood in terms of conventional statistics, offshoring is "profitable" because

the bulk of the profit from the overseas production contracted by a multinational is ultimately recorded on the apex company's balance sheet. It is only in this fashion that Apple, a company that owns no factories, is able to be listed as a "manufacturer" and derive profits from the manufacturing it contracts out to other firms. While this is profitable further down the chain, the share of profits captured by subordinate firms diminishes on average according to their level of subordination. A large multinational headquartered in the United States might contract out to a large Taiwanese contract manufacturer such as Foxconn, which will then sub-contract to myriad local manufacturers in industrial zones in China, Vietnam or India. These sub-contractors may even sub-contract further, especially through labor staffing agencies or in their own sourcing of raw materials. At the lowest levels of this chain, the sub-sub-sub-contractors are getting the smallest shares of total profit. At the same time, the actual production site is itself often still profitable and the contract manufacturer is able to capture enormous profits even while the apex corporation in the United States may be taking the bulk of the profit from the entire process for itself. In a series of case studies looking at the production chains of individual commodities, Smith demonstrates how these sub-contracting relationships operate and the deceptive methods used to account for the output in official business statistics (Smith 2016, Ch.1).

But far more integral to his theory is his later explanation of how this operates at the level of value, rather than in the official accounting measures used by national statistics agencies. It is only at this level that potential effects on the general profit rate can be understood. The key fact here is that, *within a given branch of production,*

> the value generated by productive workers in a given amount of time is independent of their productivity, even if the value-added captured by their employers remains highly dependent on this. This is so fundamental, it must be repeated: a steelworker operating

> more technologically sophisticated machinery *does not* produce more exchange value, s/he simply allows her/his capitalist employer to capture a larger share of it. (ibid, pp.241-242)

Again: this holds within a given branch of production. It then leads to a divergence in the rate of profit between individual producers based on productivity due to their relative efficiency *at value capture* in the market. It is only *between branches,* however, that the convergent tendency toward an average economy-wide profit rate will be evident. This is because these producers "do not confront each other directly as competitors in product markets, but indirectly, as capitals competing for new investors" (ibid p.243). Thus, between firms in different branches of production, the divergences in socially necessary labor time required to produce a given good, on average across all producers, will still determine the relative prices between that good and others on the market on average and in total—at least once transit costs are taken into account with regard to price formation, which occurs through a similar process determined by a combination of geographic distance and the relative efficiency of various logistics firms competing to link these production sites together in space. Value is therefore captured indirectly, through "profit-equalizing transfers" that are "generated by differences in the organic composition of capital" for different producers (ibid).

The distinction is significant because it defines the nature of the value transfers that will prevail between countries (or regions) based on how much of their trade lies within similar or dissimilar branches of production. This then determines whether spatial relocation is actually capable of counteracting a fall in profitability on its own—as in the example given by Marx himself—or whether some other measure is required. If the trade between two countries or regions is predominantly in similar goods (in other words, within the same lines of production)

then “trade differences in productivity are a prime cause of value-transfers and a prime determinant of above- or below-average profits” (ibid, p.243). This tends to be the case between imperialist nations, where firms are actively competing in the same branches of production (for instance, Japanese vs. American car producers). But this seems to raise a problem for the theory, since it implies that international trade can only allow firms to sell goods above their value when it is occurring *between* imperialist nations, where countries can exploit differentials in productivity in order to capture greater shares of value on the market. In these cases, more capital-intensive firms in one location are able to capture more of the total value not because their local process of production actually produces more value (again, value doesn’t operate at the level of individual capitals) but because they are able to capture more value in the sale of their goods, effectively siphoning it from the more labor-intensive producers in other regions. This seems to account for some of the competition reviewed by Itoh and Brenner in the case of the trade war between the US and Japan, for instance, but it doesn’t capture either the earlier period of Japanese industrialization (when Japanese industry was centered on lower-end branches of production not in competition with US producers) or more recent industrialization in China, nor does it explain how declining profitability in an entire country or region could be counteracted by relocating firms from that country or region to another in order to establish a relationship wherein trade between the two countries/regions is mostly in dissimilar goods—but this has precisely been the result in the most extreme instances of offshoring.

In order to explain this latter case, Smith turns to a different example from Marx. In this example, Marx explains the higher profit rates secured by firms with investments in the colonies by referring both to lower levels of industrial development in these locations (i.e. the siphoning of value from labor-intensive to capital-intensive firms within the same branch) and to the more

extreme exploitation of their workers, specifically mentioning the role played by enslavement and coolie labor. Though it is only a brief mention, it does seem to acknowledge the possibility for a different sort of countervailing tendency capable of buoying profits. Smith calls this tendency "super-exploitation," which is defined by the reduction of wages below the average value of labor power elsewhere—Smith affirms that this is simply a driving-down of the portion of value allotted to labor in general and should not really be understood as paying "less" than the "value" of labor as such, though others have characterized super-exploitation as a case in which wages are reduced below the value of labor-power. Regardless of exactly how we might define it, the phenomenon has the ability to push the up the economy-wide rate of exploitation (which is the total amount of surplus value divided by the total value allotted to labor in the form of wages), which will result in upward pressure on the profit rate, presuming no drastic changes in the amount spent on capital goods. This means that super-exploitation—or, in other words, global labor arbitrage operating between lines of production that allows the value apportioned to labor economywide (i.e., at the global level) to be driven down substantially—is prone to become self-reinforcing, as low profitability will promote more offshoring of production, unbalancing trade further and thereby both enabling more extreme labor disparities in the relative sense (measured in, say, comparative unit labor costs between countries) and in the absolute sense, as firms scour the globe for ever-cheaper labor inputs and begin to reproduce traditionally coercive labor regimes less and less dependent on the wage relation itself (as in the use of slave labor on West African cocoa plantations or on construction sites in the UAE).

With regard to the more general problem that profitability poses for the system as a whole, the key takeaway here is less the attempt to calculate country-by-country rates of profit (which is not actually possible in value terms, as argued by Moseley 2018), and more the issue of

whether or not claims on this profit up and down the chain exceed the profit itself—and, if they do (in which case they increasingly take the form of financial assets), is there sufficient liquidity to keep the circuit of capital turning over regardless and (returning to the key role played by treasuries) what, exactly, are the lynchpin assets underpinning global liquidity in its myriad forms of credit? No matter how distant finance seems to take this process from rudimentary questions of production, profitability remains a problem throughout, even if the problem of population was (temporarily) solved through massive waves of dispossession—which will, in the end, only recreate the problem of population anew, in a different aspect. It is on the grounds of profitability that Smith's account overlaps with the explanation of stagnation and crisis, since "these claims [on profit] rose faster than world GDP in the decade preceding the [Great Recession] and have grown even more strongly since then" (ibid, p.248). This is clearly visible in rising global liquidity, as documented by Howell. More importantly, Smith notes that the apparent "imbalances" of global trade, which are "otherwise known as overaccumulation" are marked by the increasing divergence "between the claims on surplus-value and the capacity of the productive system to meet these claims." This will initially appear as an excess of financial assets over the economy's real productive power. Moreover, the only way to solve this disproportion is not through some sort of cooperative rebalancing, as implied by Klein and Pettis, but only through "a partial but substantial reduction in these claims, in other words, a major destruction of financial assets" (ibid, p.249). As pointed out by Mattick and Brenner, however, destruction of the physical assets serves much the same purpose and, historically, has proved to be even more effective at restoring the prospects for growth.

**Finance**

While there is no space here to dig further into these questions, since the focus below will be on industry rather than finance, a brief overview of the role of finance and the growth of global liquidity, as identified by Howell, will be helpful to frame some of the dynamics explored below. The key thing to understand here is that large financial superstructures emerge in the most advanced economies both because the profitability of industry declines—and capital therefore is funneled into increasingly speculative portfolios of financial assets, rather than direct investment in production—and because the global value chains being constructed by the remaining productive industries require a managing infrastructure. Finance, therefore, can be seen as speculative and unproductive and nonetheless integral to production at the same time. If the vast chain of global liquidity and its perpetual re-financing machine ever came to a stop, production would freeze as well. These financial dimensions, moreover, are not limited to the upper echelons of the global economy, but are woven through value chains at almost every juncture. It is extremely common, for instance, for base-level producer firms in China to seek financing for each season's production rounds through claims on future revenue. When any element of this chain breaks, the firm is then unable to pay back wages or to cash out workers' social insurance funds. This has been an increasingly common source of worker protests in recent years (Chuang 2019 and Jiang 2019).

Tony Norfield explains the overall structure of global finance and its relation to production through the lens of the City of London—which is, effectively, the world's financial capital. Throughout, he is clear that "finance neither produces new value nor even transfers any value from its operations to commodities. All of its profits, as well as its costs, are a deduction from the total surplus value produced elsewhere" (Norfield 2017, p.144). This returns to similar themes as were explored by Mattick and (Jason) Smith previously, with regard to the

involvement of the state and the extension of supervisory and managerial activities. In measuring profitability, it poses the further problem of a divergence between seemingly higher profits when finance is included in the measure versus persistently low business investment. This is mentioned by Jason Smith as the justification for using the latter investment measure in favor of profitability and it is one of the reasons that Brenner utilizes profitability data for the manufacturing sector alone. Here it is the signal that a particular financial position enables the capture of larger shares of profit, without any correlated rise of real investment. This, of course, is made possible by the very global structure of imperial inequity pointed out by John Smith, with regard to commodity chains, not to mention the vast financial infrastructure in wealthy countries that links seemingly mundane features of many peoples' lives (such as retirement funds) to the "health" of the stock market. For Norfield, however, the focus returns to the factors explored by Brenner: the role of currency values in structuring global trade.

Norfield argues that it is the US seignorage position that enables much of its imperial power. Seignorage here refers to the fact that the US Dollar is the predominant currency used to settle international transactions. In 2013, the USD "was on one side of 87 percent of all global currency deals, far beyond the US share of international business and more than twice the 33 percent share of the euro" (Norfield, 2017, p.162). This enables US companies to benefit from lowered risk of changing exchange rates, putting this risk largely on firms from other countries, since most global commodities are priced in dollars. Similarly, it allows the US to exploit this position in order to siphon financial investment into the country as large portfolio investors seek "safe" assets, per Howell. Those priced in dollars are deemed the safest best precisely because of this seignorage position. Even better are US Treasuries, since these link even more directly to the

political power behind the monetary and financial infrastructure of the world. This then enables US foreign investment an "exorbitant privilege" relative to competitors. Overall,

> the interest costs on US foreign borrowing have been far less than the returns on US foreign investments. This has enabled the US to maintain a positive net investment income, despite the persistent, large net *deficit* on its foreign investment position. (ibid p.169)

In other words, American firms have proven able to capture greater shares of global value even though foreign ownership of US assets is higher than US ownership of foreign assets. This disproportionality is the best representation of the imperial power of the US state. Similarly, it demonstrates a *shared* dependence on US seignorage among other imperial countries, such as the UK, where the many of the key financial mechanisms undergirding the global dollar system are tended.

# Section 4
# –
# The Geography of the Global Glut

**Introduction**

It now becomes possible to tie these disparate observations together into a more concrete idea of how and where capitalist development takes shape in space. Essential to this account is the idea that such development is fundamentally driven by technical change and the general replacement of humans with machines, which proceeds alongside the growth of a general surplus population.

At the same time, the problems of population and profitability attenuate this trend somewhat, since the expansion of the surplus population is itself accompanied by an extension of all sorts of unproductive or low-productivity activities. All of these trends are global in scope. But they tend to take different shapes in different locations depending on relative levels of development, which are of course shaped by the history of imperialism and by the current position at which a nation is integrated into the global hierarchy of production. In the wealthiest countries at the top of this hierarchy, rising surplus population in excess of productive needs is visible in deindustrialization and the subsequent, general expansion of underemployment, the increase in unproductive supervisory, state, reproductive and managerial work and in a growing population subject to more absolute immiseration, in the form of the homeless and incarcerated. These are the nations where ownership of the financial assets associated with "overaccumulation" (in John Smith's sense, not Harvey's) are concentrated and where value capture reaches its apex, host to the headquarters of primate firms within global value chains.

By contrast, in poorer countries the production of the surplus population is defined by depeasantization—as part of a more general proletarianization that also includes the incorporation of the working classes of the formerly "socialist" countries into global capitalism as well. But the prospects of these newly-proletarianized workers diverges in different countries. In the few locations that have been able to successfully attract new rounds of industrial development, this surplus population was at least partially absorbed, even while its remainder helped to suppress wages for the majority. In some countries, such as Vietnam, Cambodia, Bangladesh and Pakistan, industrialization is proceeding rapidly in its earlier stages and has not yet hit its initial interlinked limits of population and profitability. In others, namely China, the problem of the surplus population is now returning in the form of deindustrialization, similar to

that experienced by Japan and manifesting in a sharp demographic turn, a rise in informal service employment and the decreasing competitiveness of Chinse industry in labor-intensive manufacturing. Here, the absorption of the initially "depeasantized" rural surplus population has now reached a saturation point and wages have been on a clear upward track for years—it is even likely that the total population of migrant workers will begin to decline in the near future, as has already occurred in the midst of the pandemic.[34] But China is also a large country with persistent geographic unevenness in per capita incomes and general levels of development. Certain areas are still seeing active industrialization and proletarianization of the rural population even while other areas are undergoing rapid deindustrialization. This will all be explored in much more detail below.

But there are other poor countries where depeasantization has occurred without sufficient industrialization to accommodate it. In these cases, often referred to as instances of "premature deindustrialization," the informal economy expands rapidly, grey and black markets intermix with formal exchange, new forms of non-market subsistence activity become necessary for survival and greater shares of the surplus population experience conditions of absolute immiseration. In some locations, these conditions are enabling factors for the continuing dominance of extractive industry and its chain of dependencies, similar to those theorized by the dependency theorists some forty years ago—but linked to a more complex lattice of global imperialism than they themselves identified. In other places, limited industrialization and the rapid build-out of basic infrastructure by Asian firms (namely, but not exclusively, China) has created the image of rapid industrial development even while industrialization proper has not

[34] This is evident in the most recent surveys released by the National Bureau of Statistics in the lead-up to the release of the results of a larger Census. The decline in the migrant worker population is visible here: <http://www.stats.gov.cn/tjsj/zxfb/202104/t20210430_1816933.html>

always followed—at least as measured by employment in manufacturing or increasing industrial output. This is the case across much of East Africa. The accuracy and limits of this picture will be explored with regard to Tanzania in the final chapters.

**Spatial Patterns of Industry**

While it is possible to roughly categorize these trends according to country, the reality is that many of the geographic inequalities produced by capitalist development exist as much within as between nations. Certainly, the unifying factors of different currency markets lead to a rough unification of labor costs by country—often shaped by subnational migration flows—but even in this regard, substantial internal differentiation exists with regard to wage rates, not to mention with regard to other cost factors such as transportation. This is particularly important in China because it is the driving factor behind the movement of investment into landlocked interior provinces (and the interior areas of coastal provinces), as well as into the far west. Chinese industrial relocation will be explored in more detail below, but it is notable that this was occurring alongside the outsourcing of Chinese production to places like Vietnam. On average, internal wage differentials will certainly be lower than international ones, but numerous other benefits remain—lower risk, easier financial transfers, no dependency on outside currencies (namely the dollar) to settle foreign transactions, and the benefit of simpler management through a shared language. These factors themselves are, of course, best understood as active economic and institutional processes rather than static amenities, and they are clearly not evenly distributed. Colonial histories, for instance, have shaped the degree to which many of these givens, such as stable currencies and shared languages, may or may not hold in a given region. Even in China, for example, the existence of a mutually intelligible spoken language has been an

intentional social project, deeply related to the state-building project. Similarly, when industrial relocation is occurring alongside industrial restructuring, new locational logics arise that privilege certain sites over others. This can be seen in the case of the original deindustrialization of China in the late 1990s and early 2000s, as the northeastern industrial hub was systematically dismantled even as a coastal sunbelt arose in the south, shaped by the spatial requirements of the new export-led production regime (Lee 2007).

It is therefore essential to increase the geographic resolution of the inquiry. Doing so will reduce the tendency toward an exaggerated methodological nationalism inherent in essentially all the writing on global "trade imbalances" by focusing on the construction of the specific territorial complexes where production actually takes place. The notion of the "territorial industrial complex" or "territorial production complex" is introduced by Michael Storper and Richard Walker (1991) in one of the very few attempts to translate some of the key concepts from Marx's critique of political economy into a narrative of regional, rather than national, development. On the one hand, Storper and Walker were drawing from earlier, more general work by the new wave of critical geographers focusing on questions of space in regard to the division of labor (Massey 1984) and uneven development (Harvey 1982, Smith 1984). On the other, they were adopting conventional econometric techniques that had been developed in the more conservative realm of regional science, attempting to repurpose these methods for critical use.

Central to their project is the idea that regional industrial development has to be understood using a schema of reciprocal causality, wherein "industries produce economic space" because

> contrary to Weberian location theory, industries are capable of generating their own conditions of growth in place, by making factors of production come to them or causing factor supplies to come into being where they did not exist before. (Storper and Walker 1991, pp. 70-71)

This also means that the simple factors of cheap labor or resources are not sufficient to account for industrial relocation: "It is decidedly not the case that peripheral locations are cheaper sites for production, because the same conditions of underdevelopment that make labor or land inexpensive generally make them less productive in use" (ibid, p.73). Thus, peripheral locations become attractive locations only when specific conditions are met. One condition is when general structural limits have been reached in mechanization, profit is stagnating and, as John Smith points out, supply of dissimilar goods between industrial lines can be cheapened by forcing down the cost of labor through global arbitrage. Without any reference to the value terms Smith uses to explain the dynamic, this appears simply as: a) the sort of systematic offshoring of entire branches of production to new locales where wages are extremely low and b) the more nefarious but equally important dissolution of the wage relation as such, through informal supply relationships linked to "artisanal miners" (who supply as much as 30% of the cobalt mined from the DRC according to Baumann-Pauly 2020), or new forms of slave production (as in the West African Cocoa industry, as documented by Whoriskey and Siegal 2019, or the construction industry in the UAE, as overviewed by Cornwell 2020), many of which involve renewed forms of child labor. In such conditions, while it still may not make sense to relocate a firm's core, capital-intensive production line, it nonetheless becomes lucrative to relocate labor-intensive suppliers elsewhere. In fact, this seems to drive periodic returns to labor-intensive methods of production in certain supplier industries *even when* more mechanized production may be possible

(see Itoh 1990, Ch.10), since it ensures that mechanization, in these specific instances, is not yet profitable, is too risky or is not affordable or feasible given the average size of firms (i.e., when particularly cheap pools of labor enable the persistence of workshop-size production units). This is clearly the case in mining, where large capital-intensive facilities are often located literally side by side with "artisanal" operations conducted with hand tools in extremely dangerous conditions.

Another condition that makes distant relocation possible is the relative age of the industry or its product. Storper and Walker point to "windows of locational opportunity" that open when entirely new production lines open, since these fast-growing industries "can ignore the traditional locational calculus" and are "not yet pinned down by enormous investments in big factories and industrial complexes" (ibid, p.75). This applies both to entirely new industries (such as the production of home computers) as well as to moments of revitalization in older industries triggered by "improvements in process or product, new employment relations, or other forms of restructuring" (ibid). But even within these "windows of locational opportunity," other factors remain important. Relocation requires necessary infrastructure, including industrial infrastructure in the form of roads, ports, utilities, etc. but also social infrastructure in the form of an education system giving workers a base level of literacy and technical training, as well as policing, carceral and other supervisory infrastructures to enforce labor discipline and ensure a baseline of social stability. Thus, even many "cheap" locations may still be "expensive" in all these regards. Similarly, classic agglomeration effects remain important throughout, particularly for the higher-tech production lines that benefit from proximity to the producer services offered in major metropolitan zones (Sassen 1991).

Overall, Storper and Walker offer "four basic patterns of geographic industrialization" (Storper and Walker 1991, p.71, Fig. 3.1). These are presented as abstracted schemas, each of which is later explored with relation to certain real-world examples with the understanding that there are always real constrains on the underlying spatial logic and some admixture of the schematic patterns. The first is the pattern of "localization" in which "a new industry arises at several points away from older industrial areas" (ibid). This tends to occur within the context of an opened "window of locational opportunity" driven by new technical innovations. It is important to note, however, that "localization" does not entail that all locational constraints are lifted in these windows of opportunity. Instead, the idea is simply that the new industry or new production line does not necessarily benefit from any proximity effects to old industrial clusters, even while it may benefit from entirely new proximity requirements. Some of these new requirements will certainly be industrial in character, but they aren't necessarily determined by the same logic as those of the older production lines. An example here (not used by Storper and Walker, given the year in which their text was published) would be the rise of data centers. These are a new industrial line with unique locational requirements that leads to a somewhat predictable pattern of industrial location determined primarily by cheap land, cheap energy costs and efficient and secure linkages to high-speed fiberoptic networks—of course considered alongside important institutional amenities associated with national regulatory regimes and network proximity to large potential pools of underserved customers.

While this might give the impression that localization only occurs at the forefront of technological innovation, the reality is that this pattern tends to dominate in any sort of new industry. A low-tech example can be found in the example of the *mitumba* market in Tanzania and Kenya (which exists in many other African countries under a different name), wherein

secondhand clothes donated from wealthy countries are arranged in large bundles (*mitumba* in Swahili) that are then unloaded, warehoused and ultimately exchanged in a chain of transactions that ends with vendors in local markets selling the clothes to consumers. Here, many affiliated activities arise, not only insofar as other goods are then sold alongside these clothes but also in washing, modifying and reselling. This is an example in which a new product line emerges from the commodification of a good (discarded used clothing) that had not been commodified. The result was a complete transformation in the spatial shape of the clothing market, including the bankruptcy of many local textile producers and the subsequent reorientation of commercial supply networks away from old manufacturing districts hosting textile workshops and toward the central ports receiving *mitumba* in container ships. This fact has now led to the gradual attempt to ban *mitumba* imports in order to revive local garment industries (Gabagambe 2013, Wangwe et. al. 2014).

The second spatial pattern that Storper and Walker identify is that of "clustering" wherein "one startup area surges ahead while others decline or grow more slowly" (Storper and Walker 1991, p.71, Fig. 3.1). This phenomenon is particularly important to understanding the rise of the sunbelt industries in China and will be central to my own field work examining the industrial zones of Dar es Salaam, Tanzania, where clustering effects have tended to concentrate much of the country's manufacturing capacity (outside lines linked directly to mining and agro-processing). A related spatial pattern is found in the form of "dispersal," wherein "growth peripheries arise away from the core territory of the new industry" (ibid). Dispersal can be understood as the push factor that then creates new cores where clustering takes place. Both are far less dependent on the emergence of entirely new lines of production and tend to be conditioned by changes in the "locational calculus" of unit labor costs, access to new markets

and changing institutional conditions. An example of the paired dispersal-clustering dynamic can be found in the consumer electronics sector, where assembly work was regularly relocated between East Asian production sites as changing labor costs dictated. But this dispersal was then accompanied by new processes of clustering *within* the new host countries, as areas such as Shenzhen, in China, outcompeted early potential competitors elsewhere to become a hub for assembly work.

Similarly, clustering and dispersal are often capped by the fourth and final spatial pattern, the "shifting center," where "a new center of an industry rises up to challenge the old" even as "peripheral dispersal may continue under the sting of new competition" (ibid). The shift of center only occurs after spatial dispersal has ensured that a new cluster forms and after that new cluster becomes so cost-effective that it leads to a subsequent shrinkage in the old core. Storper and Walker point to several examples here, including the relocation of the meatpacking industry from the Ohio River Valley in 1850 to Chicago by 1895 and from there to a band of heartland states stretching from Iowa south to Texas by the 1980s (ibid pp.94-95); the shift of cotton textiles industry from Massachusetts (1919) to North Carolina (1939) in the early twentieth century (ibid, p.93); and the relocation in the semiconductor industry from its old industrial clusters in the American Northeast out to the new hubs of California and Texas, which, at the time, had recently undergone dispersal to peripheral locations across East Asia (ibid, p.72, 90). These peripheral sites would soon see their own competitive clustering, resulting in a more general shift of center toward Asia.

**The Territorial Production Complex**

Throughout, Storper and Walker are careful to emphasize that, despite international relocations of production, "most industrial activity remains concentrated in a few regions of each country and in a few countries in the world, and regions still tend to have rather specialized industrial bases" (ibid, p.83). Thus, these patterns are not primarily occurring at the national scale, but at the regional one. This is important, because it explains how a country like China could undergo an almost simultaneous process of industrialization in one area and deindustrialization in another, founded on entirely different product lines (Lee 2007). Similarly, even within more general regions, industry will tend to concentrate in particular areas rather than others. These areas are what Storper and Walker define as "territorial production complexes," and sometimes refer to as "territorial industrial complexes." Following their usage, I will treat the terms interchangeably throughout.

In some cases, these may be commensurate with cities or mega-urban regions like China's Pearl River Delta. Given the global context at their time of writing, they point to historical examples such as the declining American Northeastern manufacturing belt as well as the newer "Osaka-Nagoya-Tokyo belt of southern Japan" (Storper and Walker 1991, p.141). These represent the largest-scale, regional examples, wherein industry effectively produces mega-urban economic spaces "so immense that they have been virtually identified with national or continental industrialization as a whole" (ibid). But these prominent examples are not necessarily the average case. In more general terms,

> a "territorial complex" is an extensive work site that brings disparate production activities into advantageous relation with each other, at a larger scale and scope than the individual workplace, firm, or even, in many cases, the industry.

> Territorial formations are fundamental to the operation of industry because they offer means of integrating production systems […] (ibid, p.138)

This integration occurs through a variety of geographic amenities, including proximity, which minimizes costs and maximizes access to effectively pooled services and resources, "locational fixity of infrastructure and daily activity" which lowers risk and helps cultivate the resource base, as well as "geographical boundaries" that limit movement and thereby help to both enforce divisions of labor and encourage inward social cohesion (ibid). Altogether, such complexes

> not only lower tangible costs of transport and communication, but ease information-sharing, allow pooling of labor and fixed capital, stabilize physical and social relations, help people identify with each other (and against outside competitors), and generate distinct cultural practices over time. (ibid)

While this is an expansive definition, it nonetheless narrows the focus of attention onto particular urban complexes or even specific industrial zones within cities, wherein all the spatial patterns outlined above can be observed at multiple scales of focus.

The underlying infrastructural variables that determine the paths of growth of such a territorial complex are equally important. Storper and Walker present the different patterns of development in purely schematic terms where industrial areas are initially treated as points floating in a neutral space. Later, they flesh out how these schemas take shape in real geographies of production. But they offer little in the way of an intermediate analysis exploring the infrastructural and institutional factors that help to determine the transition from the schematic logic to real development. On the institutional side, this is more easily done through an empirical narrative of recent economic history with careful attention given to the geopolitical

dimension, in the fashion of Brenner and Glassman. On the infrastructural, side, however, it is possible to identify certain patterns at a similar level of abstraction. Cowen (2014) points to "gateway and corridor cartographies" (p.62) as being increasingly essential within global value chains, and even culminating in the creation of "logistics cities" where an entire urban area's industrial base is as much or more dependent on circulatory activity rather than production or consumption (p.163). The growth of a territorial complex, in any of the patterns identified by Storper and Walker, will be alternately constrained or incentivized in its precise direction and possible distance from its industrial center according to the underlying geography of key infrastructural gateways and corridors which, at a certain degree of agglomeration, ascend to the status of entire logistics cities. China plays a particularly central role in this regard, since it is "a logistics empire" (ibid, p.67) both in the sense of supplying the containers, cranes, ships and other equipment necessary for global supply chains and insofar as Chinese firms, namely those in construction and finance, have been increasingly integral to the extension of basic infrastructure worldwide over the last two decades. Thus, the territorial complex includes and reciprocally develops a more fundamental infrastructural complex that both undergirds and extends out from individual production hubs.

Territorial complexes, then, can be observed at multiple scales. Their geography is constrained and channeled in particular directions by physical characteristics of the landscape as well as underlying infrastructural amenities. In order to investigate their spatial development, it is necessary to have data suitable to the scale of inquiry. But these complexes often both extend beyond conventional administrative boundaries and exhibit unique regional or local spatial patterns at more granular scales. Inherent limits in the data often prevent detailed inquiries beyond or below a particular administrative level due to the institutional nature of data gathering

and aggregation. On the one hand, commensurate measures may not be available between different areas. On the other, data may not be available at all below a particular unit of aggregation, even when it is gathered locally. This is one reason why my own field work, presented in the final chapter, attempted to obtain extremely granular spatial detail about industrial location in Dar es Salaam, since such data was completely missing from other sources. It is also why, in the next chapter, I am forced to rely mostly on provincial-level data to explore industrial relocation within China, since more local data was not consistently available for all necessary variables.

Throughout, though, the real point of focus is the "territorial complex" understood at a multi-scalar level including the smaller industrial zones of individual cities in East Africa as well as the mega-urban regional complexes, as seen in the Pearl River Delta. Similarly, I draw attention to the infrastructural complex that undergirds these zones and extends out from them, linking one to the other. At a larger scale, the particular spatiality of contemporary logistics infrastructure also privileges certain larger spatial forms (Cowen 2014), such as the development of corridors of industry in littoral areas which are themselves best understood not as isolated productive territories but as part of a larger integrated coastal rim (Park 1997, Neel 2016). But industrial restructuring always takes place within the greater geo-economic dimension of class conflict that itself shapes geopolitics. Coastal rims, therefore, are not purely factors of physical geography, since the development of such areas into a functional economic space is itself a political project that always leaves out certain sites of potential investment and privileges others. This process has a discursive dimension, visible when it is translated into intentional policy decisions. But this discourse is generated by more concrete military interests, which are high-level expressions of both factional battles within the global capitalist class (i.e., competition

between competing trade blocs, which can take on a military character) and expressions of class conflict at the global scale, as visible in Pacific development projects justified in the language of anticommunism and, of course, in the military conflicts in Korea and Vietnam (Connery 1994, Glassman 2019).

In this case, the Pacific Rim territorial complex is particularly important, since we can track its development into the immediate past in the very dynamics of international industrial competition discussed by Itoh and Brenner; and also project its growth into the immediate future, as industrial relocation extends the same littoral development pattern further south along the Pacific Rim, into Vietnam and Indonesia. Just as the development of the Pacific Rim complex was inextricable from imperial politics, so too is its present extension across the South China Sea (particularly visible in the rapid industrialization of Vietnamese port cities). Similarly, the identification of these spatial patterns will allow more speculative inquiries into the potential for the industrialization of the Indian Ocean Rim and can help explain the formation of early growth peripheries in places like Myanmar, Bangladesh and Pakistan in connection to both the current pressures placed on Chinese capital and the potential future development of more integrated commercial and industrial networks extending all the way across the Indian Ocean to East Africa. Broad observations are possible here, even if the precise political coordinates that will determine this development are not yet set in stone. Thus, my own survey of the industrial zones in Dar es Salaam is framed as only one early component of a much larger, ongoing inquiry into what conditions might be necessary to see the gradual formation of a potential Indian Ocean Rim complex as central to global industry in the future as the Pacific Rim is today.

**Measures**

This chapter has provided both a general theoretical context while also offering some suggestions as to the best methods to measure crisis in both its long-run and short-term forms. Similarly, the geographic aspects outlined in this last section give hints of how to narrow down this focus to the role of Chinese economy in global production, the spatial character of its internal industrial restructuring and the patterns of its outward expansion. To conclude, I will briefly preview some of the key measures that will be used in Part 2 below, leaving more detailed descriptions of the data and exact methodology for the forthcoming chapters. I will begin with an overview of the general summary statistics and then discuss options for narrowing the focus onto specifically geographic questions focused on the development and restructuring of territorial production complexes and their affiliated infrastructure.

As is made clear in the work of Benanav, Smith, Brenner and Itoh, it is important to first contextualize the relative performance of the Chinese economy relative to the other major economies during their own industrial booms and long periods of deindustrialization, even while keeping in mind that these national separations are merely tentative conceptual tools capable of capturing only the general contours of internal differentiation within the more general trends of the global system. We should, therefore, not expect the Chinese economy to simply mirror the experience of its Japanese, German, Korean or even Taiwanese precedents, as just one more late developer. Instead, China is clearly more central for the system today than any of these economies were historically. Whereas other moments of industrial restructuring were split between multiple countries—Japan and West Germany, or the four East Asian Tigers and, subsequently, several Southeast Asian nations—the Chinese ascent over the course of the 1990s and, especially, the first decade of the twenty-first century was unique insofar as it concentrated the vast majority of the world's new industrial growth in a single country, which was one of the

few locations with both rapidly increasing industrial output and sharply rising rates of industrial employment (at least after the deindustrialization of the northeast). In order to properly frame this narrative in the same terms posed by Benanav, Smith, Brenner and Itoh, we will need measures of output growth, employment growth and productivity growth. A review of trends in Chinese TFP growth will also be helpful, for comparison to the trends identified by Gordon. Similarly, measures of profitability and investment will be essential, especially insofar as it is possible to focus on profitability and investment in fixed assets within industry specifically.

Itoh's work also emphasizes the importance of population. One of the reasons that so much of the recent growth in global industry could be concentrated in a single country rather than split between multiple hubs was because China's population size meant that its incorporation into global capitalist production was among the largest single booms in the growth of the global labor force in the history of capitalism.[35] Similarly, China not only mirrors the Japanese case reviewed by Itoh insofar as its industrial boom was linked to the persistence of a large and extremely flexible rural labor surplus, but the Chinese example takes this amenity to further extremes, since the *hukou* system has allowed for even greater exploitation of surplus labor than in the Japanese case (Chan 2010). Though recent reforms to the system have been framed as measures toward its abolition, the reality is that these reforms have only intensified the application of spatial restrictions on the population in the major cities where most migrants have sought to settle (Chuang 2020). At the same time, the overhang of the stringent family planning policies implemented in the early years of reform, combined with rapid urbanization and the

[35] In absolute terms, it is obviously the largest. The subsumption of the Chinese population into global capitalism essentially doubled the size of the global labor force over just a few decades (Jaumotte et. al. 2007). Measuring the relative impact is much more difficult, but it seems apparent that even this substantial process of rapid incorporation pales in comparison to the historical potential for the expansion of capitalist social relations that was possible in earlier centuries, when the capitalist mode of production only reigned over a minority of the world population.

subsequent downward pressure on the birth rate, has now produced the opposite problem, causing a gradual labor shortage. This has led to a somewhat premature inflection in the dependency ratio, contributed to rising wages and, overall, resulted in China reaching the Lewis Turning Point years earlier than might have otherwise been the case (Zhang et. al. 2011, Wei et. al. 2017). Although the trend was initially dampened by lower than average consumption and higher than average productivity among the middle aged population (Han and Cheng 2020), there are no immediate prospects for its reversal.

Though there have been recent attempts to raise the birth rate, there has been little success. More importantly, increasing births would have the immediate effect of aggravating the problem by adding more dependents under the working age. Using international migrant labor is another alternative but, at present, this is minimal. Thus, demographic factors are important to understanding slowing growth and building crisis both as push factors behind industrial relocation due to higher wages attending shrinking rural surplus labor and as pull factors for the relocation of industry, both domestically and internationally. Similarly, the centrality of a reserve army to capitalist production—as a necessary means to address the problem of population—means that new territorial production complexes will necessarily need access to a large pool of potential workers. Ideally, such workers should have a minimal level of literacy, technical training and some ability to offset other social reproduction costs. On the immediate population horizon, it seems that only South Asia and sub-Saharan Africa offer such labor pools. The exact size and demographic prospects of these areas will have to be explored in more detail and compared to the relative impact of Chinese incorporation into the global labor force, keeping in mind that China's workforce had many of its social reproduction costs offset through the

overhang of socialist-era education, healthcare and industrial training institutions, none of which exist to the same degree in almost any other country today.[36]

At the more granular level of geography, FDI data by country and an overview of changing trade patterns and portfolio investment are all important. At the same time, this is where data issues will start to become more pronounced. Similarly, necessary limits on the scope of the study also arise here. This accounts for the very minimal engagement with existing trade and financial data that would be necessary (following [John] Smith and Norfield) to determine the precise role of China within the contemporary imperial hierarchy.[37] My focus will instead be on the role played by capital exports in these imperial dynamics. I will discuss the general problems with data on Chinese investment through a series of comparative exercises between major data sources, locally gathered statistics, ground-truthing through field surveys and, in the case of Tanzania, a more systematic investigation of investment projects reported in the media. Meanwhile, the need to break down the inherent methodological nationalism of most FDI accounts leads me to the use of more wide-ranging data. I will explore Chinese provincial-level statistics about the relative growth rate of industrial enterprises as well as the reported profits and assets (total and fixed) of these enterprises, alongside trends in provincial electricity consumption (to avoid potential criticism based on alleged overreporting on balance sheets). Similarly, I will draw from a wide range of ethnographic and field survey work on Chinese outbound investment in particular areas. To this, I add my own field survey conducted in Dar es Salaam, Tanzania. This is the instance in which I am able to explore the character of the

[36] Vietnam is one slight exception to this, which partially accounts for the rapid relocation of industry there despite the fact that relatively large populations of even lower-wage workers can be found elsewhere.
[37] Mapping the changing role of China within the global trade structure through reference to John Smith's work on Imperialism remains an important task for future work following up on the account I have provided here.

industrial territorial complexes currently under development in the most granular detail, mapping out Chinese factories and warehouses while also obtaining a general sense of the development of individual industrial zones based on interviews with local workers and residents and getting some impression of the character of Chinese investment in the city through interviews with Chinese workers and managers.

# PART II

# The Measurement of Crisis

## Profitability and Industrial Location

### Introduction

Since patterns in profitability and investment are important measures of how well firms are, in aggregate, overcoming the "problem of profit," this section is geared toward framing the basic, macroeconomic performance of the Chinese economy with an ultimate focus on measuring trends in the profit rate. The chapter is split into three sections. The first gives an overview of Chinese macroeconomic data on output, total factor productivity, labor productivity, investment, the absolute number of industrial firms, as well as the total assets, fixed assets and net profits of these firms. In order to address concerns about the reliability of output data, alternate GDP measures are included, alongside less falsifiable correlate variables such as total electricity consumption. The second section then moves on to the measurement of the rate of profit, giving an overview of the general approach, listing past attempts to measure the rate of profit in China and visualizing all of the attempts for which data is available and comparable. I then add to these my own series of different rate of profit measures, each of which derive from slightly different sources. Finally, I aggregate the measures into several mean measures of the rate of profit, each of which captures slightly different aspects. The third and final section of this chapter then disaggregates the rate of profit measures by size of firm and ownership structure. Finally, it

proposes a more granular geographic focus on divergent provincial trends, which will be addressed in Chapter 4.

It will be helpful, however, to first review the basic theoretical terms in which such measurement takes place and how it relates, if at all, to Marx's theory of value. Above, I loosely distinguished between relatively "superficial" empirical work on economic history, with a heavy reliance on conventional econometrics, and the attempts to connect this superficial level of measurement to the more fundamental "deep structure" of value theory. The work of Brenner (2003) and Benanav (2020) are two examples of work that is deeply informative but, on their own, have no need to go below this superficial level of measurement in order to narrate events. This is true even though both authors show elsewhere that it is possible to connect these narratives to a more fundamental theory of value and both have regularly described their own work as Marxist. In contrast, Itoh (1990, 2020), Smith (2020) and Mattick (2011, 2018) make the connection between the surface-level econometric narrative and the "deep structure" of value theory more explicit, even if the two are kept distinct (as is especially visible in Mattick 2011, with justification given in Mattick 2018). Ultimately, the exact role and theoretical nature of measurement in Marx's work and in subsequent critical theory inspired by his method is worthy of its own devoted study, drawing on contemporary complexity theory, philosophy of science and philosophy of the social sciences more generally. Here, I will simply list some of the broad contours of the attempts to connect the more fundamental concepts of value theory to their necessary forms of appearance in actual prices in the economy, the measure of which comes in the form of conventional business statistics. While these prices appear in a more unadulterated form on individual firms' balance sheets or in other direct records of production and labor costs, the process of producing macroeconomic statistics at the larger scale is heavily mediated by

various institutional actors who understand such measurement within the framework of economic folk-science (see: Morgenstern 1966).

Recent decades have seen a general revival in the use of econometric methods by self-described "Marxist Political Economists." As the contradictory designation indicates, these are often better understood as continuations of classical political economy rather than continuations of Marx's critique of political economy. They are often based on a fundamentally Ricardian misreading of Marx that echoes many aspects of what Heinrich (2012) critiques as "worldview Marxism." At their most mathematically and theoretically detailed, as in the work of Anwar Shaikh, they are essentially a form of Neo-Smithian or Neo-Ricardian political economy that calls itself Marxist and which, to make matters more confusing, explicitly contrasts itself with self-described Neo-Ricardians (Shaikh 1982, 2016). Nonetheless, the absence of the critical dimension is the starkest difference between such accounts and the work of Marx himself, indicated by their portrayal of Marx's work as a continuation of political economy rather than an attempt to critically overturn it by exposing the relationship between the core concepts of the political economic tradition and the material process of real abstraction that inheres in capitalist relations of production, naturalizing political economic as the folk-scientific ground of inquiry. This "Marxist political economy" certainly opposes mainstream economics. But instead of a critical opposition, it offers a classical one, counterposing an equally folk-scientific foundation rooted in the very tradition that Marx himself critiqued.

In some cases, this work in "Marxist political economy" has a more ambiguous theoretical basis that is, at least in part, critical in nature. This characterizes the work of Itoh reviewed above, as well as that of Guglielmo Carchedi (2012), Andrew Kliman (2007, 2011) and John Smith (2016). In other cases, many thinkers offer econometric work that they themselves

characterize as “Marxist,” and which offers extremely useful insights, but which does so without attempting to connect this empirical work systematically to deeper debates about the nature of value theory in more critical interpretations of Marx. At most, this work might make passing and eclectic reference to many such theories, but is ultimately concerned more with pragmatic application and less with exegesis of the original texts or purely theoretical inquiry into their structure. By far the most influential of such thinkers is Michael Roberts, whose popular blog is probably the most-cited source of self-described “Marxist” empirical work and the go-to resource for measurements of the profit rate. Roberts often collaborates with Carchedi, and the two have produced and edited several texts laying out the basic empirical techniques used and gathering together other scholars’ attempts to measure various national profit rates and aggregate them into a single global rate (see, for instance, Roberts 2016 and Carchedi and Roberts 2018).

In this effort, they often engage in debates with others who use such empirical techniques—for instance Shaikh and Kliman—about the proper methods of calculation of the profit rate in the sense used by Marx. At times, they address the work of more critical scholars such as Heinrich (as in Carchedi 2012) but tend to do so in a way that fails to accurately lay out key differences or which mischaracterizes the basic points of these critical inquiries. This is indicated by the fact that Roberts and Carchedi seem to endorse the conclusions of many other critical scholars, such as Paul Mattick (Jr.) and Fred Moseley, despite the fact that these latter thinkers pose their inquiry on similar terms as Heinrich—the key distinction being that they come to different conclusions about the utility of profit rate measurements. Thus, Roberts and Carchedi frequently offer extremely useful empirical material but tend to conflate these measures with Marx’s own categories in an incorrect, one-to-one fashion. Overall, their empirical approach is rooted in a pragmatic or eclectic attitude toward the complexities of value theory and

an almost complete dismissal of the historiographical questions raised by scholars such as Heinrich. In subsequent chapters, then, I will frequently make use of econometric methods drawn from Roberts, Carchedi and others who might refer to themselves as "Marxist political economists," but I do so without any illusions as to whether or not these methods represent the proper "Marxist" measurement of things like the profit rate. This is because the relationship between econometric inquiries and value theory is not so simple.

Overall, Mattick (2018) and Moseley (2018) offer much more coherent explanation of the relationship between empirical measurement and Marx's critique of political economy, driven by his scientific inquiry into the nature of value in capitalist production. Moseley and Mattick both conceive of the structure of Marx's *Capital* in essentially the same way, even while both acknowledge that only Volume I was ever printed in a final form approved by Marx and that what has come to be seen as Volume III and treated, therefore, as a more complete document is, in fact, a fundamentally incomplete draft that was written around the same time as Volume I and never returned to. In that sense, the structure identified by both Mattick and Moseley does not necessarily involve a temporal shift between "models" but instead the simultaneous development of multiple levels of abstraction (in this case, Heinrich 2013 is in agreement) that were intended to be presented somewhat sequentially. Moseley's explanation of this structure is succinct:

> the subject of [Marx's] theory is a '*single system*'—the actual capitalist economy—which is first analysed at the macro level of the total economy and is then subsequently analysed at the micro level of individual industries. (Moseley 2018, p.3)

Similarly, Mattick argues that we can understand Marx to be engaged in a process of abstract modelling designed delve down to the core laws of motion of capitalism which are not directly measurable, in the conventional sense, even while we make use of empirical measures that, by

their very definition, do not exist at or capture this level of abstraction. Ultimately, the core point is that we should not conflate the two as if they were one and the same or as if it were a matter of abstract and unmeasurable value (understood in theoretical terms) being directly "transformed" into price (i.e. empirical terms). Echoing Moseley, Mattick argues that Marx's model of capitalism, meanwhile, is actually two models: the first, in Vol I and Vol II of Capital, focuses on the total system in its abstraction and therefore speaks of the creation and circulation of "total value" through society's total labor, while the second, in the Manuscripts of 1864-65 (i.e. what would become "Volume III"), zooms in on this total system in order to see the intricacies of how this value is then distributed through competition between different firms and how this drives further technical change.

In regard to the measurement of the profit rate, Mattick explains that "Marx's law of the tendency of the rate of profit to fall does not directly refer to what is conventionally called the rate of profit in business statistics" (Mattick 2018, pp.254-255). This is because Marx's law refers exclusively to *total* capital investment, in value terms, and the related sum of surplus value produced by this investment. There is no problem of how "values" transform into "prices" in Marx, because they don't. Here, again, Moseley offers a succinct summary of the position: "there is *no* 'transformation problem' in Marx's theory" (Moseley 2018, p.4). Instead, as argued by Mattick, value "as a term of economic theory, refers to the social practice of representing labour time by money prices" (Mattick 2018, p.153). In Marx's analysis there is no "transformation" of distinct value-quantities into price-quantities, "because the two play different theoretical roles […] Value is an explanatory category, price an empirical one" (ibid). Prices are the necessary form of appearance of value. But this is not to say that they are either the same as value nor that they are "merely" an appearance without any substantial effect. It's important to remember here

that one of Marx's key theoretical contributions is that even seemingly superficial appearances that are, in the last instance, dependent on more fundamental dynamics of the system are also, in fact, "real" and exert a reciprocal influence on these dynamics.

Thus, while Mattick's conclusion might seem to displace empirical categories out of theory entirely, it actually just points out the fact that they bear a different and essentially unproblematic relation to it:

> in application to real-world situations, Marx's theoretical model provides a 'pattern of explanation' than [sic] can be instantiated by historically specific constellations of economic phenomena, in which the value relations fundamental to Marx's 'law of motion' […] can be identified conceptually, though not in terms of quantitative data. (ibid, p.23)

This does not mean that this theoretical model has no relation to empirical data, however. Despite the different terms, it predicts concrete trends that should eventually be made evident in all sorts of data, not to mention more qualitative accounts. Because, in the end, Marx does make concrete predictions, and these can be tested by comparison to the real world:

> The goal of Marx's theory, the 'law of motion' of capitalism, is a causal explanation of long-term trends in the development of capitalist society […] Marx aims to demonstrate that the normal operation of the system, over a long enough time period, will lead to increasing mechanisation of production, concentration and centralization of capital ownership, polarization of social class interests, and recurrent economic crisis, all of which he believed would open the way to the creation of communism. (ibid)

Returning to the rate of profit specifically, Mattick makes clear the distinction between the impossible calculation of a precisely-Marxist (and therefore "unobservable") rate of profit and the use of conventional business statistics to measure profitability:

> […] since the mass of surplus value available constitutes an absolute limit to its appropriation by individual capitalists as profit, the tendency to the decline of this (unobservable) rate is clearly related, given the centrality of business profits to the society, to a tendency for conventional profitability to decline, even though this tendency may be offset at a given time […] In this way, the concretization of the abstract model makes possible the utilization of empirical data (whatever their underlying limitations) […] (ibid, p.255)

This liberates such empirical measures from the burden of exhaustively and precisely matching up to the much more abstract schema used by Marx. They simply are not the same things, even if empirical measures can be used as elements in a more expansive sort of proof more in tune with the scope of the theory itself.

Without this burden, it is not necessary to engage in the many lengthy debates that have taken place about the most accurately "Marxist" way to measure the profit rate in individual countries, or how to combine such measurements into an approximation of a global rate of profit. This is because it simply isn't possible to do so—as Mattick says, value itself is uncountable even while it is a causal force in the world, and it necessarily appears in the form of prices that, by their very nature, diverge from value itself in a theoretical sense. But figures derived from these prices can be measured, and there is reason to believe that the tendencies we observe in them should follow certain movements in the system as a whole. More importantly, they can still give us insight into those central predictions made by Marx, and the notion of profitability is

central to understanding recurrent economic crises and the more general tendency toward long-run stagnation and breakdown in the system as a whole. But the vast majority of the time that Marx refers to the rate of profit he is referring to a single profit rate as a turbulent average structured by competition and which exists at the scale of total social capital only. He periodically refers to the profit rates of individual firms but makes clear that these fractional rates of return are ultimately determined by competition over shares of the total social surplus value, since some firms are able to make above the prevailing average rate of profit and others are forced to make less. A similar logic prevails in sectors of industry, and in different national fractions of industry. The potential national divergence of rates of profit (more accurately: divergent abilities to capture value within the total social capital) is an important component in understanding the emergence of trade wars and, in more extreme cases, military conflict. Not only does the rate of profit tend to fall over time, but the distribution of surplus value across all the world's industrial firms—divided according to their many sectors, countries and localities—is itself a turbulent process, and is the driver of territorial transformation under capitalism as new, more competitive industrial complexes rise and old ones fall to ruin. Similarly, the general expansion of unproductive investment identified by Smith and Mattick exerts a geographically uneven downward pressure on the production of surplus value.

It therefore makes no sense to try and measure "the rate of profit" in some pure "Marxist" sense for any single country.[38] At the same time, it makes perfect sense to attempt to measure

[38] This is not exactly to claim, as Mattick 2018 seems to, that such a task would be impossible. Since Marx largely speaks of value at the level of the total social capital and treats value itself and the tendencies derived from it in a somewhat probabilistic fashion, it would conceivably be possible to construct probabilistic and dynamic mathematical models that might be capable of closer and closer approximations of the "total social capital" and its associated value terms *all of which only apply at the global/systemic level*, at least insofar as Marx uses them. The main limit here is not actually mathematical, but instead has to do with whether or not there currently exists (likely not) sufficient data structured in an adequate fashion to feed into such a model, whether such data could conceivably be constructed (and if it ever would be), whether it can be derived from conventional statistics

what Mattick calls "conventional profitability" as well as conventional theories of technical change—as they are commonly used in the field of industrial economics/industrial organization (see, for example: Cohen and Levin 1989)—because we can, a) assume that these tendencies bear some relation to the global profit rate and overall mechanization, likely moving generally in tune over long periods of time even if these latter phenomena remain *practically* (though maybe not ultimately) unmeasurable and b) argue that the conventional profitability and level of mechanization of national industry is in and of itself an informative factor with regard to economic crisis. Thus, measuring the rate of profit in China at multiple levels of sectoral and geographic detail will, in fact, be informative for a general inquiry into global crisis tendencies and their influence on the changing geography of production. In particular, it is perfectly reasonable to use conventional measures of unit labor cost and industrial profitability to confirm the hypothesis that Chinese production was, comparatively, more profitable than other potential sites for industrial investment at the turn of the millennium—the reason that China was such a major locus for global industrial relocation in those years, outcompeting neighboring states in Southeast Asia—and that this profitability has in the last decade peaked and begun to stagnate.

Trends in profitability, undergirded by technical change (in the more material sense used by Marx, not the vague, residual sense gestured toward by TFP), can then be seen as the driving force behind both factory relocations (to cheaper provinces within China, and to other countries) as well as the changing composition of investment and employment within the domestic economy. This is at least what we would expect, because it seems to tie together several trends already evident: as industrial returns decreased, more money was funneled first into massive

designed for altogether different purposes, and ultimately whether there are other, close enough proxies to construct parallel models showing something similar but not quite identical.

stimulus-led construction bubbles, and now also into services. Throughout, speculation and financialization grew, creating stock market bubbles, a ballooning shadow banking sector, and an intricate infrastructure to facilitate grey-market capital flight into “secure” assets (like real estate) overseas (for an overview of these trends, see: Jiang 2018). Only by exploring how deeper problems of value accumulation appear in empirical data can we understand the relationship between the drive toward crisis, understood at the most abstract level, and the countervailing measures that then restructure the real geographies of production and lead to the construction of new territorial industrial complexes.

# Chapter 3

# The Profitability of Chinese Industry

**Introduction**

Before moving to profitability, it will be helpful to first provide a general overview of the data that will be used. As new variables are introduced, their exact source will be explained in more detail alongside any potential measurement issues. The majority of the raw data comes from China’s National Bureau of Statistics (NBS) and, specifically, the National Industrial Census.

Alternate measures for GDP, capital stock, total factor productivity, total labor cost, labor productivity and unit labor cost come from either the Penn World Table (version 10) or the Conference Board's International Labor Comparisons series, both of which utilize the deflated GDP measure of Maddison and Wu (see: Maddison 2006, Wu 2011 and Maddison and Wu 2008) rather than the one given by the NBS ("National Accounts…" and Wu 2014). Since the focus here will be on profitability, the best approach will be to address each of the main variables of concern in measuring the profit rate first. After this, the section will move on to describe the source of secondary variables that will be used elsewhere.

Conventional econometrics often measure various rates of return (ROR) in order to approximate profitability, rather than the "rate of profit" (ROP) as such—which is associated with the now-heterodox tradition of political economy. Mathematically, the distinction is often inconsequential, since rate of return, intended to capture average return on investment in general, is usually calculated more literally as "rate of return on capital," "rate of return on assets" or as some variation on this theme, measuring total firm profits (in an economy, sector, etc.) divided by total firm capital. Often, this is paired with measures of "return on labor" (often conceptually similar to labor productivity even if calculated differently), as well as some calculation of TFP. For Chinese industry, a standard method of capturing profitability and comparing profitability according to ownership structure, firm size or location, as recorded by the National Bureau of Statistics, is to divide the total profits of all industrial enterprises by the total assets of all industrial enterprises (as in Brandt et. al. 2020). Doing so comes close to a classical "rate of profit" measure but is not exactly the same. It also completely ignores factors of turnover time by arbitrarily treating the data in annual units—at the macro-level, this is almost always a necessary sacrifice and it's one wherein the effect on the overall trend is minimal *except for* extremely

slow-turnover investments, as in the case of large state-funded infrastructural projects concentrated in particular regions, which will be addressed in the next chapter. It will be helpful to begin with an introduction to these various measures before moving on to the question of what data to draw on for the different components of the equation.

Ideally, the goal here is to obtain both a broad measure of the national profit rate and a measure more specific to industrial enterprises. The national measure will gauge the trends in all sectors for China as a whole, while a specifically industrial measure will focus in on the productive core of the economy without the potentially divergent effects of high profitability on paper among, say, real estate firms, caused by rampant speculation and the bubble in asset prices. For self-described Marxists who do attempt to measure what they think of as a Marxian "profit rate" at the national level, the practice is generally to use the classic profit rate equation from political economy, as used by Marx, but to substitute measures from conventional business statistics for each of its value-denominated elements. The traditional equation is as follows:

$$\frac{S}{C+V}$$

Where $S =$ Surplus Value, $C =$ Constant Capital, and $V =$ Variable Capital

This equation differs from the standard ROR used in business statistics, described above. At the most granular level, the measure of ROR usually takes the current value of an investment, subtracts from it its initial value, and divides the resulting sum by this initial value. This is a good measure for individual investors, since it derives a precise sense of trends directly from the balance sheet of individual firms. But this granular ROR only captures one limited dimension of profitability in a given year and it fails to really portray the magnitude of profits relative to total investment accrued in things like plant and equipment. It therefore gives a very poor idea of how

corporate profitability as such fares over time. When economists attempt to compute larger economy-wide ROR measures it's common to scale this up by using some larger measure of the capital share in total output, at which point ROR increasingly approximates ROP as calculated by political economists.

Variations on the ROP equation derived from Marx—but, again, fundamentally different from what ROP is in Marx's model since they're only calculated on fragments of total value—are more slightly appropriate, since they give a sense of the magnitude of total output to total costs, and the equation can be decomposed into both the rate of exploitation (i.e. S/V, also called the rate of surplus value), and the organic composition of capital (i.e. C/V). The rate of exploitation as used by Marx measures the overall social rate of surplus value extraction. Decomposed, the equation above looks like this:

$$\frac{\frac{S}{V}}{\frac{C}{V}+1}$$

If conventional statistics are substituted here, the meaning of these components changes somewhat. Using output or profit as *S* and the overall wage bill for a given country or industry as *V*, we just get a comparison of their relative magnitudes over time. In other words, it gives price-units of output per price-unit of labor input. This is significant, however, because if the total profits or output are increasing but total wage costs are not, it indicates either more intensive work for diminished pay (relative to output) or a change in the production process itself, whereby firms are opting for more labor-intensive types of production enabled by some sort of decomposition in the wage relationship of the sort John Smith gestures toward. These changes in the production process, insofar as they are derived from technical sources (i.e. new machines,

new infrastructure, etc.), should be picked up in the measurement of the organic composition of capital. Using conventional statistics, C/V can be converted into some measure of materials costs + fixed capital divided by the wage bill. Even though this fundamentally just shows the relative magnitude of the total cost of assets in a given year vs. the wage bill in that same year, since plant and equipment compose a relatively large share of industrial firms' assets we should expect this value to increase as production becomes more capital-intensive.

Again, issues of turnover time *have to be* completely abstracted from here, since turnover times differ between branches of production, between regions and even between individual firms. These are things that would, however, ideally be considered if focusing on a single production branch or a small subset of individual firms. They are absolutely essential to understanding, for example, the intricate financial dependencies that exist for many Chinese producers, where any break in the continual circuit of financing, production, sale and re-financing can doom a company. Similarly, determining some sort of general turnover time for the entire social circuit of capital averaged across all industries in all locations in order to estimate something like the "velocity" of the total social capital would be important for any mathematical model attempting to replicate the most macro-scale dynamics of the system. At the scale ("macro" in conventional economics, but "meso" compared to the value terms of Marx) explored here, however, these are largely either non-issues or marginal concerns. This is evident in the nature of what is being measured by the conventional (essentially always annual or quarterly) statistics input into the equation. Turnover times more rapid than a year (this is the most common case) cannot possibly effect any divergence from the trend because of the nature of the measurement itself. The only instance in which turnover time can influence the annualized trend is in cases where turnover is extremely slow, since large upfront investments in things like

infrastructure will depress profitability by raising C in a given year (or a number of years) without any affiliated returns in S in those same years. In other cases, even if we imagine a firm with an insanely rapid production process allowing for an extreme velocity of turnovers, each cycle of input and each cycle of output is accounted for in the annual average statistics. Many micro-rates will average into a macro-rate. Even if it were possible or desirable to include intricate data on turnover times, this would still run against the methodology here, however, since an essential part of the process is the comparison between pre-existing measurements of the rate of profit and rate of return in China, all of which are conducted in this annualized fashion.

The turnover issue is often a matter of concern when attempting to replicate one-for-one the logic laid out in *Capital* (namely, Volume II). But, again, none of the measures I'm using here are that dissimilar from conventional macroeconomic understanding of profitability, especially when paired with measures of productivity. I *am not* attempting to model the total social capital, which is what Marx is speaking of when referring to turnover time in Volume II. This speaks to the difficulty of linking a "Marxist" conception of the rate of profit to any one specific econometric method over others. But it also points to the fact that the mere use of conventional econometrics doesn't—at least not in any fundamental way—threaten the critical dimension of the social-scientific project instigated by Marx, who himself made wide-ranging use of similar measures drawn from the political-economic orthodoxy. The point is simply to use such measures critically. This means that the process of selecting stand-ins for the individual elements in the ROP equation must be attentive to the precise differences between the sense of each variable in Marx's usage and the character of the stand-in variable drawn from conventional data. In this equation, S represents total social surplus value, and the business statistics stand-in is usually some measure of the value of total output or simply net profit for the year. V

represents variable capital, which means money spent on workers, so the stand-in is usually the sum of wages or some combination of wages and expenditure on benefits to capture a more complete measure of the total wage bill. For C, a measure of capital stock is usually used (for an overview of which stand-ins are usually selected, see: Kliman 2011, Roberts 2016 and 2019b, Axelsson 2016). There is much debate on exactly what kind of measure of capital stock is best (how to calculate depreciation, whether to use historical v. current costs, etc.), but there are even more severe problems in the Chinese case. The most salient is the fact that there exists no official measure of capital stock. In conventional national accounts, capital stock is usually measured as the sum of the nominal values of all the fixed assets in use in the economy. Since measuring profitability requires understanding the relative magnitude of capital—especially as tied up in fixed assets—in a given year as compared to the magnitude of yearly returns, the complete absence of any official capital stock measure creates a major problem.

# Section 1 – The Underlying Data

**Variables**

Faced with these problems, the most robust solution to measuring profitability across Chinese industry is to iterate the procedure using several different sets of variables capable of standing in for measures of S, C and V. This can be done at the national level using a few different data sources. But problems are posed by national measurements of output (like GDP) and capital stock or total assets, since these can often include things like residential housing stock, speculative investment and other forms of non-productive activity that move in the opposite direction as industrial profitability, since poor returns on investment in productive industries

encourage funds to flow to more speculative ventures. This means that countervailing tendencies to the fall of the profit rate are exaggerated in such measurements. At the same time, the recording of things like housing or construction bubbles in national statistics can be informative in its own way, since bubbles do provide an actual buoying effect on the rest of the national economy, helping to stave off crisis even while threatening to deepen that crisis when it strikes. So such measures are not useless, they must simply be supplemented with others estimating the performance of industry specifically. Nationwide measures of profitability derived from total economic output will need to be accompanied by similar measures relating to industrial enterprises specifically.

A few main sources will be used. First is the Penn World Table (PWT). This is a widely used aggregate database designed to be able to compare different countries' macroeconomic indicators by converting all national currency values to a standardized international US dollar value using purchasing power parities (PPP, i.e. how many goods could be purchased with a given amount of currency). Previous versions of the PWT have been a major source for political economists attempting to measure the profit rate, and an Extended Penn World Table (EPWT) was composed by Adalmir Marquetti and Duncan Foley (2011), which includes a gross profit rate calculated for all available countries in an old version of the database. For other calculations, either the PWT 9.1 or 10.0 will be used, both of which draw from the work of Maddison and Wu (2008) to construct a deflated measure of GDP. These values will be recorded in standardized international USD. I will draw from the raw numbers they offer, rather than the PPP-converted series, since international comparability is not the goal.

Second is the Conference Board's International Labor Comparisons (ILC) database. The Conference Board utilizes a similarly deflated GDP series also drawn from the work of

Maddison and Wu (2008). But, unlike the PWT, they also record more detailed estimates of productivity and unit labor costs. Their estimates are similarly meant for international comparisons and, in this instance, they are particularly useful since it is their series on output, productivity and employment that is used by Benanav (2020). Drawing the same figures from the ILC for China allows for point-by-point comparisons with the trends identified by Benanav, keeping in mind that output growth may be somewhat muted given the deflation. It's also important to note that the ILC database was formerly the responsibility of the Bureau of Labor Statistics but, under pressure from budget cuts, it was transferred to the Conference Board in 2013 despite the Conference Board being an industry-funded non-profit rather than a federal agency. The ILC is still produced using the same methodology and conceptual framework as it was under the BLS, preserving its capacity for longitudinal comparison over time.

Finally, the bulk of the data that will go into my own measures is drawn from the Chinese National Bureau of Statistics (NBS), which are recorded in units of 100 million RMB. Rather than beginning with an overview of the entire bureau, more detail will be given on the general methodology used by the NBS for each variable used. Since most relate to industry, their standards are set out in the National Industrial Census and they are sensitive to definitional changes in which enterprises are of sufficient size to be recorded. Key changes in this definition occurred in 1998, 2011 and 2017, and these will be reviewed below. In addition to these main sources, I will also reproduce, for comparison, all other measurements of the rate of profit for the Chinese economy for which public data is available. This includes basically all recent measures in English and Chinese-language scholarship except for those of Li Minqi, who has used his own method for estimating capital stock and produced his final rate of profit measurements in chart form but without making public the underlying data (reproduced in various iterations in Li 2009,

2015 and 2020). Similarly, a recent paper (Marquetti et. al. 2018) is mentioned but excluded from comparison since it is rooted in fundamentally different base data (firm-level observations from the Orbis database, which I will use below for comparative purposes in the Tanzanian case). From these other sources, I will also derive a more thorough measure of the wage bill's share in total output, producing a composite measure of the wage share to use throughout. To start, however, I will review each element of the ROP and which variables will be used as replacements.

### *S – Output, Net Profit, Value Added*

The stand-ins for S are the most varied. This is because no category in business statistics really comes close to the basic idea of surplus value in Marx's sense, and, regardless, total surplus value is not something that can practically be measured (at least at present) in this way. It is only understandable at its properly systemwide, social scale, with the money measurement of national output, individual industries' average net profits, or an enterprise's value added all hinting at different dimensions of the movement of total surplus value but none actually capturing the same meaning. Nonetheless, we'd expect such measures to roughly follow the same trends over the long term, although they should also exhibit some important local divergences from the global rate. But, again, this is simply a matter of understanding such divergences and fitting them into the larger picture of global profitability. The main divide here will be between measures of total output before its division into labor and capital costs (i.e. more universal output measures like GDP or Value-Added—these are the most conventionally used measures of output in the literature reviewed above and in the practical attempts to measure profitability reviewed below) and narrower measures of profit, representing only remainder after this division.

*Output*

The first stand-in will be variations on the measure of national output, basically the GDP measures from the PWT, ILC and NBS. Traditionally, GDP measures try to capture the market-conventional "value" (i.e. price) of all the finished goods and services produced within a country's borders, whereas GNP (Gross National Product) adds to this income that accrues to that country's citizens overseas as well. The raw price data used for GDP calculation is gathered in different ways in different countries, but there are three common methods to conduct the calculation itself. The first, used in the US, is the "expenditure" approach, which calculates GDP as: household consumption + investment + government spending + net exports. The second is the "income" approach, based on the notion of "factors of production" that exists in conventional economics. This method calculates GDP as: wages paid to workers + profits earned by business + taxes on production + the consumption of fixed capital.

Finally, there is the "production" approach, the main annual measure used in China, which calculates GDP as the sum of real value added across all the sectors of the economy. This tends to prioritize the input of the "real" productive base, against the expenditure and factors of production approaches, where consumption have equal priority. Here, the measure of real value added is particularly important. This variable is, at root, a way of calculating the remainder obtained after subtracting the price of intermediate inputs from the price of total output. It includes consumption indirectly: not at the point of purchase but instead through the total wage bill. For each sector, real value added includes the total outlay for labor, regardless of whether those wages are spent or saved. This form of measurement will inflate GDP when the savings rate is higher, since it does not only count consumption as expenditure. This stands in contrast to

the approach of the US and numerous other countries, where personal consumption expenditures as recorded by the BEA compose as much as two thirds of total GDP (Weinstock 2020). The US approach means that increasing consumer credit will tend to inflate GDP. Essentially, both the Chinese and the American GDP are inflated measures, but each inflates the number differently because of the divergence in their rate of savings and consumer credit. In China, real value added is measured separately for essentially all industrial sectors and these measurements are summed to produce the national GDP measure. This method is used to produce both the quarterly GDP estimates, as well as the official annual measure. Occasionally, however, China also produces a GDP figure calculated according to the expenditure approach. The essential difference between these two methods, aside from the issue of consumer credit, is that the expenditure approach calculates the input costs and projects forward, while the production approach calculates the final costs and projects backwards (both subtracting intermediate expenses). The details of this measurement are covered in Orlik 2011 (Chapter 2).

One invariable objection to the use of such figures is the issue of inflated figures and questions over the general reliability of NBS data, as put forward most systematically by Maddison and Wu (2008), who provide the standard model for deflating Chinese GDP (a review of this literature can be found in Plekhanov 2017). Problems of output exaggeration have historically originated largely from local governments, where there is a strong incentive to inflate the figures delivered to the central statistics agency (these problems are documented in Orlik 2011). At lower levels of reporting, this can create major distortions, and recent attempts to reign in over-reporting have led to apparent "downturns" in production (as in Jilin and Inner Mongolia) which are, in reality, just the deflation of the statistical bubble. Similarly, a revision in measurement in 2017 cleared out additional double-counts and mis-categorizations that had been

present in previous years at the local level. Since local reporting of figures is a component in the calculation of the national output, this leads many researchers to suggest a standard deflation of the raw GDP figure, usually by 1-2% for most years (as in Maddison and Wu 2008). In addition, certain years that saw extreme downturns (particularly those farther in the past, such as in 1998) have figures that are fundamentally questionable, not reflecting the severity of the downturn on the ground. That said, there have been substantial efforts in the past two decades to correct for these problems at the national level, and the NBS has for years modified the local output measures it receives with weights derived from harder-to-fake variables like freight traffic, electricity consumption and night lights satellite imagery (Orlik 2011, Chapter 2).

The exaggeration of the GDP measure is itself therefore often exaggerated in the press. Though it is undoubtedly inflated to a certain extent, the more basic inflation behind GDP growth in China has to do with the high share of investment in GDP—a topic that will be discussed below. Meanwhile, it's worth emphasizing the fact that GDP is fundamentally a political construct which, in many places, has only a distant connection to actual economic wellbeing. This is not just an issue in China, but also in the US and essentially everywhere else. Among the most obvious examples is the fact that GDP, in its very structure, actually obscures the imperial geography of production. John Smith (2016) illustrates how the measure comes to embody imperial power itself, since products produced for a US-headquartered company, even if they are produced in China, will have the vast bulk of the profit derived from them counted in the US and thereby added to US GDP. This is similar to the way that Apple can be listed as a "manufacturing" company despite owning no factories. Thus, American GDP essentially measures value capture rather than actual productive power. This is further exacerbated by the inflating effects of credit, especially with regards to consumption. As will be shown below, this

US inflation of GDP at the consumer end, facilitated by credit, is matched by a Chinese inflation at the investment end, also facilitated by credit. Ultimately, however, anyone claiming that Chinese GDP is "falsely inflated" has to face the fact that US GDP is inflated as well, albeit in a different fashion.

But for the purposes here, the more important point is that even extreme inflation simply doesn't matter, since the point of interest is not the exact price-sum of output or the comparability of Chinese GDP to other countries' GDP measures. Instead, the point of focus is trends over time and relative magnitudes of different variables from the same source. Wage data from the NBS will be calculated as a share of output, so it will include the same inflation. Capital stock, although a separate measure (see below), should be equivalently inflated at the local level. Fixed capital formation, the basis of many capital stock reconstructions, also tends to be exaggerated by local officials. What all of this means is simply that the trends should be clear regardless of the inflation since those arguing that this inflation exists in output do not argue that investment or fixed capital remain untouched. Instead, the argument is that the statistical inflation occurs across the board for these variables. So long as capital stock is not being inflated to an extent wildly different than output, then, the figures will still capture the relative weights of each component in the equation accurately. The only necessary wariness arises when looking at absolute counts and change-over-time measures derived from them. In these cases, it is simply a matter of knowing when the key statistical reforms were implemented, dividing the data into three periods guided by different conventions in data gathering that are mildly different in quality from one another.

The first output measure I will use is the Real GDP series from the PWT version 10.0, standardized into 2017 international USD for easy comparison. Originally derived from the NBS,

this GDP measure has not been corrected in other ways, and it is useful because it can be compared without modification to the national capital stock measure, also found in the PWT. A similar, but deflated, GDP measure is drawn from the Conference Board ILC database, using the methodology laid out by Maddison and Wu (2008)—this same deflation is applied to the PWT PPP GDP series, not used here. Another measure will be drawn from Herd (2020) for comparison to his capital stock series in the same database. The final GDP variable I will use is simply the same real GDP figure used by the PWT, but as recorded directly by the NBS in units of 100 million RMB. This measure is used independently because it will compare well with the two measures of capital stock, from Chinese academic sources, which are also denominated in units of 100 million RMB. A second output measure will be the Value Added of Industry, as recorded by the NBS. This is a measure that captures the value-added of all industrial enterprises "above a designated size" (see below) and is available sector-by-sector. It is essentially a disaggregation of the overall GDP measure calculated according to the production approach. It is, however, distinct from the NBS measurement of the total share of "secondary industry" in GDP, since secondary industry is a slightly broader designation, including construction and other affiliated activities (whereas "industrial enterprises" and "construction enterprises" are recorded separately elsewhere).

*Net Profit*

With regard to the measurement of the rate of profit, the major issue with GDP measures is not potential inflation, but instead the way that the value-added calculation includes all sectors of the economy. Though it seems as if this would be more comprehensive (and it is), the problem is that sectors being buoyed by extreme speculation (real estate, finance, etc.) are included

alongside core productive sectors like manufacturing. More importantly, we'd expect somewhat divergent trends over time, as was observable in the Japanese case as manufacturing profitability declined and speculative activity increased (Itoh 1990, Brenner 2003). Though it makes little sense to apply a clear-cut definition of which individual firms are productive of value versus which are non-productive it is nonetheless apparent that FIRE (finance, insurance and real estate) industries, for instance, will include a greater sum of non-productive activities that act as a drag on underlying growth and will tend to grow in a speculative fashion precisely when profitability in the productive sector declines. To approximate trends in the productive sector alone, it is common to calculate separate measures of profitability just for manufacturing. This is the method used by Brenner (2003, 2005). Others disaggregate the rate of profit by detailed industrial sector or simply split it into non-financial and financial measures (Kliman 2011, Moseley 1991, Itoh 1990, Li 2020).

In Chinese national statistics, measures are provided for net profits of industrial "enterprises above designated size" (规模以上企业). There are two key definitional aspects of this category. The first is the definition of "industry." This includes: mining and logging, agro-processing, manufacturing and the repair of industrial products. It does not include construction, transport, utilities or any direct agricultural production. It *is not* the same as "secondary industry," which is recorded separately. Secondary industry includes mining and quarrying, manufacturing, utilities, and construction. Thus, the "industry" category includes one feature (logging) that's otherwise recorded as primary industry and excludes several others that are included in secondary industry. This is not an issue here, though the distinction must be kept in mind. If anything, it ensures that the category is a bit closer to the core of the "productive" economy than if we simply measured "non-financial" businesses or manufacturing alone.

The second definitional question relates to the "designated size." There have been four significant changes in statistical definition that have affected the universe of industrial enterprises, two of which have been redefinitions of the "designated size" cutoff. The first important change was the 1993 shift between the Material Product System of accounting inherited from the planned economy to the System of National Accounts, designed to measure prices in value-added terms rather than as assigned to sheer physical quantity of output (Holz and Lin 2001, p.33). The second was in 1998, which saw both a reorganization of registration types to include more detailed data on private firms and the introduction of the "designated size" to exclude firms that fell under a certain threshold. Between 1998 and 2011, any industrial enterprise making more than 5 million RMB from its annual main business revenue would be included (ibid, p. 33, 39-48). After 2011, the bar was pushed up to 20 million RMB (Chen et al. 2019, p.6). This was the third major change and it is the most important here because it lies roughly near the midpoint of my own data. At times, this means that the series must be intentionally broken between 2010 and 2011. Then, in 2017, the most recent redefinition took place, largely geared toward auditing the system, cleaning out instances of double measurement and addressing potential issues of local inflation and overreporting. In addition, changes in taxation encouraged industrial firms to separate their non-industrial service activity into separate businesses, causing an apparent dip in some measures (this is all recorded in the metadata for the NBS online database).

While this series of statistical redefinitions will affect the raw output numbers, it should have minimal effect on the trends I am reviewing here because the relative proportions will still be comparable in the same way in each year since they are all measures derived from the population of enterprises above designated size—i.e. if the redefinition causes total output to

drop in 2011 compared to 2010, the wage share derived from this output will also drop, as will the various measures we can use for fixed capital stock for these same enterprises. When it comes to disaggregating the total national measures, however, there are a few important consequences of the "designated size" cutoff. As documented in Lardy (2014, pp.70-73), this cutoff effectively excludes all "family" or "individual" businesses (个体户). But, because these micro-firms play an important role in the private sector overall, the result is that the universe of firms "above a designated size" will tend to underestimate the relative contribution of private firms against those in various ownership types dominated by state investment. The effect is more pronounced in the tertiary sector, where most of these small businesses are concentrated. In industry, the impact is less substantial on average but it is somewhat significant in the earliest years. In 2003, for instance, firms above the designated size that were officially registered as private contributed about one-eighth of overall industrial output. In the same year, if firms below the designated size are added to this, the overall contribution of private firms rises to two-fifths (Lardy 2014, p.71). Within industry, this contribution has, on average, been diminishing. In terms of all the variables used here, the effect will be minimal since firms above the designated size compose the vast majority of total assets, profits, output, etc.

Similarly, older data is subject to increasingly alien standards of what is being measured. In particular, any measures from prior to 1993 still largely utilized methodological standards based around the measurement of sheer output quantities, rather than market prices, first established in the planned economy. In what follows, I will first use series that run from 1990 to recent years, measured for either the entire national economy or all industrial enterprises above the designated size. The only years subject to issues raised by inherited standards of measurement would be the first three in the series. These years are not particularly important to

the overall argument. But there is also no reason to believe that the slight incompatibility of standards causes any jarring shifts in the data. Instead, the entire decade of the 1990s should be seen as a transitional period in which inherited standards of accounting and firm administration are being shifted onto a market basis and increasingly recorded using conventional statistical methods used elsewhere.

Finally, one key difference with the output measures listed above is that the net profits measure drawn from the universe of all industrial enterprises above a designated size will produce a much lower rate of profit in absolute terms than the measure derived from GDP or the total value added of industry. Both GDP and value added are measures of the total output after the subtraction of intermediate inputs but prior to this output being divided into wages, capital costs, debt repayments, taxes, profits, etc. Net profits, however, only capture this profit portion of output. They are not full output measures and are therefore qualitatively different than the others. While ROP measurements usually use variants of total output in their numerator (as stand-ins for S), it is common for conventional ROR measures to utilize total profits or some similar variable. Similarly, this means that ROP measures must include both capital and labor costs in the denominator, while ROR often only includes some measure of assets since labor costs have already been explicitly subtracted from profits. Most of the measures that use net profits in their numerator below are something of a hybrid between ROP and ROR, utilizing not only capital but also labor costs (wage share multiplied by value-added of industry) in their denominator.[39] Overall, using net profits will be informative and remains comparable to the other

[39] The exception are the measures in the following chapter, where net profits is divided by total assets to produce something closer to an ROR figure. This is done more for scalability and simplicity, with comparisons between the two variants showing very minimal effect on the overall trend.

measures, even if it gives a slightly lower absolute value. That said, it is important to keep in mind that two qualitatively (albeit slightly) different things are being measured here.

### *C – Capital Stock, Total Assets, Fixed Assets, Non-Current Assets*

Constant Capital, in Marx's sense, is also difficult to pin down in conventional statistics, since it includes investments in fixed capital, raw materials, intermediate inputs, various other operating and incidental expenses, and even costs associated with inventories. The most common ways that conventional measures of profitability approximate the same idea is through the use of capital stock data or various asset measures, none of which match up perfectly with the concept. That said, any of these options tends to do a good enough job, in practice, and (as will be seen below) the major effect of choosing one over another is on the absolute value of the rate, not its trend.

#### *Capital Stock*

As discussed above, there is no official or widely accepted measure of capital stock for the entire Chinese economy. Many have attempted to solve the issue of absent capital stock with their own measures, however. The most common method has been to use the National Bureau of Statistics' (NBS) measurement of "accumulation of fixed assets" to build a perpetual inventory series—basically, since the new assets being added each year are recorded, you can take these, subtract some measure of devaluation or enterprise closure, and then add the remainder in a compounding fashion every year. This will build a capital stock measure bit by bit, but it does depend on having some sort of starting measure for at least one year (often the figures are drawn from some initial data gathered in the 1950s, over which there is much debate). A more complicated

variation on this is the method used by Li Minqi, who is probably the best-known English-language scholar offering repeated measurements of the profit rate in China (Li 2009, 2016 and 2020). In the Chinese-language literature, the capital stock and depreciation series of Shan 2008 and Wang et. al. 2016 are among the most common sources used in profit rate measures. An overview of early attempts to solve the problem of missing capital stock can be found in Wu 2008, and a more updated and expansive review of all published methods in Chinese and English is available in Herd 2020.

Herd points out that, aside from the question of what year and dataset to use as a baseline or what depreciation series to use, one of the biggest problems with the existing literature on capital stock is its failure to disaggregate the measure by sector from the beginning of the series. Herd offers his own series on the capital stock, which has since become the series officially used by the World Bank (the organization that commissioned Herd's original report)—it was computed using a variation on the perpetual inventory method, with reference to all the previous examples of this method in both the English- and Chinese-language literature. In Herd's series, capital stock is disaggregated into four main sectors: Housing, Infrastructure, Government and Business. Infrastructure and Government are then frequently combined in his charts, given their significant overlaps. This makes a few important points evident. First is the fact that the rate of growth of capital stock began to decline overall after the year 2010. Second is that this decline was not evenly distributed according to sector. Capital stock of business peaked in growth in 2010 and then declined rapidly. Housing peaked a year later and then declined a bit more slowly. Infrastructure and Government, however, have essentially plateaued in the same years, recovering from an initial dip after 2010 to lead all growth in capital stock by a significant margin. According to Herd's figures, the Infrastructure and Government capital stock grew

roughly fifteen to sixteen percent YoY for all the years between 2014 and 2016, compared to a mere six to seven percent for capital stock in the Business sector (Herd 2020, p.16, Figure 7). Despite slowed growth, the capital-output ratio continued to grow in the Business sector as well as in the Infrastructure & Government sectors, plateauing slightly in 2015-2016 (ibid, p.18 Figure 10). The same ratio was essentially stagnant for Housing (ibid, p.22, Figure 15).

The PWT also offers a capital stock measure calculated according to a similar perpetual inventory system, but with the exact process left somewhat unclear and without the same attention to precedent given by Herd. In some cases the PWT records nine asset categories, but for most countries (and for the construction of the capital stock series), four assets are used:

> structures (including residential and non-residential), machinery (including computers, communication equipment and other machinery), transport equipment and other assets (including software, other intellectual property products, and cultivated assets). ("User Guide…", p.3)

The main capital stock measure for the whole economy that will be used here is the one from the PWT, since it is comparable to the other figures from the PWT. Herd's series will be used periodically for comparison, however, and the measures of Shan 2008 and Wang et. al. 2016 will be used in unison with the NBS GDP series. The major problem with these latter two sources is that many perpetual inventory estimates seem to systematically produce a capital stock measure that is smaller than it should be. The market value (really, price) of most countries' capital stock in a given year, even after deflation is taken into account, is higher than their GDP in that same year. This makes sense, given that capital stock is essentially an artifact of past years' GDP growth, one component of which was fixed investment. Throughout other countries' national statistics, as evidenced in the PWT, the capital stock measure almost always sits higher than the

GDP measure. This is true of the PWT capital stock measure for China and in Herd's capital stock series. It is unclear, however, why this is not the case in the two most widely cited Chinese-language papers that calculate capital stock. That said, there is no need to try to "correct" these measures, because there is no reason to believe that this method seriously disrupts the *trend* in capital stock growth, which is compared to the *trend* in GDP. A mysteriously small capital stock measure will produce the same trend in profit rate, but at a much higher absolute value (which means that it shouldn't be used for apples-to-apples comparison with other rates on the same y-axis, even while the trends, severed of their anchoring to the y axis, can be compared).

*Total Assets, Fixed Assets and Non-Current Assets*

That said, alternate measures do exist within Chinese national statistics. While the NBS does not record capital stock directly, it does record various measures of the assets of industrial "enterprises above designated size." This allows for the formulation of industry specific stand-ins for constant capital, which, for Marx, is the sum spent on plant, equipment and inputs in the production process—essentially all costs other than those expended on labor or various sorts of non-productive rents. The conventional notion of a business "asset" is essentially the same, though it is subdivided into several categories. The first, tangible versus intangible, captures the difference between actual goods and materials versus ownership of intellectual property like copyrights. Here, I will only be using measures of tangible assets. This is justified by the comparison of the relative share of intangible assets in total capital stock in Herd 2020 (p. 10, Figure 2).

But tangible assets can be further subdivided into current and fixed assets, and a third slightly more expansive measure of non-current assets (including fixed) can be produced by subtracting current assets from total assets. *Current Assets* are defined as any asset that is assumed to be either sold, consumed or used up in the normal production process within the year of measurement. This category is not ideal for use here, because it measures things like cash, stock inventory that might be sold, and certain types of short-term investment. Nonetheless, current assets will be a useful measure to use, if just for comparison with the others. *Fixed Assets* are property, plant and equipment that will not be used up within a single year's production cycle. This is closer to a stand-in for the notion of constant capital in Marx, but it crucially does not include many of the intermediate products used up in consumption (which are included in inventory and therefore measured in current assets, however). *Non-Current Assets* are a more expansive category that includes fixed assets and other sorts of long-term investments and can be roughly estimated by subtracting current assets from total tangible assets.

Here, I will use the measure of Total Assets, Fixed Assets, and Non-Current Assets for industrial "enterprises above designated size" in conjunction with the other industry specific NBS variables (i.e., Net Profits and Value Added). All are measured in current year RMB (in units of 100 million) and can be easily compared. That said, the total assets measure is expected to falsely deflate the measure of profitability (since it includes more irrelevant assets that will nonetheless be recorded as a weight against profits) and both fixed and non-current assets will inflate the measure of profitability (since neither include raw materials). Non-current assets might be the most accurate, simply because its few additional non-liquid assets in excess of fixed capital will help to fill in for the missing raw materials. Nonetheless, all three should follow the

same rough trend, and they'll be visualized side by side to give the viewer a sense of the margin of error.

### *V – The Wage Share*

For Marx, Variable Capital indicated the portion of the total capital that is given to workers. This includes pay in the form of wages or salaries, but it also includes things like employer-provided health or social benefits (though this obviously grows more complex when reckoning the role of taxes). There has been much debate on exactly how to capture this total value in the American context (as in Kliman 2011), but in the Chinese case a measure of the total wage bill will suffice, since things like employer-provided health insurance or retirement plans are not nearly as common. Instead, the most expansive health and benefits packages are implemented via the social insurance system, payments into which are subtracted from net profits in the same way as a tax payment would be. Ultimately, these funds represent a fairly small share of workers' total wages and the system also suffers from deep geographic inequalities (for more detail, see: Jiang 2020).

There is, however, no single reliable measure for the total wage bill over a long time period, partially because the employment data recorded by the NBS have always suffered from undercounting of migrant laborers and, in terms of average wages, the reality is that many firms do not adequately report detailed and accurate wage data. Here, there is no choice but to use a very general measure of the labor share of income produced at a high level of abstraction. Several measures exist for different time periods, each of which calculate wages as a share of total output. The PWT contains a wage share measure for all years in its database, but prior to

1993 it just uses a duplicated figure, likely derived from some planning era standard that never matched reality. Similarly, the most recent years are duplicate values. A far better alternative exists for the years 1978-2004, produced by Bai Chong-en and Qian Zhenjie (2010, Table 4), who compose several measures of labor share drawn from multiple sources. The one used here will be their "aggregate" labor share, calculated up from sectoral values—this is chosen because it loosely matches the production method of calculating GDP by counting up from sectoral value-added. The share values and the trend evident in the Bai and Qian data tend to match the PWT data after 1993 and follow them particularly closely later on. This allows the PWT values for all years after 2004 to be appended to the series without producing any harsh breaks in the continuity of the data. The PWT values are also retained for years prior to 1978, though these will not be examined by us below for obvious reasons.

Overall, the wage share data uses the PWT values for the years 1952-1977, then the Bai and Qian values for 1978-2004 and then again the PWT values for 2004-2019. These are originally calculated as shares of GDP, but they can loosely be transferred to our industry-specific measures as well. Though it would be more accurate to use Bai and Qian's values for industrial labor share instead, these data are only recorded through 2004 and they cannot be combined with the PWT. In terms of the overall trend, however, this appears to not be an issue, since the industrial labor share and aggregate labor share follow the same pattern. In terms of calculating the organic composition of capital (i.e. capital-output ratio) and the rate of exploitation (i.e. output-labor ratio), we should expect the stand-in for V to be slightly inflated for the measure using value-added of industry, since the aggregate share sits an average of ten percent higher than the industrial share in all years after 1990, meaning that industrial workers' share of total industrial output tended to be lower than all workers' share of all output. For the

Net Profit measures, the labor share is multiplied not by net profit (since labor costs have already been subtracted from this measure) but by total value added of industry.

**The Calculations**

The rate of profit (ROP) will be calculated using each of these stand-ins, such that eight different ROP measurements are obtained. Four will be for industry specifically, and four for the entire domestic economy. These will be compared to six different ROP or ROR measures covering various years, drawn from six different sources (4 from English-language scholarship, 2 Chinese). Then several mean ROP and ROR measures will be formulated to demonstrate a series of rough consensus measures drawing on (almost) all the existing options. In addition to the original calculations offered below, the pre-existing ROP and ROR measures cover all the major attempts to calculate the Chinese ROP save that of Li Minqi, who has provided graphs but no publicly available data, and the measurements of Marquetti et. al. 2018, which are based in firm-level measures drawn from the Orbis Database. Since the logic behind measuring ROP relates to an understanding of the fundamental laws of the capitalist system, pushing this measurement further back in time tends to stretch the concept to uselessness. The basic position taken here is that profitability was an important factor for enough of Chinese industry throughout the 1990s that including this decade will be informative, even if the figure is qualitatively different, especially prior to 1993. Ultimately, the ROP only becomes comparable to similar measures for other countries around the turn of the century, when the Chinese economy became both fully integrated into global capitalism and the population at large became fully dependent on the market after the gutting of the remaining socialist era "iron rice bowl" welfare system (for the reasoning behind this, see: Chuang 2019). All values will therefore be calculated from 1990 up

until the most recent year of data (between 2017-2020, depending on the variable). However, some of the NBS data only extends back to the mid-1990s or the year 2000, so measurements using these variables will be more limited.

I will then decompose the various ROP measures into approximations for the Organic Composition of Capital (OCC, calculated as C/V), which amounts to a capital-labor ratio capturing the relative weight of non-human inputs into the production process (plant, equipment, raw materials), and the Rate of Surplus Value (ROSV, calculated as S/V), which amounts to an output-labor ratio broadly similar to conventional measures of labor productivity. This will not be done for the ROR measures (i.e. all of those using net profits), since they include no explicit wage bill variable. For the ROP measures, this allows underlying relationships driving the movement of the trend to be delineated more clearly. The capital-labor ratio is generally used to calculate trends in the mechanization of production, since it will rise as firms become more capital-intensive. Marx predicts a tendential rise in what he calls the Organic Composition of Capital (OCC, measured as C/V) for the entire capitalist economy over time, which should, in general, be emulated in individual countries' capital-labor ratios. While the ROSV, also called the Rate of Exploitation, is often used in works of "Marxist political economy" to calculate trends in the severity of exploitation of labor—in the sense of how much value is extracted from workers versus how much they are returned in the form of the wage, again at the scale of the entire economic system—here the output-labor ratio simply gives a sense of how much is produced per dollar spent on workers. It is essentially an approximation of labor productivity. For each of these, the absolute values are not particularly important, since the goal is to determine the underlying character of changes in the ROP. In general, when the capital-labor ratio increases faster than the output-labor ratio the ROP will tend to decline. Finally, each of

these figures will be compared with conventional measures of labor productivity and total factory productivity, derived from the PWT, the ILC and Herd 2020.

The measures drawn from the existing literature on the measurement of the ROP and ROR in China are described in more detail in Appendix 3. My own eight measurements are summarized here. The first is a national measure drawn from the PWT 10.0 GDP and Capital Stock figures, and we get the same result if we substitute NBS GDP in RMB (since this essentially just undoes the Penn conversion to international USD, not their PPP GDP measure that uses the Maddison and Wu deflator). The subsequent four are industry-specific measures that utilize net profits, value added and various combinations of asset measures for industry, and the final two are more limited variants using NBS GDP and alternate capital stock measures as calculated by Shan (08) and Wang et al (17):

1) PENN9

First, I calculate National ROP using the variables from the PWT 10.0. This uses the PWT rgdpna and rnna figures for Real GDP and Capital Stock, respectively, since these are closest to pure national accounts measures (converted to international USD but not distorted by the PPP modifications used elsewhere in the tables). The GDP figure is derived directly from the NBS, converted to constant 2017 USD, in millions.

The PENN9 equation is as follows:

$$\frac{GDP}{Capital\ Stock + (Labor\ Share * GDP)}$$

2) Net Profits / Fixed Assets

This is first in a series of ROP measures using Net Profits of Industrial "Enterprises Above Designated Size." In the second half of the denominator, all my measures use the labor share multiplied by the value added of industry, rather than by net profits, since the wage bill has already been subtracted from the net profits figure. The differences between measures 2-4 are what stand-in is used for C (see above) in the ROP equation. This one uses Net Profits and the Total Price of Fixed Assets at their current cost (i.e. not historic costs, though these are also available):

$$\frac{Net\ Profits}{Fixed\ Assets + (Labor\ Share * VA\ of\ Industry)}$$

3) Net Profits / Non-Current Assets

The second in the Net Profits series uses the same equation as above, but this time calculates the Price of all Non-Current Assets instead of fixed assets alone:

$$\frac{Net\ Profits}{NonCurrent\ Assets + (Labor\ Share * VA\ of\ Industry)}$$

4) Net Profits / Total Assets

The third in the same series uses the same equation again, but this time with the Price of Total Assets:

$$\frac{Net\ Profits}{Total\ Assets + (Labor\ Share * VA\ of\ Industry)}$$

5) Value Added

In addition, I calculate one variant of the ROP using the Total Value Added of Industry. This is an industry specific measure similar to the ones used for net profits but, in contrast to the preceding equations, it uses one of the component output figures that ultimately gets added together to compose national GDP. This ROP could feasibly be calculated using any of the Capital Stock measures used above, giving the same spread to the ultimate value. But since the spread is already evident, I'll just use Total Assets, since this matches the somewhat expansive definition of output used in value-added measures and brings the final line down into range for proper comparison with the others, while also including data for the 1990s. If I were to use Fixed or Non-Current Assets, the outcome would be an identical spread with the highest estimate (using Fixed Assets) producing a profit rate line just under twice that produced by using Total Assets. This is the equation that will be used:

$$\frac{VA\ of\ Industry}{Total\ Assets + (Labor\ Share * VA\ of\ Industry)}$$

6) NBS GDP and Shan 08 Capital Stock

This is the first of two measures using alternate capital stock computations drawn from the Chinese-language literature on the topic. This measure only extends to 2006. As mentioned earlier, these capital stock measures are unaccountably low compared to GDP. This means that these ROP measures will come out much higher than the others and will

therefore be visualized separately and not used in the formation of the final consensus mean:

$$\frac{GDP}{Capital\ Stock + (Labor\ Share * GDP)}$$

7) NBS GDP and Wang et al 17 Capital Stock

This is the second of the two measures using alternative capital stock computations. This measure is more recent and extends to 2016. It uses the same basic method to calculate capital stock as used in Shan 08, which can be seen by the fact that the two resulting ROP figures ultimately track one another closely. Like the one above, this capital stock measure is already depreciated and is unaccountably low, producing an inflated ROP. The equation is the same, changing only the Capital Stock value:

$$\frac{GDP}{Capital\ Stock + (Labor\ Share * GDP)}$$

8) Herd 2020 GDP and Capital Stock

This final measure utilizes the recent Capital Stock reconstruction formulated by Richard Herd for the World Bank. It extends from 1953 to 2016, is recorded in billions of RMB and includes a comparable GDP measure, on which the wage share can be applied. The equation is the same as in the two variants immediately above:

$$\frac{GDP}{Capital\ Stock + (Labor\ Share * GDP)}$$

*Capital-Labor and Output-Labor Ratio*

Each of the major ROP measures will also be decomposed into a capital-labor and output-labor ratio. I will then measure the changes in these variables over several different time periods in order to determine which of these might account for the major trends visible in the ROP. I will produce three measures of the capital-labor ratio, but since each equation uses the same labor share, all the output-labor ratios will come out identical to one another. Thus, I will only produce this one. All measures will be compared to the OCC and ROSV measures found in Wu et al (2020) and Zhao and Liu (2017). Each will be summarized for four time periods. Since I am mostly interested in trends after the destruction of the socialist-era industrial belt, which marked the completion of China's transition to capitalism, these summary figures will begin in the year 2000 and end in 2017, the last year for which all the data is available. Although some of my own ROP measures, as well as the ROP from Wu et al (2020) extend past 2017, I take 2017 as the final year in these summaries simply so that all the changes over time can be equally compared. The first three change-over-time summaries simply look at the trends in five (2000-2005), ten (2000-2010) and seventeen (2000-2017) year increments, and the final measure zooms in on the seven-year period following the global economic crisis (2010-2017). Visually, I'll separate the first three measures of ascending time from the final measure by a vertical dashed line.

The capital-labor measures are as follows:

1) PENN9:

$$\frac{Capital\ Stock}{(Labor\ Share * GDP)}$$

2) Industrial Value Added:

$$\frac{Fixed\ Assets}{(Labor\ Share * VA\ of\ Industry)}$$

3) Wu et. al. 2020 premade measure for comparison

And, using any of the output/profit measures, I can calculate ROSV as:

$$\frac{Output}{(Labor\ Share * Output)}$$

**Trends in the Underlying Data**

Before presenting the trends in ROP and ROR, it will help to orient around the overall movement in the major input variables over time. Since almost all of the raw variables exhibit a secular increase, however, these trends are more evident in comparative growth rates. The underlying increase in all variables is most clearly linked to the relatively high and relatively continuous

growth of the overall economy, as visible in the Year-on-Year growth rates of GDP in both the original (i.e. NBS) data and in the deflated version used in the PWT:

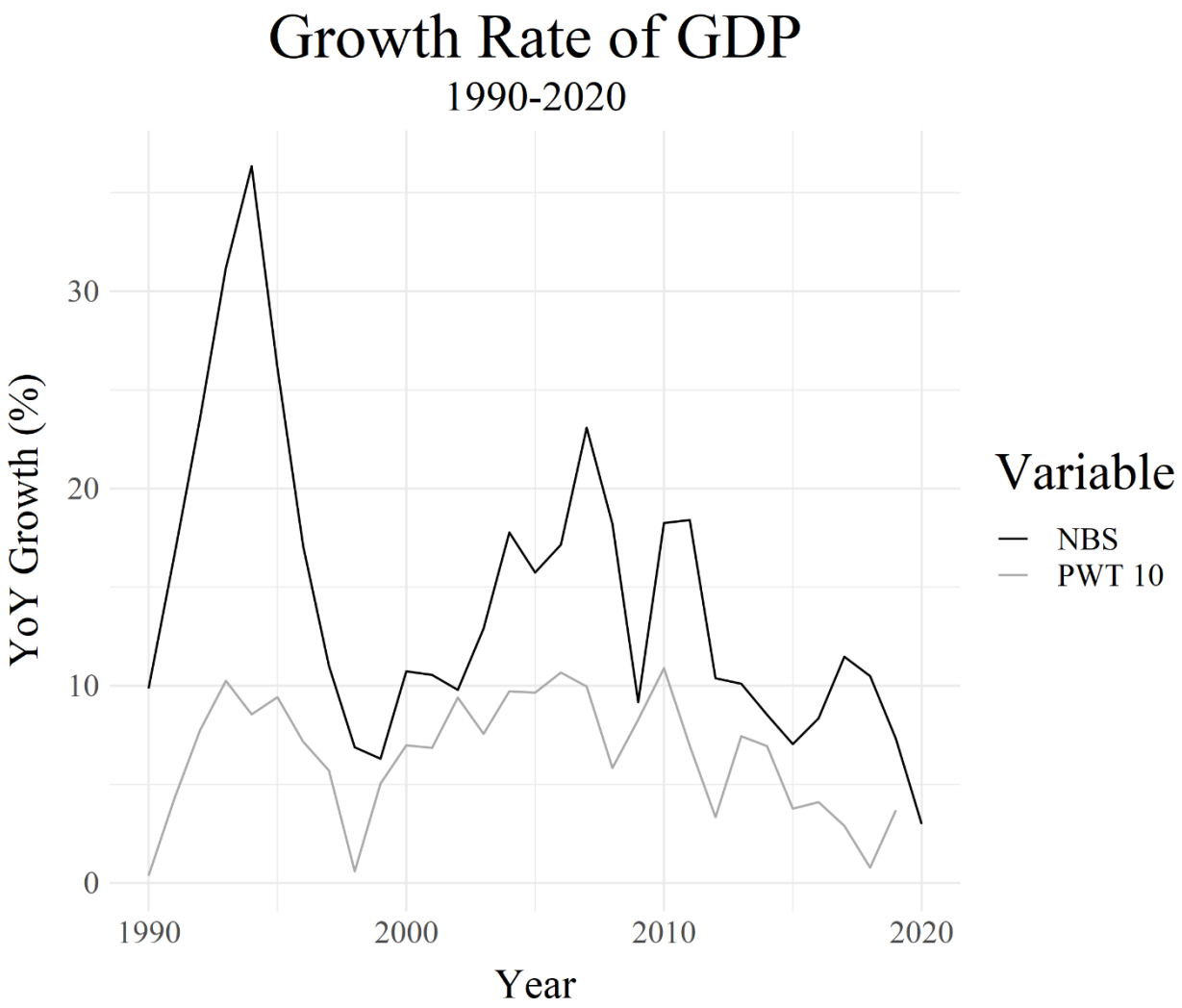



*Figure 3 - 1*

As is clear in Figure 3-1 above, both GDP series show similar overall patterns. The main differences are the fact that the NBS series lies above the deflated series used in the PWT, with particularly exaggerated spikes in the mid-1990s and mid-2000s. Both series show very strong GDP growth from 1990 through to 2010, with two notable dips in both, first during the 1997-98

Asian Financial Crisis and second during the Great Recession in 2008. A third dip related to the 2020 coronavirus pandemic is visible in the NBS series, which includes data for that year.

GDP can be decomposed in several ways and, for the sake of measuring the ROP or ROR, should ideally be compared to trends in total capital stock, total profits of industrial enterprises and various measures of productivity. First, it will be helpful to decompose Chinese GDP into the relative contributions of the primary, secondary and tertiary sectors over time:

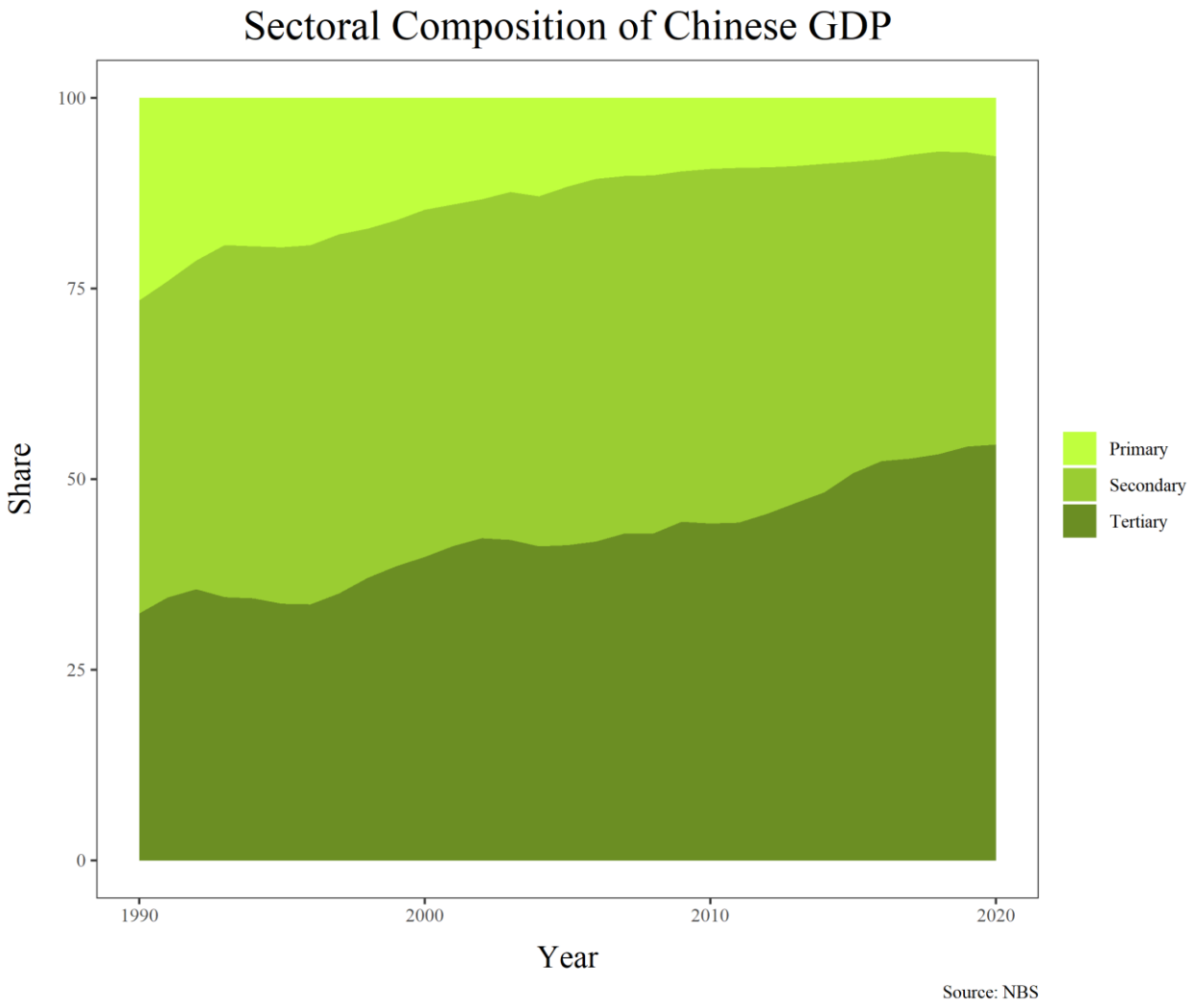



*Figure 3 - 2*

The most distinct trend in Figure 3-2, above, is the general shrinkage of the primary sector's contribution to overall GDP since 1990. It declines from a little more than 26 percent of GDP in 1990 to just under 8 percent in 2020. But the contribution of the secondary sector remains relatively static throughout, shrinking mildly in the most recent years. Value-added of the secondary sector actually peaks twice at roughly forty-seven percent of GDP: first in 1997, prior to the dismantling of the socialist-era industrial belt, and a second time in 2006, at the height of

the export-oriented production boom centered on the southern sunbelt. After this second peak, the share has undergone a slow and continuous decline, sitting at just under thirty-eight percent in 2020. In contrast, the tertiary sector has seen by far the most rapid growth in its contribution to overall GDP. The tertiary sector, by contrast, has seen more or less continuous growth and is now the largest single contributor to overall output. Value-added of the tertiary sector begins at roughly thirty-two percent of GDP in 1990 and ends at fifty-four percent by 2020, overtaking the contribution of the secondary sector sometime around 2013.

Since Chinese GDP is computed by aggregating the value-added of each industry, it is also possible to further decompose these measures into subsectoral contributions:

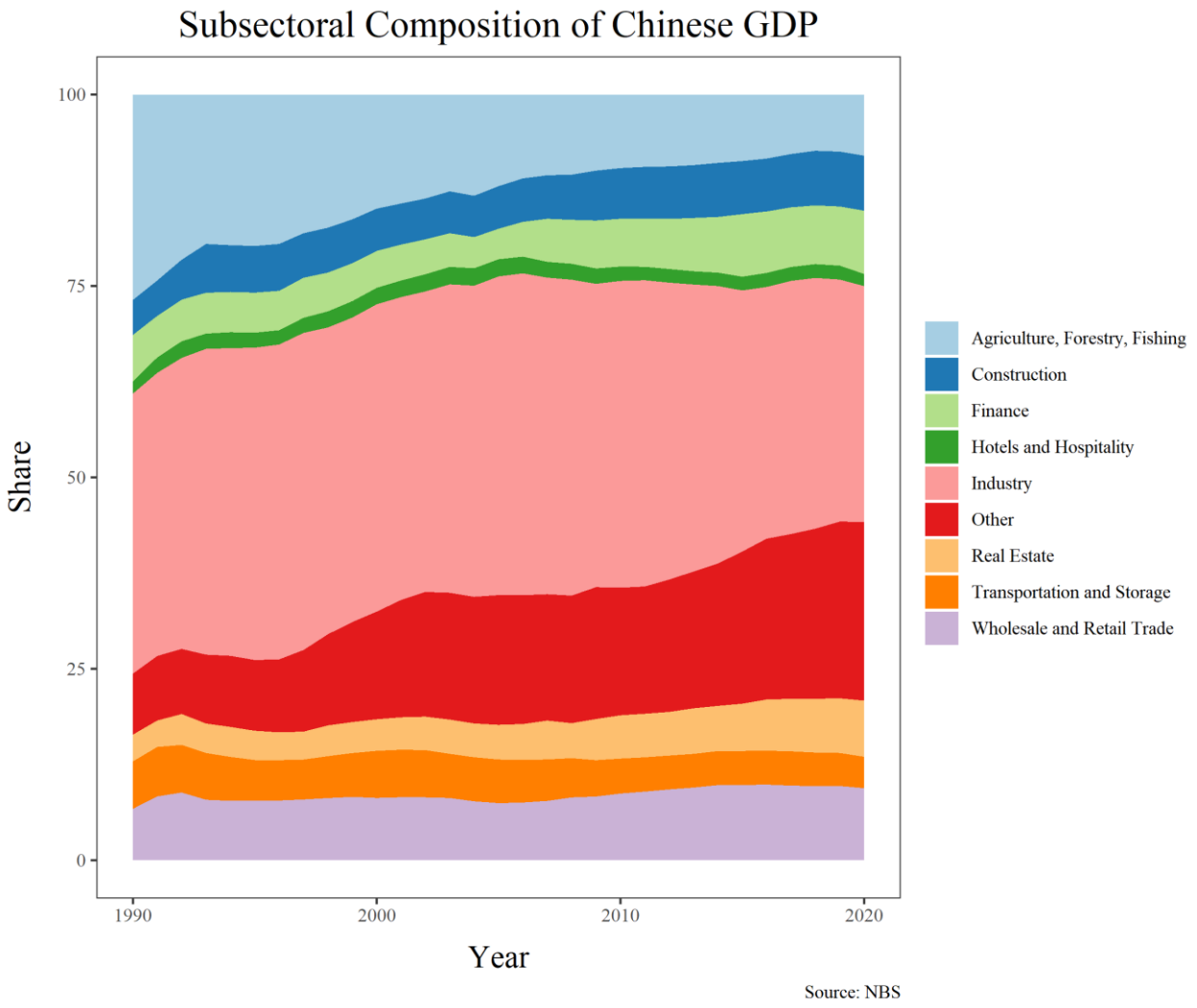



*Figure 3 - 3*

In Figure 3-3, the same trends are visible in more detail. The decline of Agriculture, Forestry and Fishing is the most notable trend, driving the overall decline in the primary sector as a whole. Similarly, it seems that the rising tertiary share is largely captured in the indistinct "Other" sector, accompanied by a more moderate rise in the share contributed by Finance and Real Estate. Based on more detailed employment data recorded by the NBS, it seems that this "Other" category likely includes healthcare, education, numerous producer services and some elements of

the tech and e-commerce sectors. In contrast, Wholesale and Retail Trade, Transportation and Storage and Hotels and Hospitality remain roughly stagnant or see only mild growth. Meanwhile, the trends the secondary sector are primarily driven by the shrinking contribution of industry to overall GDP, only mildly attenuated by the mild rise in the contribution of construction. Similar to the above series, Industry's share peaks in 1997 and 2006 at forty-one and forty-two percent of GDP, respectively, before declining to just under thirty-one percent in 2020. Construction's share hovers just under five percent in the early '90s before rising to just above 7 percent in 2020.

While the Chinese capital stock issue tends to prevent a similarly systematic approach, the basic trends can be observed in unofficial measures produced by independent scholars and through related measures such as fixed capital investment, which are recorded by the NBS. The most robust overview of the issue of capital stock measurement is provided in Herd 2020, which also provides measures of GDP, Gross Fixed Capital Formation, Capital Stock and the Capital-Output Ratio, all decomposable into four sectors: business, housing, infrastructure and government. Herd's data will also be used to compose an ROP and capital-labor ratio measure below. First, an overview of the basic trends in the raw numbers will help to scale the different decompositions:

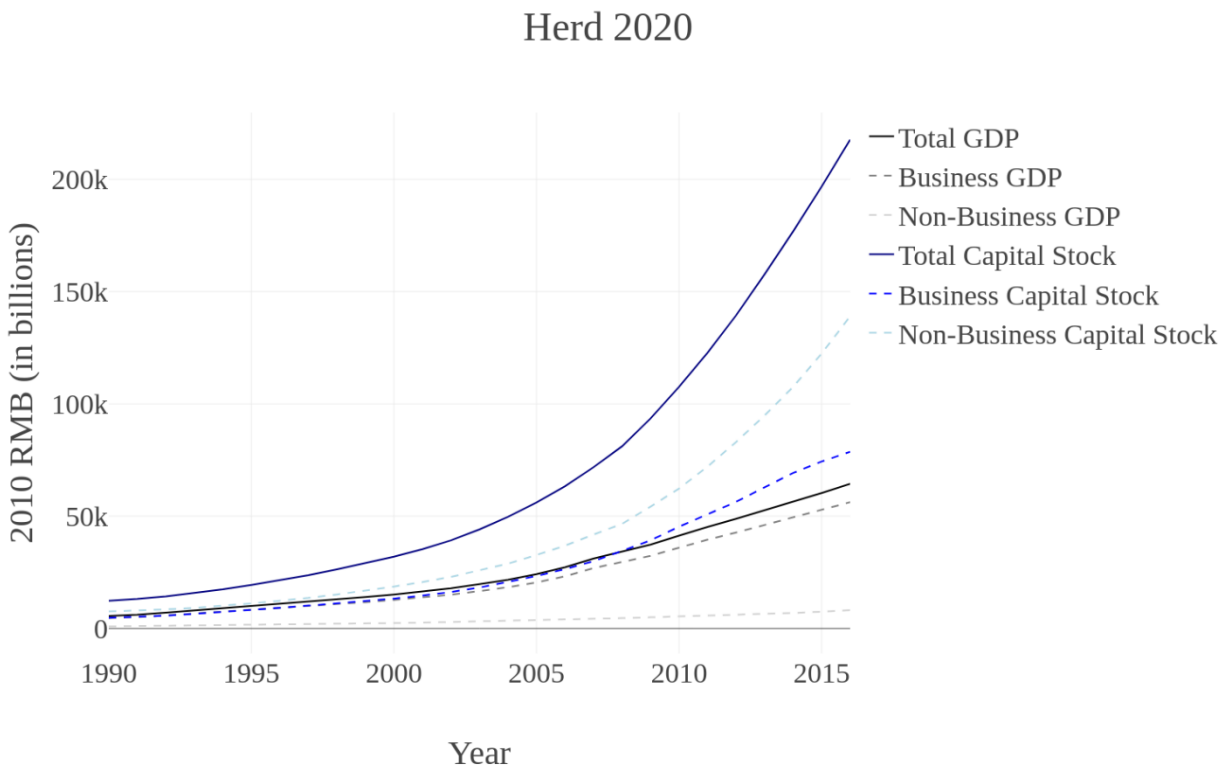



*Figure 3 - 4*

While the general growth is clear here, the precise differences in growth rates and decomposition of the variables is not particularly easy to see. In order to see potential divergences within the capital stock measure (the dark blue line in Figure 3-4), it will be better to calculate the different sectoral shares of overall capital stock:

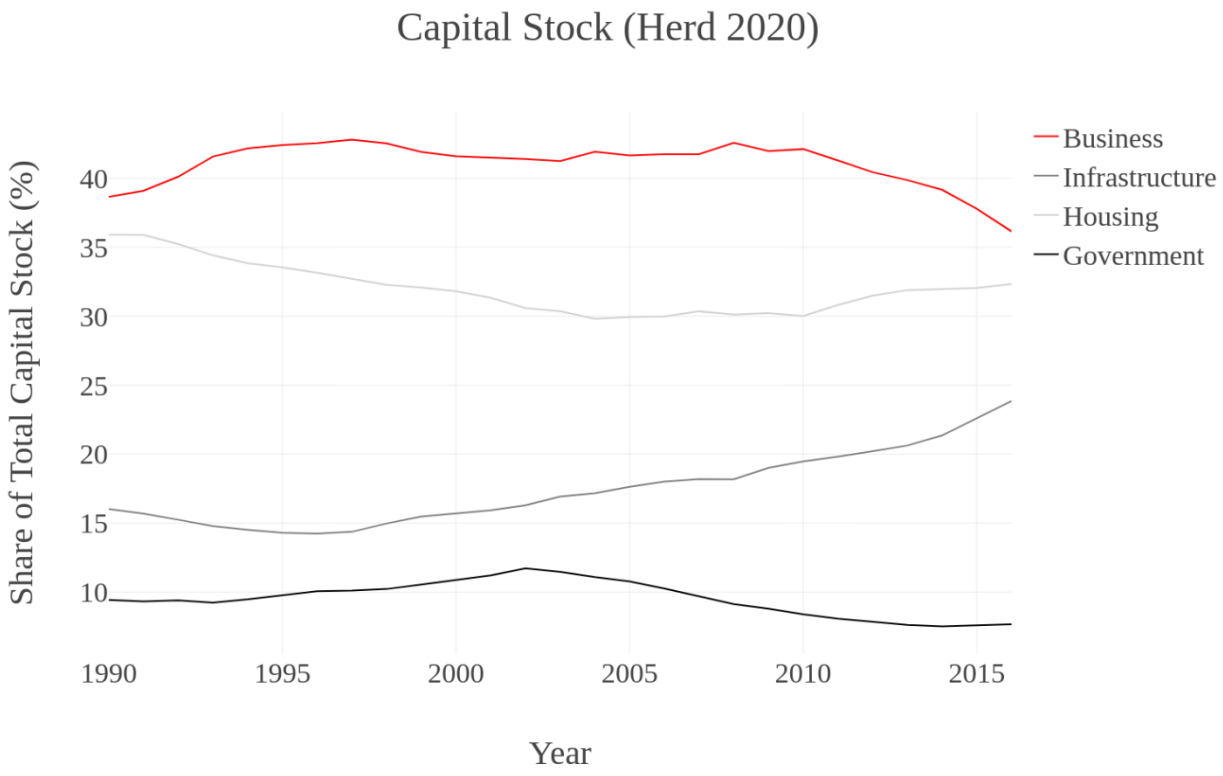



*Figure 3 - 5*

This decomposition of Herd's capital stock measure clearly shows the rising prominence of infrastructure in total capital stock and the declining share of business. It is interesting to note both that the government share has consistently declined from the early 2000s and that the housing share has only risen mildly since 2010, despite the rise of the housing bubble. These trends can be further identified in the relative divergences in the capital-output ratios of each sector. The two figures below show the capital-output ratio for the total economy (National K / National GDP) as compared to the capital-output ratio for business specifically (Business K / Business GDP) and the relative shares of each sector as divided by GDP (i.e., Sector K / National GDP):

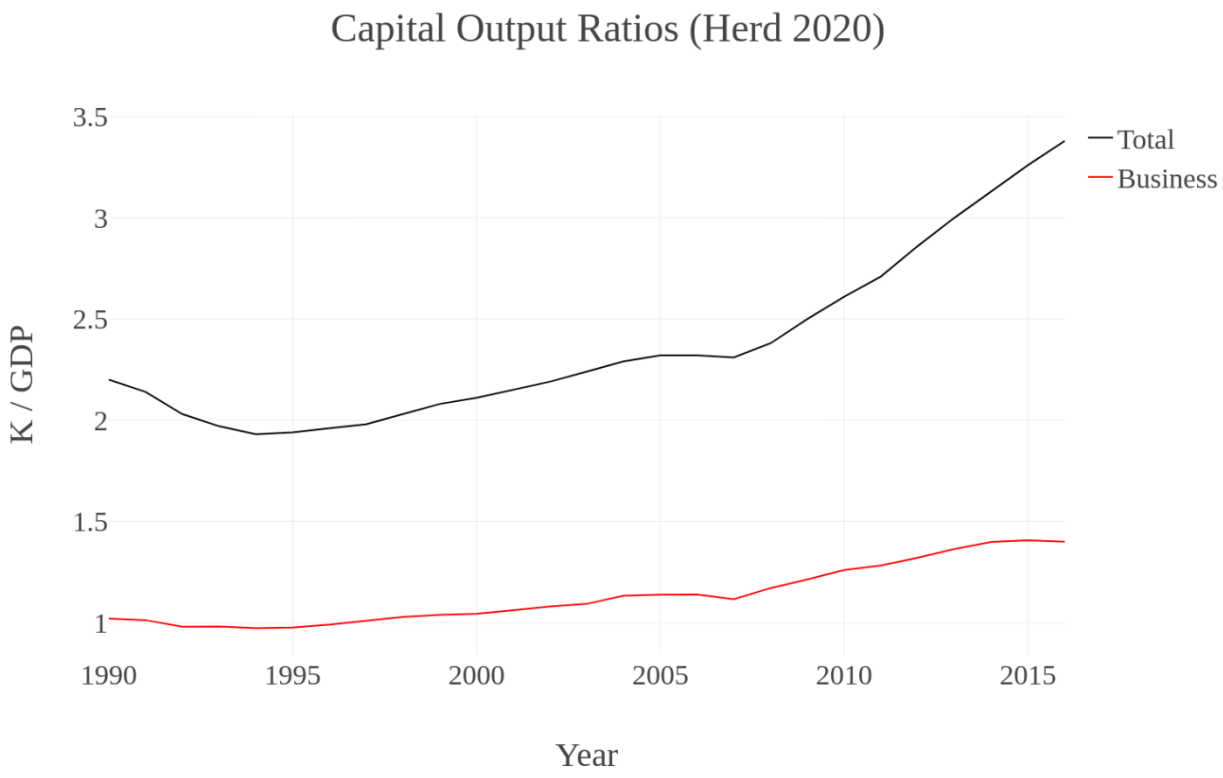

Capital Output Ratios (Herd 2020)
3.5
3
2.5
2
1.5
1
K / GDP
1990
1995
2000
2005
2010
2015
Year
Total
Business

*Figure 3 - 6*

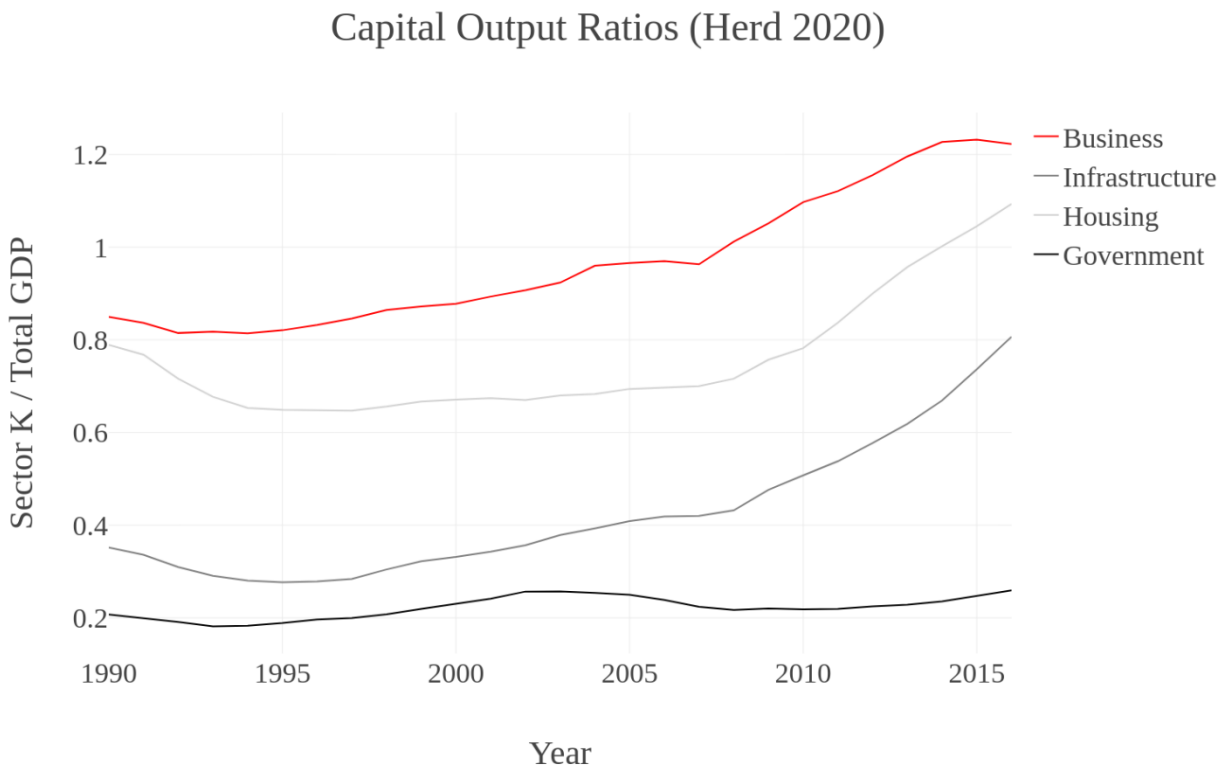



*Figure 3 - 7*

These two figures add some clarity to the initial trend visible in the capital stock series. In Figure 3-6, we see again that, while the capital-output ratio has grown for both the national economy and the business sector, the national capital-output ratio begins to grow faster than the business capital-output ratio in the mid-2000s. When the business capital-output ratio begins to plateau by the mid-2010s, the national capital-output ratio continues to grow. Figure 3-7 shows more detail, albeit using a slightly different method. Here, each sector's capital stock is divided by the total national GDP. Growth in all but the Government sector increases after the Great Recession, but Infrastructure grows particularly fast and neither Infrastructure nor Housing begin to plateau alongside Business in the mid-2010s.

The same phenomenon can also be approached using NBS data on investment in fixed capital and residential investment, which can be compared to both official World Bank measures of Gross Fixed Capital Formation and a similar series composed by Herd. The basic divergence between the series can be seen below, mostly caused by different statistical definitions (see below) but also in part due to the use of current costs (in the NBS series) versus constant costs in the Herd and World Bank measures:

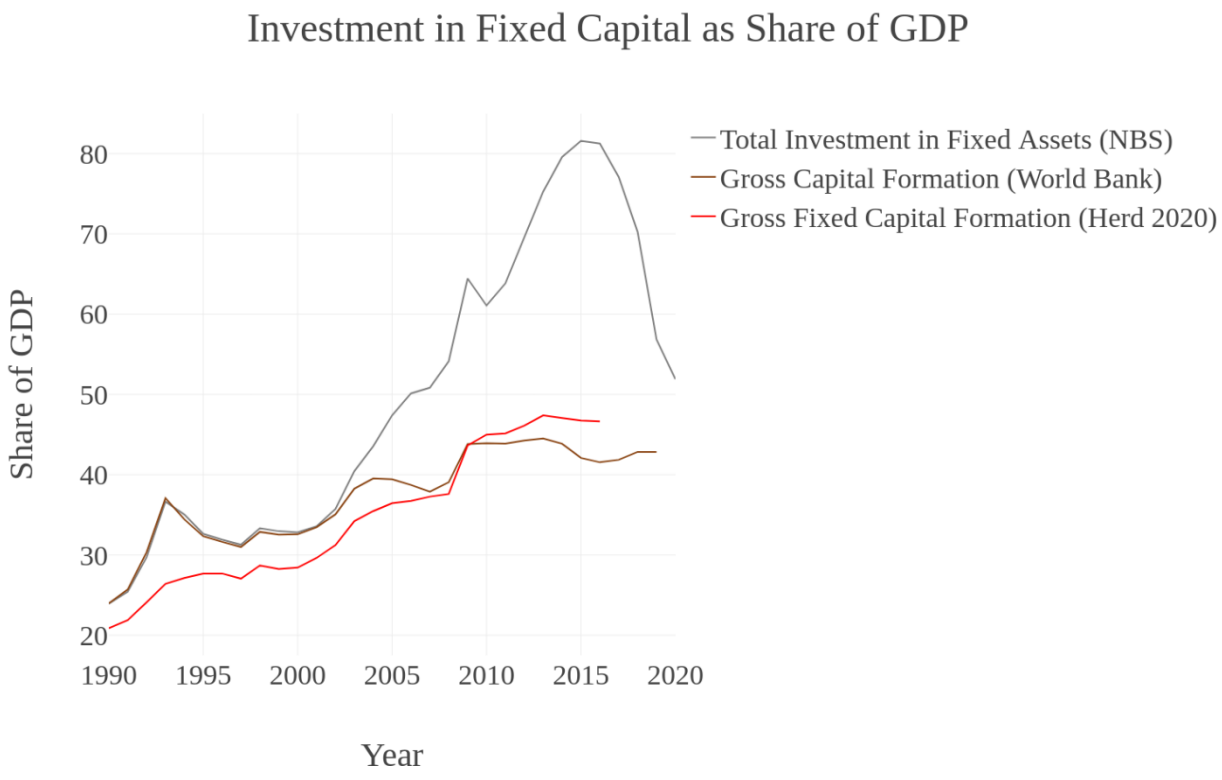



*Figure 3 - 8*

In Figure 3-8, growth is more constant, but the NBS series emphasizes the particularly rapid growth in fixed asset investment until around 2015, after which investment declines substantially. Part of that decline has to do with a statistical redefinition of this specific category after 2018, aimed at bringing it more in line with global standards for the measurement of Gross Fixed Capital Formation. Prior to this point, total investment in fixed assets had included the value of purchased land, existing buildings and second-hand equipment, including when mergers occurred. This means that corporate mergers appear in the data as "new" investments simply because pre-existing fixed capital had changed hands. Similarly, especially with regards to the value of land, the figure has been especially susceptible to inflation by local officials (Lardy 2019, p.20, footnote 12). While the same upward trend is visible in all three series here, the other two show it as less of a bubble and more as a more gradual plateau. Herd offers a more detailed breakdown of the series (in constant 2010 prices) using the same sectors as above:

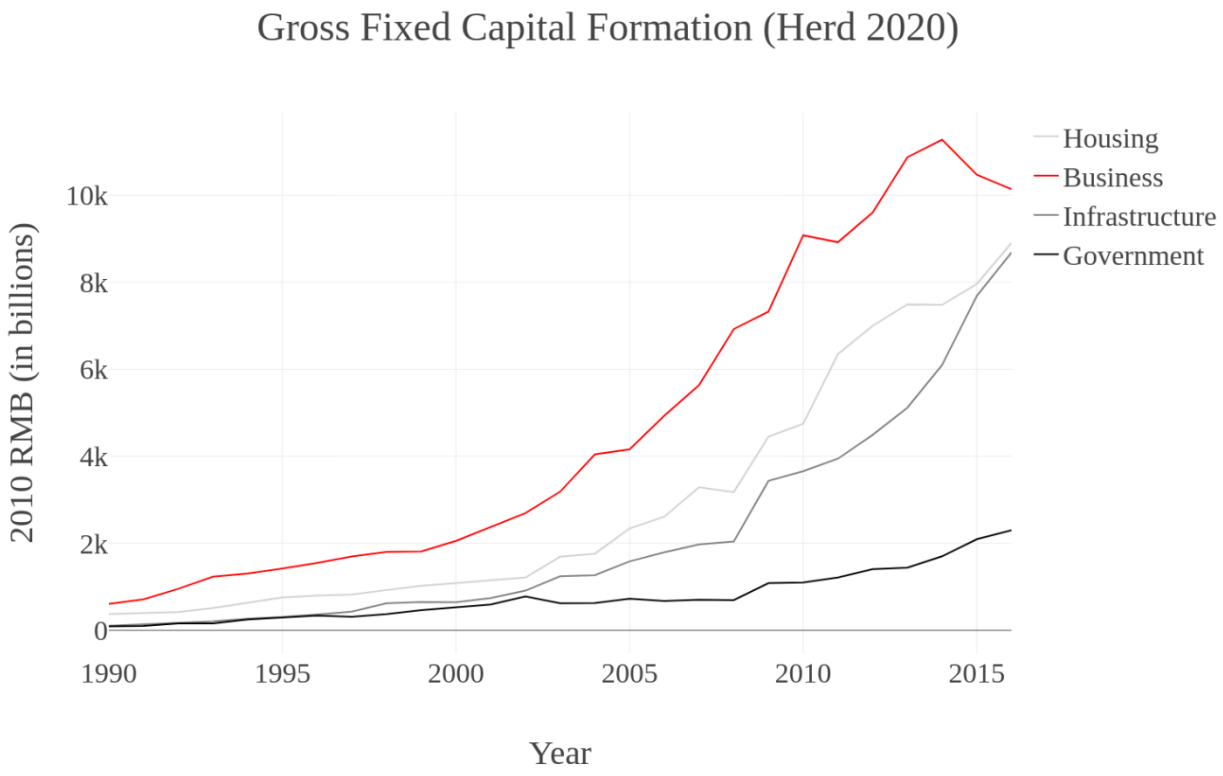



*Figure 3 - 9*

This essentially shows the same trends as in Herd's other series, with Business peaking and then declining in the mid-2010s while Housing and Infrastructure continue to rise. One slight contrast is the fact that Government Gross Fixed Capital Formation rises in absolute value after the Great Recession.

A more detailed comparison can be found in the NBS series, since it is decomposed into subsectors that can be normalized using the same year's GDP:

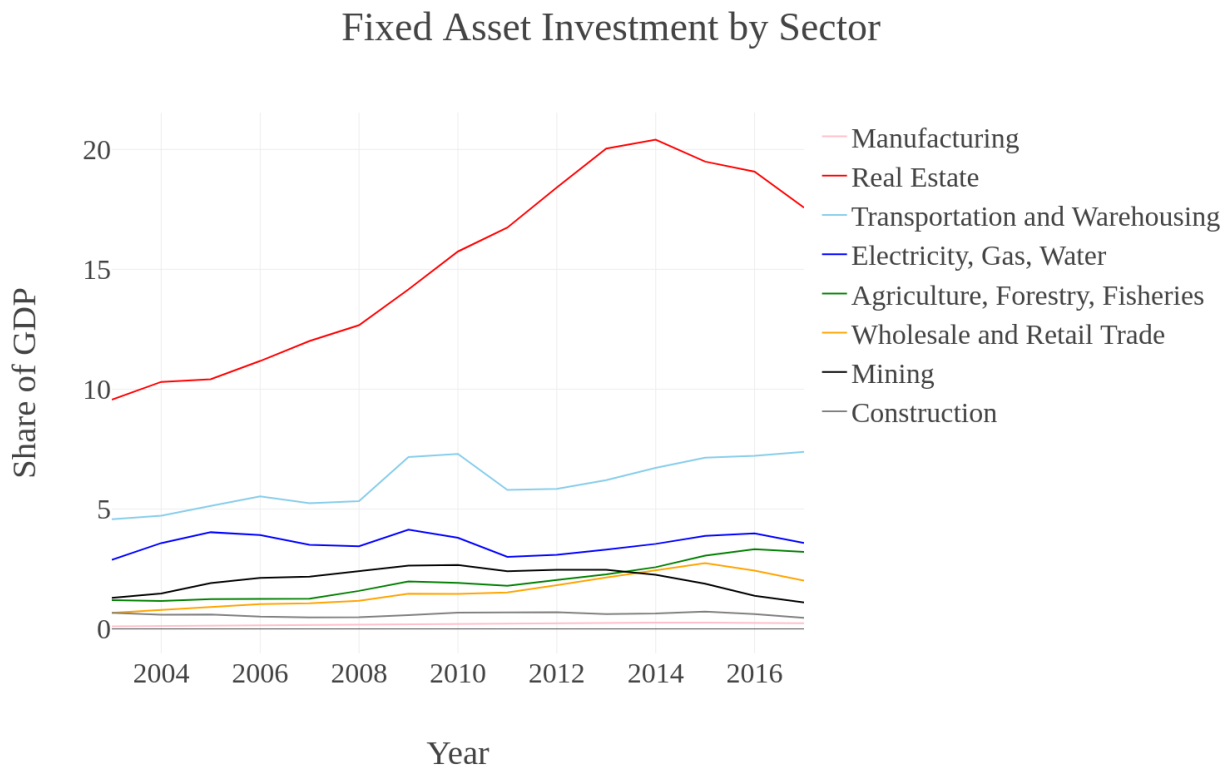



*Figure 3 - 10*

Here, it's clear that real estate dominates the overall trends in fixed asset investment, hinting that some of the peak seen in Figure 3-8 is likely related to the real estate bubble. This is likely, given that rising housing prices were one of the major factors buoying Chinese growth in the years immediately after the Great Recession. Similarly, Transportation and Warehousing see strong growth immediately after the infrastructure-focused stimulus was passed in response to the global economic collapse. After a brief increase in 2009-2010, growth falls in 2011 and then continues steadily in subsequent years. Agriculture, Forestry and Fisheries have similarly seen consistent growth. By contrast, mining sees a distinct decline and both Construction and

Manufacturing stay essentially stagnant at this scale (it is the light pink line that sits at the top of the legend, but at the bottom of the chart, not to be confused with the red line of real estate).

Below, I will offer both ROP measures for the entire national economy as well as for industry specifically. It will therefore help to place the industrial measures in context. This requires first clarifying the basic units of analysis: industrial enterprises. In NBS statistics, the category is mostly composed of firms whose main business is some sort of processing or manufacturing (construction enterprises lie in their own category here, though they are included as secondary industry elsewhere). There have, however, been several important statistical redefinitions that have changed the population of firms included in the industrial census. Technically, the measures relate to all "industrial enterprises above a designated size" (规模以上工业企业) and it is this "designated size" that has been the subject of the most recent changes. Prior to 1998, the industrial census counted all firms regardless of size. Between 1998 and 2011, it only counted firms with an annual revenue from their main business of at least 5 million RMB. In 2011, this cut-off was changed to 20 million RMB (Holz 2013, pp.6-13). In 2017, several minor changes were made to further standardize data gathering methodology and to rectify issues of double counting (this change is reported on the NBS data portal).

In all the major variables the effect of these redefinitions is minimal, since firms that lie above the designated size hold the vast bulk of all assets, take in the majority of all revenues and produce most of national output. The redefinitions are, however, important when using the absolute count of industrial enterprises to normalize data or (as in the next chapter) attempt to measure trends in the growth rate and relocation of enterprises. Whenever this is a risk, I will present the series with clear breaks at the points of the significant 1998 and 2011 statistical redefinitions. Similarly, there are important qualitative distinctions to be made in long-run

comparisons of Chinese industrial data. Prior to the late 1980s and early 1990s, most variables are accounted for using the old standards set in the planned economy (for detail, see Holz 2013). The 1990s act as a transitional decade in which measures begin to be switched to accord with international norms even while some of the firms being recorded are still operating according to the standards of the planned economy. Over the course of this decade, more and more of industrial output was subject to market prices and firms became more and more dependent on retained profit. The transition to full dependence on the market was essentially completed with the restructuring and consolidation of the old socialist-era industrial belt in the northeast and China's entry into the WTO in 2001 (Chuang 2019, Naughton 2018 Ch. 14, Lardy 2014 Ch. 1). In what follows, I will try to be attentive to these qualitative differences, using series that begin in 1990 whenever units are more or less comparable, but utilizing only data for the years 2000 onward elsewhere.

Here, for instance, we can see the effect of statistical redefinitions when portraying the total growth in enterprises over time:

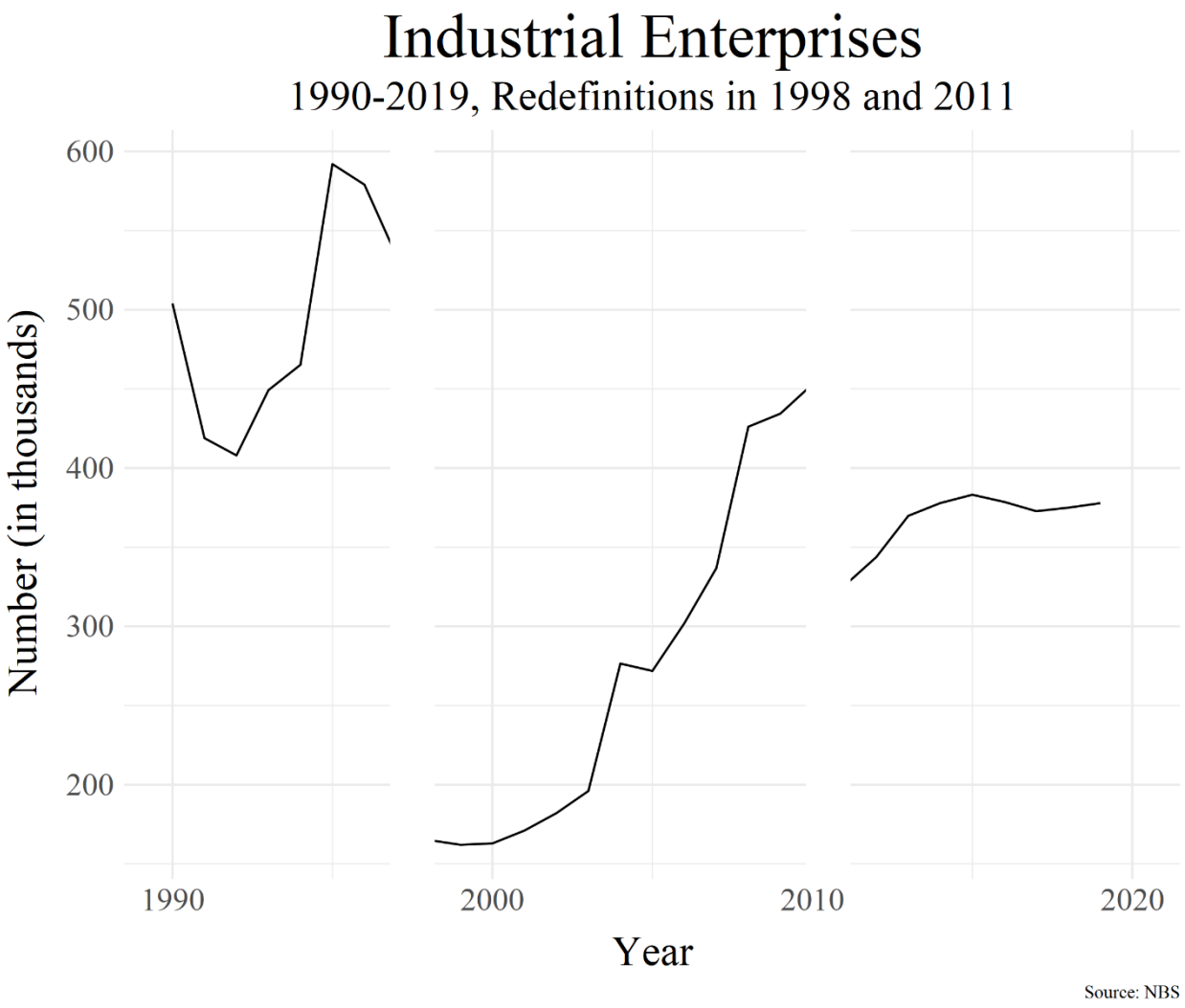



*Figure 3-12*

What is somewhat occluded here is the qualitative change in the fundamental logic of industrial production that took place over the course of the 1990s. Similarly, the statistical redefinitions sometimes obscure real declines in the number of enterprises. For example, the 1998 redefinition occurred alongside both the aftermath of the Asian Financial Crisis and the beginning of widespread factory closures and consolidations in the northeast industrial belt. These closures and consolidations effectively completed the qualitative transformation of industry. Thus, the

redefinition merely exaggerates both a real quantitative trend and marks a true qualitative divide. In 2011, by contrast, the redefinition is the major cause of the apparent quantitative decline and has no qualitative dimension. The 2017 redefinition appears to have exerted a minimal effect, at most causing a very slight decline due to the removal of double-counted firms. For this reason, it is not portrayed as a harsh divide in the final visualization.

The 2011 redefinition has little effect, however, on major variables of interest such as Net Profits, since the firms below the cut-off contribute a relatively small share of total output:

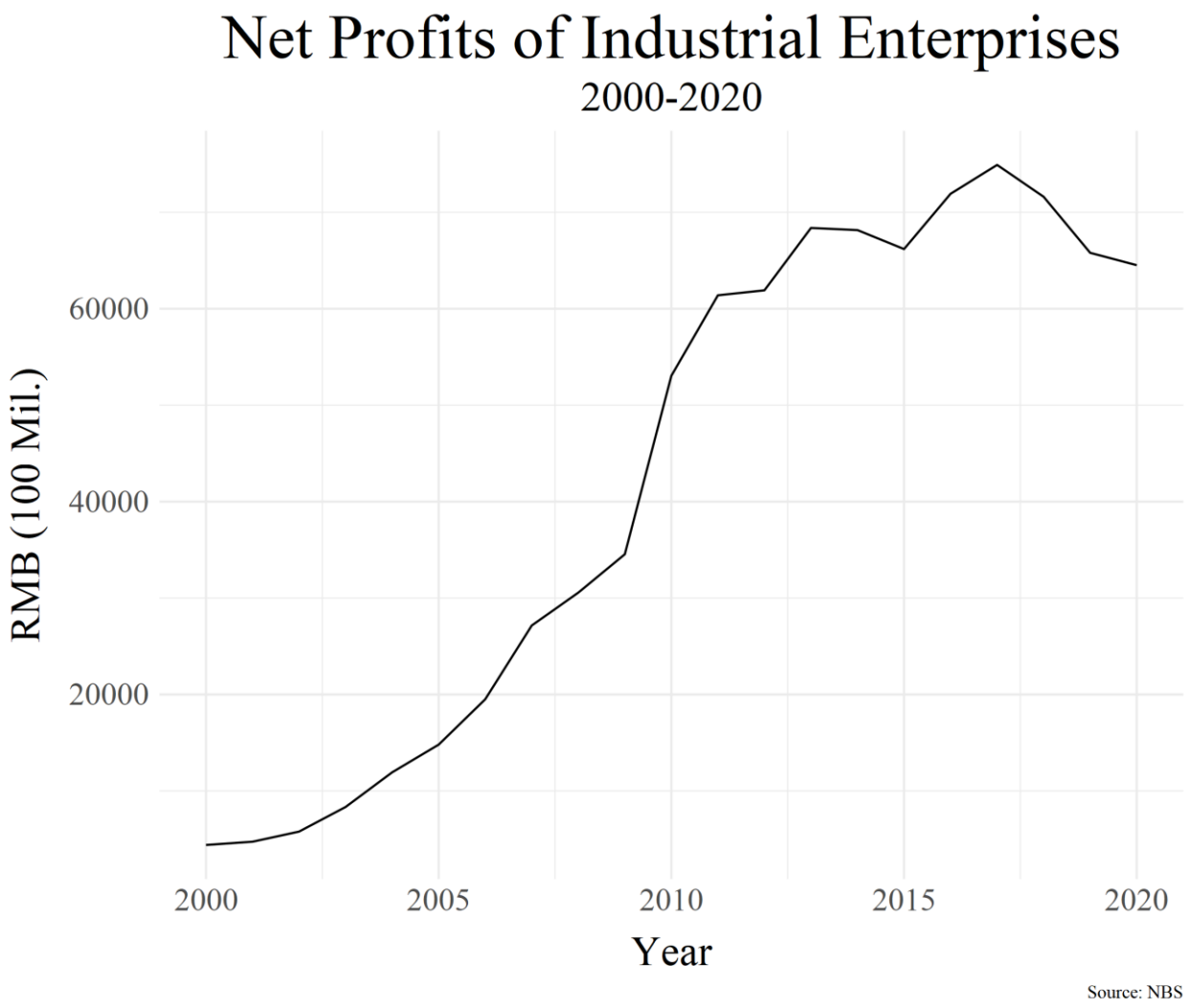



*Figure 3 - 12*

Profits clearly do not decline between 2010 and 2011, though they grow somewhat slowly. The more substantial declines occur after 2017, with one dip between 2014 and 2015. While it is possible that the 2017 redefinition could be exerting a depressing effect here through its reduction of double-counting and elimination of other methodological artefacts, we would expect that effect to be mostly visible between 2016 and 2017, not between 2017 and 2018. Instead, profits increase between 2016 and 2017 and decline thereafter. Similarly, the statistical redefinition should not produce multiple years of visible decline in reported profits. It seems that

the 2017 redefinition had no effect on the variable or, at most, may have mildly muted its growth between 2016 and 2017.

Profit growth among industrial enterprises has tended to be more volatile than other variables used below, often dipping into the negative. This is visible when calculated as percentage Year-on-Year growth and compared to a similar measure for Total Assets of all industrial enterprises:

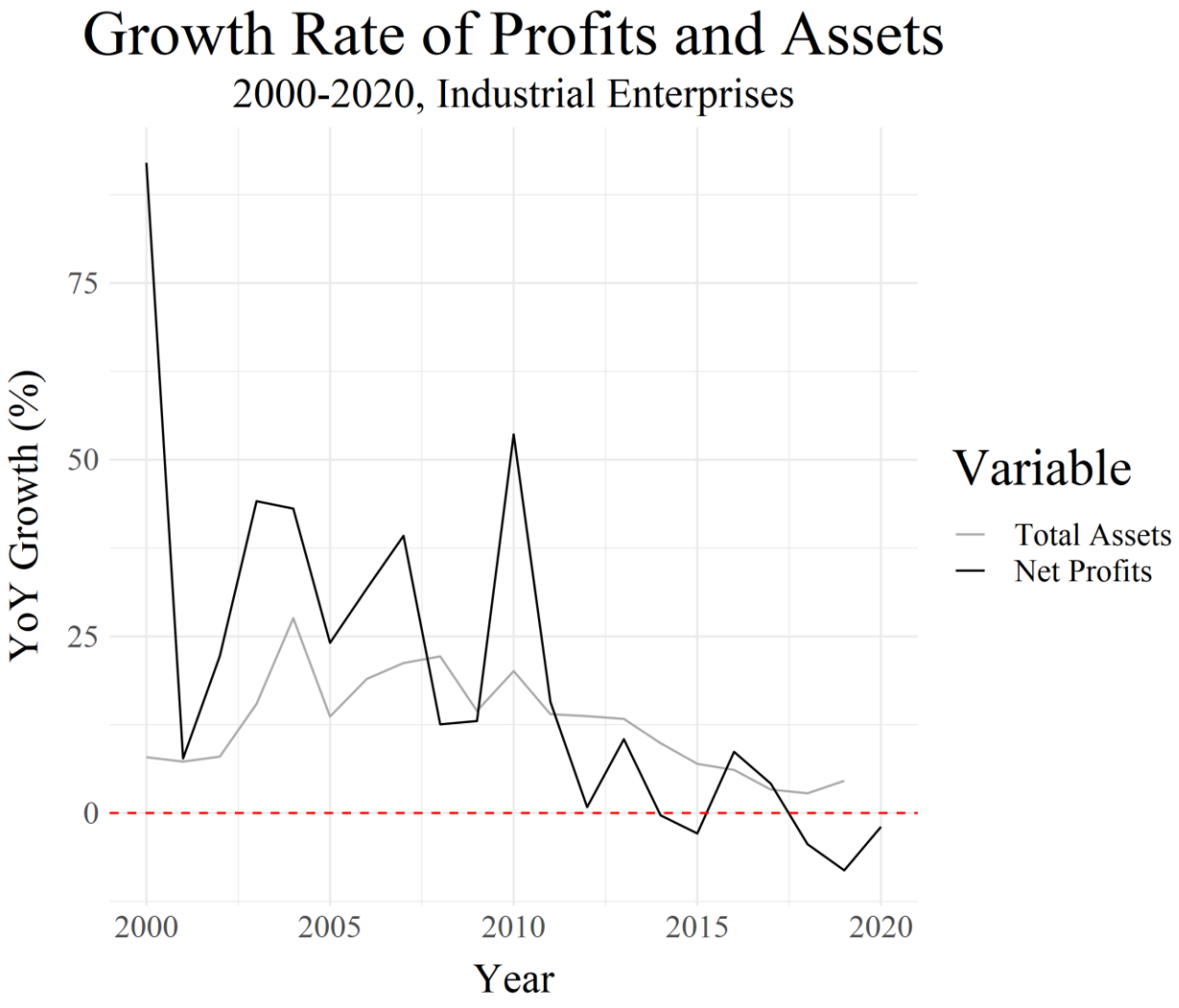


*Figure 3-13*

Even though the growth rate of Total Assets declines after 2010, it never dips below zero. By contrast, the growth rate of Net Profits exhibits more volatile spikes and dips into negative growth in multiple years.

The effect of the various statistical redefinitions are also visible when plotting total enterprises against any other variable or when using it to normalize figures, producing, for example, an average profit-per-enterprise figure. These effects will be more important in the next chapter. For now, however, it is important to point out that the larger slowing of profit and the negative growth rates seen in total profits in recent years have also been accompanied by a delinking of the roughly linear relationship between total enterprises and net profits that prevailed from 2000 to roughly 2013, as is clearly visible when one is plotted against the other:

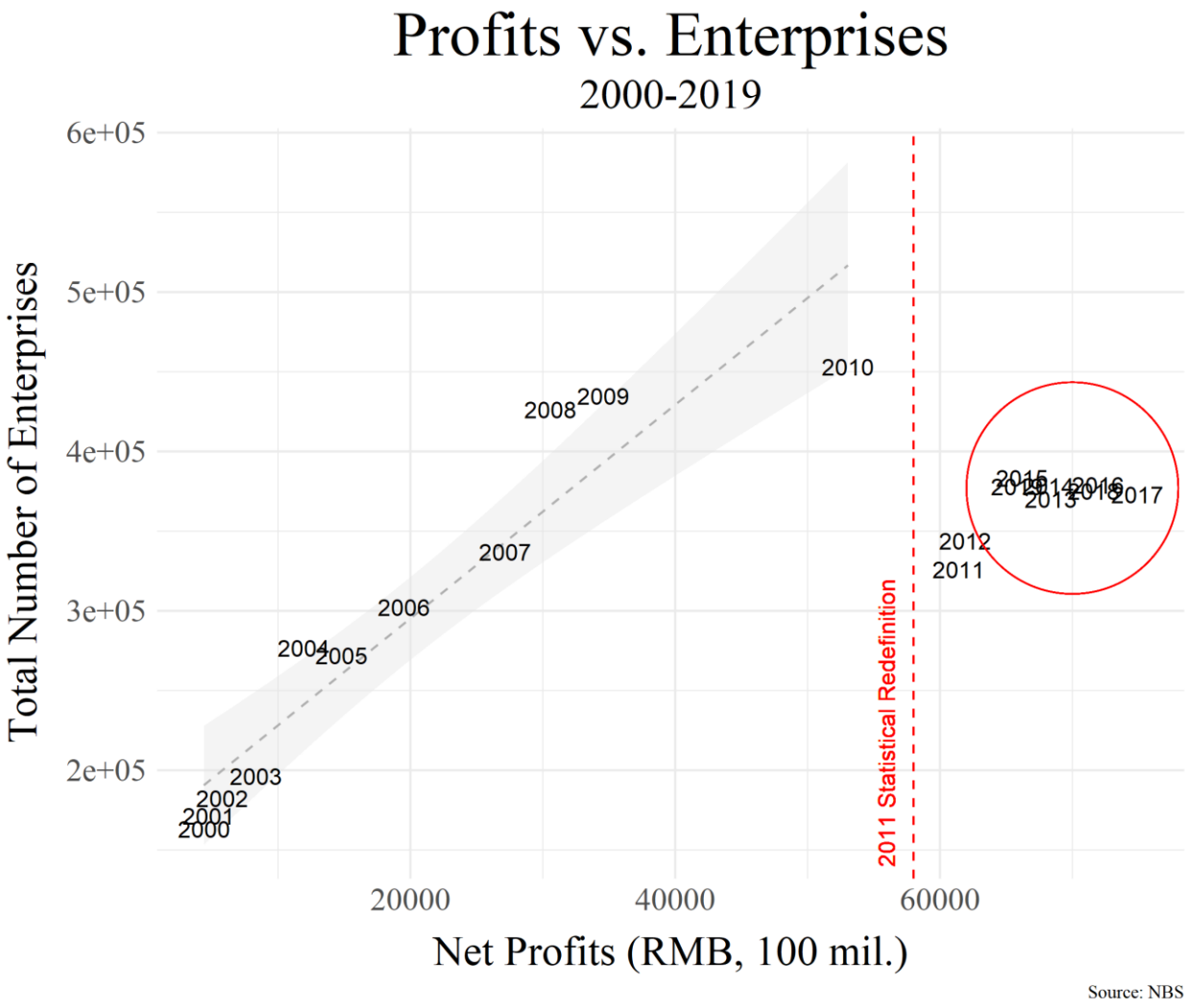



*Figure 3 - 14*

The initial vertical trend is visualized in Figure 3-14 by plotting the regression line for the years 2000 through 2010 under the scatterplot. The printing of the years on the plot shows that growth in both variables tends to move in line with their temporal order. While the 2011 statistical redefinition causes a sudden decline in the number of enterprises, the same vertical trend seems to hold for the years 2011-2013. But the years 2014-2019, circled in red, all hover around the same number of enterprises and show no clear directionality in profit. Though it's somewhat obscured in the printing of the plot, there is no linear left-right movement year by year along the

Net Profit axis. Net Profits are lower in 2019 than any in any other year after 2013. While these trends are each visible separately in the preceding plots of profits and enterprises over time, plotting them against one another more clearly indicates a turn toward stagnation over the course of the 2010s. This will also be visible in the ROP measures below. Meanwhile, it will be decomposed in more geographic detail in the next chapter.

Finally, it will be useful to look at overall growth rates of productivity, output and employment in order to frame the ROP measures within larger trends and to provide some loose comparability between trends in China and those identified in the world's other major economies by Gordon, Benanav and Smith. The main problem in this regard is that China has only twenty or so years of qualitatively comparable data (2000-2019), though it is possible to extend some of these measures back to 1990 without issue. Similarly, the methodological divides between different statistical databases merit side by side comparison. Below, the year-on-year growth rates of major output, employment and productivity variables have been averaged over 5- and 10-year intervals. The source of each variable is listed in parentheses alongside the variable name:

**Mean Growth Rates**

5-Year Intervals

| | Output | | | | Employment | | Productivity | |
|---|---|---|---|---|---|---|---|---|
| Time | GDP (NBS) | GDP (PWT) | Nominal Value-Added (ILC) | Real Value-Added (ILC) | Employment (ILC) | Employment (PWT) | Labor Productivity (ILC) | TFP (PWT) |
| 1990-1994 | 23.51 | 6.24 | 25.57 | 13.73 | 1.98 | 1.60 | 11.51 | 2.10 |
| 1995-1999 | 13.47 | 5.58 | 12.42 | 11.37 | 0.71 | 0.98 | 10.56 | 0.35 |
| 2000-2004 | 12.35 | 8.10 | 12.01 | 10.23 | 0.66 | 0.81 | 9.66 | 2.45 |
| 2005-2009 | 16.67 | 8.88 | 16.89 | 12.79 | 5.15 | 0.50 | 7.34 | 3.94 |
| 2010-2014 | 13.13 | 7.11 | 11.82 | 9.31 | 1.42 | 0.43 | 7.82 | 1.32 |
| 2015-2019 | 8.93 | 3.05 | 7.92[1] | 8.57[1] | -2.37[1] | 0.11 | 11.20[1] | 0.18 |

[1] No Data for 2019

*Table 3-1*

**Mean Growth Rates**

10-Year Intervals

| | Output | | | | Employment | | Productivity | |
|---|---|---|---|---|---|---|---|---|
| Time | GDP (NBS) | GDP (PWT) | Nominal Value-Added (ILC) | Real Value-Added (ILC) | Employment (ILC) | Employment (PWT) | Labor Productivity (ILC) | TFP (PWT) |
| 1990-1999 | 18.49 | 5.91 | 19.00 | 12.55 | 1.34 | 1.29 | 11.04 | 1.23 |
| 2000-2009 | 14.51 | 8.49 | 14.45 | 11.51 | 2.90 | 0.65 | 8.50 | 3.19 |
| 2010-2019 | 11.03 | 5.08 | 10.09[1] | 8.98[1] | -0.27[1] | 0.27 | 9.33[1] | 0.75 |

[1] No Data for 2019

*Table 3-2*

The figures above utilize several variables that are recorded in different ways. The NBS and PWT variables have already been discussed in more detail above. Those demarcated here with "ILC" come from the International Labor Comparisons series composed by the Conference Board and are originally sourced from either the NBS or the China Industrial Productivity Database hosted at the Research Institute of Economy, Trade and Industry, based in Japan

(REITI 2015). As is indicated in the cells with grey backdrops, these series have no observations for 2019, so their final time category in both tables only includes 4 and 9 years respectively, ending in 2018. These variables relate to manufacturing specifically, whereas the PWT and NBS variables relate to the entire economy. The labor productivity measure from the ILC was the only one for which they offered detailed data for China. It is calculated as real value-added per person employed and recorded as an index value, where 2010 = 100.[40] The TFP measure from the PWT is included alongside, for comparison with Gordon.

In general, the same trends are visible across the various measures. In the 5-year series, most output measures show a decline from the '90s through the early '00s, followed by an acceleration in 2005-2009 and then another decline. The 10-year series simplifies this into a secular decline for all output variables other than the PWT GDP series. In every single output measure in both tables, the most recent years have seen the lowest output growth. The largest divergence between the two series is found in the employment measures. It should be noted, however, that the ILC measure applies only to industry, while the PWT measure applies to the entire economy. Thus, we'd expect the ILC measure to be higher in the '00s, since it should pick up the rapid growth of manufacturing in the export hubs, while the PWT measure should be muted by slower employment growth in other sectors. Nonetheless, the two series move in unison in all time categories save 2005-2009, when the growth rate in the ILC series is substantially higher than in 2000-2004, whereas the 2005-2009 period in the PWT series shows

[40] This makes it less granular than the others. It is standard in growth accounting to be able to convert between growth rates of output, productivity and employment using just two of the three, since the growth rate of output minus the growth rate of productivity is equal to the growth rate of employment, and vice versa. However, some of the detail lost to rounding in the formation of the index value for productivity means that the final averaged growth rates don't retain this exact relationship. It would be possible to calculate slightly different productivity values by subtracting employment growth from output growth in the other ILC series. Similarly, it is possible to produce an alternate productivity measure using the PWT values for output and employment, but the reliability of PWT employment data is unclear. Neither is done here, mostly because it would make the table difficult to read.

slower growth than 2000-2004. The general movement observable in both tables is that productivity growth is beginning to exceed output growth, resulting in downward pressure on employment growth which, in the end, turns negative.

This is the pattern identified by Benanav as a marker of "output-led deindustrialization" (2020, p.21). China is possibly just on the cusp of this transformation. If there is a major difference, it is the relatively high level of labor productivity in the final time categories in both tables above, which is likely related to the fact that, in developmental terms, China still lies well below Japan did at the height of its industrial boom. In Benanav's data for roughly twenty-year time segments for the US, Germany and Japan since the 1950s, the consistent trend has been a secular decline or stagnation in productivity growth accompanying a decline in output growth. Since output growth has tended to decline more quickly, productivity growth surpasses it, *even while remaining lower than its historical level*. This is important, because it unambiguously demonstrates that deindustrialization in such conditions is primarily driven by the decline in output growth, not any rise in productivity. This means that "automation" cannot be blamed for the phenomenon, since general industrial overcapacity in manufactured goods and the growth of the surplus population (visible in slowing and then negative employment growth) are occurring not due to but *despite* lower productivity growth. While the inversion between productivity growth and output growth is now evident, productivity growth remains robust compared to previous time periods, at least in the ILC series. This is all confirmed by the relatively stagnant TFP growth throughout the period, peaking in the '00s (especially the latter half of the decade)

before declining rapidly through the '10s. In the 2015-2019 period, TFP growth averages a mere .18 percent.[41]

Each of these figures can be compared to similar values taken from Benanav, Smith and Gordon for the US and other wealthy countries. Benanav uses the same Conference Board dataset to calculate output, productivity and employment year-on-year growth rates for manufacturing in France, Germany, Japan and the US, averaged over three roughly twenty-year time spans: 1950-1973, 1974-2000 and 2001-2017. Japan has the highest growth rates of any country in the series, with output growth in the 1950-1973 period lying at 14.9 percent, similar to that seen in the 1990-1999 and 2000-2009 decades in China. Averaged instead over a 20-year time span, from 1990-2009, the same ILC figures give growth rates of 16.72 percent for Nominal Value-Added and 12.03 percent for Real Value added. Thus, it appears that Chinese output growth in manufacturing was broadly similar in pace between 1990 and 2009 as Japanese output growth in manufacturing between 1950 and 1973. Similar, more subdued paces of growth were seen in Germany (7.6%) and France (5.9%) in those years, with US manufacturing trailing at 4.4 percent. By the 2001 to 2017 period, all countries included in Benanav's overview saw output growth in manufacturing well under three percent, and often much lower (in the US, the figure sat at a mere 1.2%, in France, .9%). These slowdowns began in the 1974-2000 period but grew more extreme in every country except Germany, which was able to mildly raise its growth rates back up to 2 percent from 1.3 percent between the two periods (Benanav 2020 pp.19-21). No measure indicates that China has yet seem a similarly extreme slowdown, even though the most recent 5-year period shows distinctly slower output growth by every measure. The highest is the

[41] It should be noted, however, that in many "emerging" economies TFP growth is strongly correlated with growth in the export sector. For China, this seems to be the case (Lardy 2019, p.14)

NBS GDP figure, which lies at 8.93 percent between 2015 and 2019. The lowest is the deflated PWT GDP figure, which lies at a mere 3.05, on the cusp of similar rates of slowdown seen elsewhere beginning in the 1970s.

Despite this persistently high output growth, however, Chinese productivity growth clearly overtakes that of output in this final 5-year period, mirroring the pattern seen across all advanced economies after 1974. The main distinction seems to be that the hyper-acceleration required to catch up with the advanced economies leaves growth rates higher on average across the board and, so long as a severe economic crisis is staved off, the descent from this peak growth should be somewhat slower. China does, after all, still have substantially more ground to cover to truly "catch up" with the high income countries (its GDP per capita, for instance, is still well within the "middle-income" range). That said, the most recent data clearly indicate that this slowdown is proceeding apace: Real Value-Added of manufacturing, as recorded in the ILC dataset, hovers around six percent growth between 2016 and 2018 (the ILC has no data for 2019 at the time of writing), whereas the figure sat well above ten percent in seven out of the ten years between 2006 and 2015, with relative slowdowns never lasting more than a single year in a row. More significant is the decline in productivity growth as recorded by the ILC, which dropped precipitously from 18.35 percent in 2015 (and just above 10% in 2013 and 2014) to less than nine percent between 2016 and 2018. But, despite this decline, it overtakes the growth rate of manufacturing Real Value Added from 2014 onward, gesturing in the direction of the "output-led deindustrialization" identified by Benanav. Similarly, the growth of services as a share of total output will also see the growth of Baumol's "cost-disease" and will almost inevitably be accompanied by the kind of slowdowns theorized by Smith.

Nonetheless, labor productivity growth remains substantially higher than in all other advanced economies. Smith demonstrates this clearly for the United States, pointing out that, according to the BLS, average labor productivity for the entire US economy was a mere 1.2 percent between 2007 and 2017. Nor is this an artefact of the Great Recession, since growth is even slower (for all but two years, less than 1%) between 2011 and 2017, well into the supposed "recovery." But these are figures for the entire national economy. The ILC figures used above, and those used by Benanav, relate solely to manufacturing. In this regard, the picture in the US is even more grim. Smith points out that five of the seven years between 2011 and 2017 saw negative productivity growth in manufacturing (2020, p.64). Meanwhile, TFP growth (as measured by the PWT) peaked at the same time as output, in the 2000-2009 period, and has declined since. Chinese TFP growth can loosely be compared to the TFP figures used by Gordon, even though he uses much larger (approximately 50-year) time intervals. At its height, in the years from 1920 to 1970, US TFP growth averaged just under two percent. By contrast, in the years from 1970 to 2014, it sat at just over half a percent, and just below half a percent in the earlier period stretching from 1890 to 1920 (2016, p.16).

In a strange way, then, Chinese TFP growth between 1990 and 2019 emulates almost this exact same pattern in miniature, with the peak decade (2000-2009) seeing roughly triple the growth of both the decades preceding and succeeding it. The main difference is that TFP growth is lower in the final decade (2010-2019) than in the earlier one. Today, TFP growth in China appears to sit very near the average experienced by the US economy since 1970. Thus, it seems that even the quasi-magical remainder of the growth accounting equation offers no way out of the overcapacity trap identified by Benanav and Brenner. Despite their high values, labor productivity growth has begun to exceed output growth in China, leading to falling employment.

Accompanied by stagnant TFP growth, these conditions hint less at widespread automation or productive institutional restructuring and more at the type of rapid deindustrialization seen in Japan decades earlier, accompanied by a very similar trade war. The main difference is one of size and scale.

It will be helpful, finally, to clarify all of these the trends by combining the various output and employment variables into averages, which will then allow for the calculation of a separate productivity growth series. In the figures below, all output and employment figures are averaged (this mixes the qualitatively different ILC manufacturing series with the national series of the PWT and NBS, but this mixture is the goal here). A new productivity growth rate has then been constructed using standard growth accounting arithmetic, wherein output growth minus employment growth is equal to productivity growth:

**Mean Growth Rates**

5-Year Intervals

| Time | Output | Employment | Productivity |
|---|---|---|---|
| 1990-1994 | 17.26 | 1.79 | 15.47 |
| 1995-1999 | 10.71 | 0.84 | 9.87 |
| 2000-2004 | 10.67 | 0.74 | 9.93 |
| 2005-2009 | 13.81 | 2.83 | 10.98 |
| 2010-2014 | 10.34 | 0.92 | 9.42 |
| 2015-2019 | 7.12 | -1.13 | 8.25 |

Source: NBS, PWT 10 and Conference Board

*Table 3-3*

**Mean Growth Rates**

10-Year Intervals

| Time | Output | Employment | Productivity |
|---|---|---|---|
| 1990-1999 | 13.99 | 1.31 | 12.68 |
| 2000-2009 | 12.24 | 1.77 | 10.47 |
| 2010-2019 | 8.80 | 0.00 | 8.80 |

Source: NBS, PWT 10 and Conference Board

*Table 3-4*

The basic patterns are clarified somewhat when the trends are averaged across the different measures. Here, it seems more apparent that China lies on the cusp of the inversion between output and productivity growth observed by Benanav across the advanced economies. The 10-year series seems to demonstrate this fact most clearly. In this period, employment growth essentially zeroes out, since the ILC records slightly negative employment growth in manufacturing while the PWT records slightly positive employment growth in the entire national economy. In the 5-year series, employment growth turns negative in the final years.

# Section 2 - The Rate of Profit in China

With these broader macroeconomic trends clear, I will now proceed to a comparison of all existing ROP and ROR estimates for China, to which I will add my own, drawn from several sources. As a first step, it will help to simply compare all these variables back-to-back on a single chart. Qualitative differences between the measures will be symbolized using different line types: solid lines will be standard ROP measures using aggregate output variables in the numerator and including the total wage bill in their denominator; dashed lines will be conventional ROR measures, which exclude the wage bill; and dotted lines will be the variables that use net profits of industrial enterprises in the numerator rather than an aggregate output measure. All three net profits ROP measures, as well as the one using the Value-Added of Industry, are industry-specific measures. On this first chart, there is no visual distinction between industry-specific measures and those of the entire national economy:

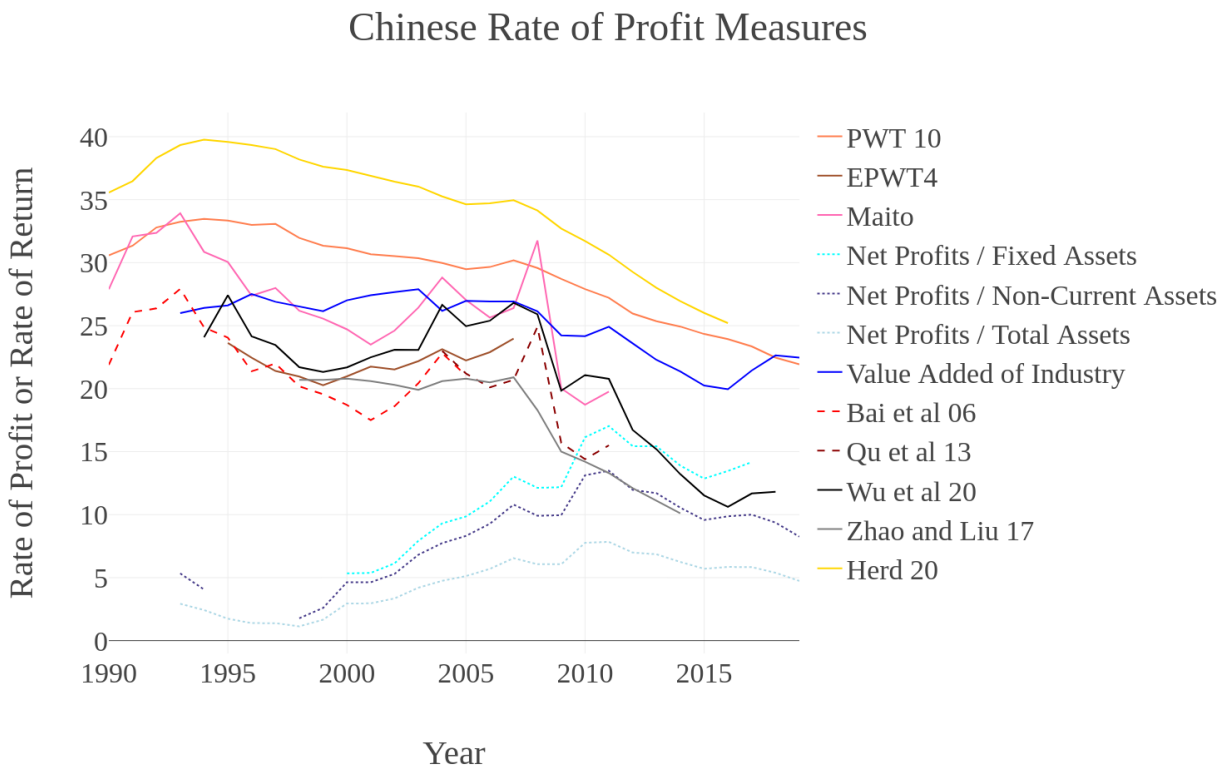



*Figure 3-15*

The general trend is clearly downward for the bulk of measures, though the dotted series at the bottom using the Net Profits variable are more clearly divisible into a period of rising profitability from the late 1990s to around 2011, followed by a gradual decline. Overall, every measure either exhibits a rise, relative stagnation or a very mild decline through the 2000s and undergoes a stronger downturn between 2008 and 2011. From here, the measures diverge, with only a few hinting at some sort of plateau or mild recovery of profitability in the most recent years (namely the Value Added of Industry measure and that of Wu et al 20). Most see the decline continue. It's also worth noting that the spreads between different NBS industrial capital stock stand-in variables in the Net Profits measures are basically as we would expect them to be. All move in unison even though their distance from one another widens slightly over time.

Similarly, they retain the same vertical relationship, with fixed assets (the smallest capital stock stand-in) producing the highest value and total assets (the largest stand-in) producing the lowest value.

It will also be helpful to compare these measures against a few others that are not included in this series. The first are the two measures that utilize the capital stock series from the Chinese literature (Wang et al 2017 and Shan 2008), which produce inordinately high values due to the unaccountably low value of capital stock:

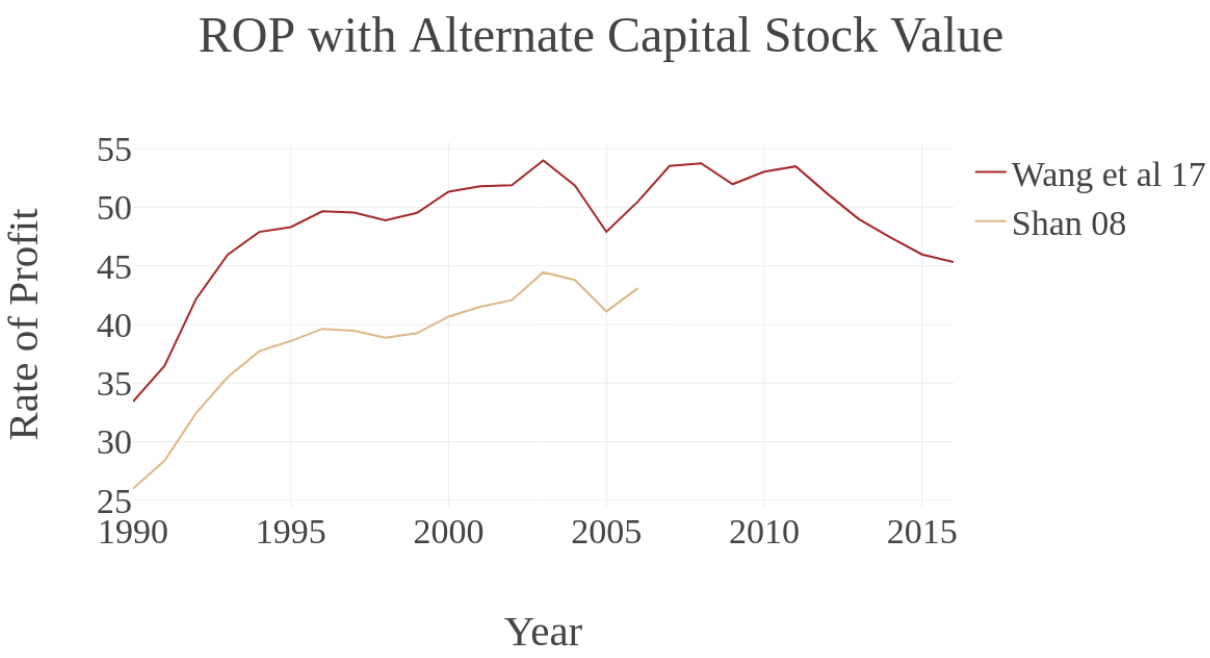



*Figure 3-16*

Even though these measures are not perfectly comparable to the others, they show the same general trend displaced to a higher absolute value. Since both use the same basic methodology for calculating the capital stock, they essentially mirror each other. In both, profitability rises

from the mid-1990s through the early ‘00s, dipping briefly in the middle of the decade. In the longer series, profitability recovers, dips very mildly at the time of the Great Recession, and then follows the pattern exhibited by all other measures above in declining after 2011.

Another measure of interest is the pre-made ROR measure included in the PWT. Since the PWT has tended to apply more aggressive deflation to its Chinese output series over time, we can visualize both the earlier ROR measure from the PWT 9.3 as well as the most recent one from the PWT 10 in order to see if there is any fundamental change in the trend:

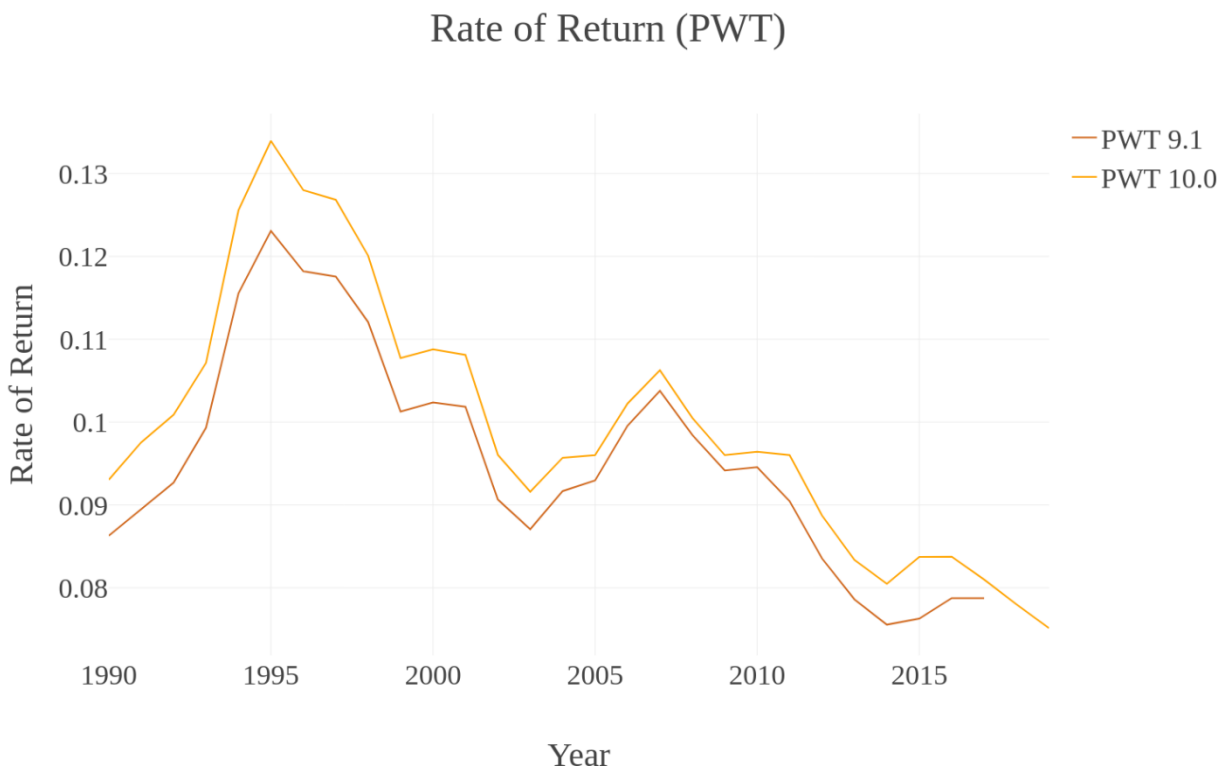



*Figure 3-17*

As can be seen above, the differences in deflation have essentially no effect on the trend. The main divergence is simply that the more recent values from PWT 10 tend to result in a slightly higher ROR. Above, the PWT 10 values are used in their raw form, producing a much more mild overall pattern. Here, the ROR is calculated behind the scenes using the fully transformed version of GDP utilized by the PWT, deflated, converted into internationally inflation-adjusted currency and standardized using comparative purchasing power parity data. But the ultimate trend here is essentially the same as that seen in the more granular measures above: profitability falls in the late 1990s and into the early 2000s, rises in the mid to late 2000s, and then begins a decline at the end of the decade. There is only a mild slowing of this decline in the mid-2010s, and it resumes in the most recent years.

In order to make the overall trends in Figure 3-15 more clear, it is possible to produce several mean measures that average across their differences. This requires some manipulation, however. Missing years in certain series (particularly the net profit measures) will create a problem since they sit lower than the others, creating artificial dips and spikes in the mean. This is mostly an issue for the early years, where some industrial asset data are missing, and for the final couple years, where asset data are missing and where certain series end early. While some data extends through 2020, enough of the measures cut off before this that it makes more sense to end the overall series in 2019. The rules for interpolation here are relatively straightforward, though they are attuned to each individual variable. For any areas where only a single year or two is missing and the values on either side of it are present, these framing values are used to calculate the missing values, treating the missing values as either simple (if one) or weighted (if two) means of the framing values. For any areas where larger stretches are missing, a qualitatively similar measure which has both overlapping data and data in the missing years is

chosen and weights are calculated using the average difference between the two measures in the five nearest years for which the two measures have overlapping coverage. These weights are then multiplied against the values for the series with greater data coverage, producing interpolated values for the missing years in the series with less data coverage. In all cases, interpolated values are not used to weight further interpolations. Finally, since the Bai et al 2006 and Qu et al 2013 ROR measures are essentially identical, they are merged to form a single series.

In the figure below, several mean ROP measures are calculated using the interpolated data, covering all thirty years from 1990 through 2019. The black line records the mean for all measures. The blue dashed line records the mean for all national measures (i.e. excluding those using Value Added of Industry or Net Profits). The final two dotted lines, both in shades of red, only average across the ROP measures calculated for industry specifically. The first, in light red, averages across all the ROP measures using values specific to industry (i.e. all three Net Profits Measures alongside the Value Added of Industry measure). The second, in dark red, simply averages the three measures that use the Net Profits of Industry. Since these follow one another closely, the result is ultimately similar to the trend seen in the middle Net Profits value in the first plot (that using Non-Current Assets in its denominator):

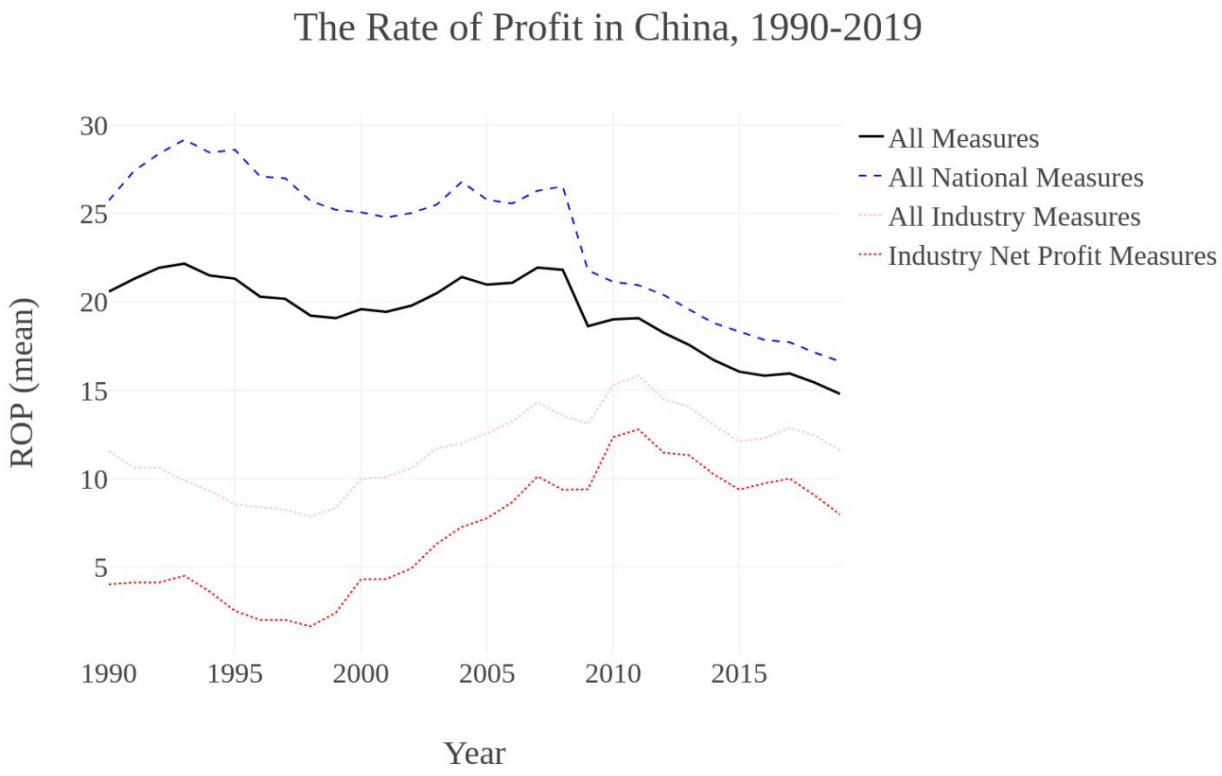



*Figure 3-18*

The trends in the earlier series are even more evident here. In all measures and all national measures, the ROP declines somewhat in the early 1990s, recovers from the late 1990s through to the late 2000s, and then begins a more or less continuous decline from around the time of the Great Recession onward. In the industry-specific measures, the same trend is evident but the increase in profitability throughout the 2000s is emphasized, a recovery is visible after the brief dip of the Great Recession and the decline truly begins only in 2011. Altogether, the mild upturn visible in the Wu et al 20 and Value Added of Industry measures in Figure 3-15 amount to only a mild slowing of the decline in three of the four measures here.

More detailed tables showing the exact values plotted here as well as the exact values of all original variables plotted in Figure 3-15 are available in the appendix. But, before moving on to a decomposition of the rate of profit, it will first be helpful to measure its growth rate in a fashion similar to that used for output, employment and productivity above. Though the relative trends are already visible in Figure 3-18, it will be helpful to quantify them:

**ROP Growth**

1990-2019, 5-Year Intervals

| | | | Industry | |
|---|---|---|---|---|
| Time | All Measures | National Measures | Industry Measures | Net Profit Measures |
| 1990-1994 | 1.11 | 2.59 | -5.19 | -1.95 |
| 1995-1999 | -2.33 | -2.36 | -2.06 | -4.37 |
| 2000-2004 | 2.35 | 1.25 | 7.71 | 27.10 |
| 2005-2009 | -2.53 | -3.75 | 1.92 | 5.65 |
| 2010-2014 | -2.13 | -2.90 | 0.27 | 2.73 |
| 2015-2019 | -2.37 | -2.42 | -2.18 | -4.67 |

Source: Author's Calculations

*Table 3.5*

**ROP Growth**

1990-2019, 10-Year Intervals

| Time | All Measures | National Measures | Industry | |
|---|---|---|---|---|
| | | | Industry Measures | Net Profit Measures |
| 1990-1999 | -0.80 | -0.16 | -3.45 | -3.30 |
| 2000-2009 | -0.09 | -1.25 | 4.81 | 16.38 |
| 2010-2019 | -2.25 | -2.66 | -0.95 | -0.97 |

Source: Author's Calculations

*Table 3-6*

The quantification of the trends makes their basic dynamics a bit more clear. Averaging across all ROP and ROR measures, profitability declines in all 5-year intervals except 1990-1994 and 2000-2004. In the 10-year intervals, these brief increases are washed out by countervailing movements in their neighboring 5-year intervals, to produce mild declines in 1990-1999 and 2000-2009 and then a more substantial decline between 2010 and 2019. The national measures essentially follow this same pattern. The measures that focus solely on industry, however, diverge in a few key respects. The most notable is the way in which both clearly show sharp increases in profitability in the 2000-2009 period, clearly concentrated in the 5-year series in 2000-2004. As above, these series seem to be capturing the boom in the sunbelt export industries and the bump in profitability created by the liquidation of obsolete firms in the northeastern rust belt. Similarly, both show only a mild decline in 2010-2019. But the decline is muted here because of 2010-2014 sees slight increases in profitability in both industrial measures contrasting with declines in the others over the same years. By 2015-2019, however, the industrial measures also see clear declines in profitability.

# Section 3 – Decomposing the Rate of Profit

**Capital-Labor and Output-Labor Ratios**

It is now possible to disaggregate the ROP measures in order to explore both the underlying capital-labor and output-labor ratios as well as sectoral differences. This will then set the stage for the next chapter, where I will explore geographic divergences in the ROP and compare these to geographic trends in enterprise location. The most common method of decomposing the ROP measure is to divide it out into an "organic composition of capital" component (C/V, in value terms) and a "rate of surplus value" component (S/V, in value terms). The relative movements of these component measures should inform us about which variables are driving the change in the ROP. The organic composition of capital is intended as an estimate of the overall level of capital-intensity within production by capturing the growth of the share of total social capital devoted to constant capital costs (namely plant and equipment) relative to the share of total social capital paid to variable capital (i.e. labor). Using conventional statistics, this value is more accurately just a capital-labor ratio. Its precise meaning depends on the stand-ins used to measure the idea of constant and variable capital.

Similarly, the rate of surplus value is intended as an estimate of the overall level of exploitation in production, since it captures the growth in total social surplus value relative to the amount of total social capital expended on labor. This is also called the rate of exploitation for this reason, but it's important to note that "exploitation" does not necessarily indicate any conclusion about the actual prevailing labor practices. It is simply an observation that relatively

more value is being produced per unit of labor input. If the rate of exploitation increases without an attendant increase in the organic composition of capital, this indicates that the increased output is not attributable to any new investments in machinery but instead to some change in the labor regime. Generally, these changes will not be beneficial to labor, since the simplest ways to get more output per cost-unit of labor input is to lower wages, lay off excess workers, encourage faster or harder work, lengthen the working day, etc. But it is nonetheless incorrect to equate increases in the rate of surplus value with increases in "exploitation" in the conventional meaning of the word, since the rate can also conceivably be increased by other means and since the S/V can increase alongside C/V, in which case it is also tracking increasing productivity due to mechanization. Using conventional statistics, this measure is more accurately described as an output-labor ratio. As above, its precise meaning will depend on the stand-in variables used.

First, I will compare the Capital-Labor Ratios from both my own measures and from Wu et al 2020 and Zhao and Liu 2017, since these authors list an organic composition of capital ratio alongside their rate of profit measurements:

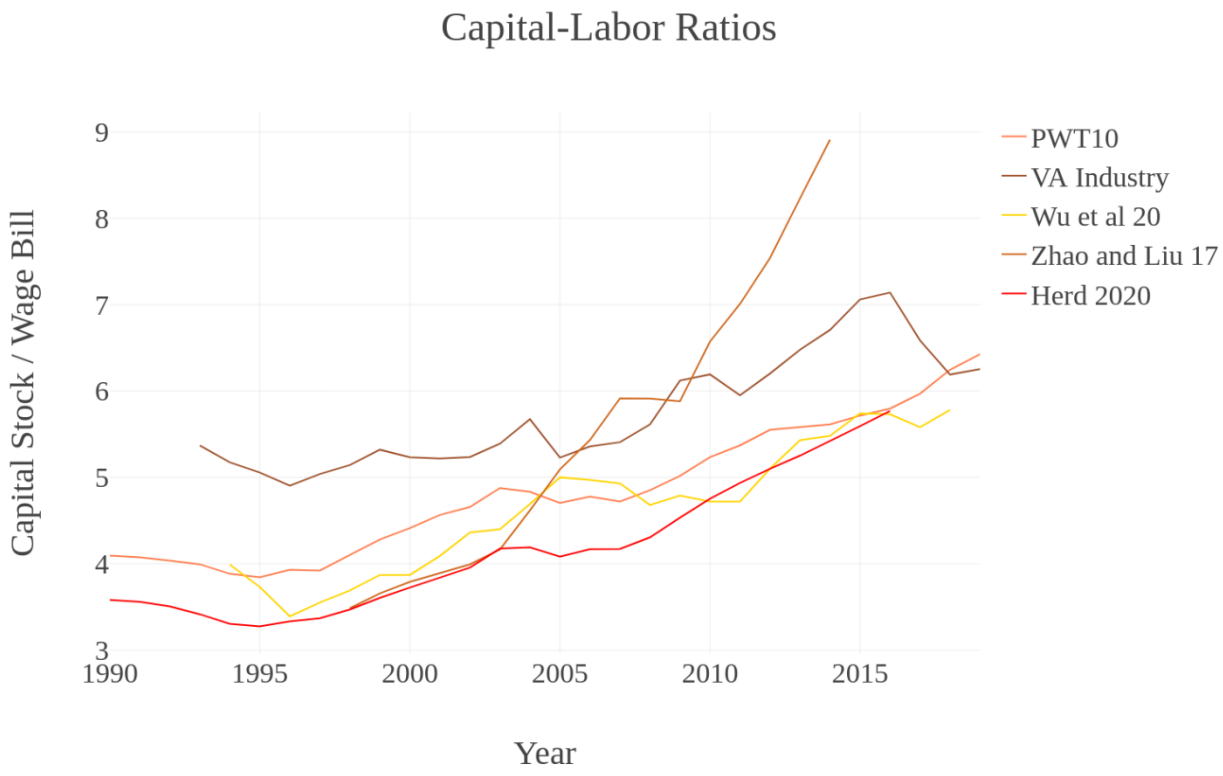



*Figure 3-19*

In the above figure, the PWT10, VA Industry and Herd 2020 ratios are my own. The PWT series divides the PWT10 capital stock variable by the labor share of output (described above, using a combination of the PWT series and the Bai and Chen 2010 series) multiplied by the PWT10 GDP variable. The VA Industry series divides the Total Assets of Industry by the labor share multiplied by the Value Added of Industry, both measures from the NBS. The Herd 2020 series divides Herd's total capital stock measure by the labor share multiplied by his total GDP measure. All produce essentially identical upward trends, though the Zhao and Liu 17 measure diverges upward from the others in the final years from 2010-2014. Most measures show an upward trajectory in the mid-1990s through the mid-2000s, followed by a brief plateau in the late-2000s, and then a resumption of the upward movement sometime between 2008 and 2011.

In Wu et al 2020 and the VA Industry measure, the last few years show a flattening or decline. But even in VA Industry, where that decline is most pronounced, the ratio in 2018 and 2019 lies above where it sat at the beginning of the decade, and well above its average over the preceding decade.

The Output-Labor Ratio can be visualized in the same way. But here, since the same labor share variable is used in all my measures, the PWT and NBS series will all show the same trend in the Output-Labor Ratio. By contrast, Wu et al 2020 and Zhao and Liu 17 use completely different labor share variables which result in different estimates of the wage bill:

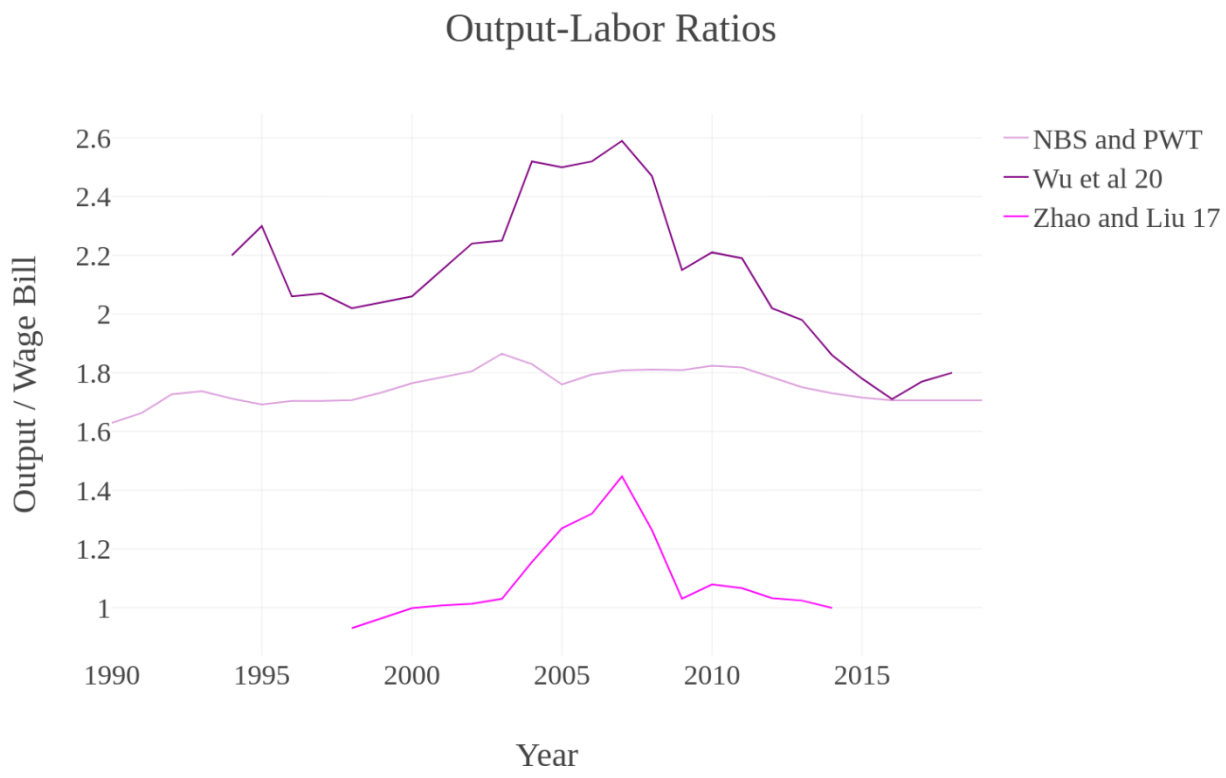



*Figure 3-20*

At first, it appears as if there is a more marked divergence between these three series. This is, however, mostly an artefact of the different wage bill measurements—though it should be noted that Zhao and Liu's measures are suspiciously low in several years. Overall, the series actually show the same general pattern. Even if we disregard the Zhao and Liu measure to just compare the figure from Wu et al to my own output-labor ratios, the main difference lies in the extremity of the spikes, not in the overall pattern. When scaled alone, my measure has a similar shape, peaking in the 2000s and declining over the 2010s. This is visible above, even though the scaling flattens the curve substantially.

There are two minor differences between my measure and that of Wu et al, however. First, while both see two peaks in the ratio over the course of the 2000s, the Wu et al measure shows the second peak in 2007 to be the taller of the two, while my measure shows the first peak in 2003 to be the taller of the two. Second, the Wu et al measure drops sharply after 2008, recovers slightly between 2009 and 2010, and then declines thereafter. In contrast, my measure shows very little decline in the late 2000s, with the real downward movement beginning after 2010. Taken together, it seems that output was increasing per cost-unit of labor input throughout the 2000s but began to decrease in the 2010s. But, again, this trend can only be understood through comparison with similar trends in the capital-labor ratio and the profit rate. Eyeballing this comparison from line charts of time series will only hint at these relationships. Instead, it will help to visualize the absolute change of each for several different time intervals, comparing the national measure derived from the PWT, the industrial measure using Value-Added of Industry (for these two, the output-labor ratio will be the same) and the series computed by Wu et al 2020.

Below, rather than average annual growth rate (which better captures some of the "bumpiness" in a trend over time, and which is used in all other growth rate measures in this and the subsequent chapter), I use the straight-line percent change method with different starting and ending years. This is because I want a simpler comparison here of the absolute movement of the series from low to high or high to low that abstracts from minor spikes and dips that lie between time periods. Those details are already visible in the charts above and are not necessary to the comparison here. Similarly, I'm most interested in the long-run trend, rather than in comparing 5- or 10-year blocks to one another (as in the other growth rate measures in this and the subsequent chapter). Thus, I will compare the variables in lengthening time intervals with the same start point: 2000 to 2005, 2000 to 2010 and 2000 to 2017. These will be accompanied by a final comparison looking only at the 2010s (i.e. 2010-2017). While a few of the measures can be extended to 2018 or even 2019, to retain comparability I will end all series in 2017, the last year for which data is available for all measures:

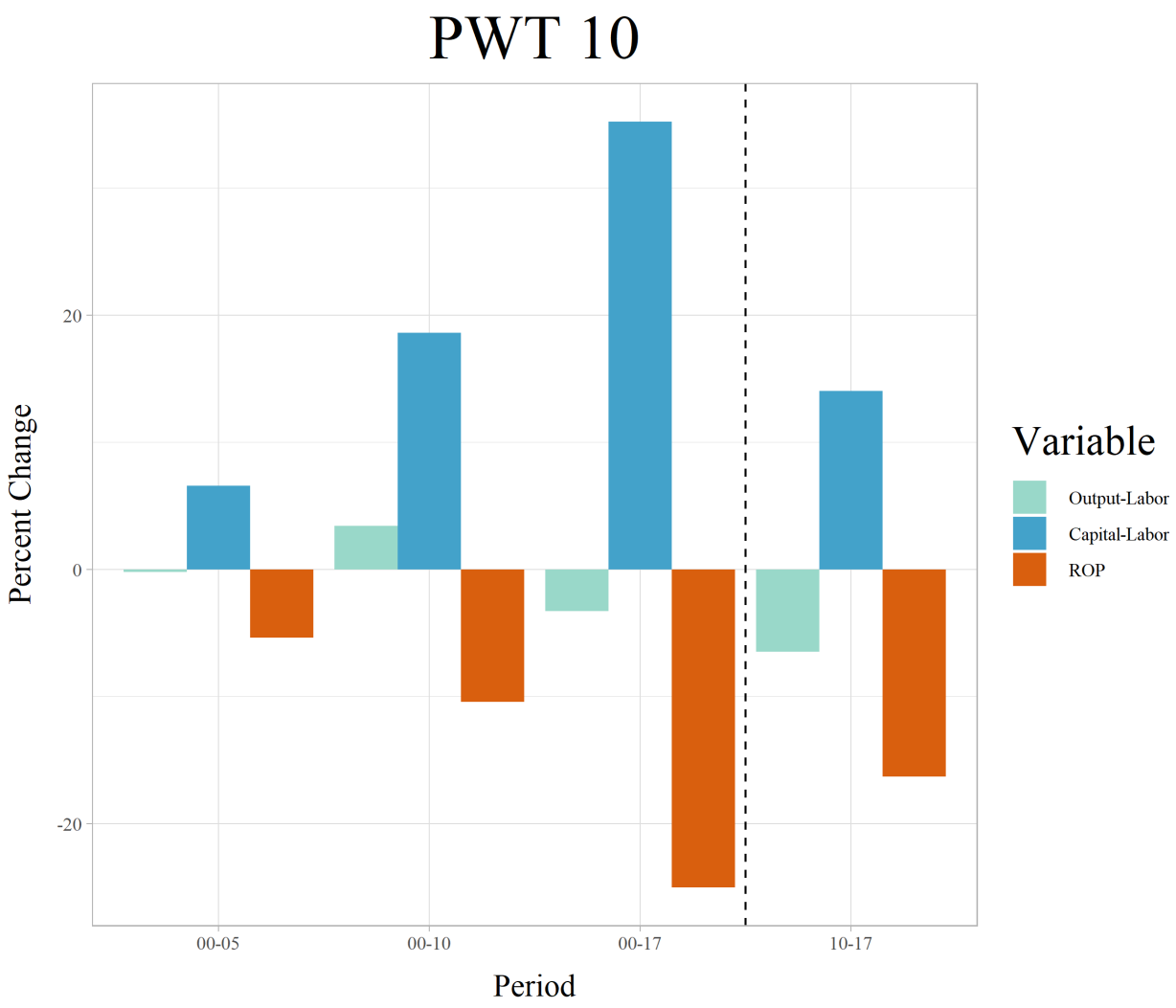

PWT 10
Percent Change
20
0
-20
00-05
00-10
00-17
10-17
Period
Variable
Output-Labor
Capital-Labor
ROP

*Figure 3-21*

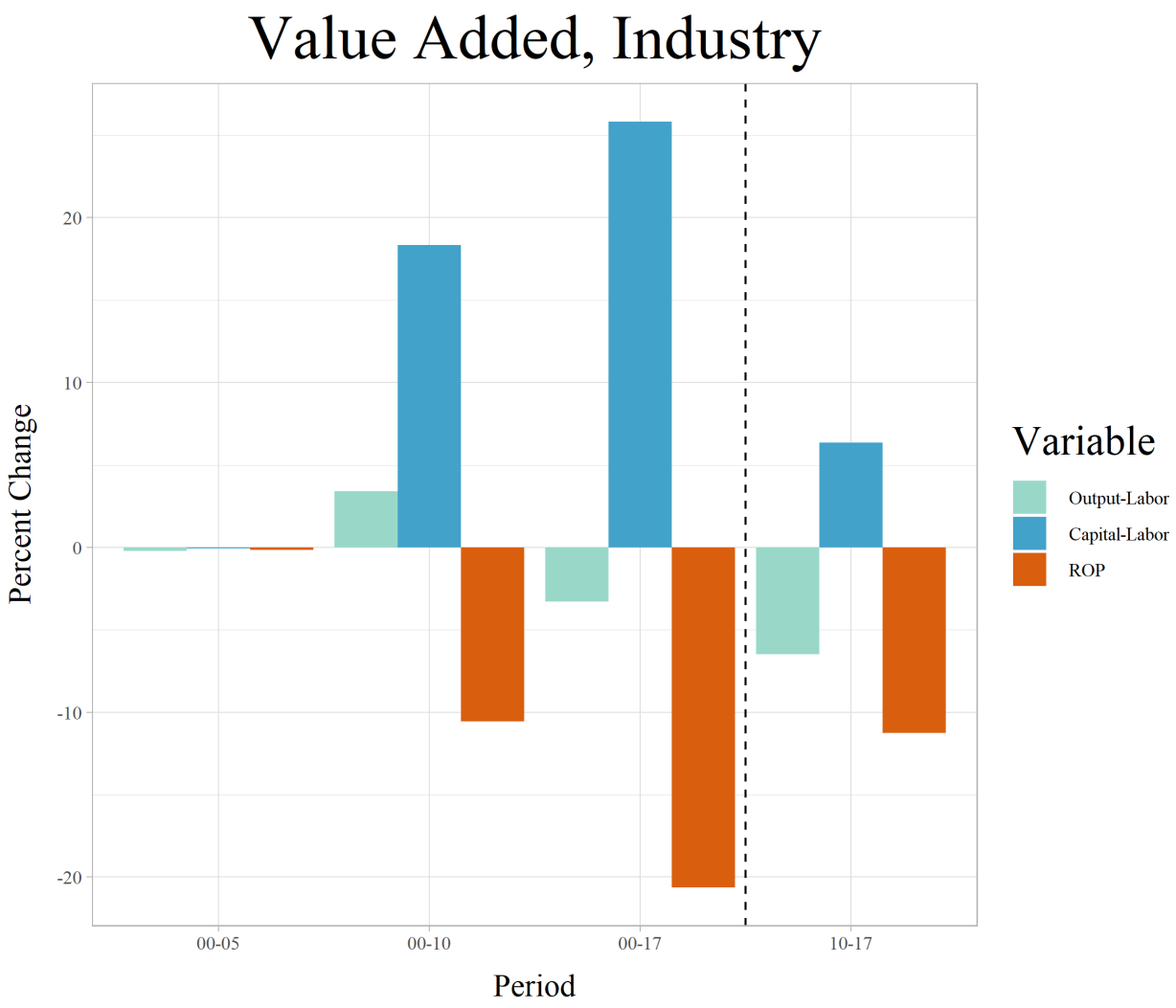

Value Added, Industry
Percent Change
20
10
0
-10
-20
00-05
00-10
00-17
10-17
Period
Variable
Output-Labor
Capital-Labor
ROP

*Figure 3-22*

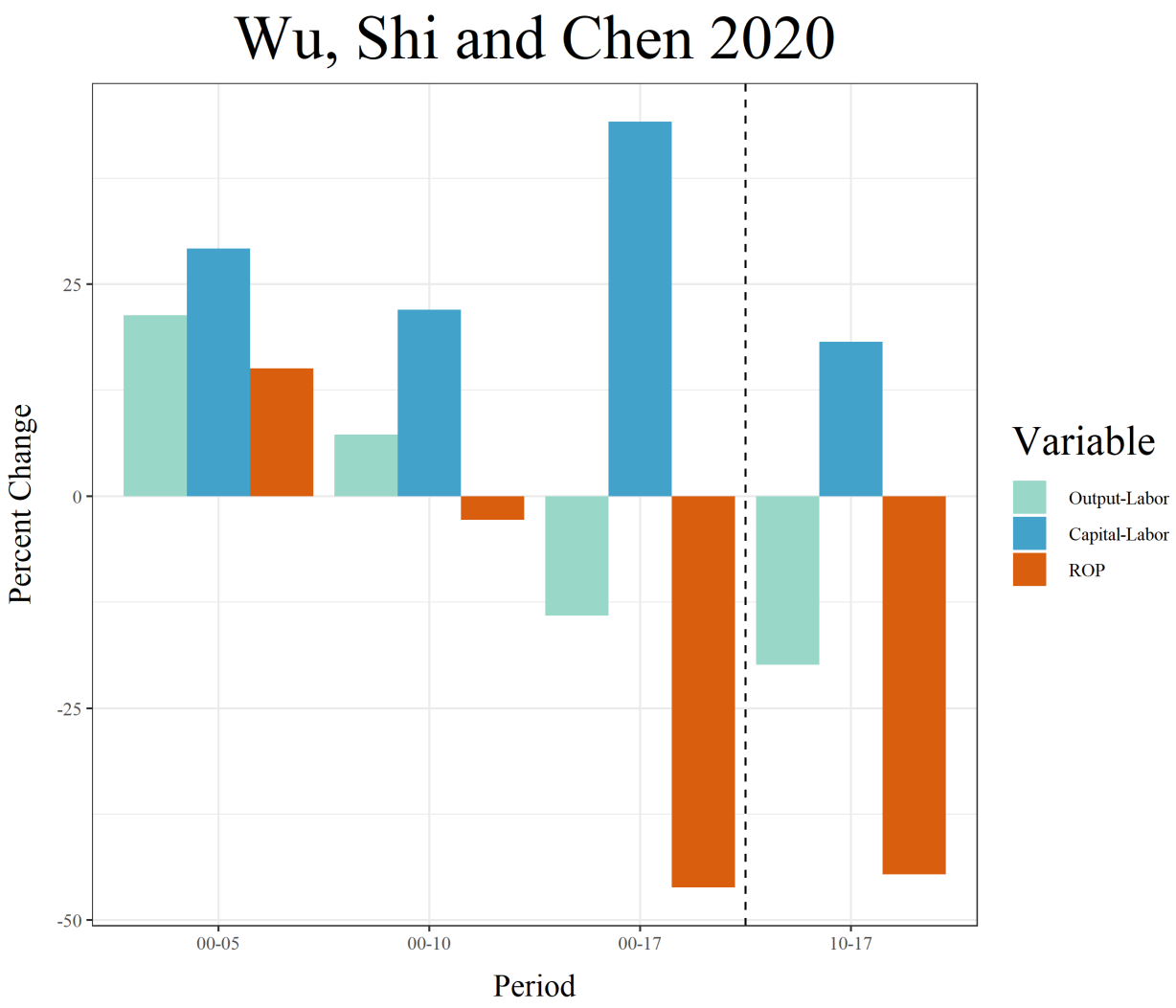



*Figure 3-23*

Very similar patterns are visible in each and the series show almost identical long-run patterns over the entire time frame (2000-2017). Differences are concentrated mostly in the earliest years of the series (2000-2005), with the PWT showing stagnant growth in the output-labor ratio, mild growth in the capital-labor ratio and a mild decline in the ROP, whereas the Value Added of Industry series shows stagnant growth in all and the Wu et al 2020 series shows growth in all, with the capital-labor ratio growing the fastest. Over the first decade, the three series show the exact same pattern, only in different degrees: the output-labor ratio and capital-

labor ratio both grow, with the capital-labor ratio growing faster, while the ROP sees a mild decline. Over the entire series, the key change is that the output-labor ratio sees negative growth, the capital-labor ratio grows substantially and the ROP declines substantially. It is clear from the final time 7-year time period, however, that this trend is concentrated in the 2010s.

Overall, these figures seem to hint that ROP growth (or more mild decline) in the early years of the new millennium was undergirded by a generally rising output-labor ratio, even while it was depressed somewhat by the rising capital-labor ratio in the same years. By the 2010s, the output-labor ratio declines and the capital-labor ratio increases, driving a much more substantial decline in the ROP. This is consistent with the trends observed in the various time series charts above, which show the output-labor ratio peaking in the 2000s, the capital-labor ratio increasing from 2000 onward, and profitability mostly stagnant or growing mildly in the 2000s and falling thereafter.[42] It seems, therefore, that the recent declines in profitability in the Chinese economy are attributable to both a rising ratio of capital costs to labor costs and an inability to increase the output-labor ratio, the decline in which likely reflects rising labor costs, at least in part. This, of course, is linked to the demographic trends and the "capitalist law of population," which takes on its conventional expression in the question of whether or not China has reached the Lewis Turning Point. Not only do capital costs seem to be increasing relative to the costs of labor, but also relative to total output, as is visible in Herd's capital-output ratios, visualized above in Figures 3-6 and 3-7. While the trend has accelerated over the 2010s due to the contributions of

[42] The Net Profits measures show a more distinct increase in profitability in the 2000s, but since these measures use profits rather than total output in the numerator they cannot be decomposed in the same way and are not compared here. This is, however, what would be expected based on the decomposed trends in the other measures, presuming that the output-labor ratio would be higher in industry if we were to use a labor share variable attuned to the particular division of output in industry. In this sense, the Value Added of Industry decomposition in Figure 3-22 likely understates growth somewhat in both the output-labor ratio and the rate of profit.

housing and infrastructure, the capital-output ratio of business also grew steadily from the mid-2000s onwards, only decreasing slightly in the last year in the series (2016). This all seems to confirm that declining profitability in China is linked to increasing mechanization and/or scale of production and an inability, due to the "problem of population," to drive down labor costs.

**Size and Ownership**

In the section above, the ROP equation itself is quantitatively decomposed into capital-labor and output-labor ratios. Here, I will qualitatively decompose the industrial ROP and its component variables in order to compare them by size and ownership structure. As above, I'll begin with the component variables. All series are portrayed here from 2001 through 2018, due to more limited data availability. Whenever the statistical redefinition of the "above designated size" cutoff for industrial enterprises might affect the trend, I have portrayed this redefinition as a break in the series.

Cutting the data up into qualitative categories requires some more intricate knowledge of exactly what these categories capture. This is especially important given the status of so-called "state-owned" (国有) enterprises. Throughout, the relevant categories will be the following, each derived from the universe of enterprises "above a designated size" (规模以上). The first table provides formal registration type according to the enterprise classification system. All enterprises except for foreign-funded enterprises are domestic enterprises registered in China. The second includes classification by size and type of industry. Only classifications used here are included in these tables. The first is loosely based on Lardy 2014 (pp. 64-65, Tables 3.1 and 3.2) with more

detailed definitions drawn from Holz (2013, pp.8-9, Table 2) and from the official NBS data definitions (NBS 2012):

| English Name | Chinese Name | Description |
|---|---|---|
| All Industry | 工业 | This is the same variable as used above, it includes all industrial enterprises “above a designated size.” In 2019, there were 377,815 registered enterprises, including both domestic- (all categories below except for the last) and foreign-funded. |
| Private Industry | 私营工业 | This includes all ownership types registered as private, defined as being established by a natural person or majority owned by a natural person. But the “designated size” cutoff excludes many smaller enterprises, the vast majority of which are private, as demonstrated by Lardy (2014). This variable therefore understates the total number of private enterprises. The understatement is muted for certain variables, however, since enterprises above the designated size tend to account for a greater share of output, assets, profits, etc. |
| State Industry | 国有工业 | This includes all enterprises registered under the 1988 SOE Law. These are the remaining “unreformed” state enterprises that have not adopted a more modern corporate structure. The measure excludes entirely state-owned limited liability and shareholding corporations as well as any other mixed-ownership categories, including enterprises jointly owned by multiple SOEs. This is important, because most state ownership today takes these more modern corporate |

| | | |
|---|---|---|
| | | forms. There is a separate, newer measure that captures "state owned and state controlled companies," but it overlaps with too many other categories to be useful here. |
| Limited Liability Companies (LLCs) | 有限责任公司 | While the name seems to imply that it records limited liability companies in the same way that the above categories record private and state enterprises, in fact, the category includes all enterprises registered as LLCs *except for* those specifically registered as Private LLCs. This general LLCs category *does*, however, include "State Sole Funder Corporations," where a state entity is the only funder of the company (they compose about 5% of the LLC category). Most of the category (95%) seems to be LLCs with more mixed ownership, including both private and state investors. Though it might be presumed that this measure excludes instances where the private investors wield a controlling share (as assumed by Holz 2013, p.8), Lardy (2014, p.67) argues that this is not entirely the case. Some firms with majority or dominant private ownership are simply not registered as Private LLCs for a number of reasons, the most common being that they wanted early access to the legal status of an LLC prior to the Private LLC category coming into existence. By the numbers, however, most LLCs with majority or dominant private ownership are categorized in Private LLCs below. The LLCs category should be read as a mixed category defined by a spectrum of *de facto* control and including a large share of state enterprises reformed into modern corporate structures. |

| | | |
|---|---|---|
| Joint Stock Companies (JSCs) | 股份有限公司 | Similar to the above, this excludes most solely or majority private JSCs but includes solely state JSCs. This is not as evident in the numbers, but it is explained in Holz 2013 p.8. The relative share of state firms is likely higher. |
| Private LLCs | 私营有限责任公司 | These are LLCs that are privately owned. Presumably, this means that they are companies wherein private investors wield a controlling share. In 2019, there were far more enterprises listed as Private LLCs (223,656) than as just LLCs (75,513), due to the distinction explained above. |
| Private JSCs | 私营股份有限公司 | These are JSCs that are privately owned. They work similar to the Private LLCs category above. But here the number of companies listed in 2019 as Private JSCs (9,966) was not so far from the number listed as just JSCs (11,557). |
| State Sole Funded Corporations | 国有独资公司 | These are any companies solely funded by state investment. This does not include mere majority or dominant investment, but instead companies that are "wholly owned" or "solely capitalized" (独资) by the state, including by other state-owned companies. |
| Domestic-Funded Industry | 内资工业 | These are all domestically funded enterprises. It includes all the registration types listed above. In 2019 these were the vast majority of all enterprises (334,227). It excludes enterprises registered in Hong Kong, Taiwan and Macao. |
| Foreign-Funded Industry | 外资工业 | These are all foreign-funded enterprises, *excluding those registered in Hong Kong, Taiwan and Macao*. In 2019 there were 23,544 such enterprises. Although the Hong Kong, Taiwan and Macao figures are recorded separately and compose an equally substantial share |

| | | |
|---|---|---|
| | | (20,044 in 2019) they will not be used on their own below. They are, however, included in the “All Industry / 工业” category at the top. |

*Table 3-7*

| English Name | Chinese Name | Definition |
|---|---|---|
| Small | 小型 | These are small-size enterprises, defined according to the nature of production using the methodology explained in NBS 2002. Note, however, that these are still enterprises “above a designated size,” meaning that “small” here excludes all family and individual companies (个体户) and any firms that make less than 5 million RMB from their main business between 1998 and 2011 and 20 million RMB thereafter. |
| Medium | 中型 | These are medium-size enterprises, defined according to the nature of production using the methodology explained in NBS 2002. |
| Large | 大型 | These are large enterprises, defined according to the nature of production using the methodology explained in NBS 2002. |
| Light Industry | 轻工业 | These are enterprises producing consumer goods and hand tools, as explained in the general definitions laid out in NBS 2012. |
| Heavy Industry | 重工业 | These are enterprises producing capital goods, including raw materials as well as firms specializing in the repair of equipment used in such production. |

*Table 3-8*

First, it will help to scale the data according to the absolute numbers of each enterprise in each category, as well as their shares of total enterprises. The following figures are basically the same as Figure 3-12 above, but disaggregated into the various categories listed in Tables 3-7 and 3-8.. They are then followed by charts showing the same categories calculated as a share of all enterprises above the designated size:

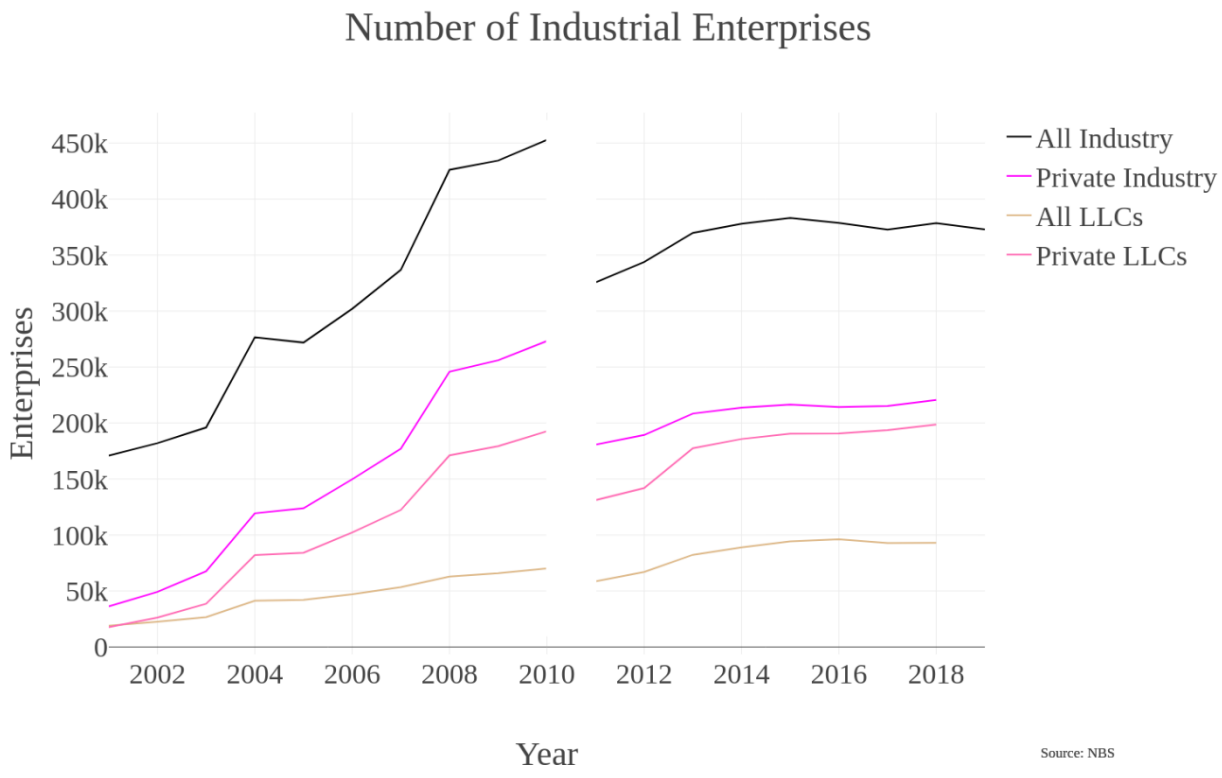



*Figure 3-24*

In this first chart, we see all the largest enterprise categories, scaled against the total number of industrial enterprises above the designated size for the entire country. Private Industry and Private LLCs are clearly the largest. But the mixed All LLCs category is also substantial. In moving from one chart to the next, it is important to note the changes in the y-axis. Here, All LLCs peaks at just under one hundred thousand enterprises in 2018. The following figure visualizes the lower range of the y axis:

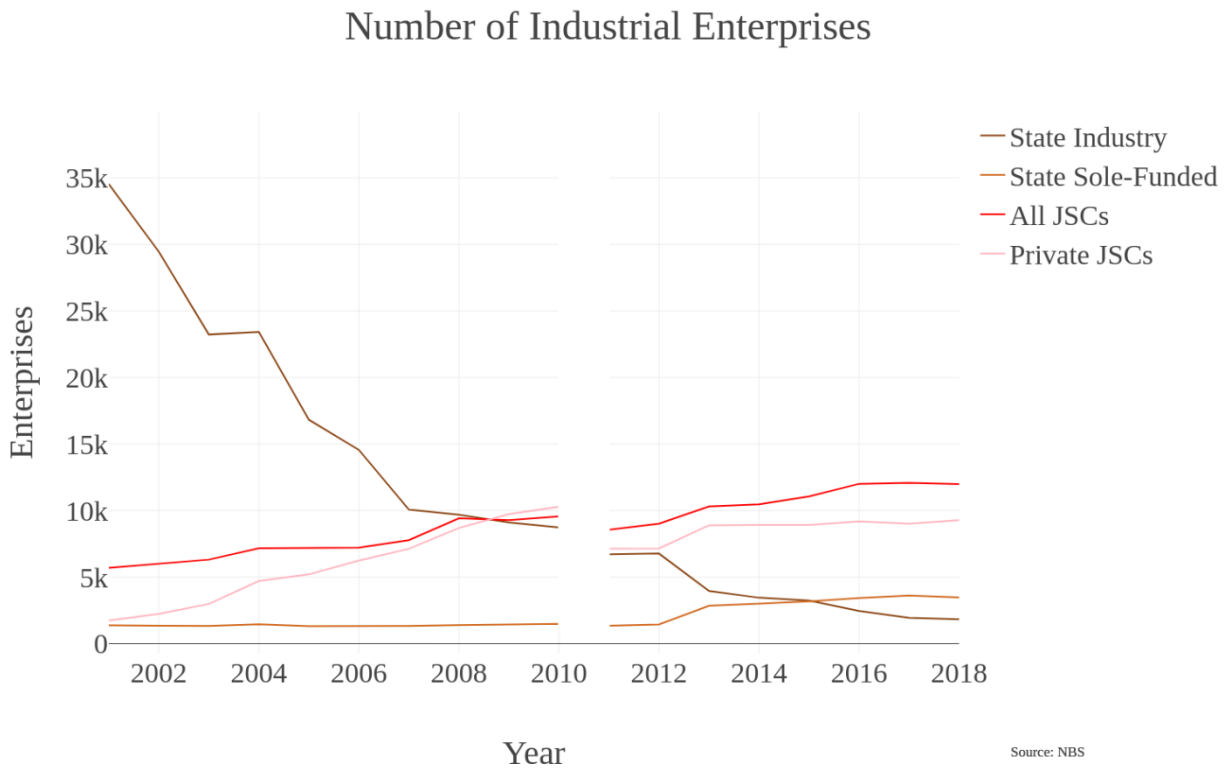



*Figure 3-25*

Here, the restructuring of traditional state industry that took place in the late 1990s and early 2000s is clearly visible. By 2018, very few "unreformed" state enterprises remained. By contrast, the rise of JSCs (both Private JSCs and the mixed category, dominated here by state investment) is also clearly visible and tracks, in part, the restructuring of state-owned enterprises into modern

corporations with shares traded on global stock exchanges. Finally, this chart shows a marked rise (albeit low in absolute terms) of corporations solely funded by a single state-owned investor. These increase in the years after 2012, growing very gradually thereafter and plateauing in more recent years.

These trends can be better visualized when each category is portrayed as a share of the total number of industrial enterprises above a designated size in a given year:

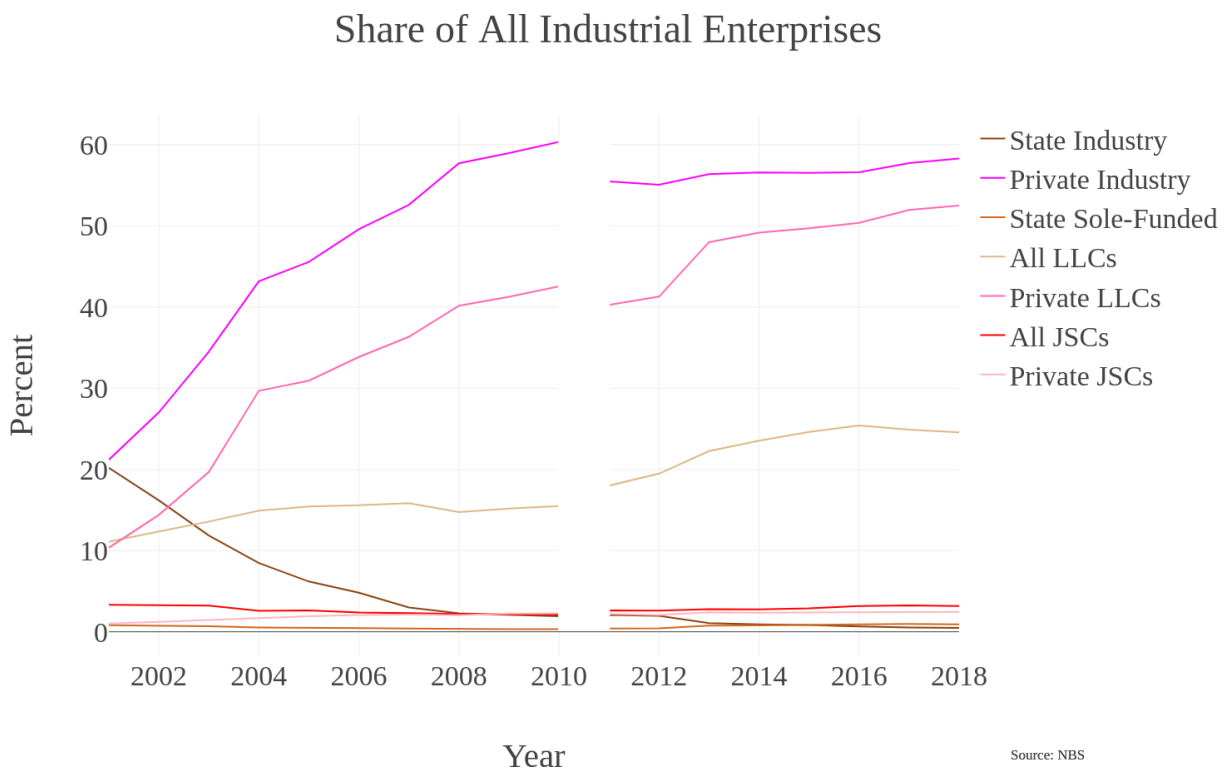



*Figure 3-26*

In this chart, the shares do not add to one hundred percent since many categories overlap. Nonetheless, private enterprises dominate the industrial sector in terms of their sheer number—

and this is keeping in mind that the exclusion of individual and family enterprises mutes this quite a bit. By comparison, all the categories in which state ownership is more heavily concentrated tend to compose a much lower portion of the total industrial enterprises.

The numbers can similarly be divided according to domestic vs. international investment, size of enterprise and type of industry:

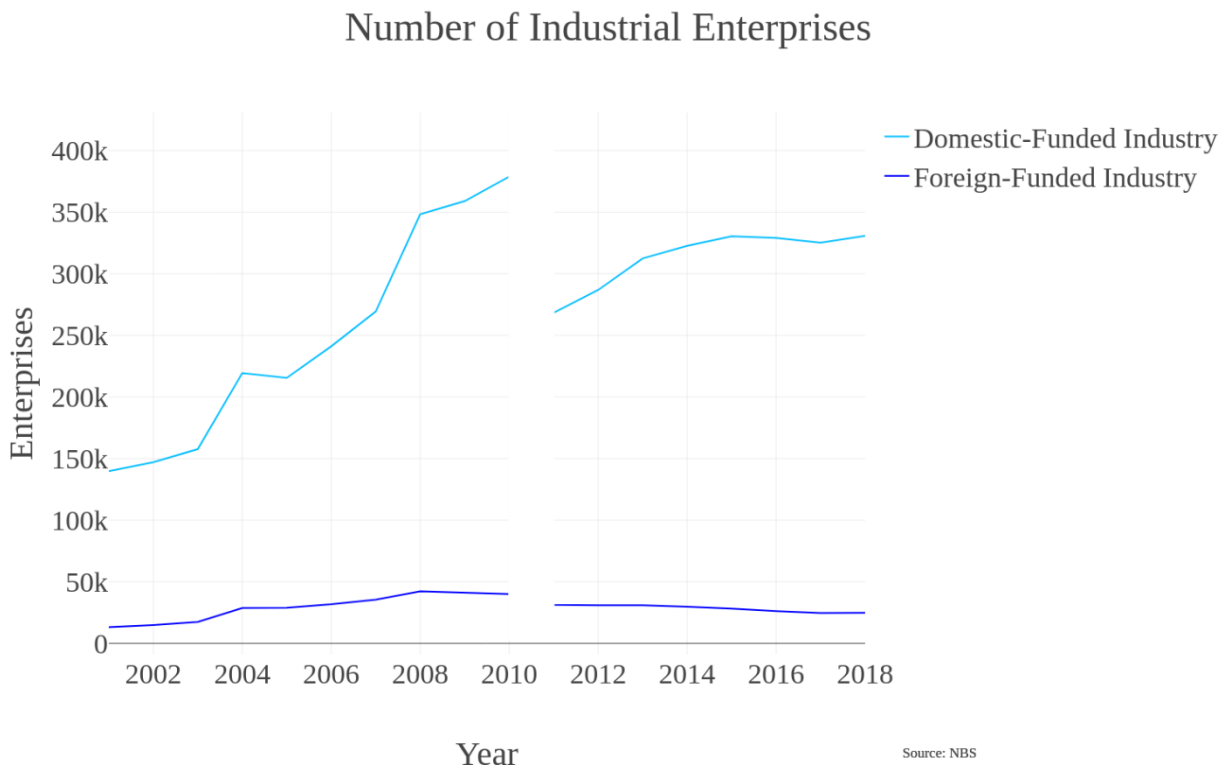



*Figure 3-27*

Here, it is apparent that the vast majority of enterprises are funded with domestic capital, with foreign-funded enterprises composing a significant but much smaller chunk. Although investment from Hong Kong, Macao and Taiwan is not visualized here, it would form a line similar to that of Foreign-Funded Industry. At first, the special role of Taiwanese manufacturers and Hong Kong finance would seem to imply that the figure should not only be included, but

also added to the Foreign-Funded category to effectively double its size. But because large amounts of domestic capital are recycled through these locations, the reality is that investment from Hong Kong, Macao and Taiwan should more reasonably be split between domestic and international sources and would exert very little effect on the basic trends visible above. These trends are clear enough that no accompanying share chart is needed, since it shows the exact same relationship, only amplified.

The reality, however, is that the sheer number of enterprises does not necessarily relate to the relative economic power of each category's enterprises. This is especially true if we assume that state-invested enterprises, for instance, might be larger on average. As can be seen from the next chart, large enterprises naturally compose a significantly smaller portion of the total:

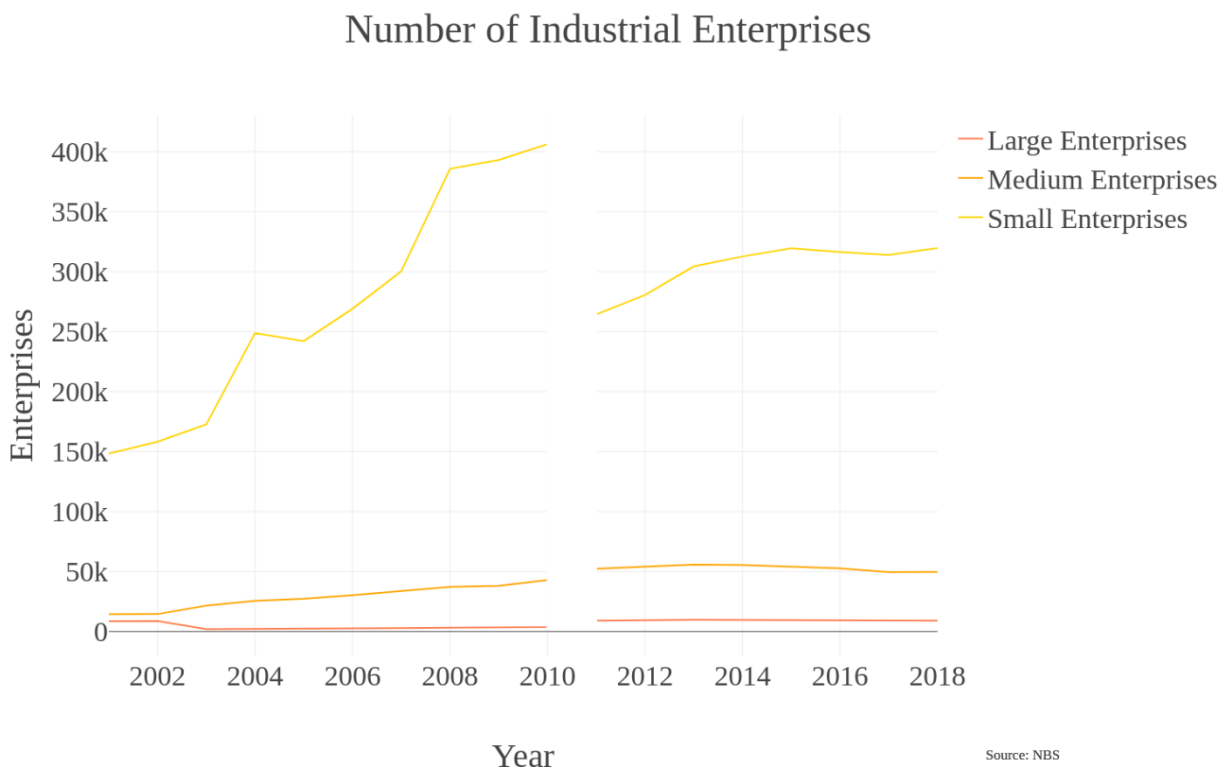



*Figure 3-27*

The vast majority of enterprises are “small” enterprises. If individual and family enterprises were included, the figure would be even larger. Though it’s not apparent due to the scaling, there was a substantial decrease in the number of large enterprises affiliated with the restructuring of state industry and an affiliated rise in both small and medium-scale enterprises. The patterns are basically the same when visualized as shares of the total.

Finally, it might be presumed that heavy industry and light industry will have different concentrations of different ownership categories, while also contributing in different ways to overall output and profit trends:

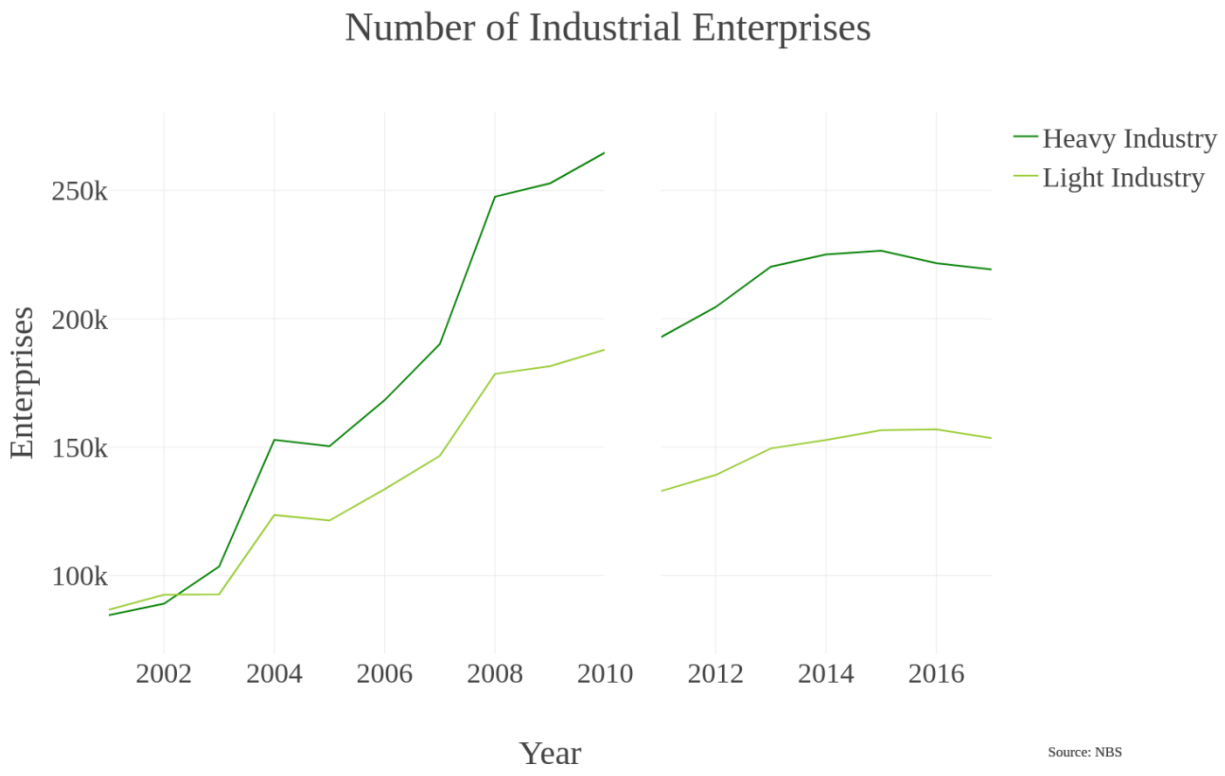



*Figure 3-28*

Though the statistical redefinition in 2011 is starkest here, the same basic trends continue after the offsetting of the data. Both heavy and light industry see growth throughout the 2000s and that growth then slows over the 2010s. As a share of all industry:

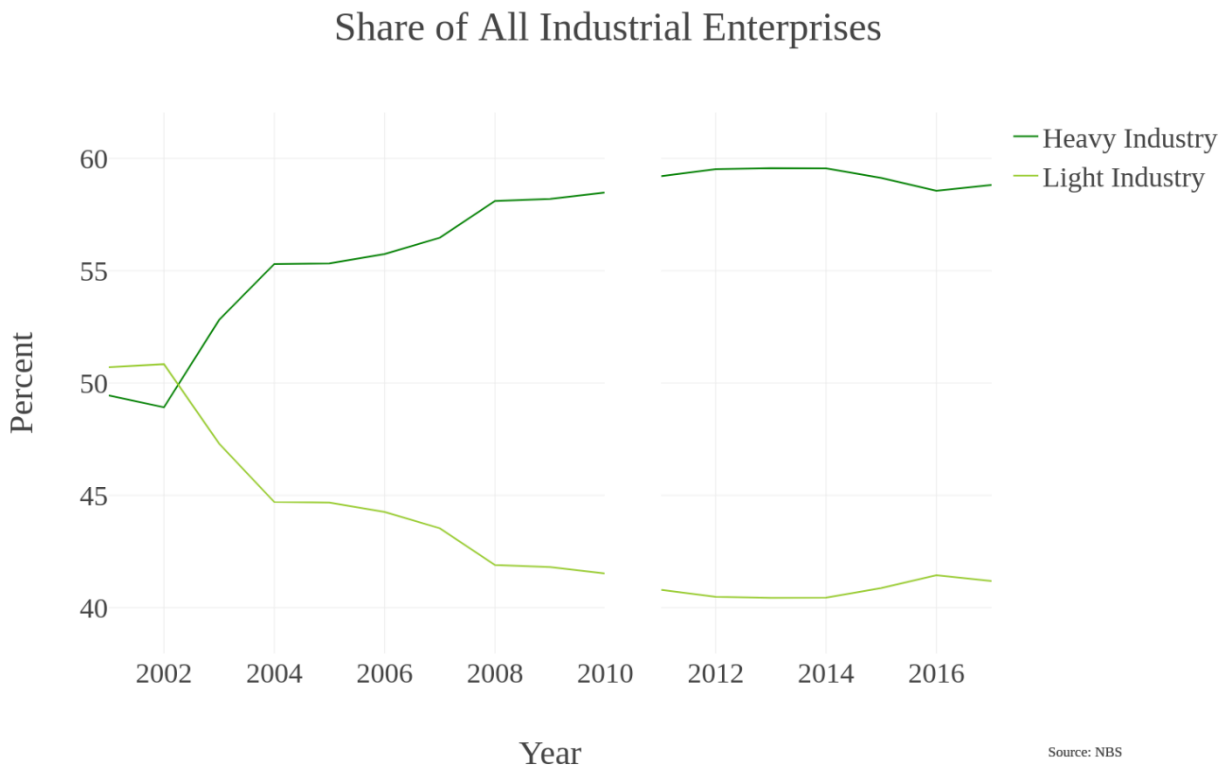



*Figure 3-29*

Here, the same relationship is visible in a cleaner fashion. In particular, the divergence between the two becomes much more apparent here, with heavy industry actually composing a greater share of all industrial enterprises over time.

While the total number of enterprises provides some initial details about the distribution of Chinese industry, it will make more sense to look at the relative contribution of each category to the major variables used in the ROP and ROR equations. For industrial enterprises, since there are no detailed wage bill data for each category, it makes more sense to calculate a simple Rate

of Return on Assets measure dividing Net Profits by Total Assets for each category.[43] Within each category, Total Assets tend to move upward in general over time with larger-scale enterprises in heavy industry having greater shares of the overall assets. For the ownership categories, there are a few notable trends:

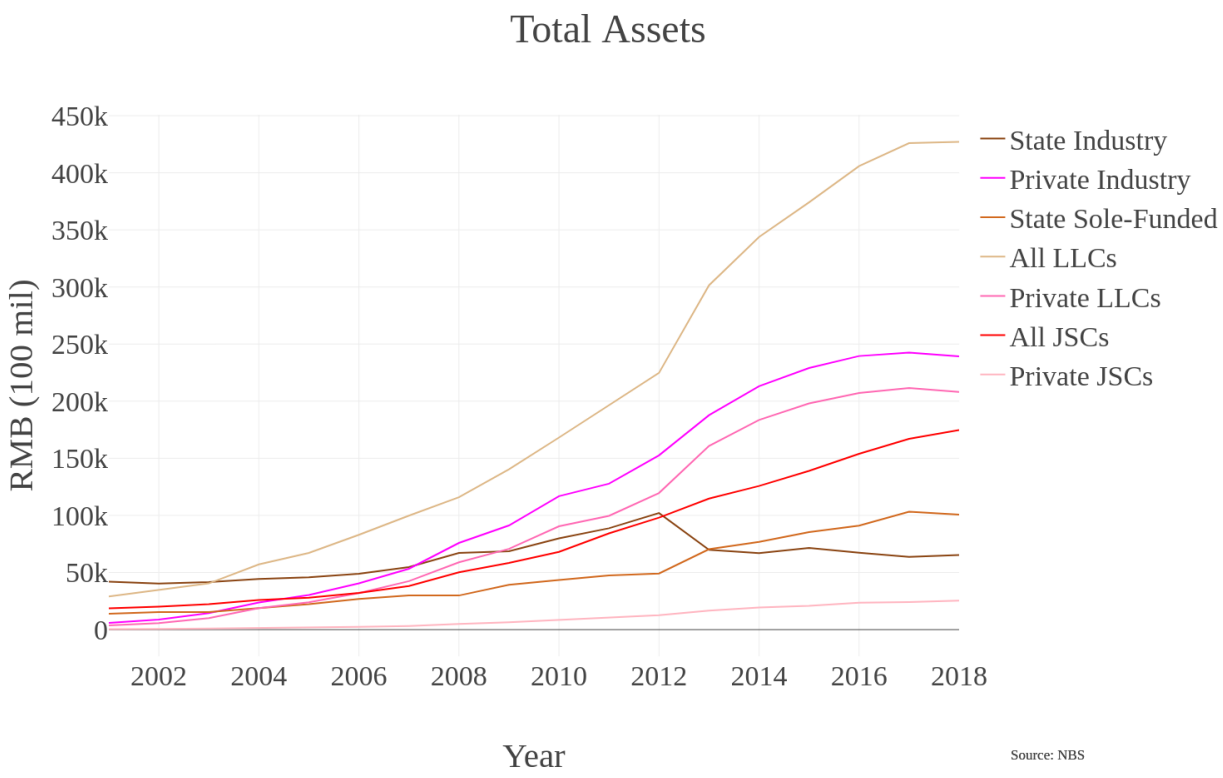



*Figure 3-30*

The first is that the All LLCs category clearly controls a larger amount of total assets than any other single category, including Private Industry and Private LLCs. In addition, this category increases its assets rapidly after around 2012. Similarly, the state industry sector actually

[43] Technically, this could be done for fixed assets as well. But Total Assets, Non-Current Assets and Fixed Assets move roughly in line and, as seen in the national measures above, will produce the same trends at different absolute values.

declines after 2012 but only to be overtaken by corporations for which the state is the sole funder. Since the All LLCs category includes state investment, both these trends point to an increase in the total assets held by firms linked to state industry.

Net Profits, however, slow similar but slightly different patterns, indicating that there will be some divergence in the ROR:

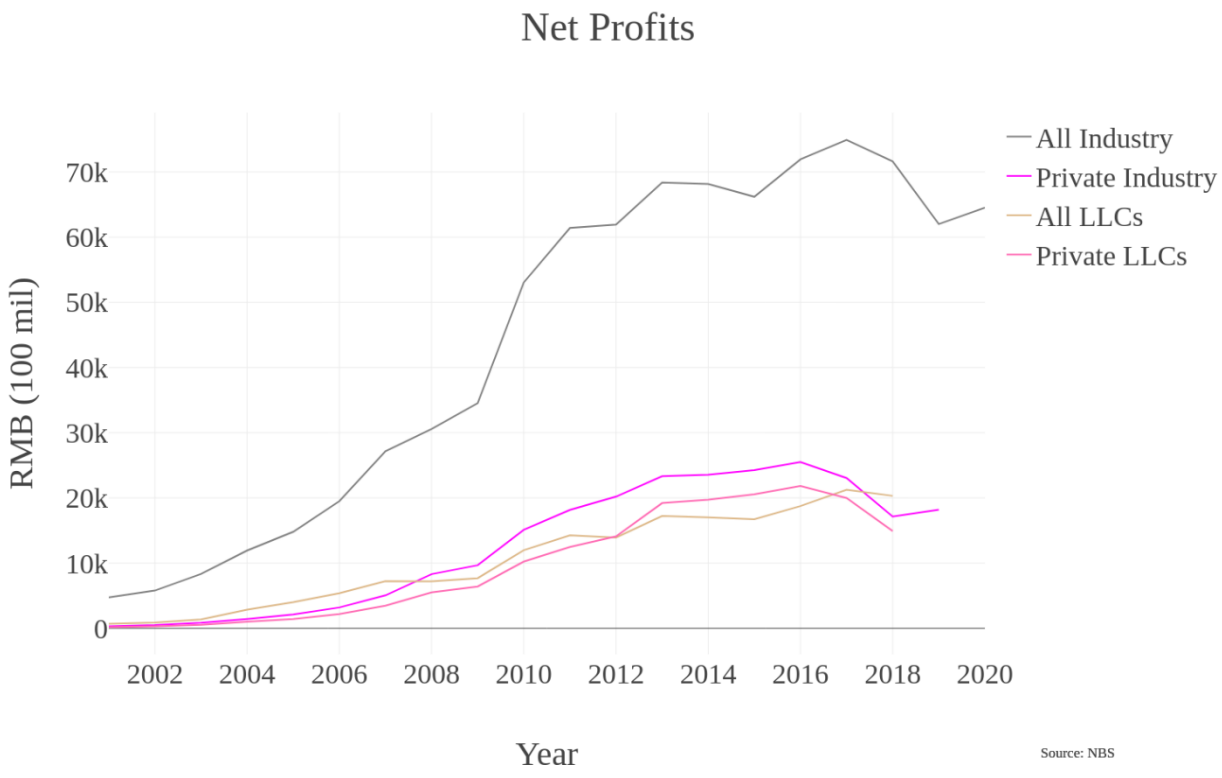



*Figure 3-31*

Private Industry, All LLCs and Private LLCs control the largest quantity of net profits overall, and their patterns tend to follow the movement of the total net profits figure over time. Between 2017 and 2018, the net profits of All LLCs pulls ahead of both the other categories, but data is

not yet available for All LLCs in 2019. The other ownership categories see significantly lower profit overall, as is clear by the rescaled y-axis:

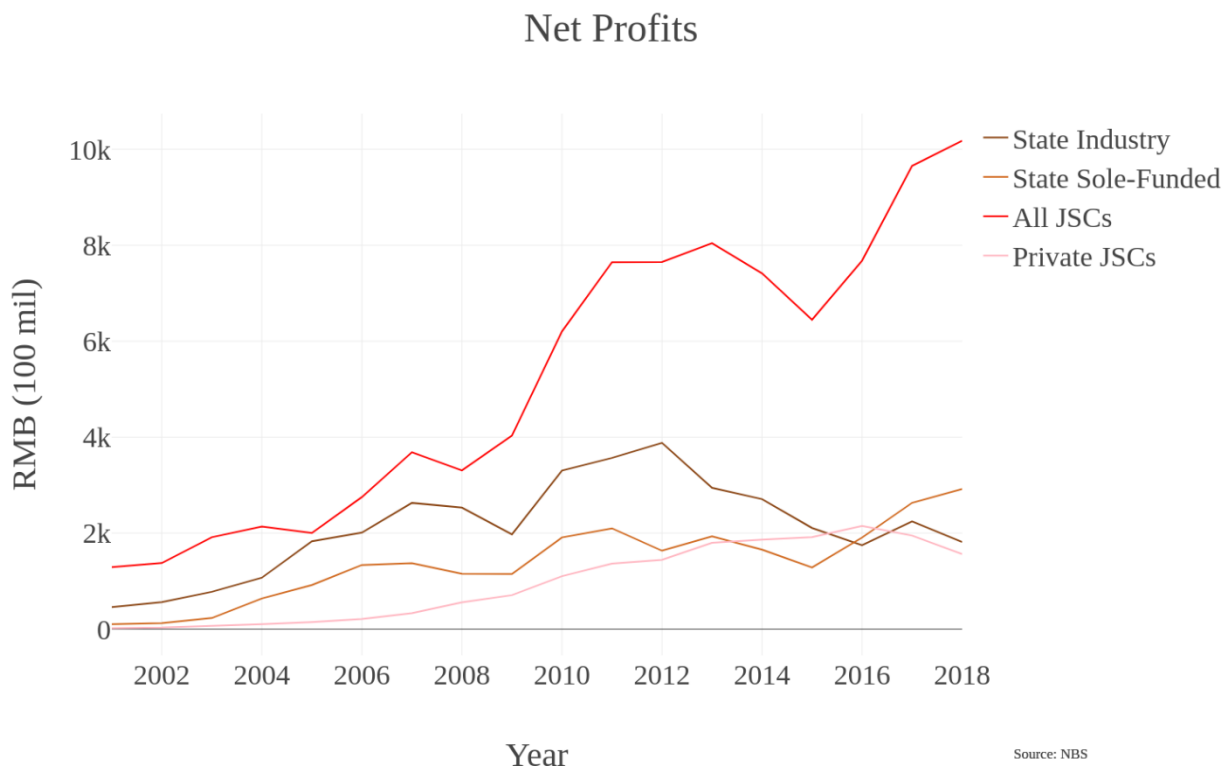



*Figure 3-32*

Here, there is also more divergence from the overall trend. Net Profits of All JSCs has grown rapidly, to lie not far below Private LLCs in 2018 (compared to Figure 3-31). Meanwhile, net profit has grown in State Industry, but peaked around 2012 and declined again thereafter. State Sole-Funded Corporations saw a similar pattern but then a rapid increase in net profits after 2015. Private JSCs have seen a fairly gradual increase, capped by a slight decrease in the final years.

Heavy vs. Light Industry and Industry categorized by Size are more clearly visualized by their relative shares of total profits:

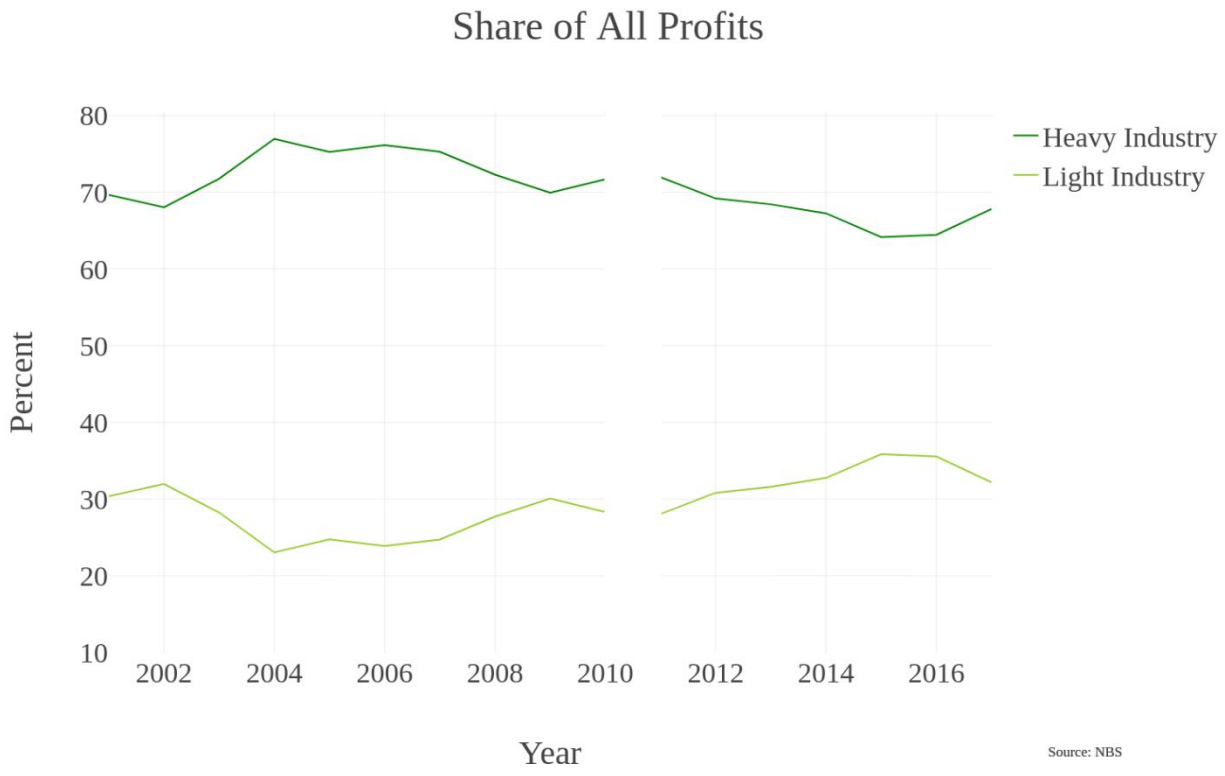



*Figure 3-33*

The relationship between Net Profits of Heavy Industry and Light Industry has remained fairly constant and was not modified by the statistical redefinition in 2011. On average, Heavy Industry has controlled close to seventy percent of the total profits of all industrial enterprises, while Light Industry has controlled closer to thirty percent. But the same patterns do not hold for industry categorized by size:

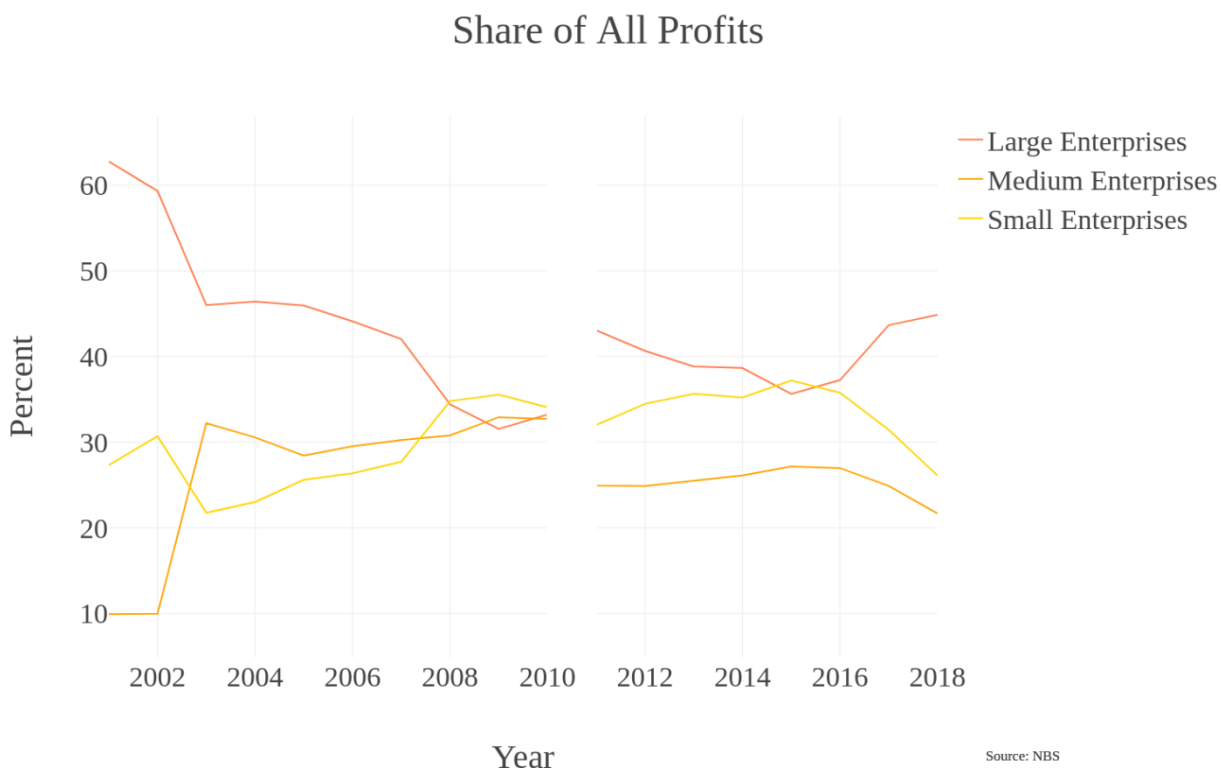



*Figure 3-34*

Here, the statistical redefinition in 2011 sees the series diverge after a brief convergence in the late 2000s, after which point Large Enterprises tend to control a greater share of total profits in most years, though a similar share (around 40%) in 2015 and 2016 as Small Enterprises. Medium Enterprises, by contrast, consistently control the smallest share of total profits in all years after 2011, hitting a trough at just over twenty percent in 2018.

While this gives a sense of each category's respective share of total profits, it is also possible to scale each category's profit share relative to its share of all enterprises. To do so, I use a measure that I call a Profit Quotient. The Profit Quotient uses the same logic as a Location Quotient, which compares the local concentration of a variable (often employment in an

industry) and divides it by the national concentration. The higher the location quotient, the higher the concentration of that variable. Specifically, the location quotient can be read in this fashion: if calculated for employment in an industry, a location quotient of 1 means that the employment in that industry in the area of interest is exactly proportional to employment in that industry in the country as a whole. A location quotient of 3.5 means that employment in that industry in the area of interest is 3.5 times greater than employment in that industry in the nation as a whole. Similarly, a location quotient of .5 means that employment in that industry is .5 times greater (which is the same as saying half as much) as employment in that industry in the nation as a whole. This means that the location quotient more intuitively communicates higher concentrations than lower, but technically it shows both. If it is visualized, however, it will tend to emphasize the higher concentrations due to the nature of the measure.

The Profit Quotient (PQ) is a variation on this theme. For a given industrial classification, I take the share of total profits and divide it by the share of total enterprises:

$$PQ = \frac{Share\ of\ Total\ Profits}{Share\ of\ Total\ Enterprises}$$

In other words, it measures the potential concentration of profits relative to the number of enterprises. A PQ of 1 indicates that a given classification's share of total profits is equal to its share of total enterprises. This means that there is no particular over- or under-concentration of profits in that class. Similarly, a PQ of 3 indicates that a given industrial category's share of total profits is three times its share of enterprises. This indicates over-concentration of profits in that class relative to its size. According to the same logic, a PQ of .5 indicates that a given industrial category's share of total profits is one half (or .5 times) its share of enterprises, indicating under-concentration of profits in that class relative to its size. This is an extremely useful tool to

measure the relative economic influence of each classification, since the vast majority of investment in China is derived from the retained earnings of individual enterprises (Lardy 2014, p.22). Even while debt-financing has grown in importance in more recent years, this still remains the case, on average, both among private companies and among all the largest, centrally-managed state-owned enterprises. Dependence on credit has grown most extreme among local state-owned enterprises (Lardy 2019).

At the same time, visualizing the PQ on a line chart will visually exaggerate over-concentration and mute under-concentration. It therefore makes more sense to simply present it in tabular form, showing each category's average over certain years with the 2011 redefinition marked by a black line:

**Profit Quotient**

2001-2018, Industry

| | Type | | Size | | | Ownership | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Heavy | Light | Large | Medium | Small | Private | Private LLCs | Private JSCs | LLCs | JSCs | State | State Sole-Funded | Domestic | Foreign |
| 2018 | NA | NA | 18.653 | 1.648 | 0.309 | 0.410 | 0.397 | 0.891 | 1.155 | 4.489 | 5.229 | 4.432 | 0.792 | 2.190 |
| 2017 | 1.153 | 0.781 | 17.614 | 1.870 | 0.373 | 0.533 | 0.514 | 1.077 | 1.139 | 3.974 | 5.739 | 3.620 | 0.864 | 2.281 |
| 2016 | 1.101 | 0.858 | 14.642 | 1.939 | 0.428 | 0.626 | 0.602 | 1.232 | 1.025 | 3.367 | 3.744 | 2.923 | 0.869 | 2.215 |
| 2015 | 1.085 | 0.877 | 14.172 | 1.925 | 0.446 | 0.648 | 0.625 | 1.243 | 1.026 | 3.374 | 3.773 | 2.341 | 0.881 | 2.039 |
| 2014 | 1.129 | 0.810 | 14.768 | 1.781 | 0.426 | 0.611 | 0.589 | 1.158 | 1.061 | 3.924 | 4.353 | 3.058 | 0.886 | 1.986 |
| 2013 | 1.149 | 0.781 | 14.647 | 1.694 | 0.433 | 0.605 | 0.585 | 1.093 | 1.131 | 4.220 | 4.024 | 3.676 | 0.910 | 1.810 |
| 2012 | 1.163 | 0.761 | 14.793 | 1.588 | 0.423 | 0.592 | 0.552 | 1.120 | 1.154 | 4.713 | 3.184 | 6.280 | 0.928 | 1.617 |
| 2011 | 1.215 | 0.687 | 15.387 | 1.554 | 0.394 | 0.533 | 0.504 | 1.015 | 1.290 | 4.736 | 2.821 | 8.304 | 0.907 | 1.692 |
| 2010 | 1.226 | 0.682 | 40.221 | 3.451 | 0.380 | 0.472 | 0.454 | 0.915 | 1.460 | 5.538 | 3.231 | 11.027 | 0.857 | 2.115 |
| 2009 | 1.202 | 0.719 | 42.113 | 3.758 | 0.393 | 0.475 | 0.450 | 0.914 | 1.466 | 5.467 | 2.725 | 9.931 | 0.856 | 2.042 |
| 2008 | 1.244 | 0.661 | 46.003 | 3.527 | 0.384 | 0.471 | 0.448 | 0.894 | 1.601 | 4.893 | 3.646 | 11.479 | 0.894 | 1.737 |
| 2007 | 1.333 | 0.568 | 48.634 | 3.032 | 0.311 | 0.354 | 0.352 | 0.577 | 1.681 | 5.874 | 3.237 | 12.811 | 0.904 | 1.750 |
| 2006 | 1.366 | 0.539 | 49.611 | 2.948 | 0.296 | 0.330 | 0.330 | 0.521 | 1.769 | 5.908 | 2.140 | 15.409 | 0.907 | 1.753 |
| 2005 | 1.360 | 0.554 | 49.901 | 2.835 | 0.288 | 0.315 | 0.313 | 0.513 | 1.750 | 5.114 | 1.997 | 12.835 | 0.909 | 1.751 |
| 2004 | 1.392 | 0.515 | 60.118 | 3.306 | 0.256 | 0.278 | 0.281 | 0.496 | 1.600 | 6.908 | 1.060 | 10.215 | 0.851 | 2.113 |
| 2003 | 1.359 | 0.597 | 45.453 | 2.918 | 0.247 | 0.299 | 0.329 | 0.485 | 1.206 | 7.130 | 0.786 | 4.079 | 0.829 | 2.497 |
| 2002 | 1.391 | 0.629 | 12.336 | 1.247 | 0.353 | 0.314 | 0.360 | 0.439 | 1.218 | 7.225 | 0.602 | 2.895 | 0.836 | 2.472 |
| 2001 | 1.409 | 0.599 | 12.494 | 1.180 | 0.315 | 0.312 | 0.356 | 0.408 | 1.277 | 8.205 | 0.480 | 2.693 | 0.850 | 2.455 |

*Table 3-9*

Above, several ownership types are colored in using a spectrum running from light pink to a light orange. This is an attempt to emphasize where private investment (in pink) or state investment (in orange) predominates, with the LLCs and JSCs categories mixed, but LLCs likely including more private investment and JSCs including more state investment. The most recent year is portrayed first for easy reference.

Several constants seem apparent. Throughout all years, heavy industry tends to seems to see slightly higher shares of total profits relative to its share of all enterprises and light industry slightly lower shares. Similarly, foreign-funded enterprises tend to control around twice as high a share of total profits relative to their share of total enterprises. The highest disproportionality, however, is by size. Though it has diminished somewhat from its peak in the mid-late 2000s, large enterprises always wield an extremely high concentration of profits and medium enterprises a slight concentration. Throughout the series, small enterprises control an unrepresentatively small share of total profits, given that they compose such a high share of total enterprises by number. Any deeper inquiry would clearly benefit by controlling for enterprise size and industrial type when comparing across categories, since it is clear that categories with higher concentrations of large enterprises will control a disproportionate share of total profits due to their scale. This is part of the reason why state-invested enterprises see higher PQ values on average.

But the correlation between the PQ of large enterprises and the various state-investment categories is not perfect. The PQ of state-sole funded corporations see the closest correlation with the movements of the PQ for large enterprises, implying that there is likely some interdependence.[44] This makes sense, given that the companies in this category (which is small overall, in terms of total enterprises) tend to be large firms in core industries and were often formed out of the consolidation of numerous older state-owned enterprises in the restructuring of the early 2000s. But the PQ of state industry (i.e. the "unreformed" state category) tends to be

[44] The series does not have enough observations to compute fully useable inferential statistics, but computing basic correlations can give some descriptive insight: the Pearson correlation coefficient is .819 and the Adjusted R-Squared of a simple linear regression model using state sole-funded corps as dependent and large industry as independent is .65.

low in the earliest years, when the PQ of large enterprises was highest. The value for state industry then increases almost continuously through the 2010s, while the PQ of large enterprises is relatively stagnant until the final two years in the series. The correlation between the two is essentially nonexistent and, based on this, we can conclude that the trend in state industry PQ is at least somewhat independent of the trend for large scale enterprises.[45] On the other end, the trend in the small enterprises PQ is clearly correlated with the values for private enterprises[46] and private LLCs.[47] Meanwhile, the value for foreign-funded enterprises is not correlated with any of the size categories and seems to move independently of it. The reasons for this are not entirely clear, but it could simply be due to the fact that foreign-funded enterprises may be less concentrated in any one size or sector, as recorded here.

With the trends in the underlying data now clear, it is possible to move on to the calculation of the rate of return on assets for each category. This is a simple figure that divides the net profits for each category by the total assets in each year. This is the exact same method used in Figure 12 of the Brandt et. al. 2020 World Bank report (p.13), but disaggregated into more detailed categories. Each series will be plotted against the trend for industry overall in order to show whether it exhibits generally higher or lower profitability. Table A1-3 in Appendix 1 shows the same data calculated as each category's divergence from the industry-wide ROR measure for each year (i.e. "Category i" ROR minus All Industry ROR). But visualizing them as time series communicates the same relationships more clearly:

[45] The Pearson correlation coefficient between state industry and large scale industry is -.33 and the Adjusted R-Squared of a simple linear regression model using the same terms is a mere .1773.
[46] Private and Small have a Pearson correlation coefficient of .93 and an Adjusted R-Squared of .87.
[47] Private LLCs and Small have a Pearson correlation coefficient of .94 and a statistically significant Adjusted R-Squared of .89.

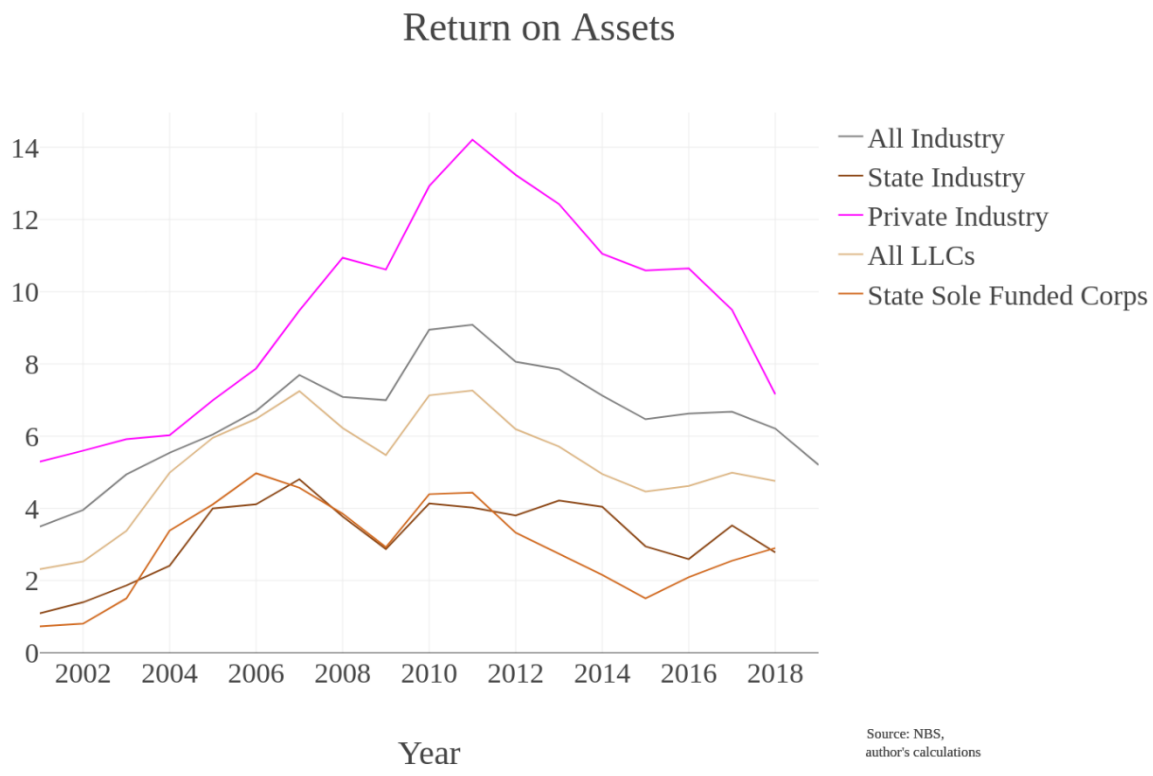



*Figure 3-35*

In Figure 3-35, private industry shows a consistently higher profit rate in absolute terms than the figure for all industry, with the greatest upward divergence visible in the period between 2008 and 2016. Over these years, the divergence averages 4.33, compared to 2.82 in 2017, .95 in 2018 and an average of 1.26 between 2001 and 2008. Similarly, the LLCs category and both unreformed state industry and modern corporations where the state is the sole funder all exhibit generally lower profitability, with the two exclusively state categories showing the lowest profitability here and the lowest of all series. Over all available years, the state industry's average deviation from the ROR for all industry is -3.39 and for corporations solely funded by

the state it is -3.70. Both LLCs and companies where the state is the sole funder show a mild increase in ROR after 2015, against the overall trend.

The second subset of ownership categories all exhibit substantially higher profitability than that seen across all industrial enterprises taken together, with two showing a more strongly divergent trend in the most recent years:

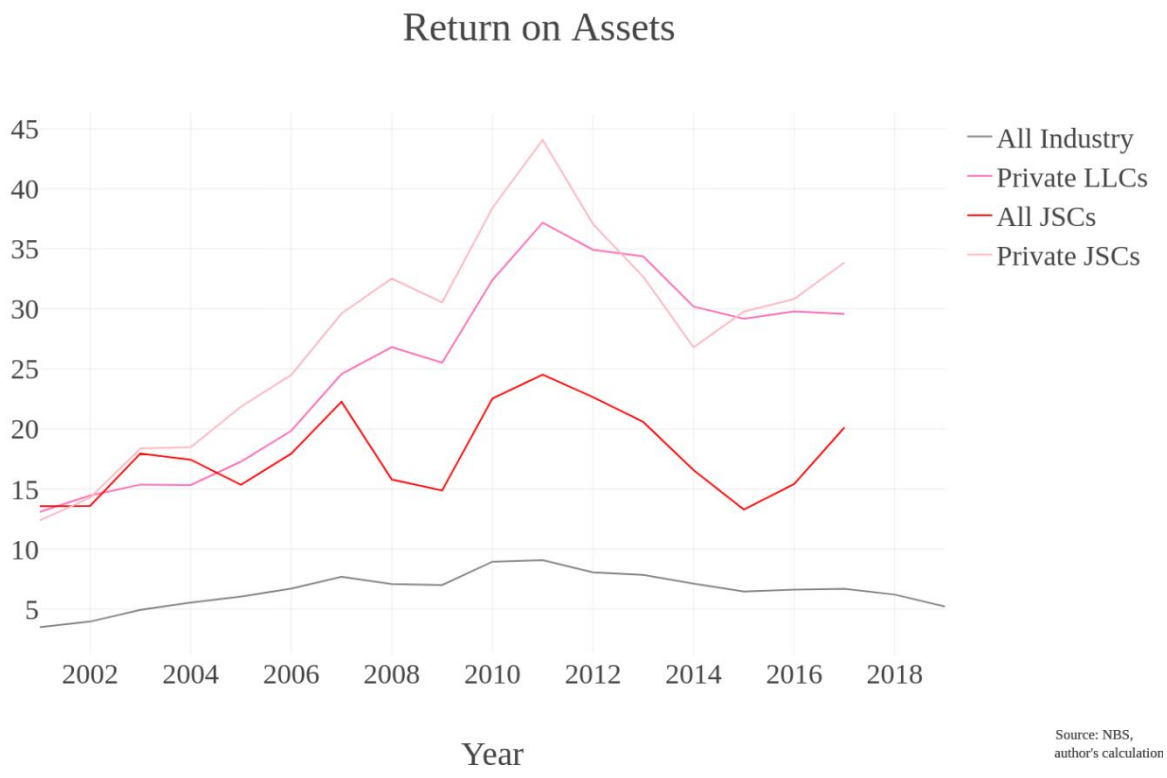



*Figure 3-36*

Private LLCs and private joint stock companies have the highest rates of return of any category. Over all available years, their average divergence from the industry-wide rate of return are 18.62 and 21.33, respectively. They also show an even stronger increase in ROR after 2015 than that seen for LLCs and state sole-funded corporations above, contra the industry-wide trend. But the

general joint stock company category, which includes state-invested companies, also shows strong profitability relative to the industry-wide figure, with an average divergence of 11.24.

At first, it seems as if the rate of return on assets exhibits an inverse pattern against the trend in the profit quotient. But a glance at the figures for foreign- and domestic-funded enterprises dispels this impression:

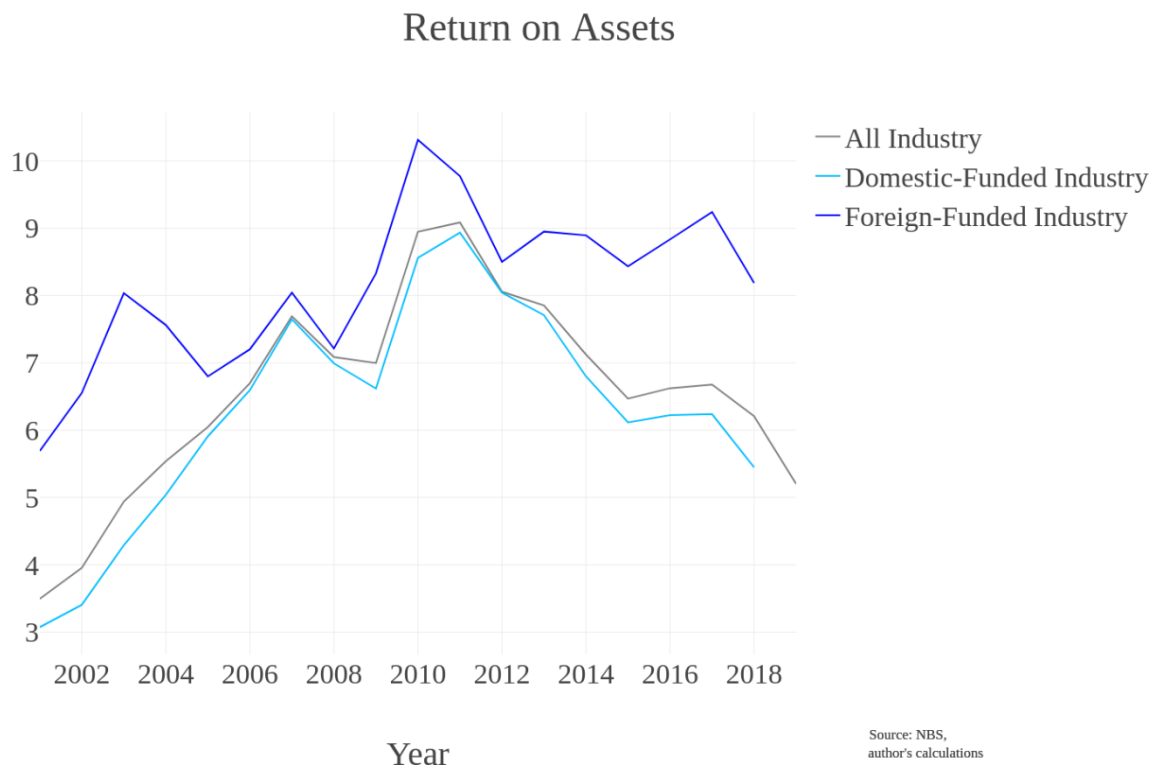



*Figure 3-37*

Here, the trend in the ROR is not inverse to the trend in the PQ above. The two instead show fairly similar trends: Foreign-funded industry tends to have a higher ROR than domestic-funded, though domestic-funded lies extremely close to the ROR for all industry since the vast majority of industrial enterprises are domestic funded. This indicates a completely different hidden

variable shaping the trends here: since the All Industry line is an aggregate of all of these categories combined, the larger ones will gravitate toward that line and the smallest ones will more easily diverge from it. This doesn't mean that categories with many enterprises cannot diverge, however—as is clear with the private enterprises above (which made up about 55% of all enterprises at its peak divergence in 2012). But it does seem to indicate that, as the scale of enterprises rises through consolidation and their number shrinks (as in the State category, where the count diminishes over time but assets and profits continue to rise) the divergence from the industry-wide ROR will increase.[48]

If this is the case, we should see the trend for large enterprises and those in heavy industry (where capital concentration is greater) also diverge below the industry-wide ROR on average, but especially in later years where the capital-labor and capital-output ratios are higher:

[48] While it would be possible to correct for this (normalizing, for instance, by the total number of enterprises), doing so would fundamentally change the nature of the variable being displayed. But the trends themselves are still significant and the positive or negative divergence from the trend is still valid, even if it helps to keep in mind that the relative divergence from the industry-wide measure should be muted in some cases.

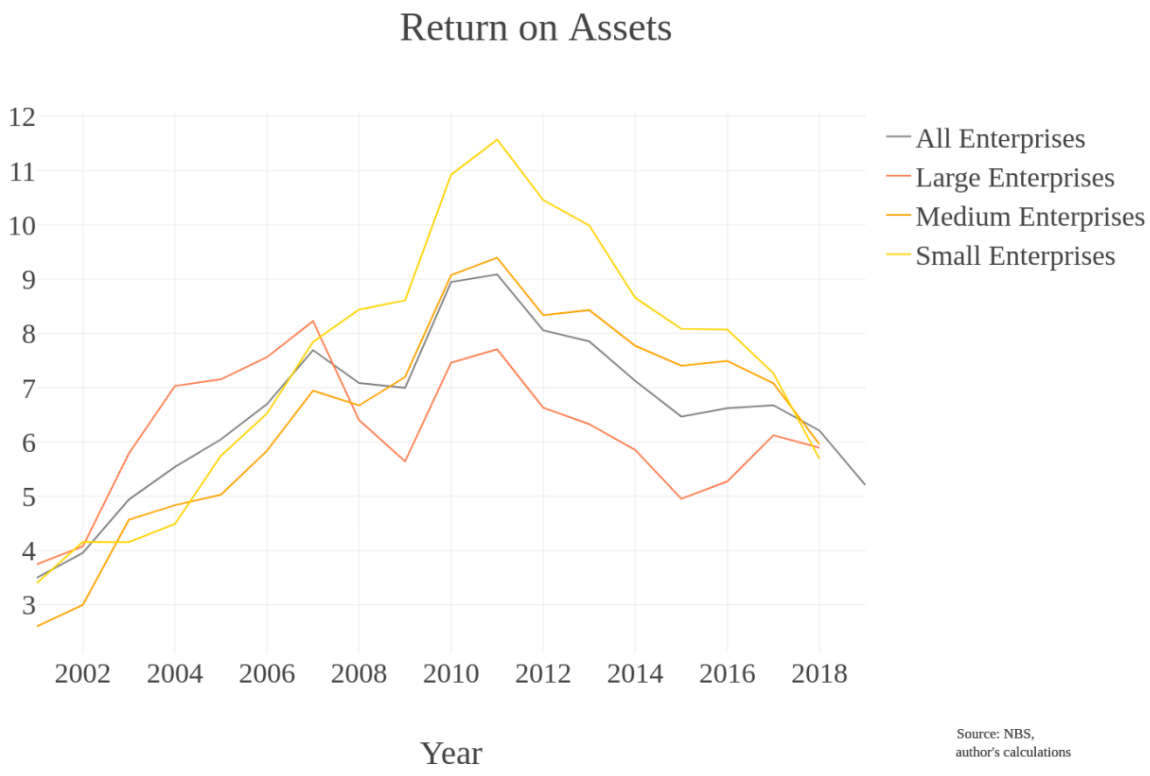



*Figure 3-38*

This is generally the case for industries categorized by size, though it's notable that large enterprises actually had a slightly higher rate of return on assets, on average, prior to 2008, while both medium and small enterprises had a slightly lower rate of return, on average, in the same years. For all, the trends essentially returned to the average in 2018. This follows a similar pattern as private industry in Figure 3-35, where a bubble of high positive divergence developed after 2008 and collapsed by 2018. Given the precise years of these inflection-points, it is reasonable to suspect that there might be some relation to the restructuring of industry after the Great Recession. The trends in Figure 3-38 seem to imply that large enterprises became relatively heavier in assets in these years and relatively lighter in profits, compared to the same ratio across all industry. In other words, even though both assets and profits were increasing

assets seemed to be increasing more quickly (causing a general decline in the ROR) and this disequilibrium between the rate of growth of assets and the rate of growth of profits was more pronounced for large enterprises. But, by 2018, the divergence had levelled off.

A very similar, albeit more muted, trend can be seen in the relative movements of the rate of return for heavy and light industry:

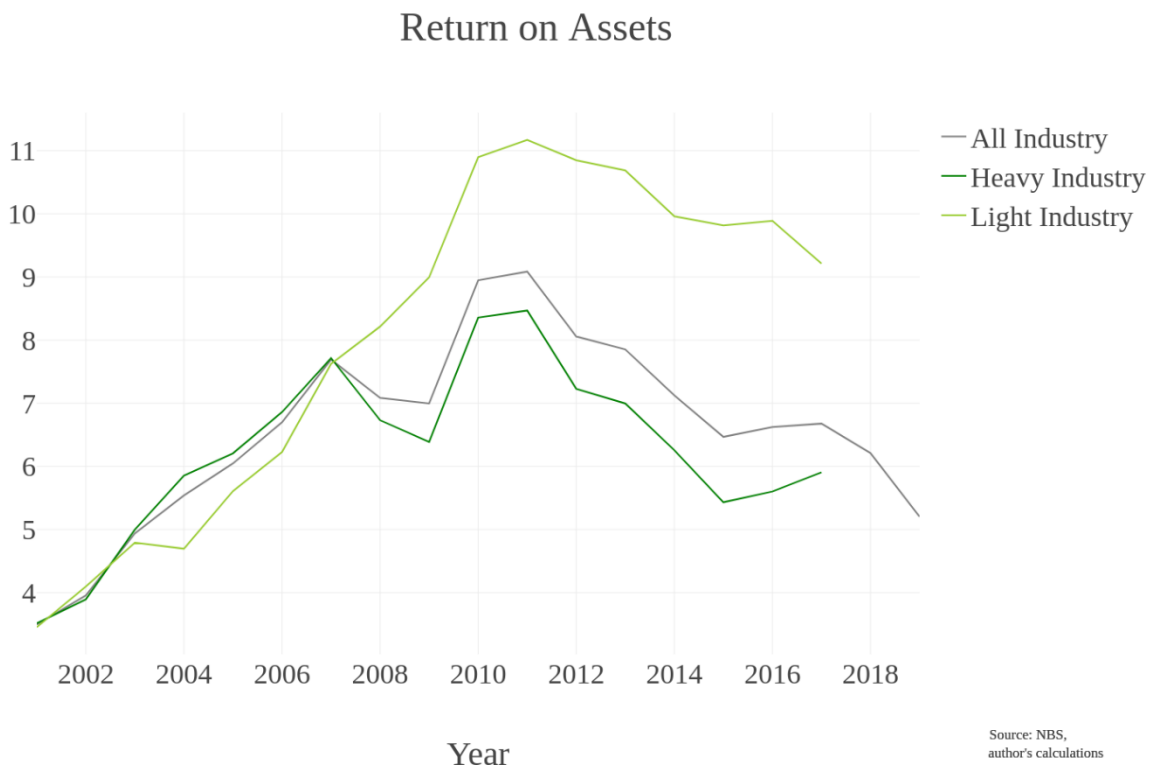



*Figure 3-39*

From 2001 through 2007, both essentially follow the overall trend with heavy industry slightly lower and light industry slightly higher. But after 2008 the relationships invert and both lines diverge from the industry-wide trend. Light industry diverges upward substantially and heavy industry diverges downward, though its divergence is less extreme—in part, this is due to the

fact that there are fewer total enterprises in light industry (about 40% of the total in most recent years) and more in heavy industry (about 60%), making the latter gravitate closer to the overall trend line. Neither have data available for 2018, so it's unclear if they re-converge, similar to the series above. But it is clear that larger enterprises and those engaged in heavy industry tended to have lower than average rates of return in the years following the Great Recession. The fact that "unreformed" state enterprises and corporations with sole state investment tend also to be concentrated in larger-scale production in the heavier industries suggests that the relatively low profitability of these categories likely has as much to do with the scale and sector of these industries as with the registration status or source of investment funds.

**Prospects**

Sifting through the data above, a few key trends stand out. The first is simply that the overall rate of profit or the rate of return—as measured in almost every conceivable way from the raw data by myself, by those who would consider themselves "Marxist economists," by consultants for HSBC or by World Bank researchers—has been falling in China almost[49] continuously since 2011. After this year, no measure shows any recovery to the rates seen in 2011 or before. When averaged across all measures, the downward trend after 2011 is monotonic. When averaged across all national measures, it is very nearly monotonic—with a single, very slight increase between 2017 and 2018. When averaged across all industrial measures, the same absolute decline is visible, but the monotonic character is lost due to a brief upswing between 2015 and 2017. When decomposed into categories based on ownership, size and type of industry, almost

[49] Here, I am referring to the measures from Figure 3-15 covering overall rates of profit and rates of return, not the return on assets measures disaggregated by category.

no category sees this trend reversed and none of the few small categories of enterprise that have experienced a mild recovery in recent years see profitability regain its peak from the early 2010s. Similarly, the trends in the profit quotient, the opposite divergences observed in heavy vs. light or large vs. small industry and trends in capital-labor and output-labor ratios, paired with the capital-output ratios computed by Herd (2020) all indicate that these trends are linked to a gradual build-up in the scale and capital-intensity of production, accompanied by a rising unit labor cost over the entire 2000 to 2018 period:



*Figure 3-40*

As can be seen in Figure 3-40, the cost of labor per unit of value-added fell in the early 2000s, increased rapidly through the latter half of the decade, peaked in 2012, fell somewhat and then resumed its upward growth in the later 2010s. Rising labor costs do not account for the fall in the rate of profit in any linear way and, in fact, the two appear to move in almost opposite directions. It is only when placed in the context of increasing capital intensity in the midst of a general global glut in manufactures that the linkage of the two makes sense. As is apparent from the trends in both the output-labor ratio and in Unit Labor Costs, China has been unable to restore the rate of profit by forcing labor costs down. Instead, the same "output-led deindustrialization" evident in the comparative growth rates of output, employment and productivity for all advanced economies, may now be becoming visible in China as well.

While Lardy (2019) and Batson (2020) suggest that one of the primary causes of falling growth is the rise of state investment and the downward drag on profits and output economywide exerted by the relatively lower rate of returns seen in state enterprises, this appears to only be partially the case. While it is true that ownership categories with higher shares of state investment tend to have lower rates of return, this is also true of all large enterprises—and it is worth noting that joint-stock companies, which include state-invested joint stock companies, have higher rates of return and see a mild divergence from the downward trend in the final years. Similarly, while all of the private ownership categories show higher than average rates of return, the private industry category also shows one of the strongest declines in the overall rate. Private LLCs, the next largest of the private enterprise categories, shows a similar, though more muted, decline. Only the relatively small private joint stock companies category shows any divergence from the trend, and even in this case it never regains its 2011 peak. While Lardy and Batson

identify the inefficiencies of state-invested enterprises as the cause of their drag on overall growth, suggesting that state investment is not responsive to differential rates of return and attributing this simply to the whim of Chinese policymakers, I would instead point to more theoretically robust accounts like those of Mattick (Jr, 2011 and Sr, 1969) and Smith (2020), which argue that all large economies in the world today are *de facto* essentially mixed economies, often dominated by large conglomerates with relatively low rates of return and, today, even long stretches of negative profitability. Many of these are precisely "zombie" firms sustained solely by state subsidization and protectionist trade policies. In other words, China seems again to simply be regressing to the mean.

Meanwhile, if there was any ambiguity about whether or not China passed the "Lewis Turning Point" sometime in the 2000s, by the end of the 2010s this ambiguity had largely evaporated. Many signs seem to point toward China entering the same "new normal" of "secular stagnation" that has long prevailed in Europe, North America and Japan, even while it is confronted with the externalities of its own rural-urban dual system, such as a rising share of "left behind children" (Chan and Wei 2019). The demographic turn is only one feature of this new normal and exists alongside rising shares of employment in services and, therefore, greater susceptibility to Baumol's "cost disease" and the attendant slowdown in growth. Often, these various features are not connected in any systematic way within such theories. There is, nonetheless, a conventional portrayal of such phenomena as the universal fate of "mature" economies, signaled by both an inverting population pyramid and a productivity slowdown (especially evident in slowing TFP growth, as argued in Brandt et. al. 2020). But simply ascribing such features—which in these accounts are often not even clearly connected to one another, even if they are spoken in the same breath—to a vague developmental "maturity" is a

somewhat arbitrary act of classification rather than an explanation. As in the work of Koo and Gordon, the only possible causes on offer are folk-scientific accounts that ultimately defer to either demographic or technological limits, both of which are portrayed as natural, inherent in development and more or less unavoidable. Lardy convincingly demonstrates, however, that the contribution of the demographic dividend has always been less than it seems (2018, p. 23-25) and that TFP growth has moved almost entirely in line with the growth of the export sector (ibid, pp. 10-11). More troublingly, the implication in all such work is that the social crises that attend this economic breakdown—skyrocketing inequality, increased austerity for the poor, more brutal racialization (in China evidenced in the mass imprisonment of Uyghurs), flaring geopolitical conflicts, the deepening of almost every form of global "imbalance" that structures imperialism and, of course, climate collapse and more general ecocide[50]—are all also natural and inevitable, with the only possible course of action being slight remediation within the wealthiest epicenters of accumulation.

At the same time, the slowdown in China appears premature. With a nominal per capita GDP of only 11,819 USD in 2019 (according to the IMF World Economic Outlook database), China still ranks as a middle-income country, below much of Eastern Europe, Chile, Uruguay and Panama and, depending on the measure (PPP comparison take it lower), only barely above or just below Mexico, Kazakhstan, Thailand and Malaysia. More importantly—and prefiguring the argument in the next chapter—the internal unevenness of development in China means that much of the country's interior has a significantly lower per capita GDP in real terms, even while the cost of labor is not equally elastic since producers rely on the same currency regardless of

[50] Few of these things, however, are *explicitly* connected to these trends in such accounts. At most, the usual practice is to acknowledge increasing inequality, rising populism and, possibly, austerity. Klein and Pettis also link this to geopolitical conflict. Other social crises are treated as more or less exogenous.

location—in other words, even if production in the interior is cheaper than on the coast, if the RMB inflates relative to the dollar it may become more expensive in global terms, regardless. While it makes for a tempting historical analogy to place China in the same position as Japan in the 1970s or 1980s, when the Japan-US trade war was at its height, the reality is that the true trade war has likely not even begun. Lardy points out that, in PPP terms, China in 2014 had the same relative per capita GDP measured against the US as Japan did in 1951 (Lardy 2019, p.4). The slowing in global trade and subsequent turn toward domestic and regional markets also hints that the sheer size of the Chinese population and the degree of concentration of global production within China provide for much more leverage in the face of crisis than was the case in Japan.

Lardy concludes with an ultimately optimistic prediction that China can, in fact, restore high growth rates when global trade accelerates again, so long as it also pushes through renewed rounds of privatization and forced bankruptcies or restructurings of zombie firms. It seems that this prediction is shaky, however, since it's not yet clear to what degree global trade will recover (or on what basis), with many (such as Howell 2020) instead predicting a redoubling of regionalism. At the same time, even if global trade were to recover, it remains unclear exactly how feasible the type of restructuring suggested by Lardy would actually be, given that the last massive restructuring of state industry created a persistent rust belt in the Northeast, led to large-scale protests and was ultimately only seen as "successful" because export production was producing new sunbelt agglomerations in the south at the same time (Lee 2007). The rapid growth of the private sector in such areas provided the economic basis necessary to restructure the remaining state industries into modern corporations, listed on international stock exchanges (Walter and Howie 2012). Batson (2020) offers a somewhat more skeptical picture than Lardy, emphasizing the fact that more recent debates on the "reform" of the state-sector no longer focus

on such systematic restructuring and are now "about different mechanisms for managing SOEs, not about whether the size of the state sector needs to be reduced" (ibid, p.22). This seems to align more closely with the present political reality.

In other words, while China may still be able to sustain high-enough growth to push itself out of the middle-income range, there is little evidence that it would be able to avoid the social crises that have attended general deindustrialization elsewhere. The extensive state interventions used to stave off the risk of individual crises have only tended to accelerate the tendency toward breakdown that generates the risk of such crises in the first place, in the exact same way that similar interventions have in other countries. Again, as is argued above, the real protagonist of the story of systemic crisis is not falling profitability itself but instead these "countervailing tendencies" deployed against it, which, when successful at preventing depression, also succeed in aggravating the conditions that created the threat of depression in the first place. In China, the clearest sign of this is the diminishing dividend of state investment after the stimulus effort that followed the Great Recession, where each unit of stimulus has tended, over time, to produce fewer units of final output, producing a "bubble that never pops," but which seems more and more difficult to keep inflated (as documented in Orlik 2020). As growth slows, the risk of a stagflationary crisis increases, putting even more pressure on financial authorities to ensure that the RMB does not rapidly inflate relative to the USD. This, of course, only further aggravates the ongoing trade war. Faced with a similar conundrum in the 1980s, Japan ultimately lost its trade war on the terrain of high finance and international fiscal politics. But China is substantially larger, less developed and more central to global production than Japan was at the height of its industrial boom—and it has never been a military subordinate or true geopolitical dependent of the United States.

In international terms, the much more alarming fact is that the ascent of China as a late-developer required an unprecedented degree of all-out economic mobilization that was fundamentally enabled by the remnant bureaucrat infrastructure of the socialist era, facilitated by a "Communist Party" that had been restructured into a vanguard organization helmed by and serving the interests of the domestic capitalist class, which has long found its interests aligned with foreign capitalists in the wealthiest countries (though this is likely changing, albeit more slowly than trade war discourse might imply). If the Chinese economic "miracle" is any evidence, it seems that the Lewis Model that Koo gestures toward and the Three Sector Model that founds the orthodoxy of developmental orthodoxy are both growing more and more foreshortened and any kind of developmental ascent more and more dependent on extensive state intervention, systematic division of the national labor force (as via the *hukou* system in China, which was essential to pushing labor costs down) and unchallenged success in the zero-sum contest of global labor arbitrage necessary to secure essential initial inflows of capital. In the case of poor countries today—as will be demonstrated in the final chapter with regard to Tanzania—very few of these conditions are met. In the midst of the general global glut, such locations are increasingly condemned to conditions of "premature deindustrialization" (Rodrik 2015, Diao et al. 2019 and 2021) wherein there is little hope of using the "China model" or any model whatsoever to catch up to the advanced countries.

Falling profitability in China, then, has a resounding significance. Ultimately, the profit rate of China, averaged across all national measures, converged with that of the US over the later 2010s and is now on track to converge with the average rate of the G7 economies if it continues to decline through the 2020s. According to the most recent estimates by Michael Roberts, the US economy-wide rate of profit lay between sixteen and twenty percent (measured in current cost

terms) between 2010 and 2019. By comparison, my average of all national measures shows China's dipping below twenty percent from 2013 onward, ending at 16.62 percent in 2019. At the same time, the US economywide profit rate is buoyed by a massive financial sector. When the profit rate is calculated for the US non-financial sector alone, the average for the years 2010 to 2019 was somewhere around 4 percent (Roberts 2020c). By comparison, China's industry-specific measures, at 11.59 percent (for Value Added of Industry) and 7.96 percent (for Net Profits) in 2019, are still substantially higher. Similarly, the mean economy-wide profitability of the G7 countries is driven down by particularly low values in Japan and Western Europe, averaging around 6.5 percent over the course of the 2010s (Roberts 2020b, with individual country trends visible in the PWT 10). This is well under the rate seen in the US and China but, since the rate of profit in the US and other G7 countries has tended to decline more slowly or even plateau in recent years, it seems probable that the Chinese rate could even converge with the G7 measure over the next decade.

# Chapter 4
## The Changing Geography of Industry and Profitability

**Introduction**

Even though trade and currency conflicts take on a geopolitical character that emphasizes the specific role of nation states, geographic divergences in profitability are not simply national matters. There is also a regional unevenness to profitability within individual countries. This geographic disequilibrium in the prospects of investment ultimately determines the location of new territorial production complexes and seals the fate of future rust belts. But this is not simply a matter of administration and advertising, with locales able to "attract investment" through city planning or tax breaks. Similarly, physical geography plays some role, but only insofar as a particular landscape's locational amenities are made into social necessities through their integration into the technosphere. As the base technologies that undergird capitalist accumulation change, so too do the particular physical amenities most valued in terms of industrial location.

This is not only true of shifts in primary resource dependencies—which will soon see the oil fields of Saudi Arabia diminish in relative importance against the lithium flats of Bolivia, for instance—but also captures the shifting locational logic that accompanies major technical changes in circulation. The basic infrastructure for globalization itself was laid by the confluence of advances that took place in logistics during the wars of American hegemony in the latter 20th century (Cowen 2014, Chapter 1). These were accompanied, of course, by a particular Pacific-facing imperial geography, as the US reinstated war criminals such as Nobosuke Kishi to lead a

renewed developmental project under American military tutelage in Japan (Glassman 2019). Together, these factors privileged cities with particular physical amenities over others: pacific-facing port cities became some of the fastest growing in the world and almost all the ascendant "newly-industrializing countries" were located in the Pacific, hosted US military bases and often saw their juvenile industrial firms nursed on American wartime procurement contracts (as in Japan, with the Korean war, and Korea with the Vietnam war – documented in Glassman 2019 and Chuang 2018). Ultimately, the physical amenity of a deep-water bay is only made into an economic advantage in a particular sociotechnical context and the ascent of "global cities" like Los Angeles and Tokyo (Sassen 1991) had much to do with their position within *industrial* circuits, rather than just their concentrations of producer services (Neel 2016, Cowen 2014).

Far more important than administrative or physical amenities are the preexisting features of industrial geography laid down in past cycles of investment. Overall, the point to be emphasized is that "most industrial activity remains concentrated in a few regions of each country and in a few countries in the world, and regions still tend to have rather specialized industrial bases" (Storper and Walker 1991, p.83). Storper and Walker argue that there is apparently no tendency for already-concentrated industries to simply disaggregate over time (ibid). Even as they increase in scale and undergo relocation, this relocation tends to be either an expansion of an existing agglomeration or the formation of new agglomerations of production (when "windows of locational opportunity" open) that exhibit just as much concentration as the old ones. Against conventional accounts that attribute such relocation to "product cycles" or the squeeze on profits caused by labor militancy and subsequent "spatial fix" (in other words, relocation as an intentional ruling class strategy deployed against labor), Storper and Walker argue that declining profitability in old industrial cores, driven by competition and accompanied

by rising labor costs, has to be understood in relation to both technological change (i.e. the "maturity" of the industries) and to "overinvestment and excess competition on a world scale" (p.89). The point is essentially the same as that made later by Brenner in relating industrial overcapacity to the build-up of trade wars, speculative bubbles and all their attendant crises. But Storper and Walker place greater emphasis on the fact that "there is no natural course of industry growth," pointing out that the apparently "natural" obsolescence of rust belt zones often appears quite suddenly, as optimal locations seem to see their advantages suddenly melt away. Throughout, the continual buildup of overcapacity and the weighing-down of industries with the sedimented capital of past rounds of investment tends to intensify competitive pressure and press the profit rate downward.

Profitability is central to this mechanic, but while "wage rates and other input costs influence profit rates […] they are secondary to the disequilibrating effects of investment, competition and technological change in explaining the dynamics of capitalist growth and crisis" (p.205). For Storper and Walker, like Itoh and Brenner, it is precisely "the continued dynamism of capitalist industrialization [that is] the chief cause of destabilization" (ibid). Storper and Walker, however, ultimately portray this dynamism in a more optimistic light, periodically hinting that precisely balanced rates of income, investment and productivity growth can see extended periods of "crisis-free capital accumulation" and that these balances can potentially take very different characters in different places. They juxtapose various "models" of accumulation associated with individual nations or groups of nations and characterized by their relative proportions of consumption, investment and productivity growth. The "Japanese model" for instance, is defined by "modest wage and consumption levels, high profit rates and very rapid accumulation" (ibid, p.213), all of which are features that many scholars (for instance Li and

Gong 2013, Ramo 2004) associate with a supposed China model today. But it is notable that all the countries that Storper and Walker point to were almost immediately thrown into severe crisis in the years just after the publication of the book (1989), hinting that the prospects for "balanced" growth were not so good after all. This, of course, is where the Brenner account steps in, using many of the same metrics at a more macro-economic scale in order to account for this series of bubbles and busts.

Today, it again seems as if China offers the model for a form of perfectly balanced growth, proving the possibility of "crisis-free capital accumulation" in much the same way that the Japanese model was used as evidence in similar arguments in the 1980s. At the very least, the ultimate outcome of the Japanese case should give those making such claims pause. But Storper and Walker, in contrast to others writing at the time, never placed much emphasis on this distant potential of balanced growth. Their mild optimism remains a background feature throughout. Meanwhile, what they provide that Brenner does not is precisely the theory of geographic industrialization essential to understand the relocation of industries and the formation of new agglomerations of production both internationally *and* regionally. While the nation-state remains an essential actor, as "the gravitational force that most tightly binds the class practices and institutional arrangements of politics, economics, and territory" (p.215), it is the workplace and the firm that act as the "basic unit[s] of production under capitalism," since "the firm is a container for capital in its various forms" and is therefore "the central actor in the competitive battle among capitalists" (pp.128-129). The firm itself is, of course, composed of various individual workplaces that make use of particular technologies purchased at particular times according to the prevailing technological level of that time. While firms can certainly survive technical obsolescence, pressures of competition tend instead to lead to overinvestment,

burdening firms with masses of fixed capital that become increasingly unable to attain average profit rates as new rounds of investment in new locales enable even more rapid increases in output. Thus, the very investment necessary to survive obsolescence becomes more and more difficult. Similarly, cost-cutting becomes a continuous necessity.

It would seem, at first, that this privileges undeveloped regions, since these areas will be unburdened with such masses of old fixed capital. But, again, the infrastructural accretion from old rounds of investment is both a blessing and a curse. As Storper and Walker argue: "It is decidedly not the case that peripheral locations are cheaper sites for production, because the same conditions of underdevelopment that make labor or land inexpensive generally make them less productive in use" (ibid, p.73). This is why infrastructural investment that lays the basic groundwork for industrialization, alongside targeted investment either directly through the state or heavily facilitated by it, has been so essential to all "late-developer" growth booms. In China, the development of the early coastal export hubs was only made possible through the systematic build-up of regional infrastructure, financed directly through the various counties and townships attempting to attract investment from across the border (in the earliest stages, this mostly meant Hong Kong and, in some cases, Japan). It was the relative flexibility of local governments, informal kinship connections across the border with Hong Kong and the new financing and administrative tools made available through the township-and-village enterprise (TVE) structures that were central to the geographic industrialization of the Pearl River Delta, for instance, where "there was no excessive expansion of populations and production activities in the primate city of Guangzhou, as the conventional wisdom of urban transition might have predicted" (Lin 1997, p.17) and instead urbanization took place "from below," led by small towns, local officials and their affiliated TVEs.

Infrastructure developed in a similar fashion, as a new settlement type termed “rural-urban integration” (城乡一体化) predominated in the early reform period, wherein small-scale industrial and agricultural activities took place side by side (ibid, p.71). The funds from these activities were then recycled by local officials into infrastructural investments that enabled even more lucrative investments. While the central government decision to designate certain southern locales as the first special economic zones was obviously essential to their success, it was only at a much later stage that higher levels of government stepped in to conduct wider-ranging infrastructural investments and guide industrial policy in the new production hubs. But, regardless of its source, the early outlays on infrastructure and the early involvement of the state (especially in the form of local officials) was essential to gestating what would soon grow to be one of the world’s largest territorial industrial complexes. By the 2000s, both the Pearl River Delta and Yangtze River Delta areas were undergoing ever-more-rapid urbanization driven by agglomeration economies centered on export-oriented manufacturing industries, while the interior was dominated much more widely dispersed low technology industries, many in protected sectors and/or related to resource extraction (He and Wang 2012). Large-scale state industrial policy (especially currency and capital controls) also proved to be indispensable when the Asian Financial Crisis ripped through the region in 1998. While the effects were certainly felt and Southern China saw a wave of bankruptcies (capped by the collapse of CITIC) that fundamentally reshaped certain locales such as Hainan, the crisis was ultimately the opportunity that enabled China to outcompete the myriad Southeast Asian nations that had, until then, appeared to be far more secure sites for investment (Chuang 2018, Brenner 2004). Ironically, these are the nations where industrialization is today proceeding most rapidly, in part driven by

investment from China after more than a decade of relative stagnation in industries that had been outcompeted by the new Chinese manufacturing hubs at the turn of the century.

The Great Recession marks an essential turning point in China's industrial history. The crisis itself was muted through rapid, large-scale state intervention in the form of the stimulus and followed by a pivot toward greater reliance on the subset of core firms more closely linked to the state and the fraction of the Chinese bourgeoisie that operates "inside the system" (内体)—the "system" here being the bureaucracy of the party state, and this fraction including most of the country's largest capitalists, who are also high-level officials and party members. While the stimulus itself was a response to the Great Recession, its aftermath was what necessitated continuing state intervention to restore stability, sustain growth and try to force through a second "spatial fix" by encouraging industrial relocation to the interior. In the years after the Great Recession, global trade slowed and demand for Chinese exports of consumer goods followed suit (Lardy 2019, pp.1-5). Similarly, the crisis was not limited to the collapse in the housing market and financial system in the West. It was soon accompanied by the sovereign debt crisis in Europe (Tooze 2018, pp.319-422). Meanwhile, financial volatility elsewhere was soon paired with increasing speculation and volatility in the Chinese real estate and financial markets, culminating in the overheating of the Chinese stock market in 2015 and subsequent state intervention (Orlik 2020, pp.111-117). All of these events seemed to hint that China could only retain stable growth and stave off the risk of crisis by enhancing the administrative control of the CCP—a vanguard organization of the country's leading capitalists—over the entire economy, *even if this meant sacrificing growth*. This was an integral dimension of the "new normal" declared by Xi Jinping in 2014.

The first stage of this process was marked by the stimulus, which also shaped much of the nature of state intervention in the years to come. The passage of the stimulus package was the immediate trigger for the uptick in the growth of infrastructure's share in total capital stock visible in Herd's measurements. It also set the groundwork for the rising share of housing in total capital stock visible in the same series after 2010. The government share of capital stock, by contrast, has fallen continuously since the early 2000s, hinting that state intervention has mostly been filtered through these other categories. The rise of the infrastructure and housing shares of total capital stock are responsible for the decline in the share of total capital stock devoted to business, which had been steady or growing mildly between 2000 and 2010 but fell rapidly thereafter. Herd's series unfortunately ends in 2016, so it is unclear whether or not the seemingly imminent convergence between the infrastructure and business share of capital stock actually occurred (all visible in Figure 3-5 above). Other data from Lardy hints that, by 2018 or even 2017, some of these trends may have begun to reverse (2019, pp. 15-16).

The most immediate effect of the stimulus was its ability to create millions of jobs to sop up the sudden growth in unemployed migrant workers created by the closure of factories producing consumer goods for export to the high-income countries (Fang and Chan 2009 and Chan 2010). But the longer-run goal of the package was to boost infrastructural development and use the crisis as an opportunity to encourage industrial relocation to China's interior in the hopes of levelling out the deeply uneven rural-urban and interior-coastal divides that had become increasingly stark. This was why

> China's massive and very costly US $586 billion fiscal stimulus package (equivalent to an eighth of the Chinese economy) to cushion the recession and maintain GDP growth

> (the target was set at 8% for 2009) […] poured tens of billions of dollars into public works in road and rail transportation in the inland region. (Chan 2010, p.671).

It was recognized that the Great Recession would lead to both a short-term decline in consumption in the wealthy importing nations and a longer-term reorientation of global production chains to serve more markets in China itself, in wealthier Asian countries and across the "Global South," broadly defined. By the late 2010s, global value chains had, in fact, reoriented in just this fashion (Suwandi 2019), even if they were far from any decoupling with the high-income importers (Gereffi 2014). Relocation of industry to the interior, then, was both a firm-level market response to rising costs and falling demand in the coastal export industries and a state-level industrial policy intentionally designed to deploy a second "spatial fix" in China's favor by retaining firms that might otherwise simply close or relocate to Southeast Asia. The stimulus was designed to accelerate the infrastructural investment required for that spatial fix, but not to replace the fundamental market incentives it was attempting to serve. This is why the initial goal was to phase out the stimulus and thereby undercut the reliance on direct public works investment as early as 2011.

But a few essential features of the stimulus ensured that this would not be so simple. Overall, the spending package that was ultimately rolled out was, technically the same size as that originally promised: 586 billion USD, close to 4 trillion RMB. Then, two weeks after the stimulus was initially announced, its details were laid out, making it clear that the spending would in reality be a joint effort between the central government and the local governments in the locations receiving funds. Of the total 4 trillion that was promised, premier Wen Jiabao "committed 1.2 trillion in central government funds. The balance, 2.8 trillion yuan, was to come

from local governments and state-owned enterprises" (Orlik 2020, p.84). Meanwhile, the overall division of the funds remained the same, prioritizing infrastructure:

> Spending on transport and power infrastructure would be the largest element, accounting for 1.8 trillion yuan of the 4 trillion. There was a trillion yuan for reconstruction following a massive earthquake in May 2008 in Wenchuan […] that killed sixty-nine thousand and left 4.8 million homeless. Rural infrastructure, environmental investment, affordable housing, technology, and health and education filled out the total. (ibid)

Throughout, emphasis was placed on development in the interior. The national road and rail systems were expanded substantially and numerous cities in Western provinces built out subways, highways and numerous real estate developments, often far beyond their necessary scale.

The shortfall of central funds for investment was to be made up for largely by local governments, but not through sole reliance on tax money or their existing fiscal budgets. Instead, local governments turned to the banking system to secure loans to build out infrastructure in line with the guidelines in the stimulus. Nor was this begrudging acquiescence to central mandate. Instead, it was seen as a once-in-a-lifetime opportunity to both make short-term profits through land sales (not to mention intricate networks of local corruption) and to gamble on the potential of becoming the core of a new industrial agglomeration, if investment was successfully attracted. The shape this took was a sort of two-stage bidding war, in which localities first attempted to effectively market themselves to financial institutions capable of granting them loans to build roadways, business parks and high rises, and then attempted to use these amenities (often while they had not even yet broke ground) to attract new rounds of investment from manufacturers. The first stage proved far easier than the second.

In the initial bidding war for access to credit, local governments nationwide built up their debt loads through various local government financing vehicles (LGFVs), often using land as collateral. These LGFVs were facilitated by central bank policy and driven by the interests of local officials and capitalists, who often coordinated to significantly overbid the price of parcels in order to increase mortgage and leasing revenue to fund investment projects (Huang and Du 2018). The infrastructural stimulus thereby created the conditions for a massive real estate bubble—and this is why the housing share of capital stock begins to rise with a lag, while the infrastructural share rises almost immediately. Overall, the rate of new loans increased more or less in line with investment in the transportation network, which accelerated from an already significant 19.7 percent growth in 2008 to 48.3 percent growth in 2009 (Orlik 2020, p.85). As local government debt began to skyrocket, the central state immediately recognized the need to tighten credit conditions, even while it continued to encourage new investment projects. In 2010, this tightening took aim at the real estate market specifically, with central banks raising down-payment requirements, mortgage rates and reserve requirements for lower-level banks. The result was a very sudden drop in home sales which resounded into far-flung industries dependent on the bubble such as steel. Between April and July of 2010, the Shanghai Composite Index "fell from a peak of 3,166 […] to a trough of 2,363 […] losing 25 percent of its value" (ibid, p.91). But growth soon recovered despite the new credit limits. More troublingly, the RMB now began to inflate alongside skyrocketing property prices, risking China's carefully-cultivated currency position, which had enabled its export industries to flourish in the years leading up to the global collapse and to remain competitive even in the midst of the crisis (ibid).

Even more stringent controls in 2011, which was supposed to be the year that dependence on the stimulus funds was phased out, proved unable to deflate the bubble and—with the

emergence of the European sovereign debt crisis driving down export demand yet again—there was even a renewed necessity to unleash the real estate speculators yet again. It wasn't until the first few years of the Xi administration that more decisive action began to be taken, pairing anti-corruption crackdowns with reforms in the evaluation of local officials' performance, more targeted credit tightening and the selective sacrifice of a number of former industrial areas as their firms went bankrupt or were forced to relocate. By this point, there was a stronger realization that industrializing the interior would also entail allowing the market to clear out the less competitive firms in the old coastal industrial hubs, now hampered by older equipment, rising wage costs and skyrocketing rents or mortgages. At the same time, evidence points to the reality that the extension of infrastructural networks had merely succeeded in accelerating the urbanization of the pre-existing urban centers rather than disaggregating existing agglomerations (Yu et. al. 2016).

Many localities seemed to be failing in the second stage of the bidding war, having successfully attracted loans to build out business parks and high-rise complexes but unable to secure sufficient flows of investment in productive industries afterwards. On their own, credit controls and anti-corruption drives would never be enough to truly reign in the risk of the bubble popping. Instead, the state pivoted toward a far more expansive program that accelerated the consolidation of ever-larger firms directly linked to the state, extended more effective forms of party oversight into a greater number of firms across the economy and established administrative tools to prevent capital flight and even liquidate the assets of renegade capitalists through forced mergers (Lardy 2019). This was paired with a careful attempt to deleverage without bursting the bubble, which entailed both direct crackdowns on new bursts of speculation (as when the stock market overheated in 2015) and wide-ranging loan write-offs and accounting reforms led by the

People's Bank of China that sought to better separate its dual role as financial regulator and monetary policy maker. This succeeded in pushing down the share of bad local government loans through a massive debt swap, but this did not entail a debt write-off. Instead, loans obtained at short-term market rates were systematically refinanced through the issuance of 5-year bonds, halving their interest rates and extending the repayment schedule to align it better with the expected horizon of returns for long-term infrastructural assets (Orlik 2020, pp.128-133). In aggregate however, this simply stopped the inflation of the debt bubble, without reversing it: "from 2008 to 2016 China's debt-to-GDP ratio rose from 142 percent to 250 percent. In 2017 and 2018, it levelled off, ending 2018 at 254 percent" (ibid, p.134).

Similarly, the various interventions did nothing to rectify the underlying problem of slowing growth as larger, lower-return firms reliant on state funding (many of them now already or at least dangerously close to being loss-making "zombie" firms) composed a larger share of output and the decline in global trade ensured that the several percentage points of growth traditionally added by the export industries would be substantially cut down. As argued by Mattick (Jr. in Mattick 2011 and Sr. in Mattick 1969) and Smith (2020), regardless of how direct the involvement of the state becomes or whether or not the firms dependent on state funding are categorized as "private," this dependence ultimately acts as an unproductive drain on the total social surplus and will tend to generate diminishing returns over time, *even though it is often necessary* to stave off crisis and sustain short-term growth at much higher levels than would otherwise be possible. The diminishing dividend is visible in rising capital-output ratios, as measured by Herd (Figures 3-6 and 3-7 above) and others (Huang 2018), which measure "how much capital spending is required to buy an additional unit of GDP growth" (Orlik 2020, p.181). Herd's figures clearly demonstrate that the rise in the total capital-output ratio has been driven by

the infrastructure and housing sectors, which confirms Lardy's (2018) claim that part of the depression in growth rates (and much of the downward move in return on assets) has been due to rising state investment, consolidation and intervention. Overall, "the additional GDP generated by each new 100 yuan of credit fell to 32 yuan in 2018, down from 95 yuan in 2005" (Orlik 2020, p.181). In part, this is because the plateau of the local government and real estate bubbles did not entail any real debt write-off, with many of the longer-term bonds still requiring continuous debt servicing, such that, by 2018, "close to 20 percent of GDP ha[d] to be used to service debt" which is a figure "higher than the United States on the eve of the financial crisis" (ibid).

On the ground, these years were marked an intentional pivot by provincial and local officials in the coastal hubs toward various rebranding efforts, focused on attracting new cores of service or higher-value-added production. In these areas, the bidding wars were therefore less about bidding against interior locales in an attempt to retain pre-existing industrial agglomerations and more about competing against other coastal hubs and various international challengers to attract investment in new sectors. Similarly, ongoing infrastructural development was less focused on building out base infrastructure, instead placing more emphasis on the development of lucrative high rise apartment buildings and office complexes, the wholesale replacement of old factory buildings with new "high-tech" business parks or the redevelopment of such buildings into hip arts and service centers, and the linking-together of pre-existing hubs via new high speed rail systems. These efforts were encouraged at high levels but were only made a material necessity when it became clear that smaller firms would be cleared from many high-value locales through either bankruptcy or mergers regardless of whether or not local officials had an interest in retaining them.

While living in the Pearl River Delta in 2014 and 2015, I saw both the reverberations of earlier factory closures and the early efforts toward rebranding and recentering different portions of the territorial industrial complex on various "new" industries. In Guangzhou, I explored two ends of this process in various "urban villages" (城中村) within the city. Urban villages are portions of the city where an old village had been absorbed by an expanding urban center but wherein some villagers had not yet sold the land off to urban developers or had not agreed to deals made by their village leadership. Such areas are marked by tight-knit, informal construction defined by squat multi-story buildings intercut by narrow alleys (since villagers are usually paid per square foot of their land parcel), often entirely surrounded by towering modern skyscrapers. Once important sites for housing migrant workers and bustling with small textile workshops, these urban villages have since faced both dimming economic prospects and repeated crackdowns by the authorities who, in league with real estate developers, sought to buy out the villagers and sell off the increasingly valuable land (Buckingham and Chan 2018 and Wang 2016). The most explosive of these cases was in Xian (冼) village, lodged right in downtown Guangzhou and abutting the central business district, with the China Development Bank skyscraper looming over rubble from partial demolitions and tightly-packed informal construction in the village below. The attempt to demolish Xian village was marked by years of protests, often turning violent as gangs hired by the developers clashed with local villagers who refused to stay and checkpoints had been set up at all the entryways (McMurrow 2015).

When I toured Xian village in 2014, the village was partially sealed off from the rest of the city by security checkpoints after the protests, a good portion of it had already been demolished, the vast majority of its once-substantial population of migrant renters had left and only a small share of its businesses continued to operate. But other urban villages had not

suffered such an extreme crackdown. In these villages, the old economic base of renting to migrants and small workshops (the sort that would easily fall below the "designated size" in NBS measures) had begun to shift more fully into services. In Beigang (贝岗) village, on the same island as Guangzhou's University Town (大学城), hip cafes and restaurants had opened to serve students, who also provided steady demand for rentals. The same was the case with Baishizhou (白石洲) in Shenzhen, which I visited in 2015. Like Beigang, Baishizhou lies near a major university. It also abuts the hip OCT neighborhood, itself full of cafes and artist lofts in converted factory complexes. While Baishizhou seemed to have retained more of its share of migrant renters, this was in part simply because the share of migrants in Shenzhen's total population was far higher (they compose the majority of the city population). But by 2015, it was equally common for students or poorer service workers to be renting there.

In Guangzhou, another important economic base for the urban villages was wholesale trade, often in the same textile sectors that the workshops had once operated within—and frequently in the exact same buildings. For instance, African traders had settled in various villages in and around Guangyuanxi Road (广园西路) and Xiaobei Station (小北站), both in Yuexiu District (越秀区), where they worked in wholesale trade, purchasing bulk goods for export back to Africa. At first, they lived alongside the city's migrant factory workers, but as more industrial firms moved outside the city these districts saw larger concentrations of Africans and much more dependence on trade. By the time of my visit in 2014, the African migrants themselves were facing constant crackdowns from immigration officials. Subsequent years have only seen accelerating efforts to gentrify the area (Huang 2018). Similarly, in Lujiang (鹭江) and Kangle (康乐) villages, near Sun Yat-sen University, numerous buildings that once hosted textile

workshops had since been converted to wholesale storefronts, selling textile products in bulk but not longer producing them nearby. The larger suppliers for such storefronts in these two villages and in Yuexiu District were located in large wholesale warehouse malls in the city's north, which often sat on sites that had themselves once hosted textile factories.

But by the mid-2010s these areas seemed increasingly under threat of demolition. Guangzhou officials were seeking to reorient the city more exclusively around high-end services. In numerous locations, old factory infrastructure had been demolished and replaced by new high-rises. In a few areas some of the old buildings were converted to hip art spaces and cafes. But ground-up redevelopment was the more common goal, as in Xian village. Meanwhile, on the other end of the delta in Shenzhen, the shift was made not toward services but toward higher-end electronics assembly. When I lived there in 2015, the bulk of Apple's iPhone production had been concentrated in the Foxconn plant in the city's north and the marketing of the city as the "Silicon Valley of Hardware" was well underway (Demuth and Dixon 2016). By contrast, the neighboring city of Dongguan had been less able to pivot onto a new economic base. Heavily reliant on the textile sector, Dongguan began to hemorrhage firms and population after the Great Recession. The process was often accompanied by strikes and protests on the part of workers seeking back wages and the payout of their social insurance before they left the area. One of these, at the Yue Yuen shoe factory, was the largest strike in China to date (Schmalz et al 2016). While some of Dongguan's townships subsequently recovered (and Dongguan in fact led the resumption of manufacturing after the coronavirus lockdown), many have experienced secular shrinkage of population and economic activity over the 2010s (Lu and Du 2019). Overall, the 2010s saw both greater regional diversification within the older territorial industrial complexes as many old manufacturers closed down and/or moved elsewhere and a more limited re-

concentration of certain hold-out industries, creating even further internal diversification at the township-to-township level within cities like Dongguan.

Overall, the efforts to relocate industries to China's interior seem to only have been partially successful, as compared to initial claims. Many parts of the Pearl River Delta did see a real decline in the number of manufacturing firms, concentrated among foreign firms producing for export. In Yantian, just east of Shenzhen, for instance, there had been over four hundred foreign firms in 2001. By 2013, that number had declined to a mere one hundred and fifty, since "higher labor costs, a dearth of available land, and weaker export demand in the wake of the great financial crisis [which] drove some into bankruptcy; others to cheaper locations" (Orlik 2020, p.105). Similarly, the total population of migrant workers in the city was cut in half and "the factories that remained faced lower profitability," in part due to the inflation of the RMB (ibid). Factory relocation was often accompanied by decoupling from regional ties established in the old territorial industrial complexes in Southern China and a recoupling with new territories in the inland provinces. This process was often facilitated by individual factories' transnational ownership, but it was not simply a relocation of the same export-oriented production to a new site. Instead, it saw the relative institutional power of suppliers increase and was often accompanied by a reorientation toward serving both the domestic market and/or new markets elsewhere in Asia or in middle-income or even low-income countries worldwide (Yang 2013 and 2014, Zhu and Pickles 2014, Gereffi 2014). The relocation of an Intel assembly line in Shanghai and its reopening in Chengdu, the opening of a new HP laptop manufacturing facility in Chongqing and the relocation and Foxconn's decision to open almost all its new facilities after 2009 in interior locations were all high-profile examples of this phenomenon (Yang 2014). Similarly, the regionalization of international trade and the declining dependence on imports

have been clearly visible in global trade data and in the total value of China's exports as a share of GDP (Lardy 2019, p.3).

The effects of these relocations were also visible in provincial statistics, with Chongqing and Henan, for instance showing increases in the growth rate of total value-added after 2008 (ibid, p.146). Similarly relocations were hinted at in the boom in fixed capital investment and a proliferation of industrial parks.[51] However, these parks were almost always built in advance of actual investment in new factories and were a necessary precondition for locales to participate in the second round of bidding wars that occurred in an attempt to attract such investment. Between 2007 and 2010, Chongqing's fixed asset investment "had been increasing approximately 30 per cent per annum" and "Henan Province's urban fixed asset investment [increased] at a speed exceeding 30 per cent" between 2006 and 2014 (ibid, p.147). As has already been mentioned, local government debt was the key factor fueling the initial bidding wars over debt and the subsequent bidding wars to attract investment. But there was also a high degree of variability in which firms tended to relocate where based, in part, on the manufacturing sector within which they operated and the global, regional and local context of that industry at the time of their relocation (Zhu and He 2013, Yang and He 2016). The resulting geographic "rebalancing" has therefore itself been somewhat uneven, since different localities have seen divergent levels of success in the final round of bidding wars and different product lines have seen divergent success in their ability to relocate within China at all. Ultimately, the internal "spatial fix" seems guaranteed to be somewhat temporary and, in the long term, its basis in debt-fueled local

[51] Fixed asset investment, however, also increases when mergers and acquisitions rise. Since this was occurring in the same period, it is somewhat difficult to disaggregate the two. Nonetheless, since all other measures of capital formation show positive (albeit more modest) growth in this period, there's good reason to believe that the two are at least correlated even if the NBS investment in fixed assets measure overstates real growth in fixed assets.

government bidding wars “may even worsen rather than ease structural contradictions such as the lack of innovation and domestic consumption” (Yang 2014, p.150).

But the national context also creates certain distinct constraints that are not so easily overcome. First are the demographic trends, which are no longer in China’s favor even if some scholars such as Lardy (2019) argue that the influence of demographic factors is somewhat exaggerated. The fact remains that, no matter how far inland firms relocate, they will never again find the same high shares of young, capable workers that flooded into the first coastal production hubs in the 1990s. The family planning policy of the reform era, combined with the downward pressure on fertility that accompanies urbanization in general, all but guaranteed that the Chinese growth pyramid would invert. Even if the Lewis Turning Point was, in fact, passed as early as the turn of the millennium (Lardy 2019, pp.23-25), the 2000s and early 2010s were still years in which a strong demographic dividend persisted since the population pyramid had not yet seen a strong inversion. Moreover, the inversion can’t simply be reversed by a change in policy since it is no longer primarily policy-driven but instead an outcome of economic development itself (Orlik, pp.108-110). Lardy suggests that investment in “human capital” and a forced increase in the retirement age would both offset the tendency (2018, p. 24), but these are clearly rearguard measures that would merely be capable of diminishing the decline. His more important observation is simply that the demographic dividend may not have ever been the primary source of growth and continues to be secondary to trends in global trade and domestic productivity.

The second important constraint is the fact that, despite cheaper labor in the interior, all Chinese provinces still rely on the use of the RMB and, therefore, depend on a lucrative RMB exchange rate to ensure their competitiveness in the global market. The decline in global trade has been by far the biggest factor in the decline of Chinese growth overall (Lardy 2019, p.3).

Being able to cut labor costs by relocating to areas where subsistence goods are somewhat cheaper is a rearguard defense that does nothing to change the diminishing competitiveness of the yuan against the dollar, euro or yen. While China's size and relatively low GDP per capita (as measured at purchasing power parity) ensure that increases in domestic consumption could conceivably sustain growth at higher levels and for longer than were possible in, say, Japan in the 1980s, relying mostly on domestic growth will almost always result in slower overall accumulation than that contributed by the export sector (McCombie and Thirlwall 1994). While it's perfectly possible that Chinese growth rates and even rates of return could recover, as Lardy suggests, this would entail the dismantling of the emergency intervention measures that the state implemented to stave off crisis, another round of restructuring (including forced bankruptcy) for under-performing large enterprises, and a renewed exposure to both global capital and goods markets. All of these options bring a renewed risk of crisis itself and many seem to imply that China would need to pause the slow inflation of the RMB already underway. In the end, it seems more probable that growth will continue to slow, though not as much as is predicted by pessimistic commentators (like Pettis), and that currency appreciation will induce further relocations of firms beyond China.

In fact, relocation overseas was already proceeding in the same years as domestic relocation. While higher-tech firms like Foxconn or HP may have seen continued benefits in relocating to the interior, lower-tech firms (or even lower-tech segments of the same transnationals) often found it more lucrative to relocate overseas. Often, these moves were being made by transnationals headquartered elsewhere, even if they may have involved Chinese management with production experience gained on the mainland. But offshore relocations were also common among domestic manufacturers (Yang 2016 and Yuan 2018). In all cases, the

underlying pull factors were the same: As early as the 2000s, Chinese Unit Labor Costs had ascended above those in neighboring Southeast Asia, but by the 2010s they began to accelerate even further, approaching those in the US and seeing the relative costs of production in Southeast Asian countries drop to between one third and one half of those in China (Yang 2016, p. 5, Figure 1). According to official data from MOFCOM (and confirmed by other sources, such as Strangio 2020) Southeast Asia seems to have been the major recipients of china-origin manufacturing FDI, though growth was also substantial in Bangladesh and Pakistan (Ramachandran 2019 and Garlick 2018). Similarly, more scattered investments in manufacturing have been part of the general increase in outbound FDI (Yu et al 2019, Yuan 2018). In the lower-tech production lines most susceptible to offshoring, however, there has been a continual tension between the potential for automation within China and the comparative costs of simply relocating to a cheaper labor pool even where automation might be technically feasible (Altenburg et al 2020).

While the ultimate field focus of this study is the character of Chinese firms operating in Dar es Salaam, Tanzania—covered in the next chapter—it will still be helpful to briefly frame these distant relocations relative to domestic changes in the geography of Chinese industry. The following chapter will therefore explore some of the statistical evidence relating to the decline in profitability, disaggregated by province and region, and the relative geographic trends in enterprise, profit and asset growth. The method follows from that used in the last chapter but attempts to decompose the various rate of return measures by geography. This will be followed by a brief overview of the global data on Chinese outbound investment, as well as a more focused look at Chinese investment in Africa specifically, since the African continent is one of the few locations where long-run demographic factors seem to lie in favor of future

industrialization. This will prepare the ground for the final chapter, focusing on the results of my own field work in Dar es Salaam's industrial zones and the role of Chinese firms within them.

# Section 1
# –
# Trends in Profitability and Enterprise Growth in China

As above, it will first help to frame the overall data by looking at a few of the key underlying variables and their interrelationships. Here, it is possible to divide by both province and region. But there are a few important caveats: First, relocation to the "interior" can also mean relocation to the interior of an individual province, including coastal ones. Since this sort of relocation lies below the geographic units of analysis used here, it will be entirely unaccounted for. Second, there is no consistent definition of which provinces are "interior" and which are not. Similarly, the "coastal" provinces that have seen the fastest growth do not actually include all provinces that face the Pacific. Liaoning in the Northeast and Guangxi in the South never saw the same rates of growth as Guangdong or Zhejiang. Third, the official regional divisions used by statistical authorities often lump together "interior" and "coastal" provinces or include overly large categories that fail to capture internal differentiation. Fourth, Chinese provincial-level data also includes data for four centrally-administered urban areas, similar in status to Washington

DC in the US: Beijing, Tianjin, Shanghai and Chongqing. Of these, Chongqing is essentially the size of a small province and includes a substantial rural hinterland. Beijing, Tianjin and Shanghai are each centered on their respective cities but include much of the surrounding metropolitan area. Measured by land area Beijing is about half the size of Hainan, the smallest province, Tianjin is about a third of the size of Hainan and Shanghai, the smallest geographic unit shown here, is about a fifth the size of Hainan. By contrast, Chongqing is more than twice as large as Hainan.

While I will use the conventional province classifications, including each of the four centrally-administered municipalities, I will be using my own regional definitions, visible in Map 4-1:




*Map 4-1*

These are based on the Economic Regions identified in various development plans issued by the government, but with a few key changes. I have split the overly large Western China region into distinct Southwest and Northwest categories and assigned Inner Mongolia to the Northern region rather than either of the Western ones. Similarly, I have attempted to differentiate the coastal region by splitting it into three categories. The South Coast region is centered on the Pearl River Delta in Guangdong. It includes neighboring Guangxi and Fujian, as well as the island province Hainan. The East Coast region is centered on the Yangtze River Delta in Jiangsu, Shanghai and

Zhejiang. This region also includes neighboring Shandong since the province's primate city, Qingdao, lies on the Eastern coastline but does not face the Bohai Sea. The Northern region includes the economic cluster in Beijing, Tianjiin and Hebei, all of which face the Bohai Sea. This region also includes neighboring Shanxi and Inner Mongolia, in an attempt to differentiate better between relocation to "central" provinces vs "western" or "northern." The Northeastern category includes Liaoning, Jilin and Heilongjiang, which are all consistently classed as such in conventional regional categorizations. The Central region focuses on landlocked provinces in the very center of China's populated area, but it excludes: Chongqing and Guizhou, which are both classed as Southwest; Shaanxi, which is classed as Northwest; and Shanxi, classified as North.

**Trends in the Component Variables**

The regions and the provinces within them see substantial differentiation in the absolute values of each of the variables, with the most economically dominant provinces and regions distinctly outpacing others. This is visible, for instance, if we look at the Value-Added of Industry for each province, faceted by region:

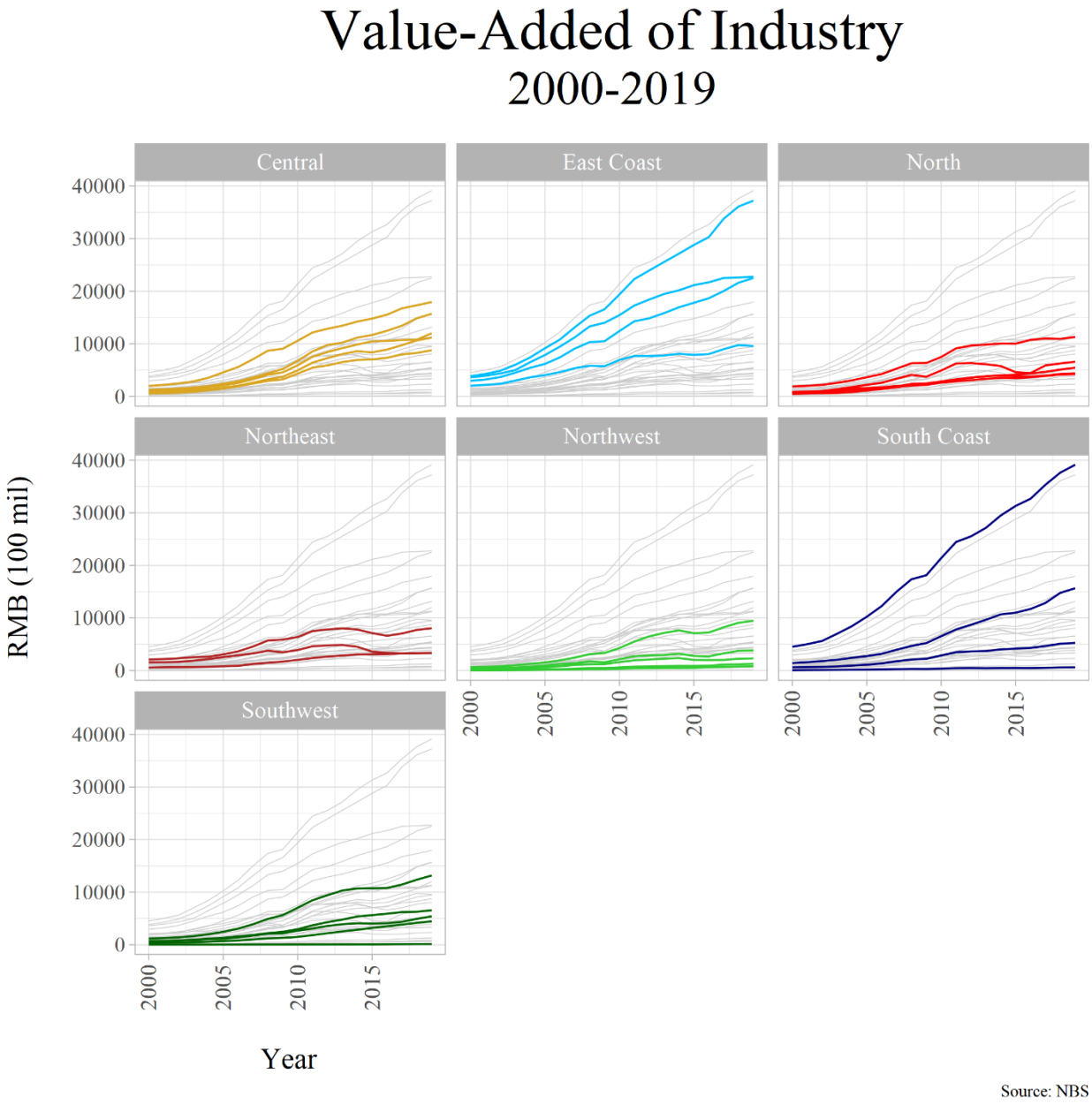



*Figure 4-1*

Even though the Value-Added of Industry is not what will be used in the return on assets measure below, it gives a better sense of the provincial and regional divergence in economic development since it is a more inclusive measure of total output. In the figure above, the trendlines for all provinces are shown greyed out, with each individual province highlighted and

grouped by region. Clearly, the South Coast and East Coast lead the trends, with Guangdong and Jiangsu showing the fastest growth and highest total value-added. At the same time, the internal differentiation within individual regions is also clearly visible. Guangdong leads the other South Coast provinces substantially, while the East Coast is split into three groups, with Jiangsu in the lead, Zhejiang and Shandong in the center, and Shanghai well below. Some of this dispersion is due to the differing size and units of comparison. But in some cases, similarly sized provinces within the same region, such as Guangdong, Guangxi and Fujian, show distinctly different output levels.

This internal dispersion is ultimately disguised when the values are aggregated by region. But providing the regional sum also shows the divergence between regions more clearly:

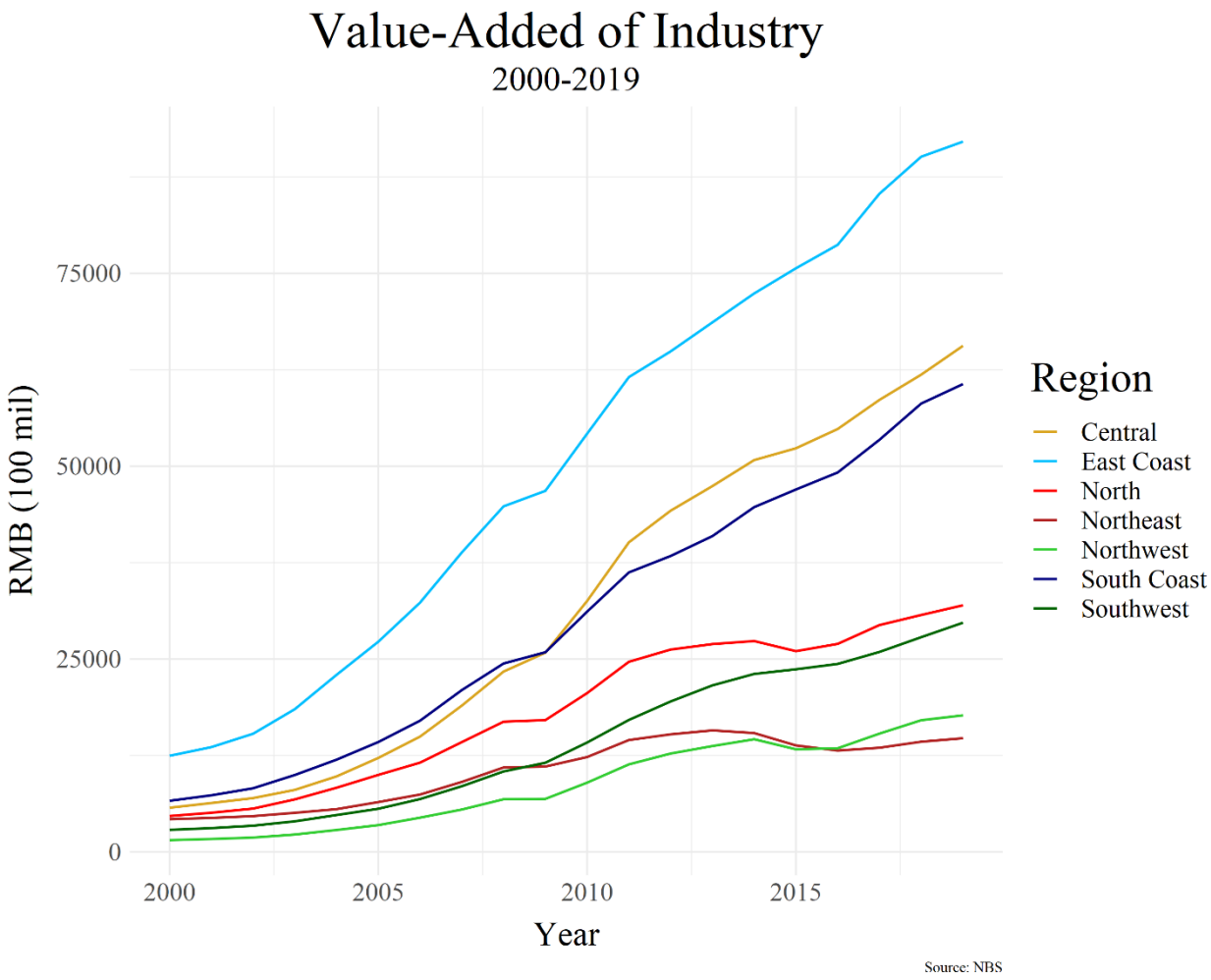



*Figure 4-2*

Notably, summing value-added by region switches some of the relative weights that might have been presumed when looking at Figure 4-1. Here, even though the East Coast and South Coast regions have the same number of provinces and the South Coast includes the province with the highest value-added of industry out of all provinces in China (Guangdong), it's clear that the East Coast has a far higher value-added in total. Similarly, the Central Region actually matches the South Coast, with the relative position between the two inverting after the Great Recession. Thus, summarizing by region can help to clarify many of the larger-scale relationships between variables.

Total Assets essentially show the same pattern of general increase over time, though the North region rises to the same relative level as the Central and South Coast regions. Since the trend is the same, it won't be shown here. The same is not true, however, for net profits of industry:

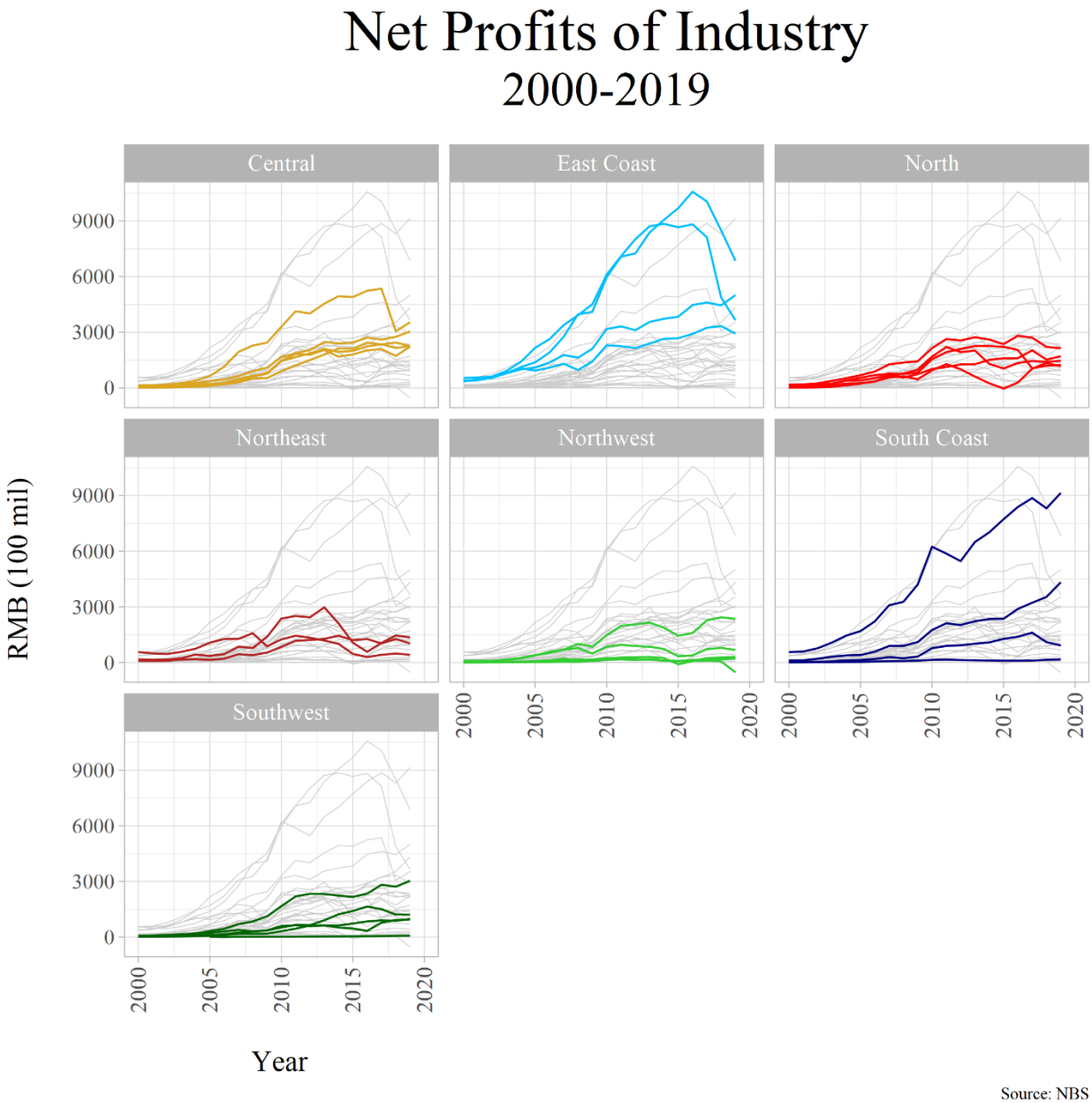



*Figure 4-3*

The trend here shows greater divergence between provinces, across regions and within regions. In particular, strong growth and then decline is visible in three provinces: Henan in the Central region and Jiangsu and Shandong provinces in the East Coast region. Earlier dips can also be observed in some Northeast and Northern provinces. Summing profits by region can help to clarify some of these trends and compare across regions:

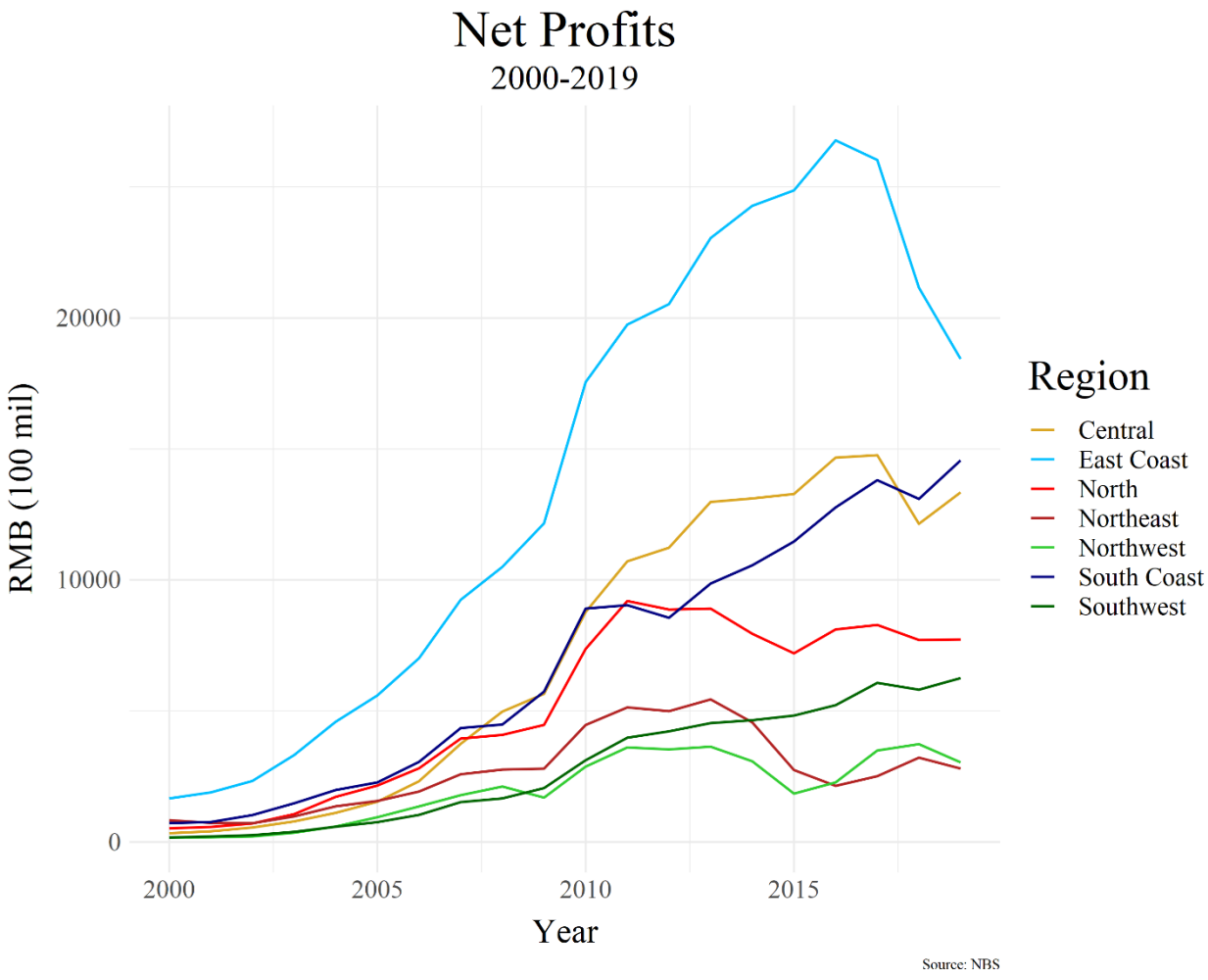



*Figure 4-4*

The same basic trends can be observed here, even if the aggregation mutes some of the individual provincial trends seen in Figure 4-3. The East Coast sees a substantial decline in total profits after 2016. While several other regions see some decline in the same years, none sees it to such an extreme extent. This is important, because the trend in the East Coast clearly exerts a strong influence in the overall trend visible in the last chapter. In the Central region, a somewhat similar trend is visible in a more subdued form, with a recovery in final years. But this is almost entirely driven by the influence of Henan. Most other provinces in the region show a steady growth in profits throughout. Similarly, in the South Coast and Southwest there has been no overall decline in total profits in the 2010s, though there have been certain years where the number has decreased slightly before continuing its ascent. In the North, Northeast and Northwest, an increase in the early 2010s was followed by an early decrease toward the middle of the decade, followed by either a plateau (in the Northeast), a mild recovery (in the North) or a full recovery (in the Northwest).

The total number of enterprises follows roughly the same trend but with the variation between peaks and troughs softened:

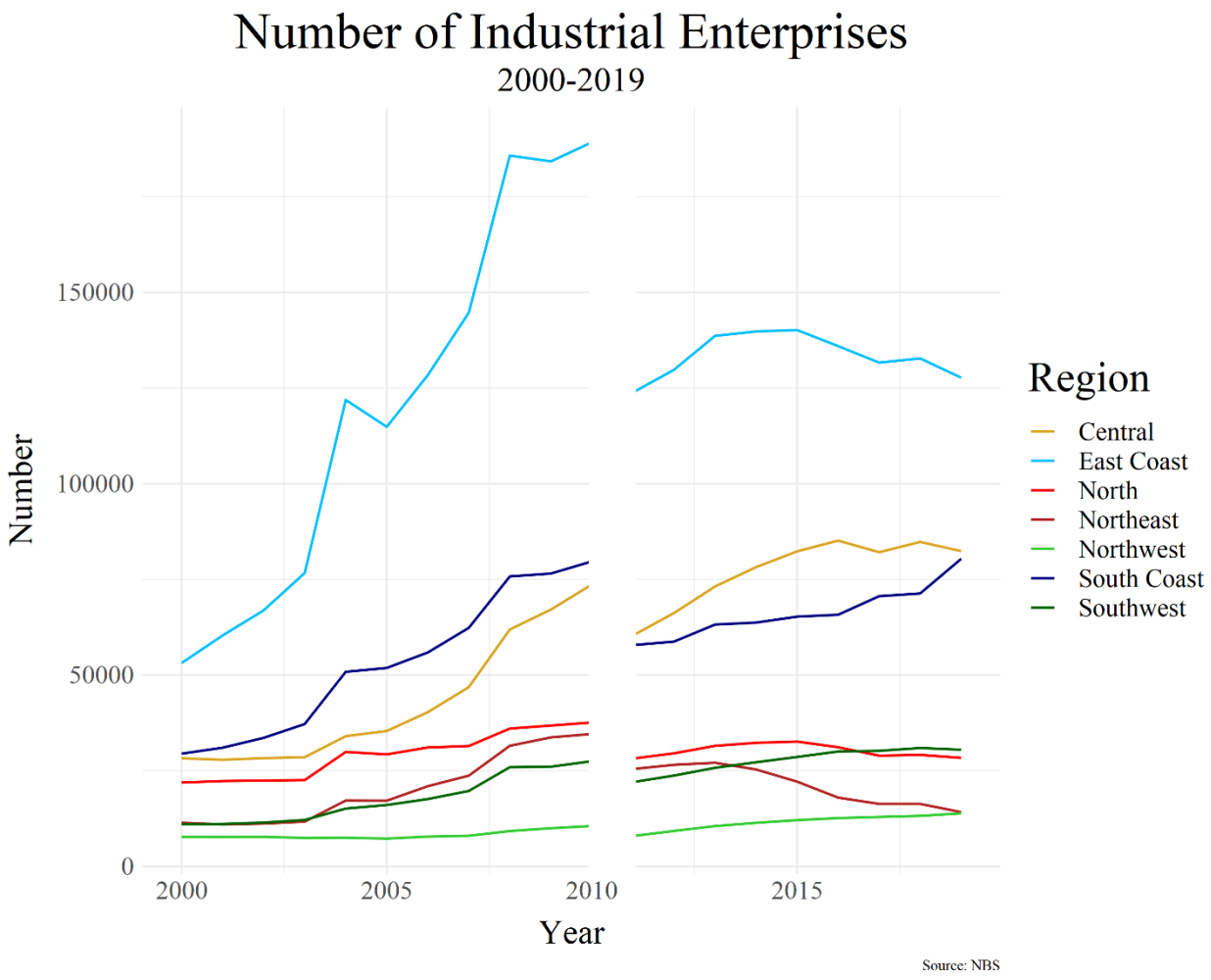



*Figure 4-5*

Here, the statistical redefinition in 2011 is portrayed as a harsh divide in the data. The redefinition exerts a downward pressure on all the counts, but the effect is most distinct in the regions with the greatest number of enterprises. The relative positions of all the series are preserved before and after the redefinition, with the exception of the Central and South Coast regions. The two were on the path to convergence prior to 2011 and, after it, inverted their relative positions, with Central lying above the South Coast region for the remainder of the decade, though in the final year of the series the two appear to be approaching another convergence. Similar inversions are seen due to the continual decline in enterprises in the

Northeast after 2013 or so and the gradual rise in enterprises seen in the Southwest, which surpasses the Northeast around 2014 and the North around 2016. The Northwest also sees a gradual increase and, by 2019, appears to have converged with the declining Northeast.

Combining net profits and the total number of enterprises also allows for a sense of the changing scale of production and relative divergence across provinces and regions in the relative performance of industry. The simplest way to do this is to calculate a simple measure of net profits per enterprise:

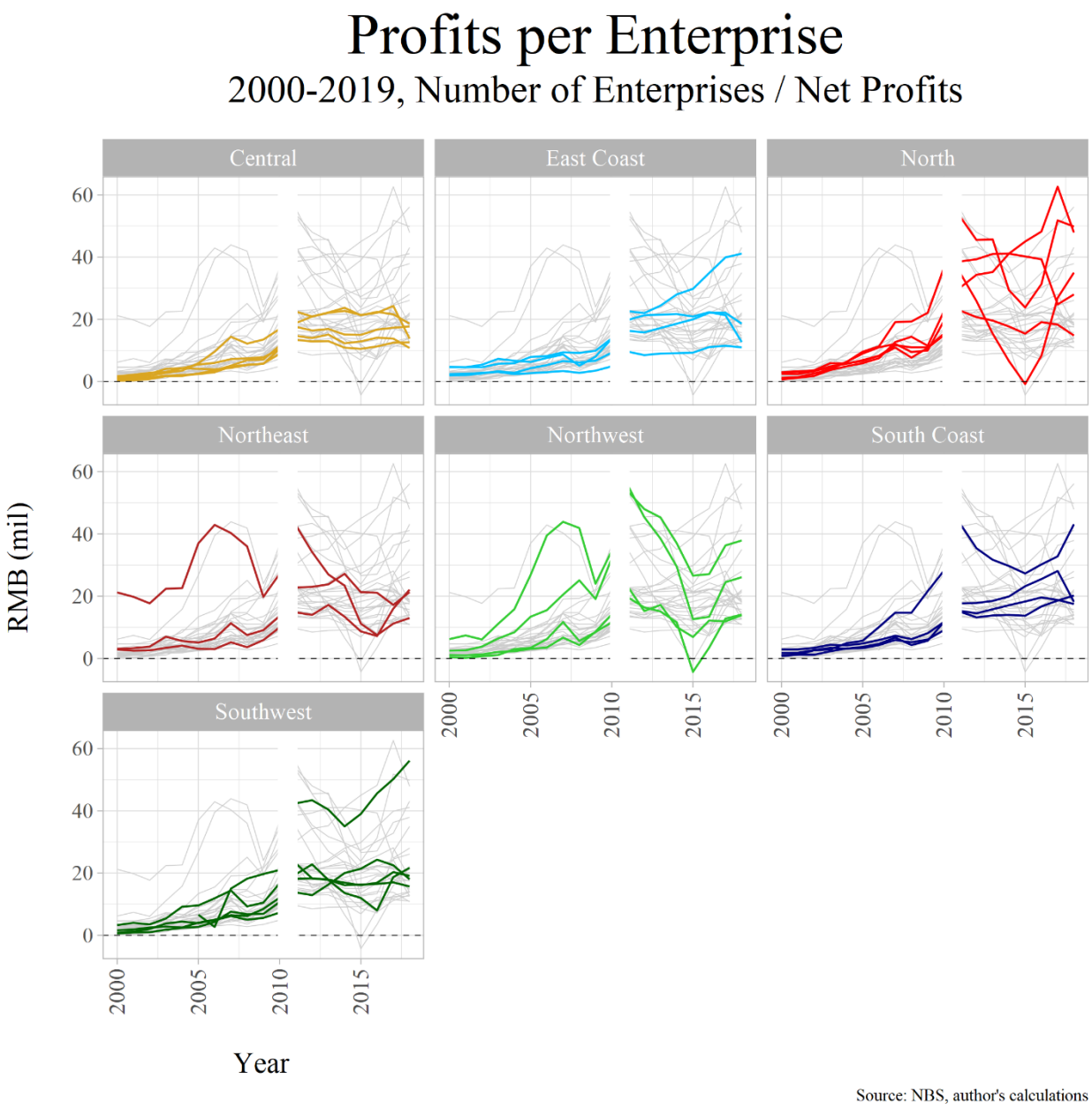



*Figure 4-6*

The above figure shows profits per enterprise for all years except for Qinghai in 2019, which had an extreme negative outlier that distorted the scaling of the plot. Here, the trends are much more divergent both between and within regions and at the provincial level areas with smaller numbers of enterprises see the most exaggerated upward outliers—Tibet in the Southwest, Shanghai in the

East Coast, Beijing and Inner Mongolia in the North and Hainan in the South Coast. These can be corrected for by calculating the same ratio using the regional sum of profit and regional sum of enterprises instead:

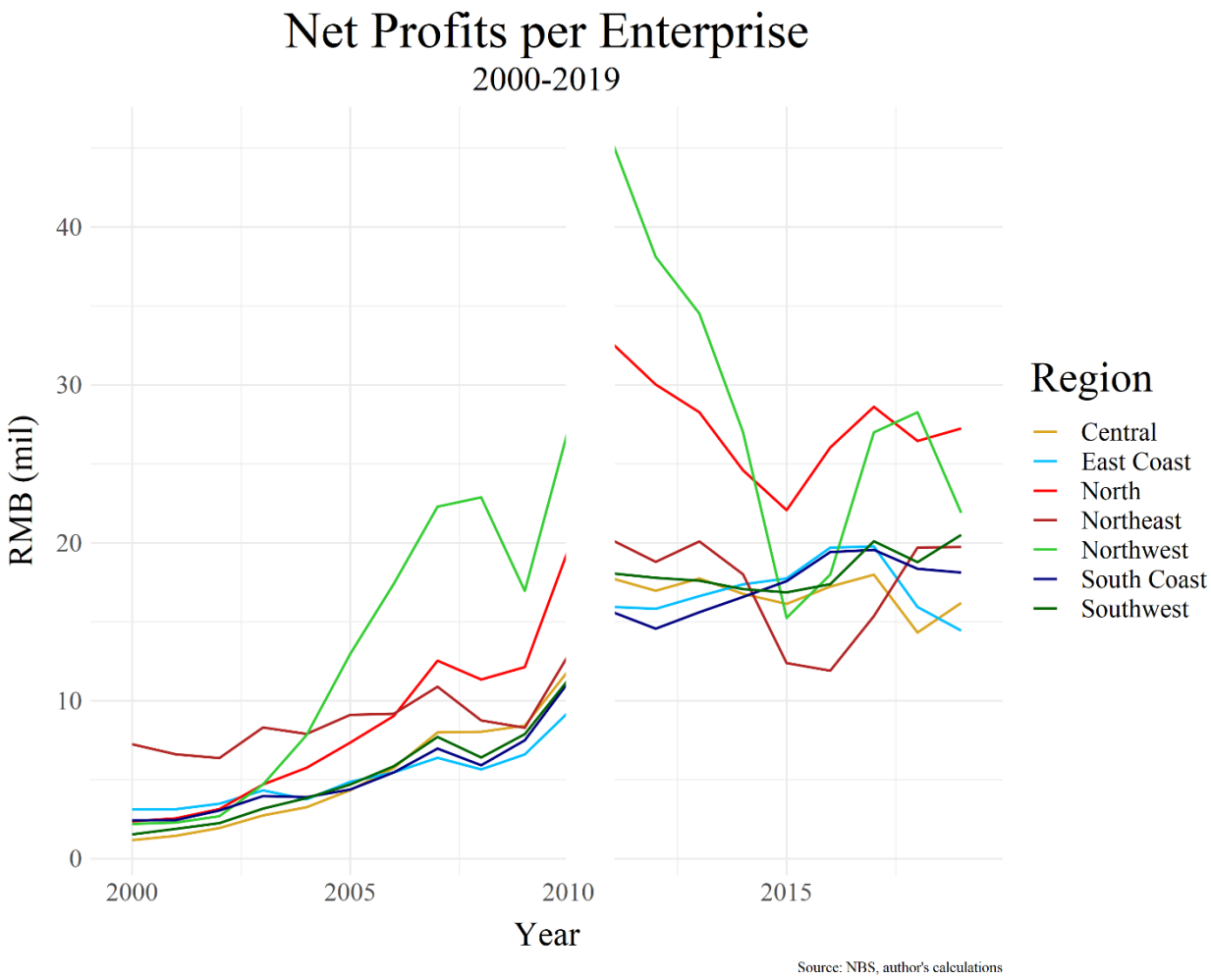



*Figure 4-7*

The figure above clarifies the relative movements of the series, showing that regions tended to see profits per enterprise increase over the 2000s and stagnate or drop over the 2010s. [52] The most extreme cases are the Northwest and North, likely due to a combination of low numbers of total enterprises (see Figure 4-5) and a greater concentration of enterprises within sectors such as mining, natural gas and petroleum, where both the average size of enterprises is larger and the potential for super-profits much greater. The harsh drop followed by a slight recovery seen in the Northwest, for instance, may represent the process of diversification of industrial activity in Xinjiang, the reforms of the coal industry in Shaanxi or the general decline and then mild recovery in profits in Gansu (where the figure dipped into the negative in 2015)—in every location these trends were related to the relative performance of industries producing base commodities.

**Trends in Profitability**

But the overall trend toward stagnant profits per enterprises over the 2010 across all regions is likely due to some combination of increasing scale of production and declines in relative efficiency, which Lardy (2019) associates with the rise in state investment. Since Lardy uses return on assets to make this case—which is also what is used in the chapter above—it will be useful to disaggregate the return on assets series by geography in the same way:

[52] Note that this is not an average of the figures in 4-6, but instead the same ratio calculated on the sum of net profits in each region and the sum of enterprises in each region. This reduces the relative influence of the smallest provinces (in terms of either net profits or enterprise numbers).

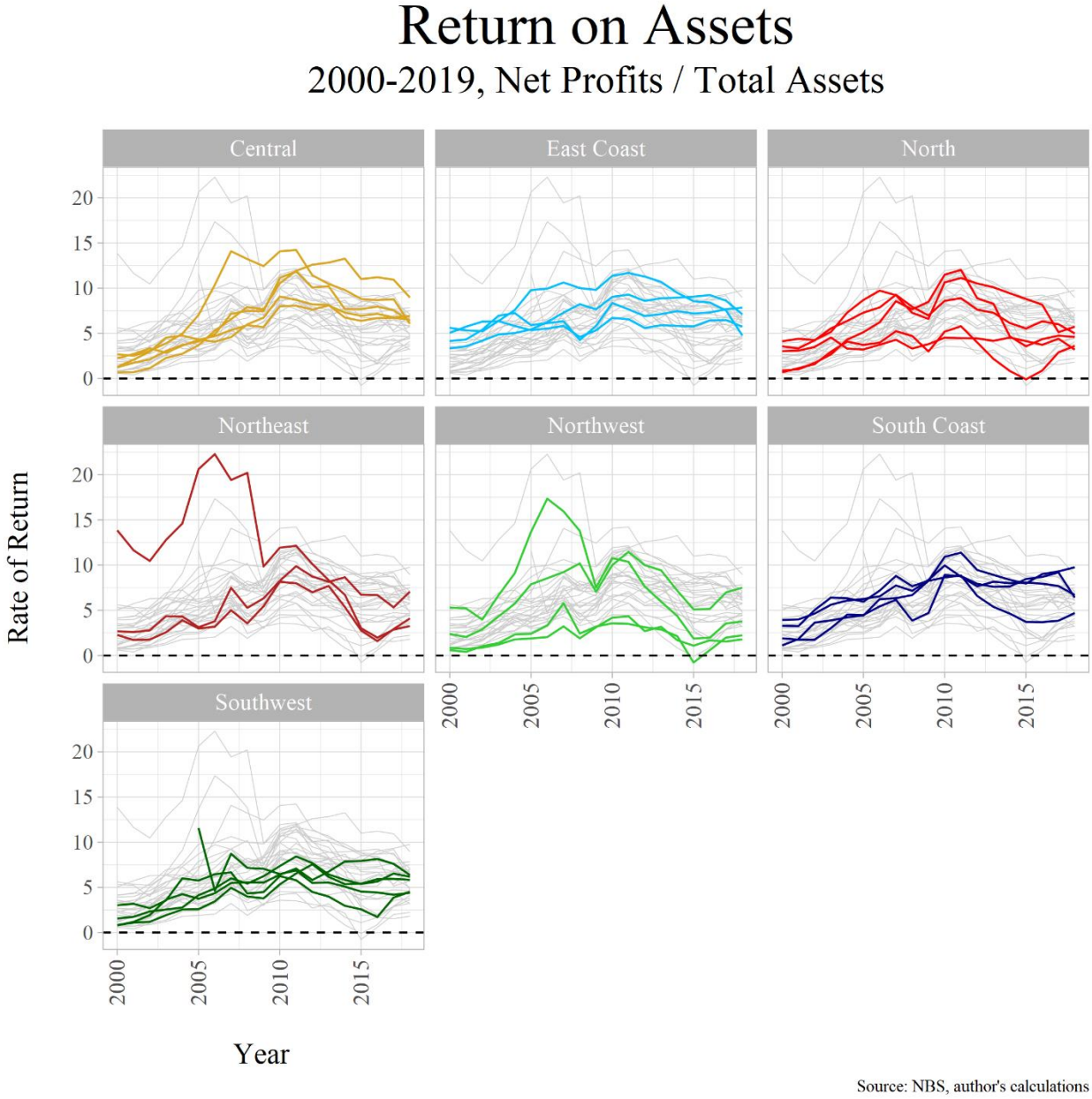



*Figure 4-8*

The figures here tend to lie within a similar range of one another, particularly in the 2010s. In the 2000s, there are a few major outliers, all of which are likely linked to natural resource and agro-processing booms: Heilongjiang in the Northeast, Xinjiang in the Northwest and Henan in the

Central region. After 2010, the most extreme outlier is downward: Hainan in the South Coast region.

The relative convergence of return on assets is more visible when net profits of each province is summed by region and then divided by the sum of total assets by region:

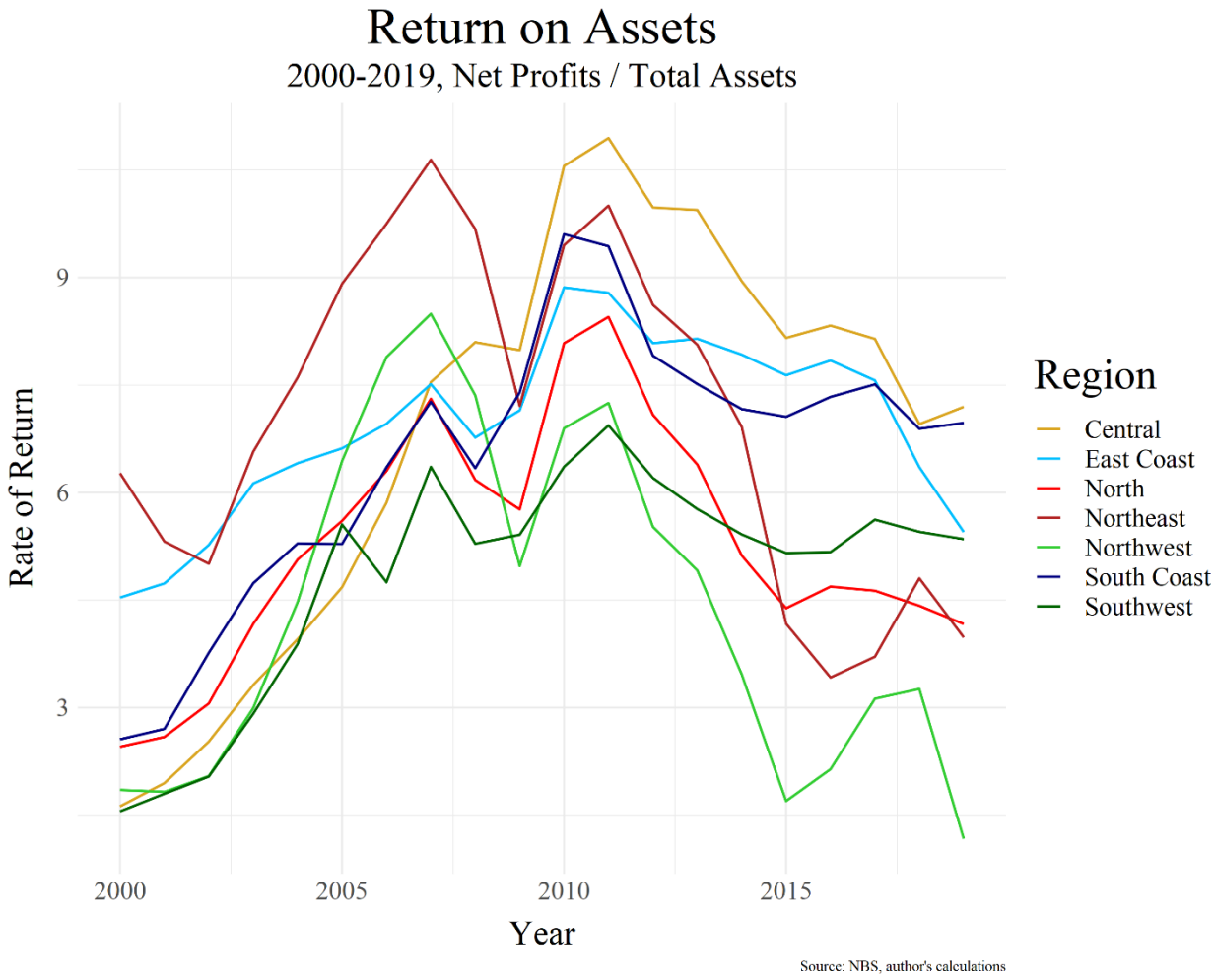



*Figure 4-9*

This clarifies both the relative differences between regions and the overall trend across all the series. As in the national case, return on assets tends to increase sharply over the 2000s, dipping

during the Great Recession, recovering along with the initial stimulus and then dropping for the next decade—though not monotonically, since many regions see a brief recovery sometime between 2015 and 2017. Today, the return on assets in many regions lies about where it sat in the early 2000s, when industry was still partially burdened with the massive debt loads of the unreformed state enterprises. No region has seen an opposite trend, though the final few years in the series show some important divergences , with mild recoveries in a few regions (though, in the case of the Northwest, the recovery is followed by another decline).

Between regions, however, there are a few extremely important changes in relative position. The first is the absolute decline in the Northeast, which sat at or near the top of the series in the 2000s, likely due to demand for coal and other natural resources as energy consumption boomed with urbanization and the build-up of the sunbelt industries. Second is the rise of the Central Region after the mid-2000s, pulling into the lead after the Great Recession and retaining that lead throughout the 2010s, though recent years have seen it narrow to the point of near convergence with the East Coast. This is important, however, since it shows the bottom-line pull factor driving enterprise relocation inland. Finally, the decline of rates of return in the Northwest in the 2010s are equally distinct, with the region declining to the lowest position in the midst of the Great Recession, recovering somewhat during the stimulus, and then declining again to the lowest position in the years after. As in the Northeast, some of this trend is likely due to changing relative demand for natural resources. But it's also likely related to the recent emphasis placed on building out infrastructural projects across the region and in Xinjiang especially. Since much of this activity has been heavily subsidized by the state and large-scale state enterprises (or state-funded infrastructural projects) tend to have lower rates of return, it

makes sense that this region would see an even more extreme decline in the rate of return over the course of the decade.

Ironically the trends in the upper and lower ends of the series are both likely to be linked to growth in the number of total industrial enterprises. Lardy argues that state investment is less responsive to return on assets. This is not exactly true, since firms receiving state investment are still seeking profitability—they are just able to do so with a longer horizon, since they can subsist on state subsidies in the interim. Thus, it is more accurate to say that state investment is more attuned to expected long-term return on assets, while private investment is more attuned to potential short-term returns. Since state investment has composed such an important part of the build-out of industry in Xinjiang and private enterprise has only just begun to develop on this basis, we'd expect to see the total numbers of enterprises increase in the Northwest region as a whole even though the rate of return remains low. On the other hand, the Central region has seen a larger share of private investment and has been especially well positioned to supply the domestic market. Since private investment will be more responsive to short-term returns, the two should be more closely linked in the Central region. Similarly, the harsher downturn in profitability in the East Coast region in recent years will likely be associated with a decline in the total number of enterprises. The South Coast, by contrast, saw most of the decline in return on assets in the earlier 2010s and has since seen a mild recovery, which should be associated with either stagnation or a renewed increase in total enterprises. Across the board, however, it's essential to remember that the scale of industry is increasing across the entire time series, meaning that, measured purely in terms of total enterprises, growth will slow over time regardless. The fastest enterprise growth in all regions should be in the 2000s.

**Trends in the Number of Enterprises**

While the general trends in enterprises by region are visible in Figure 4-5 above, a few alternate approaches can illustrate the trends in more detail. First, the relative relationships between regions become more clear when each region's industrial enterprises are calculated as a share of the national total, giving a sense of where the country's industry is concentrated and how this concentration has changed over time:

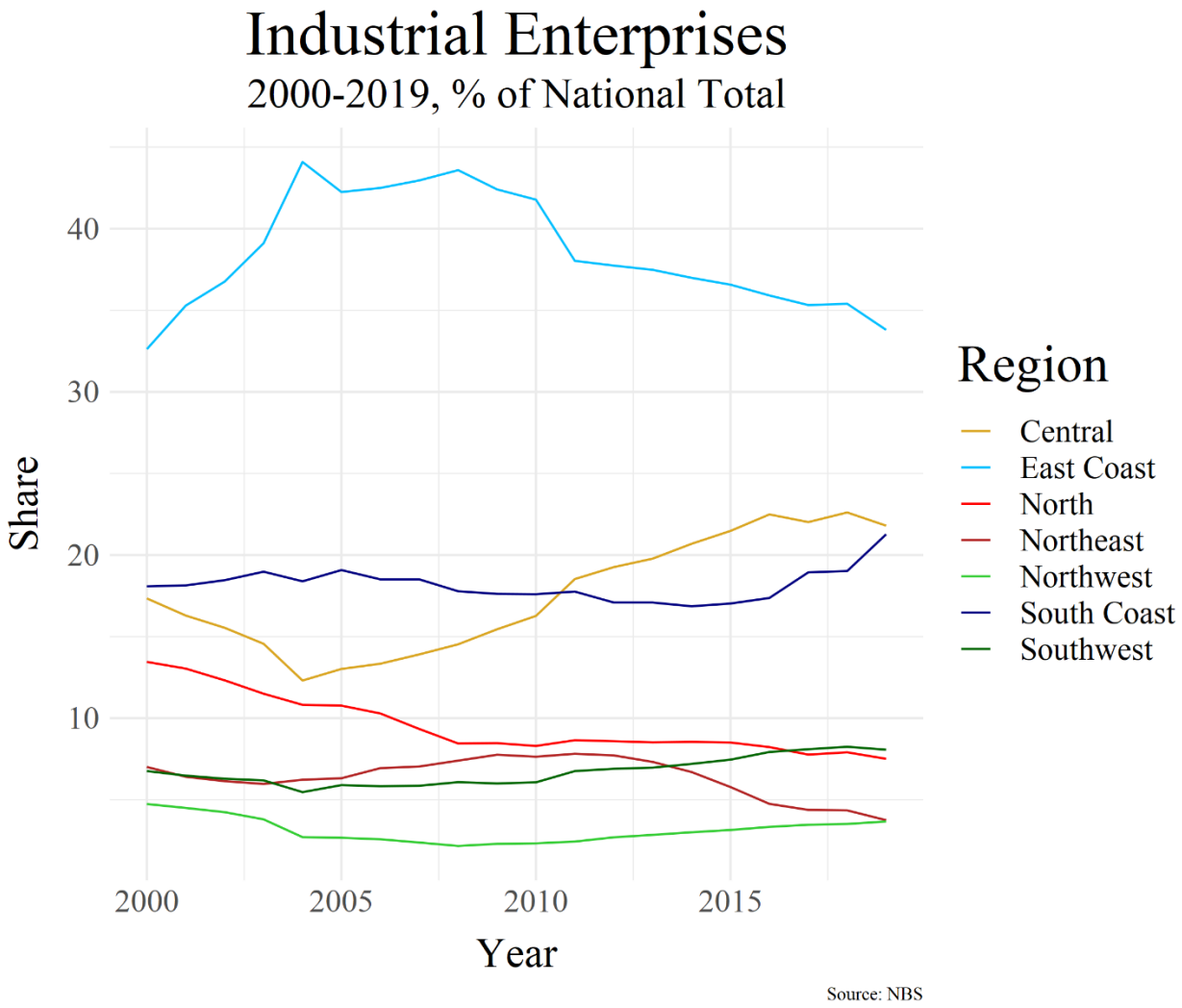



*Figure 4-10*

Here, the same relative positions visible in Figure 4-5 are portrayed more clearly. In particular, the continuing dominance of the East Coast in its share of total industrial enterprises is evident, but this dominance peaked in the 2000s and has been declining since 2010. Similarly, the inversion of the South Coast and Central shares is more evident here, as is their near-convergence in 2019, driven by a more recent upsurge in the share of industrial enterprises located in the South Coast and a plateauing of the share located in the Central region. Finally, the North appears to have seen most of its decline in the early 2000s and has been stagnant or declining only slowly thereafter. By contrast, the Northeast has seen most of its decline after 2011 or so, while the Southwest and Northwest have seen moderate increases over the same period.

To better summarize these trends, it will be useful to calculate the annual average growth rate of enterprises over 5-year intervals within each region. Here, as above, the total number of enterprises is summed by region, each year's straight-line growth rate is calculated relative to the previous year and then the average of year-on-year growth rates is taken in 5-year segments from 2000 to 2019. This approach retains some of the variability from the individual years, whereas a simple straight-line average from the beginning of the five-year segment to the end, divided by the number of years, would eliminate all of this variability. I exclude 2011, however, since the statistical redefinition distorts the figure. This means that the 2010-2014 period is calculated with 2011 as an NA value:

**Average Annual Growth Rate**

2000-2019, Number of Industrial Enterprises

| Time | Central | East Coast | North | Northeast | Northwest | South Coast | Southwest |
|---|---|---|---|---|---|---|---|
| 2000-2004 | 5.07 | 24.50 | 8.88 | 12.46 | -0.71 | 15.25 | 8.62 |
| 2005-2009 | 14.94 | 9.25 | 4.39 | 14.97 | 6.07 | 8.77 | 11.97 |
| 2010-2014 | 9.22[1] | 3.79[1] | 4.05[1] | 0.63[1] | 11.07[1] | 3.53[1] | 6.88[1] |
| 2015-2019 | 1.12 | -1.77 | -2.52 | -10.74 | 4.02 | 4.86 | 2.33 |

[1] Excluding 2011 due to distortion from statistical redefinition

Source: NBS, Author's Calculations

*Table 4-1*

A few things become clearer when looking at average growth rates by region. First, it's evident that the number of enterprises increased the fastest in the 2000s, where the highest average 5-year growth rate sits (24.5% in the East Coast during 2000-2004) and where growth rates regularly exceeded ten percent. In the subsequent decade, by contrast, only the Northwest in 2010-2014 sees the number of industrial enterprises increase by more than ten percent, while the Northeast sees *negative* growth of the same magnitude in 2015-2019. Another notable trend is the fact that growth rates tend to stagnate or even decline across the board in this final 5-year segment, with four to five percent rates in the Northwest and South Coast the highest in the country, the East Coast, North and Northeast all suffering negative growth and the Central and Southwest regions seeing growth under three percent. Similarly, in the first half of the 2010s, the Northwest, Central and Southwest regions led all others. For the Central and Southwest regions, this simply widened the lead they had established in the latter half of the 2000s as the growth rate in the coastal regions declined. But it also saw the Northeast stagnate and the Northwest succeed

it, stepping into the lead in both 2010-2014 and nearly leading in 2015-2019. Finally, the recovery of the South Coast in the final five-year period stands in contrast to the negative growth rates seen in the East Coast and North in the same years.

But the number of total enterprises may not be the best measure here. In part, this is because of potential statistical over-reporting during the growth boom in the 2000s, which may contribute to the particularly rapid growth rates in the number of total recorded enterprises in the Northeast in these years, since these were the provinces most associated with overreporting their provincial output (Orlik 2011, 2020, Lardy 2014). While the growth boom in the sunbelt certainly saw increased demand for the heavy industrial products of the Northeast, these were years in which employment growth was weak. The rise in the number of industrial enterprises could be recording a proliferation of smaller-scale firms that emerged after the restructuring of the industrial belt, without any expectation that this would be matched by rising employment. Regardless of the cause of high enterprise growth rates in the 2000s, however, the 2010s saw a far more rigorous attempt on the part of statistical authorities to reign in statistical overreporting (as documented by Orlik 2020 and Lardy 2014 and 2018). Thus, the growth rates after 2010 should be treated with less suspicion and they can be read as at least somewhat related to the "Northeast China Revitalization Strategy" (called by various names, the Chinese is slightly different: 振兴东北老工业基地) pursued after 2010, which focused on further rounds of restructuring, the attraction of higher-tech industries and the clearing-out of inefficient firms (Ren et al 2020). In terms of the total number of enterprises, the effects appear to mostly be negative, however, and the Northeast had slipped almost to the bottom of the rankings when measured by share of total enterprises nationwide.

Regardless of improvements in statistical methodology, there will always be complaints about over-reporting distorting such numbers. In addition, enterprise number is not itself an ideal stand-in for the relative concentration of industry since it does not capture the size and scale of production. If employment and output numbers are equally suspect, however, it becomes necessary to turn to correlated variables that are more difficult to falsify. As early as the 2000s, the National Bureau of Statistics began to search out and correct overreporting by comparing output numbers delivered by provincial officials to those same provinces' consumption of electricity and freight traffic—both of which were much more difficult to falsify. These have since become important measures (alongside nighttime light data) to compose alternate output figures for China. Depending on the measure, however, researchers have concluded both that Chinese GDP is overstated (Rawski 2001, Owyang and Shell 2017) and that it is understated (Clark et al 2017). At the large scale, then, these attempts to correct for potentially misrepresentative data are probably most useful for historical statistics, when the NBS was using more rudimentary methodology and had far less capacity to correct for local over-reporting. In later years, the NBS itself began to incorporated many of these alternate methodologies into its own fact-checking process (Orlik 2011, Holz 2013) and new methods such as using night lights data, for instance, came the opposite conclusion as earlier studies (Clark et al 2017).

Still, including at least one alternate measure that correlates to industrial location will help to allay any concerns while also giving more insight into potential divergences between the sheer number of industrial enterprises and the changing scale of production. Electricity consumption (measured in units of 100 million kwh) is the best measure to use for these purposes, since the industrial sector accounts for the bulk of electricity consumption, about two-thirds in 2018 (ChinaPower 2021), and it is recorded for all years examined here except for 2019.

First, I'll compare each region's total electricity consumption as a share of the national total, producing a plot comparable to Figure 4-10 above:

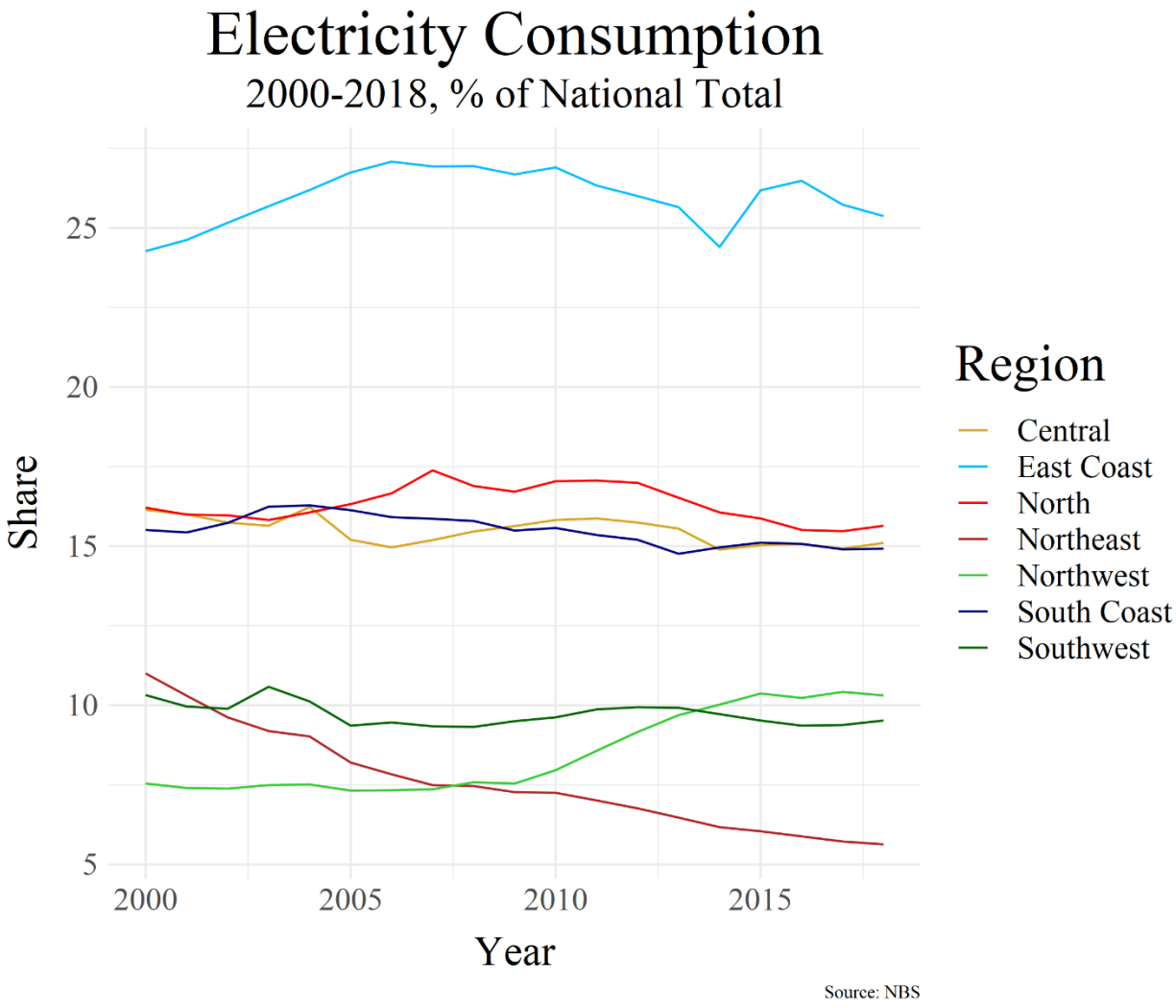



*Figure 4-11*

Much of the overall pattern is the same. The East Coast remains dominant, and the Northeast, Northwest and Southwest all lie at the bottom while the Central region and the South Coast compete with one another in the middle range. But a few key differences also emerge. By far the most notable of these is the ascent of the North to the same level as the Central and South Coast regions. In fact, the North overtakes both in the latter half of the 2000s and retains that rank

throughout the remainder of the series even if its lead narrows in the latter 2010s. In part, this is due to a rescaling effect, since the coastal regions seem to have higher shares of total enterprises on average than they do shares of total electricity consumption—in the East Coast, the share of total enterprises lies around forty percent, but its share of total electricity consumption drops to twenty-five percent. But the decline in the share of total enterprises in the North visible in Figure 4-10 is not matched by much of a decline in electricity consumption, signaling that a smaller share of industry in the region is using a growing share of electricity over time.

Other trends are also more clearly visible in this series: The Northeast sees a continuous decline in its share of electricity consumption over the entire period. The southwest sees its share remain essentially unchanged at around ten percent. But the Northwest sees the most rapid rise of any region in the 2010s, growing from a stagnant seven percent plateau in the 2000s to around ten percent in 2013, after which it plateaus at this new level, outranking both the Northeast (as early as 2008) and the Southwest (by 2014). Similarly, the inversion in the ranks of the Central and South Coast regions visible in Figures 4-5 and 4-10 is less clear cut here. While the share of total electricity consumption in the Central region grows to outrank the South Coast after the Great Recession, as in the other series, here the two converge again as early as 2014 and move in unison thereafter. By the late 2010s, then, the North, Central and South Coast regions all seem to occupy the same middle space. Throughout the entire period, of course, electricity consumption is rising across the board. This is clear when the same data are visualized in raw numbers, rather than as shares of the total:

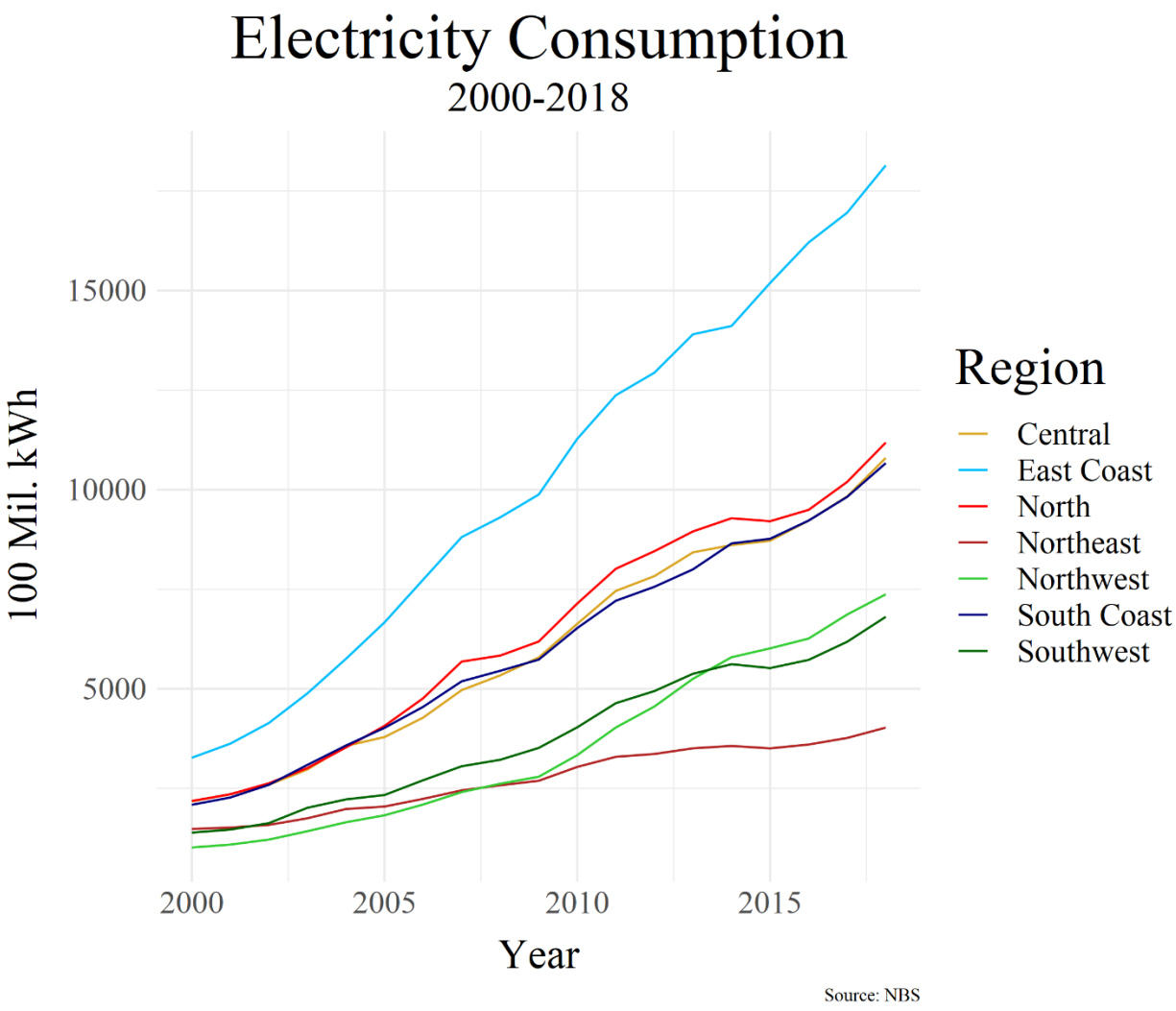



Figure 4-12

Here, the significance of the increase in the Northwest, the stagnation of the Northeast and the unchallenged dominance of the East Coast are all emphasized. But the relative positions remain the same as above.

As in the case of total enterprises, the changes in total energy consumption by year can also be represented in a table of annual average growth rates, calculated in the exact same fashion, though without the need to exclude 2011:

**Growth in Electricity Consumption**

2000-2018, Annual Average Growth Rate

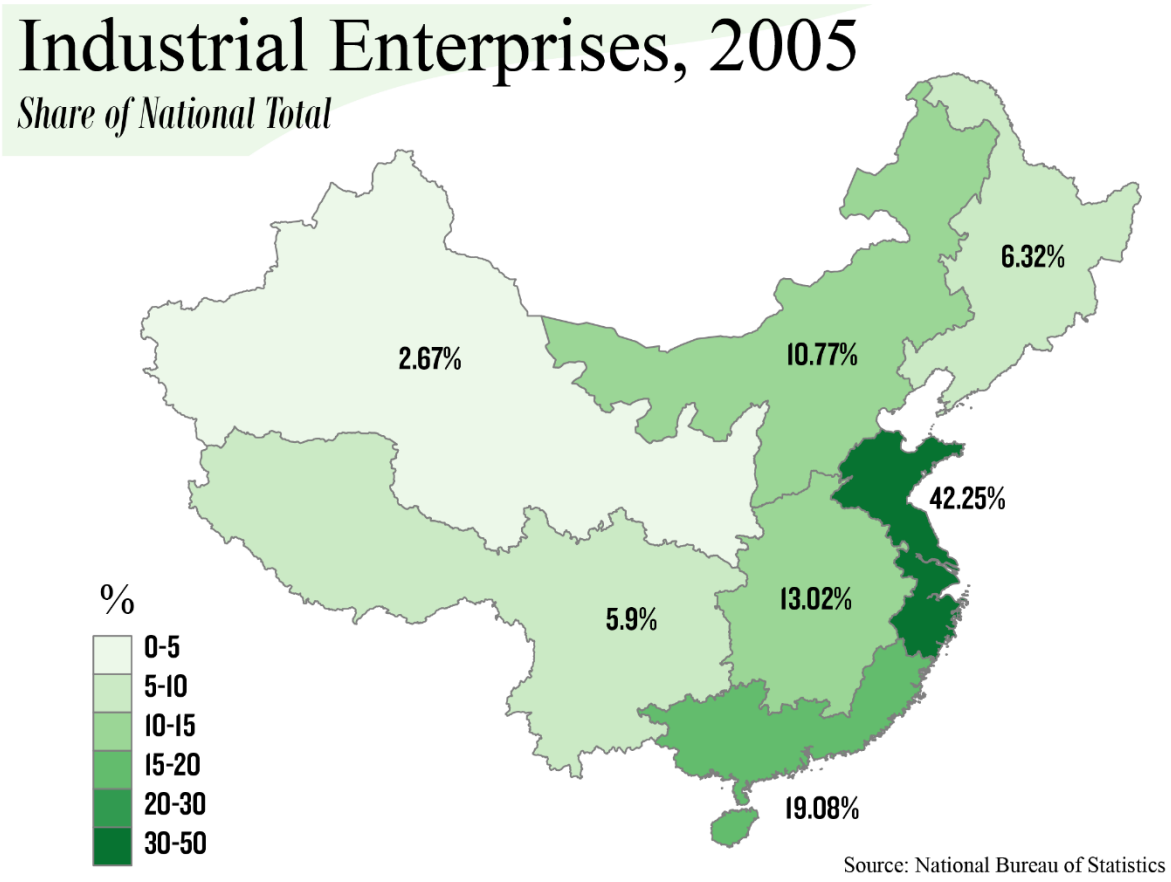
| Time | Central | East Coast | North | Northeast | Northwest | South Coast | Southwest |
|---|---|---|---|---|---|---|---|
| 2000-2004 | 11.89 | 15.81 | 12.12 | 6.22 | 11.60 | 15.18 | 10.93 |
| 2005-2009 | 10.23 | 11.51 | 12.08 | 6.34 | 11.15 | 9.97 | 9.68 |
| 2010-2014 | 8.37 | 7.48 | 8.54 | 5.87 | 15.77 | 8.62 | 9.90 |
| 2015-2018 | 5.58 | 5.87 | 5.00 | 3.25 | 6.34 | 5.63 | 5.24 |

Source: NBS, Author's Calculations

*Table 4-2*

In contrast to Table 4-1, no areas see any decline in their total electricity consumption even if electricity consumption grows more slowly in all regions as time goes on. That fact alone signals that the scale of production itself is likely changing and smaller and/or more labor-intensive firms may be being cleared from the market in favor of larger and/or more capital-intensive ones. In particular, the 2015-2018 period here is marked by relatively robust growth (though certainly lower than before) in the East Coast and North, both of which see negative growth in the total number of enterprises in Table 4-1. Similarly, although the Northeast sees a mere 3.25% average annual growth rate in electricity consumption between 2015-2018, that's paired with a *decline* of over ten percent in the total number of enterprises in those same years. This signals that even the Northeast—which lies at or near the bottom whether ranked by total number or share, enterprises or electricity consumption—seems to be seeing a shift to more energy-intensive production. This confirms the observations of Ren et al (2020) that the region has seen some success in attracting capital-intensive new industries even if this has not led to a more general regional rebalancing.

Finally, it will help to portray the overall regional trends in enterprise location and electricity consumption explored above in a series of maps comparing the most recent shares of each (2019 for enterprises, 2018 for electricity consumption) to the same shares in 2005, a representative year in the middle of the 2000s. First, I will show each region's share of enterprises in 2005, then in 2019:

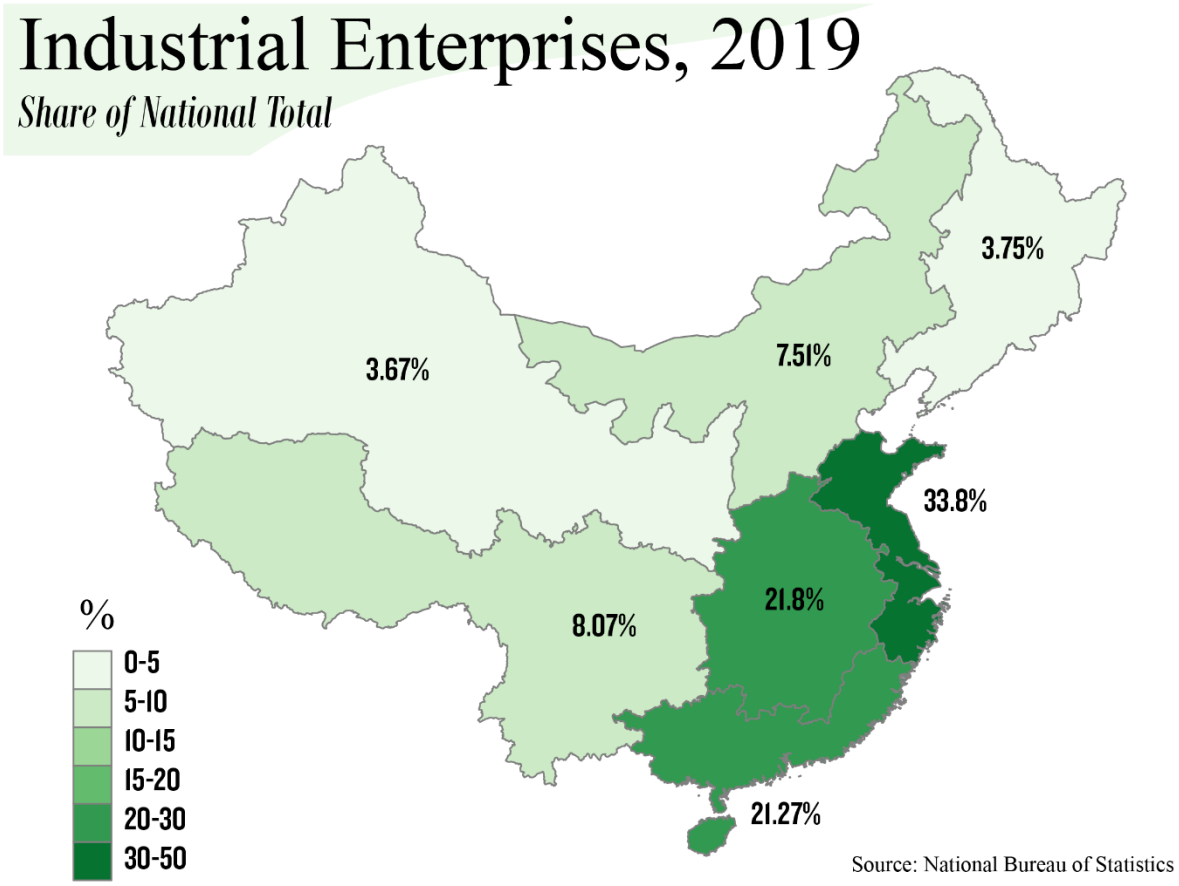



*Map 4-2*

Here, the dominance of the East Coast is abundantly clear, with more than forty percent of all industrial enterprises concentrated in these provinces, followed by the South Coast with just over nineteen percent. The Central and North regions lie in the middle, but their shares are fairly low,

just over ten percent each. The Northeast still had around six percent of all industrial enterprises in this period, just a bit more than the Southwest. The Northwest had less than three percent.

By 2019, the basic relationships remain the same but a subtle shift in the geography of industrial location is nonetheless visible:



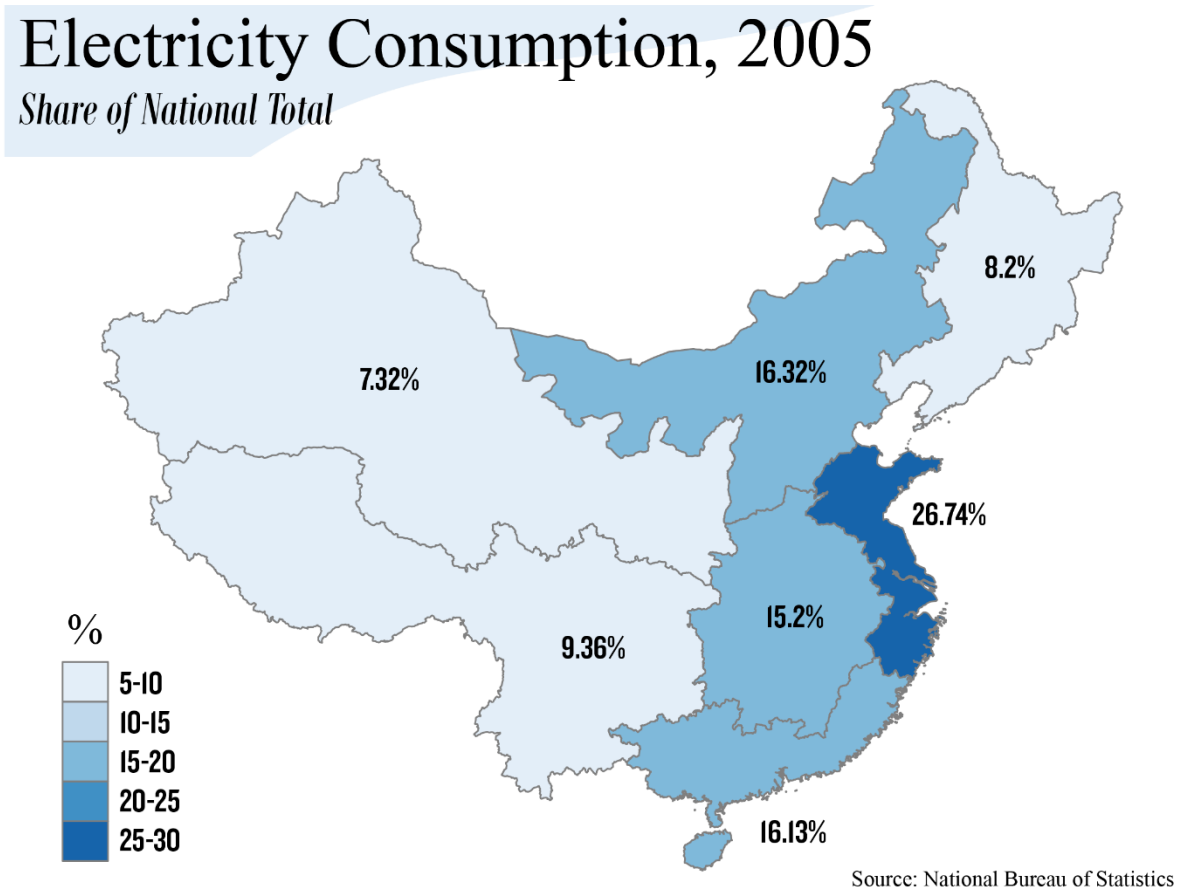

*Map 4-3*

Here, the East Coast is still dominant, but its share has fallen a full ten percentage points, to just above thirty percent of all industry. Similarly, both the Central and South Coast regions have seen their shares rise by roughly ten percentage points to lie just above twenty percent. The North has seen its share decline somewhat, with this decline driven by a sharp drop in the total

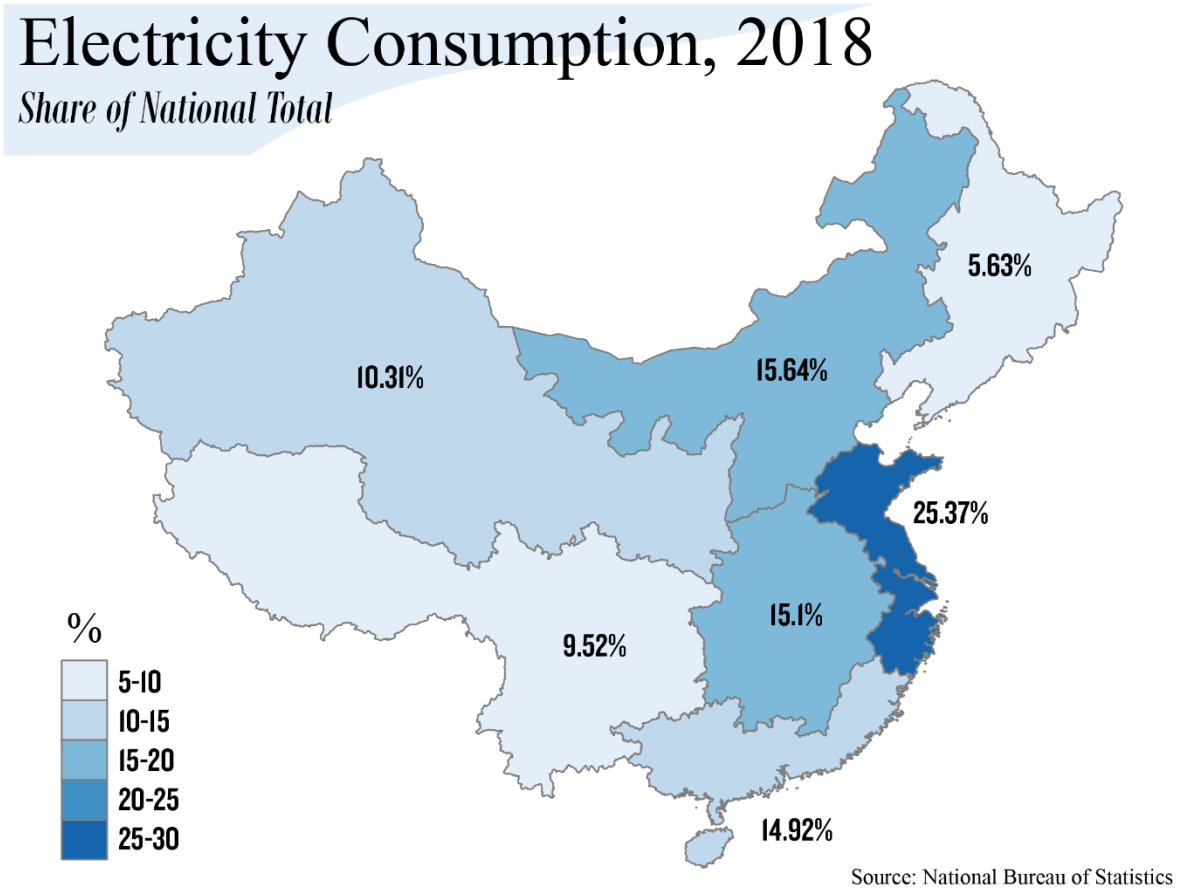
number of industrial enterprises in Beijing from a peak of 7,205 firms in 2008 to less than half that, a mere 3,121 firms, by 2019—the lowest of any year after 2000—and a slightly more moderate decline in Inner Mongolia, from a peak of 4,611 firms in 2010 to 2,965 firms in 2019. Meanwhile, the other provinces in the region saw relative stagnation or mild declines over the same period.

Between 2005 and 2019, the Northeast saw its share essentially halved, lying at a mere 3.75 percent in the most recent year, only barely exceeding the Northwest's 3.67 percent, the result of sustained growth (but still a low absolute number) over the 2010s. Over the same time period, the Southwest grew to hold eight percent of all enterprises, exceeding the shares in both the North and Northeast. This was driven by healthy growth in essentially all the provinces in the region, including Tibet's recovery from declining enterprise numbers in the 2000s to rapid increases in total enterprises in the 2010s (though, again, the figure is still low in absolute terms). Tables showing the provincial detail for each region can be seen in Appendix 2 and the tables for the Central and South Coast regions will be explored in the main text below.

First, however, the enterprise shares should be compared on the map to the regional shares of electricity consumption in both 2005 and in the final year of available data, 2018:


Electricity Consumption, 2005
Share of National Total
8.2%
7.32%
16.32%
26.74%
15.2%
9.36%
16.13%
%
5-10
10-15
15-20
20-25
25-30
Source: National Bureau of Statistics

*Map 4-4*



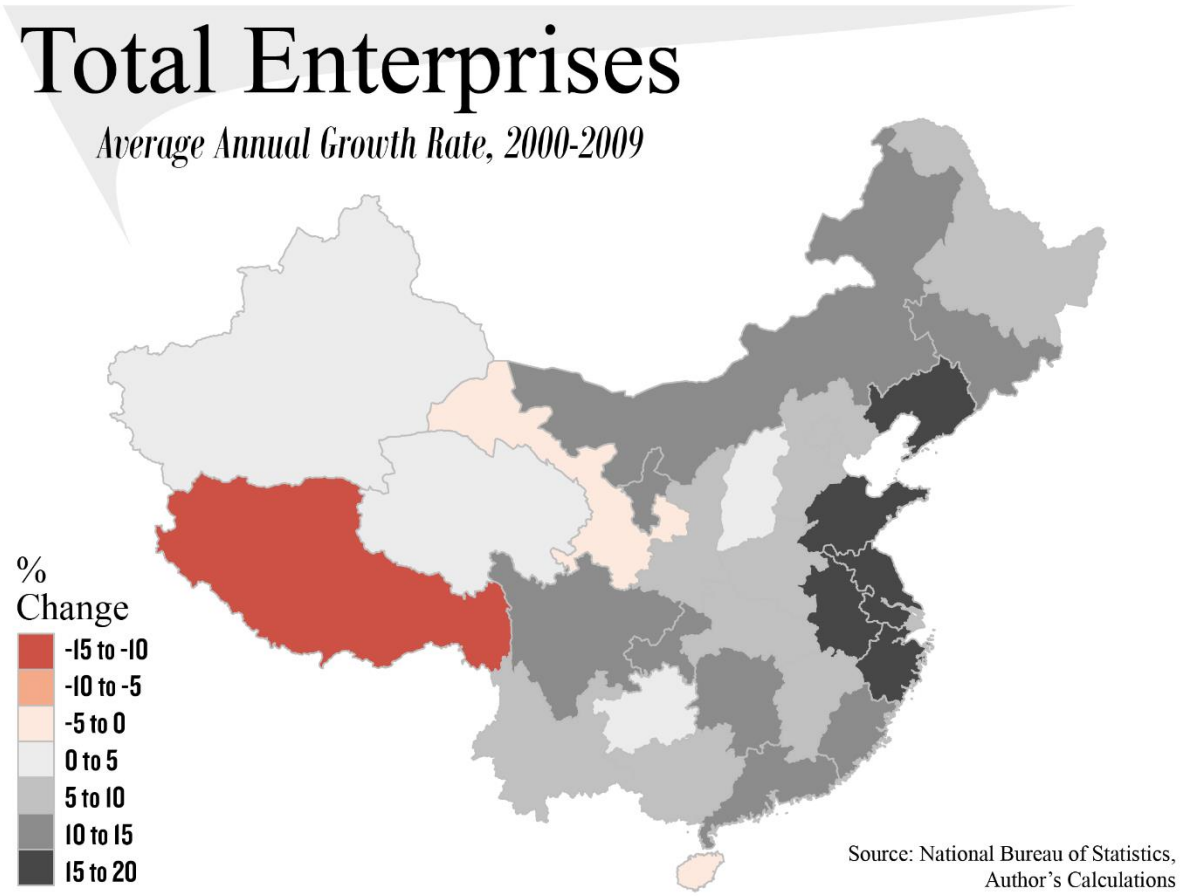

*Map 4-5*

Very similar trends are visible when showing the changing shares of electricity consumption. But there are also a few key differences between this series and the enterprise share series. The first is that the relative position of the South Coast declines rather than rises when measured by its share of electricity consumption than when measured by its share of enterprises, dropping from 16.13 percent in 2005 to 14.92 percent in 2018. Similarly, the relative position of the Northwest moves upward a bit more rapidly when measured by its share of electricity consumption, rising from 8.2 percent in 2005 to 10.31 percent in 2018. Meanwhile, the position of the North is more

stable here, sitting at 16.32 percent in 2005 and dropping only slightly, to 15.64 percent in 2018. Finally, the Northeast declines overall, just as in the other series, but the Central and Southwest regions don't see much of an increase, with their overall shares remaining almost identical between 2005 and 2018.

**Mapping the Trends**

In order to explore these trends in more geographic detail, the average growth rates of profitability, the number of enterprises, the net profits of enterprises and overall value-added of industry can be compared back-to-back at the provincial level. Rather than the share of total enterprises, I will show the annual average growth rate of total enterprises for 10-year periods by provinces to capture general patterns over time:




*Map 4-6*[53]

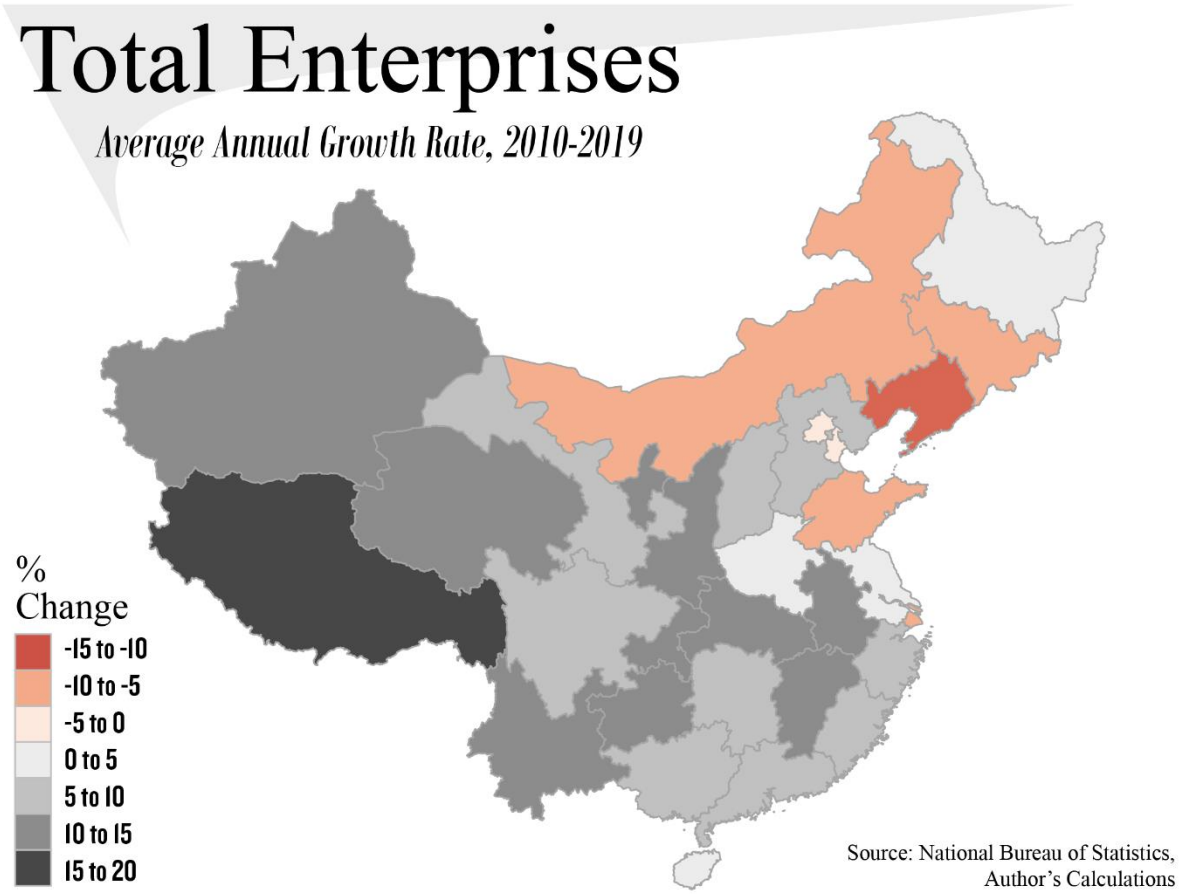
Here, the particular provinces that drive overall trends in each region are also visible, as are internal divergences within the regions. In the 2000-2009 period, enterprise growth was particularly rapid in all the East Coast provinces (except for Shanghai, where it was a moderate 9.45%) as well as in Liaoning, across the water in the Northeast, and in Anhui, in the Central

[53] In all the maps in this section, an attempt is made to use equivalent breaks between maps when those maps are showing the same variable for the same geographic units, just in different time periods. The range of values does not allow the same breaks to be used across all maps, however, since doing so would completely eliminate most variation due to the relatively high outlier values on certain maps. *At times, the breaks must even be changed between time periods for the same variable and the same geographic units.* This is not a mistake. It is because the scale of variation in growth changes so decisively between the two periods that using the same breaks would produce a map in the less variable period that shows essentially no variation—all geographic units would fall within one or two of the categories traversed by the more variable period

region abutting Jiangsu and Zhejiang provinces. In all of these provinces, the number of enterprises increased annually on average by between 16.7 (Anhui) and 18.18 percent (Zhejiang).

The map also shows a few other important divergences within regions. In the South Coast, for instance, Guangzhou shows average growth in the total number of enterprises of just over twelve percent between 2000 and 2009 and neighboring Fujian is slightly higher, just over thirteen percent. But in Hainan the total number of enterprises actually declines by 1.7 percent in the same period and in Guangxi growth is much more moderate, at 7.35 percent. Similarly, in the Southwest there is an entire range of growth rates visible. Sichuan (13.64%) and Chongqing (14.57%) show growth rates above the leading South Coast provinces but in mountainous Yunnan and Guizhou, the rates are much lower: six percent and under four percent, respectively. Meanwhile, Tibet saw the strongest decline in the total number of enterprises out of all provinces (-11.6%). The Central region is more consistent (between 7 and 10%), aside from the particularly high growth rate in Anhui (16.7%) and the moderately high rate (12.18%) in Hunan, where the total number of enterprises grew slightly faster than in neighboring coastal Guangdong. In the North, rates were also more even (though slightly lower, between 5 and 7%) with the downward outlier of Shanxi (3.13%) and the upward outlier of Inner Mongolia (14.49%). In the Northeast, the total number of enterprises appeared to grow the fastest the closer one got to the coast, with Liaoning leading (17.42%), followed by Jilin (10.65%) and Heilongjiang (6.78%). The Northwest saw declining growth in Gansu (-3%), but positive, though mild, growth in Xinjiang (4.03%) and Qinghai (2.22%), moderate growth in Shaanxi (6.8%) and fairly rapid growth in Ningxia (11.32%), each of which had low numbers of total enterprises in absolute terms.

This can be compared to the same values for the subsequent decade:

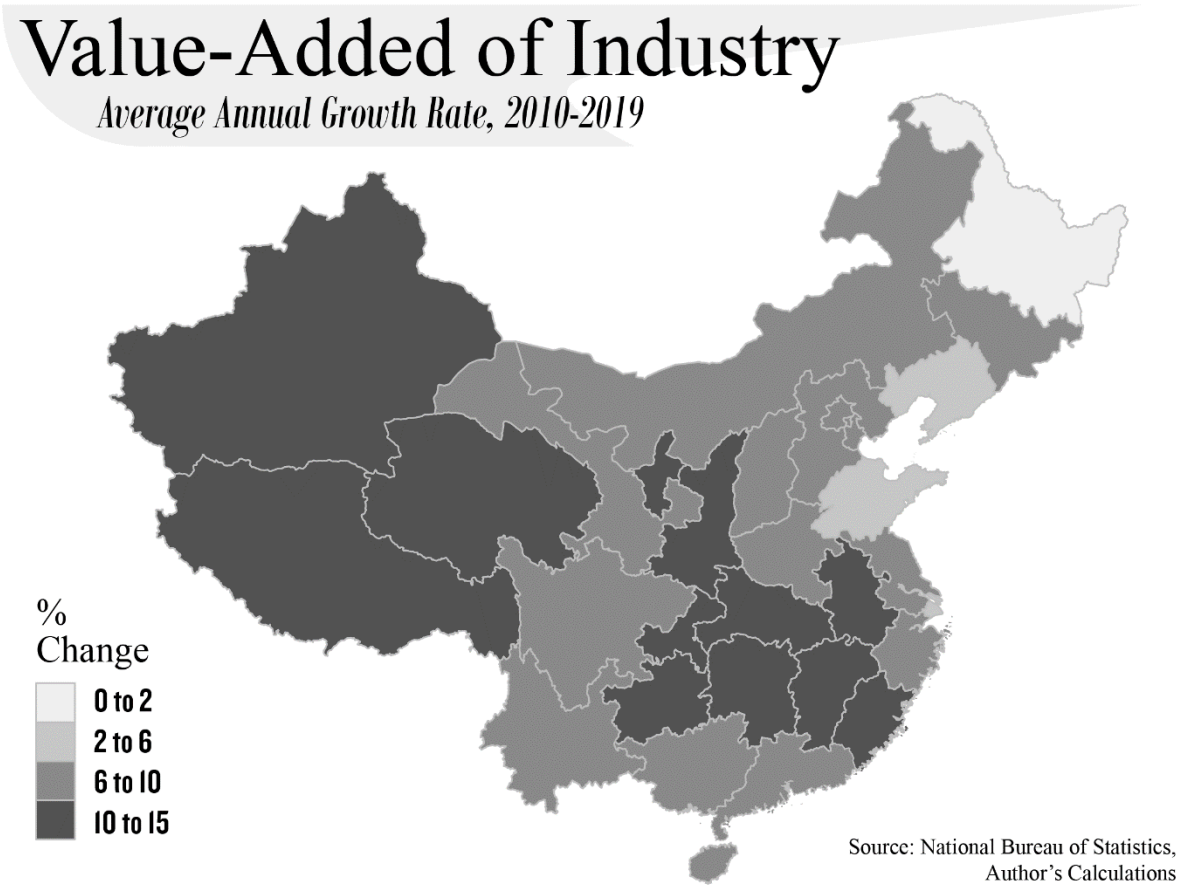



*Map 4-7*

Here, certain regional patterns gain some consistency. Growth patterns in both the Northwest and Northeast essentially invert. Between 2010 and 2019, the total number of enterprises increases rapidly across the Northwest, where Xinjiang sees growth just under ten percent, and Shaanxi, Qinghai and Ningxia see total enterprises increase at 7.82, 5.67 and 5.49 percent, respectively. Similarly, the Northeast sees harsh declines in Liaoning (-7.1%) and Jilin (-3.32%) and near stagnation in Heilongjiang (1.23%). Trends in the North bifurcate between slight enterprise declines in Tianjin (-1.99%), Beijing (-2%) and Inner Mongolia (-2.53%) against slight enterprise growth in Hebei (2.38%) and Shanxi (3.93%). The East Coast sees a similar

bifurcation, with losses in Shandong (-2.79%) and Shanghai (-2.08%), mild growth in Jiangsu (1.34%) and moderate growth in Zhejiang (4.03%).

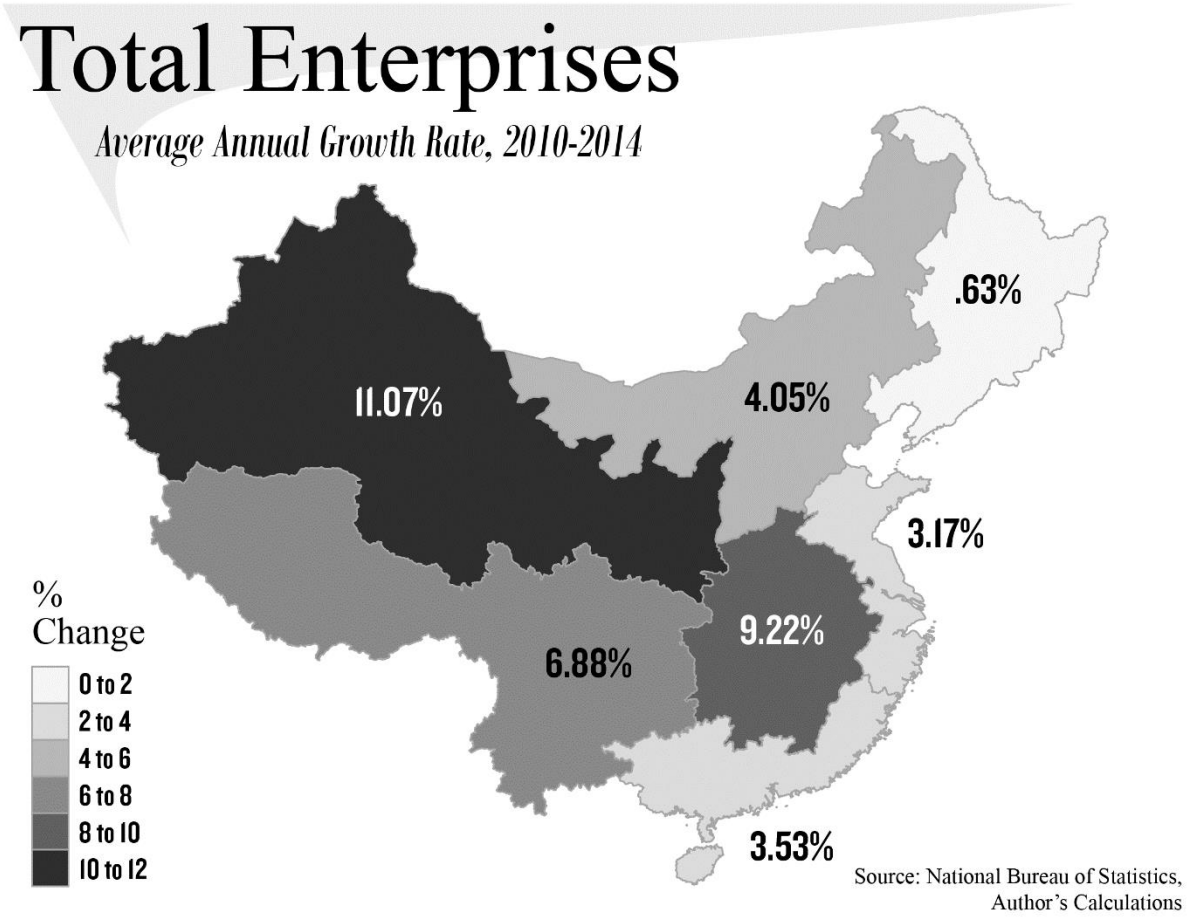
While growth in the total number of enterprises is on average lower in this period, it is clearly concentrated in a particular string of Central, Southwestern and Northwestern provinces. In the Central region, Jiangxi (8.71%), Hubei (6.24%) and Anhui (6.02%) led growth, trailed by Hunan (3.66%) and Henan (1.77%). In the Southwest, Tibet average annual increases in total enterprises of 12.59 percent meant that industry more than recovered from the losses suffered in the preceding decade. Similarly, Guizhou inverted its position to lead growth in total enterprises with a strong 9.47 percent, followed by neighboring Yunnan (5.65%) and Chongqing (5.16%) and trailed by Sichuan (2.5%). At the same time, growth across the South Coast had moderated but did not see the same decline apparent in the East Coast. It was also more uniform: Guangdong led with growth of 4.6 percent, followed by Guangxi at 4.09 percent and Fujian at 3.67 percent. Hainan trailed with slower growth of 1.97 percent. A similar geographic clustering of industrial growth is even more clearly visible in the annual average growth rate of industrial value-added by province for the same 2010-2019 time period:



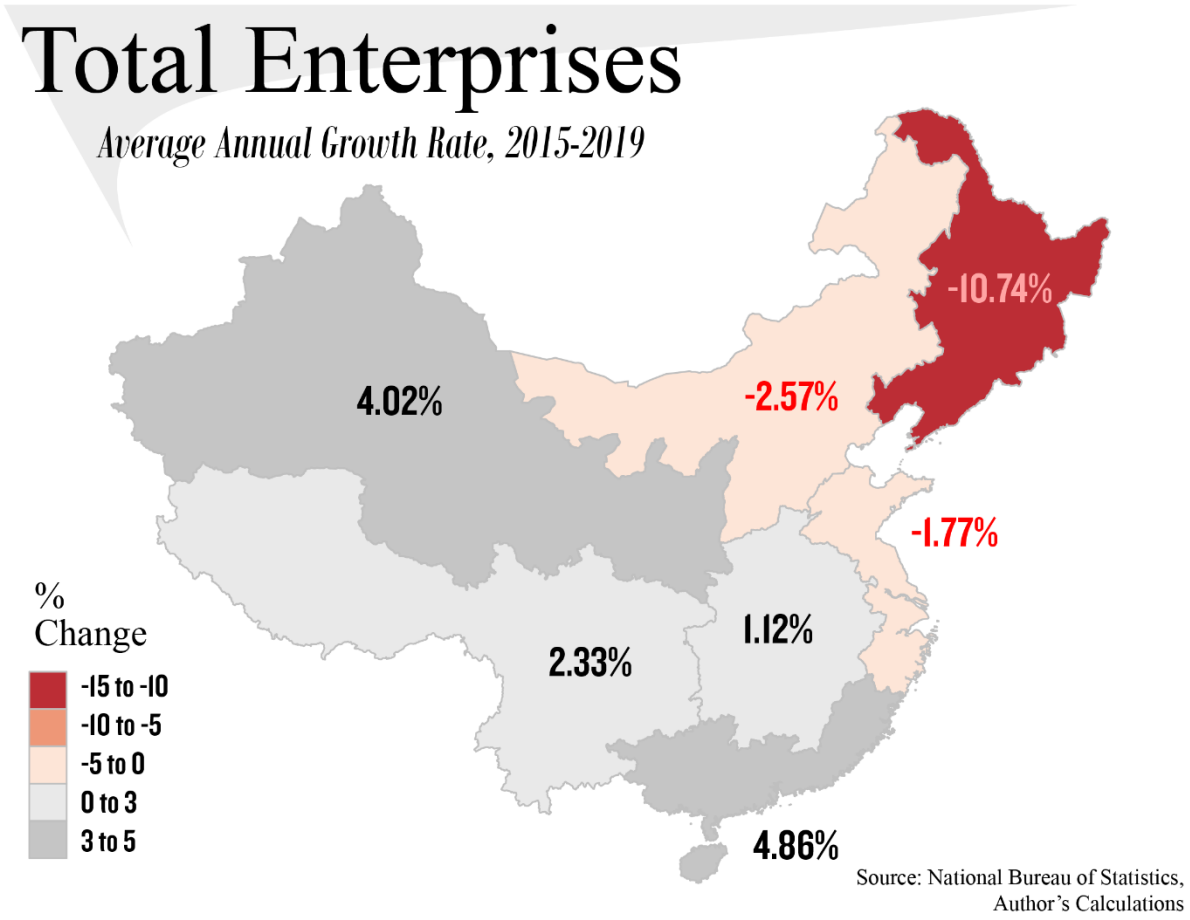

*Map 4-8*

Here, it's clear that industrial output has increased the fastest in a subset of enterprises located in the Central Region, spilling over to Guizhou and Chongqing in the Southwest and Fujian in the South Coast, with a proximate cluster in the near Northwest in Shaanxi and Ningxia and a second cluster in the far Northwest in Xinjiang, Qinghai and far Southwest in Tibet (where industrial value-added increased the fastest, at 14.47%). By contrast, every other province saw growth lower than ten percent, with Heilongjiang essentially stagnant (.14%) and Liaoning (3.45%) and Shandong (5.04%) growing only mildly.

But as is clear from the time series visualized above, this 10-year period itself included some important divergences in its first and second half. These can quickly be visualized by looking at annual growth rates of the total number of enterprises averaged across regions between 2010-2014 and 2015-2019 back-to-back:




*Map 4-9*

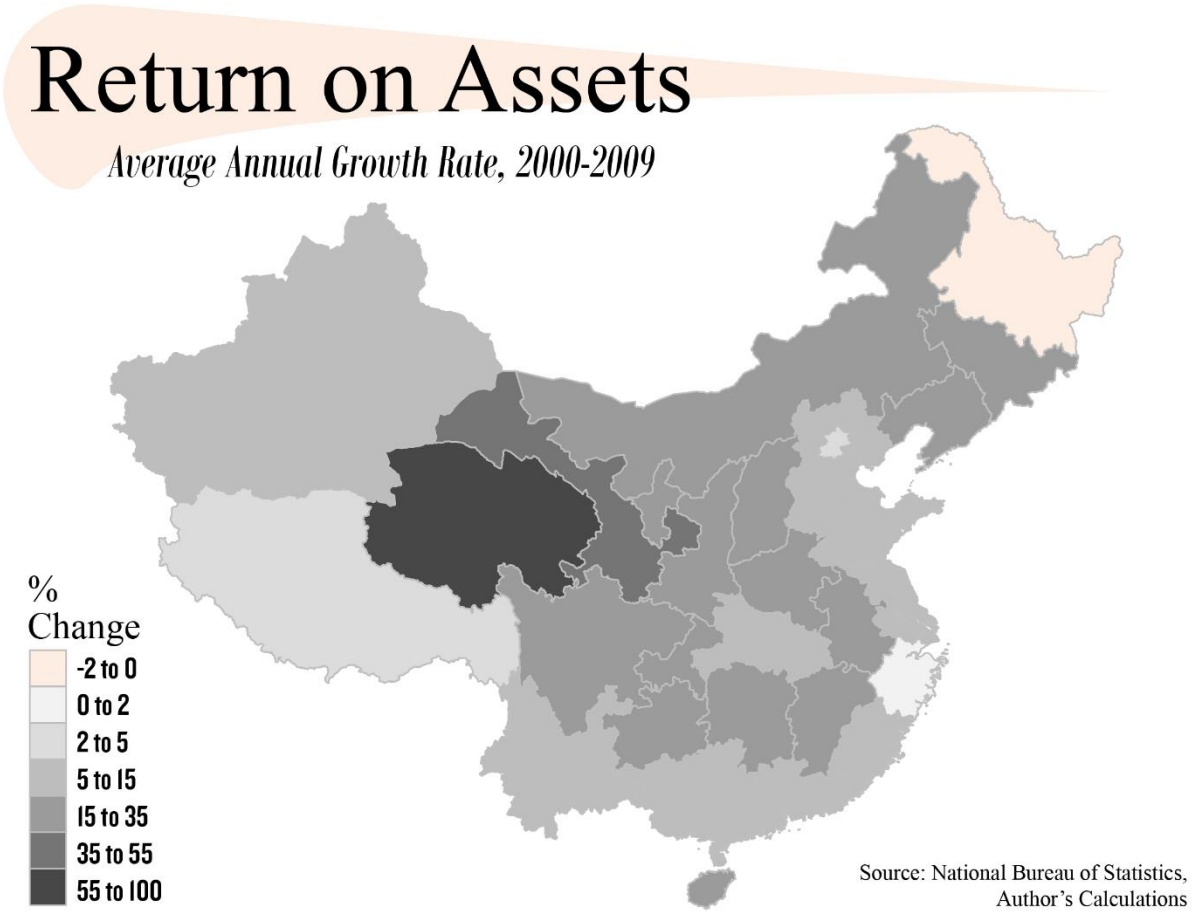



*Map 4-10*

From this comparison, it's clear that much of the period's highest growth was concentrated in the first five years, especially in the Northwest, which saw 11.07 percent annual average growth in total enterprises and the Central region, which saw 9.22 percent annual average growth in total enterprises. Even while growth was essentially stagnant in the Northeast (.63%), this 5-year period saw no declines in overall growth, at least when calculated at the regional scale. By contrast, declines were the most salient feature of the 2015-2019 period, with the Northeast losing 10.74% of its enterprises annually, on average over the latter half of the decade, the north losing 2.52 percent and the East Coast losing 1.77 percent. Growth moderated in the Northwest

but remained robust at 4.02 percent. The Central and Southwest regions saw much more mild growth in these years, at 1.12 and 2.33 percent, respectively. Meanwhile, the Southwest saw a relative comeback, with growth of 4.86 percent slightly exceeding the rates in the Northwest.

In order to begin comparisons between growth in enterprises and trends in profitability, the same methods can be used to map the average annual growth in the return on assets across provinces and regions. One important caveat with these maps, already mentioned in a footnote above, is that the strong absolute differences between high rates of return in the 2000s and low rates of return in the 2010s requires different classification breaks to be used between maps. If this is not done, the number of breaks needed exceeds what is reasonable or the maps showing less variable periods are entirely flattened, classing data in only one or two bins. As above, I will start with the provincial map for the period 2000-2009:



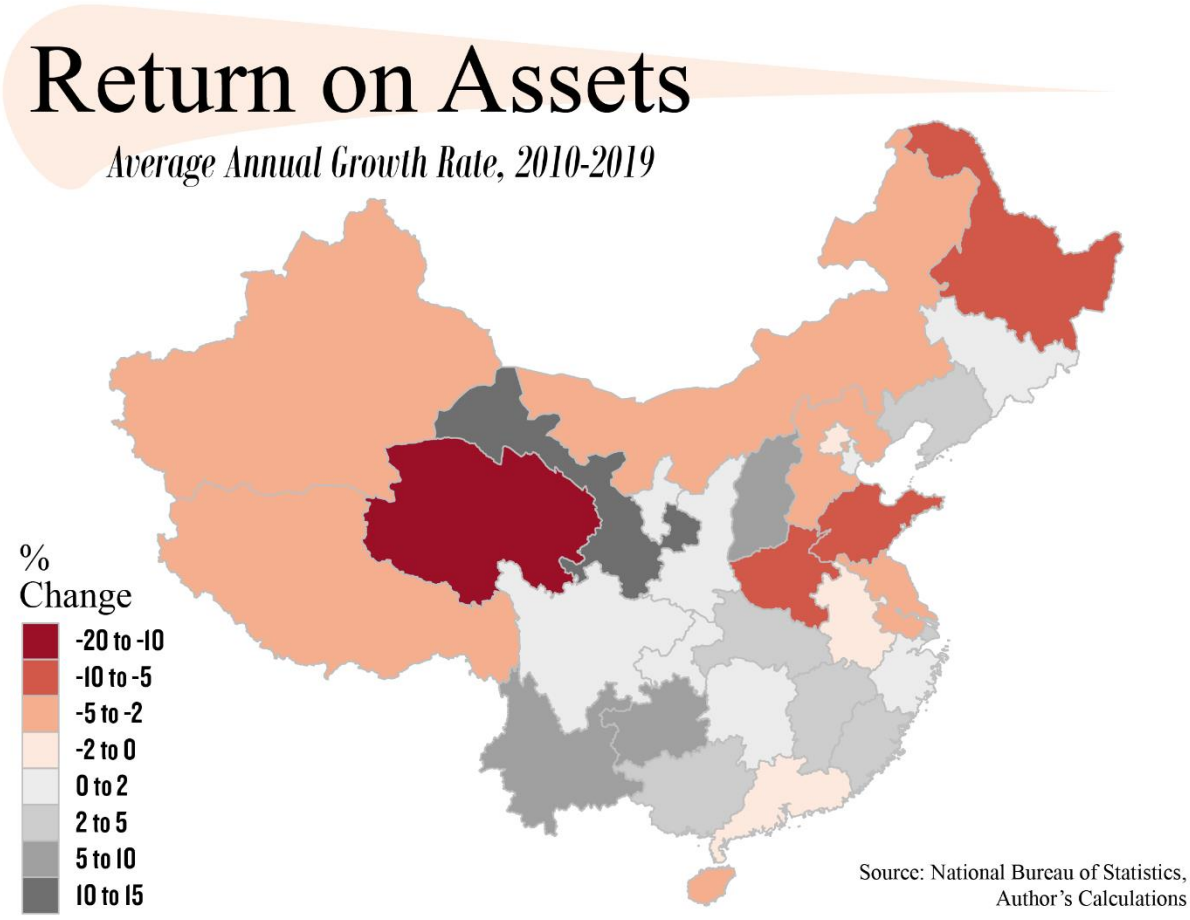

*Map 4-11*

The average annual growth rates of return on assets calculated by province shows more dispersion across the entire series than that of total enterprises. For the entire 2000-2019 period, the standard deviation of annual growth rates in the rate of return by province is 37.85, with a mean of 8.53 and a median of 3.13. By comparison, for the same period, the standard deviation of annual growth rates in the total number of enterprises is 12.89, the mean is 5.93 and the median is 4.01. As is evident from these summary statistics, both have positive skews in their distribution but the annual growth rates in the rate of return are more skewed (4.78) than the

annual growth rates in the total number of enterprises (.885). This is relevant for viewing the map above since the years 2000-2009 include most of the extreme positive outliers.

The most extreme outlier is clearly Qinghai, with average annual growth of 89.78 in its rate of return over the 10-year period. This is almost entirely because the province starts with extremely low profitability in the early years of the decade. In general, since it's not uncommon for rates of return in the slowest-growth areas to be extremely low or even fractional, the growth rate of this figure will be subject to more extreme variability. Averaging those growth rates over 10-year periods only partially rectifies this issue. In most cases, this is why it makes more sense to calculate these figures at the regional level. But showing the provincial trends is still useful here, since we can see, for instance, that Heilongjiang was already dipping into a negative rate of return in this decade, that the growth in the rate of return in Zhejiang (1.25%) and Beijing (4.27%) had already slowed relative to other provinces and that many of the Central, North and even Northeast provinces saw faster growth in their rates of return than any of the coastal provinces save Hainan (28.2%), where high rates are, as in Qinghai, due to the extremely low rates of return that prevailed at the start of the decade.

By the 2010s, growth had declined across the board and turned negative in many provinces:



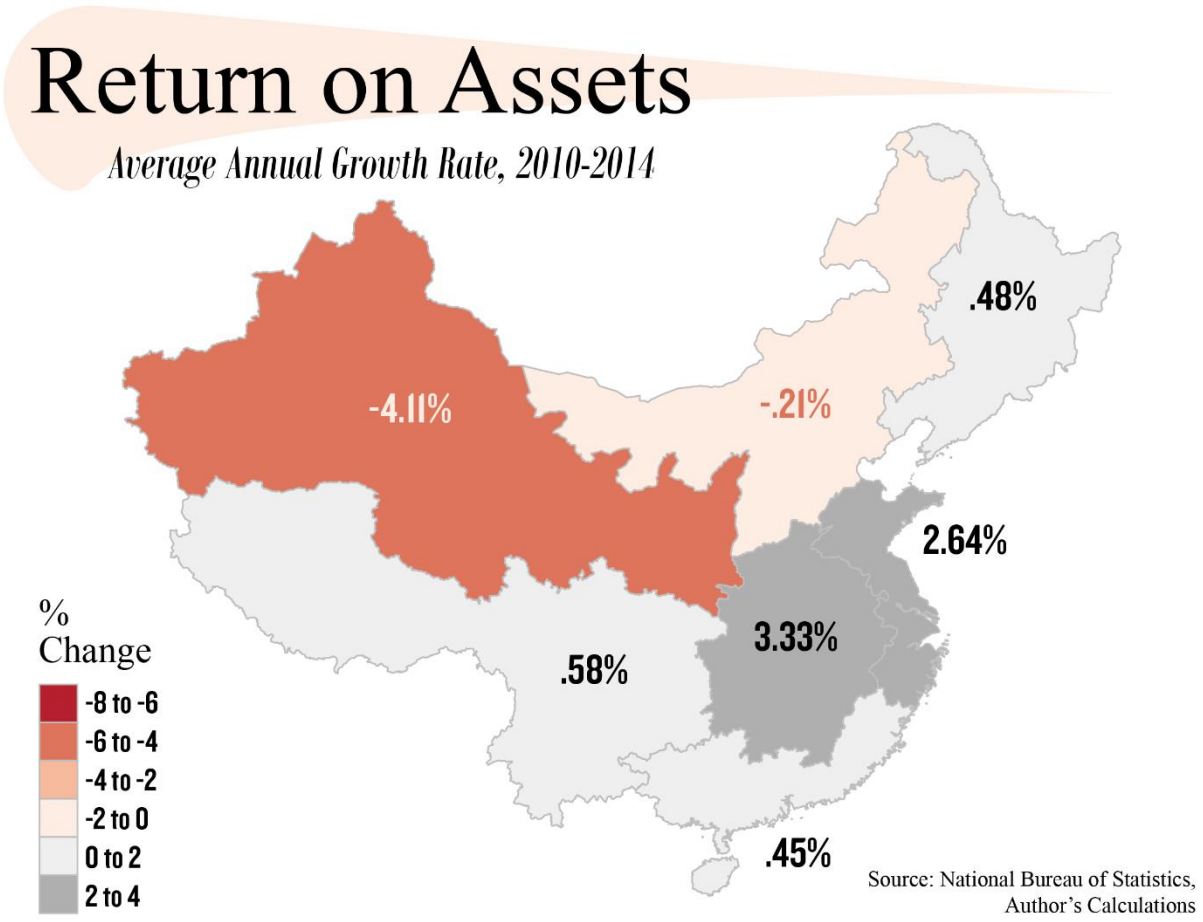

*Map 4-12*

Again, the extreme value Qinghai is a result of relatively minor changes in its rate of return on assets within an industrial sector that is among the smallest in the country. More important is the negative average rate of growth in neighboring Xinjiang (-2.42%), which saw particularly strong growth in enterprises and in its share of electricity consumption in the same years. This contrasts with many of the Central and Southwestern provinces, plus Fujian and Guangxi in the South Coast, where growth in the rate of return diminished compared to the previous decade but remained far higher than in neighboring coastal provinces. The exceptions were Henan in the Central region, which saw growth decline by 5.04 percent, and Tibet in the Southwest, with a

decline of 4.06 percent—though in Tibet part of this decline is due to its relatively low rates of return, similar to Qinghai. In Zhejiang and Shanghai, growth in the rate of return stayed more or less stagnant at 1.53 and 2.04 percent, respectively. In the other two East Coast provinces, growth declined—fastest in Shandong (-7.97%) and a bit slower in Jiangsu (-2.34%).

The South Coast saw a similar split, though Fujian (3.60%) and Guangxi (4.37%) saw slightly stronger growth, exceeding that of the Central provinces but lower than in neighboring Yunnan (6.64%) and Guizhou (5.9%). In contrast, Guangdong saw a slight decline of 1.85 percent and Hainan a decline of 3.46 percent (though, as in Qinghai, this was somewhat exaggerated by low rates of return to begin with). Meanwhile, the North saw stagnation or mild declines almost across the board, with the exception of Shanxi, where growth was relatively strong (8.63%). In Inner Mongolia, the rate of return on assets declined by 3.44 percent on average over the decade. In Hebei, the figure was -3.44 percent. Beijing also saw relative stagnation, though mildly negative (-.59%) and Tianjin saw a similar pattern, though mildly positive (1.12%). In the Northeast, performance again seems to have scaled by proximity to the coast, with rates of return declining further in Heilongjiang (-8.39), increasing mildly in Jilin (1.41%) and moderately in Liaoning (2.82%).

As above, these trends can be summarized better at the regional level, with the final decade disaggregated into two 5-year periods. Here, this also helps to correct for the outlier effect of provinces like Hainan and Qinghai, where the rates are low or fractional to begin with. First, the 2010-2014 period:

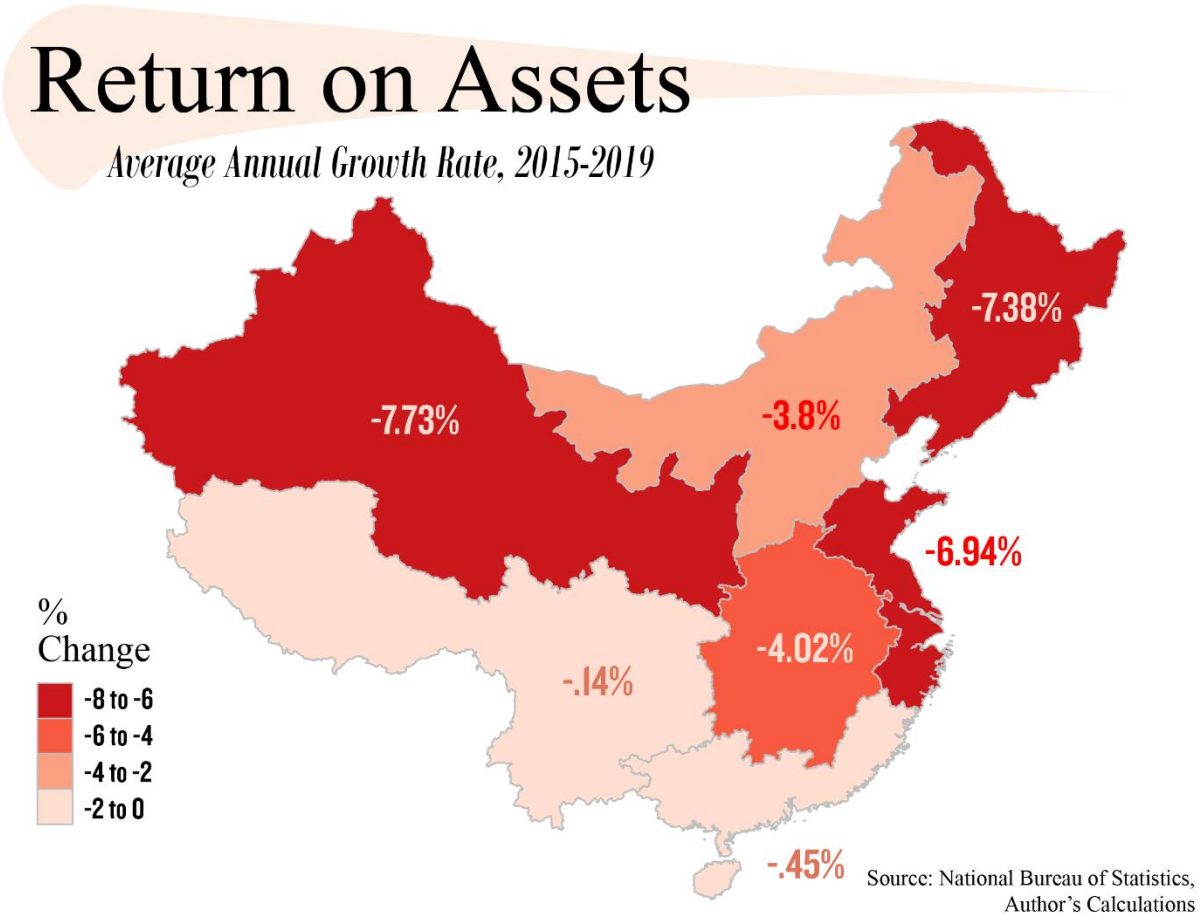



*Map 4-13*

Here, it's clear that much of the positive growth picked up in the 10-year period was concentrated in the first five years of the decade, which saw moderate positive movements in the rate of return on assets in both the Central (3.33%) and East Coast (2.64%) regions. Profitability growth was negative in the Northwest (-4.11%)—and this was not the period that saw the most extreme declines in the rate of return in Qinghai, so that outlier is not exerting an undue influence on the figure here—which contrasts with its strong growth in enterprises (11.07%) over the same period. Everywhere else growth in the rate of return was essentially stagnant, though

mildly negative in the North (-.21%) and mildly positive everywhere else. But no other regions saw positive or negative growth above one percent.

By the latter 2010s, the trends had become more uniformly negative:




*Map 4-14*

No region in this period saw any positive movement, on average, in its rate of return. Declines were universal. They were strongest in the Northwest (-7.73), but this was due mostly to the influence of Qinghai, which saw rates of return dip into the negative in 2019.[54] In Xinjiang, these

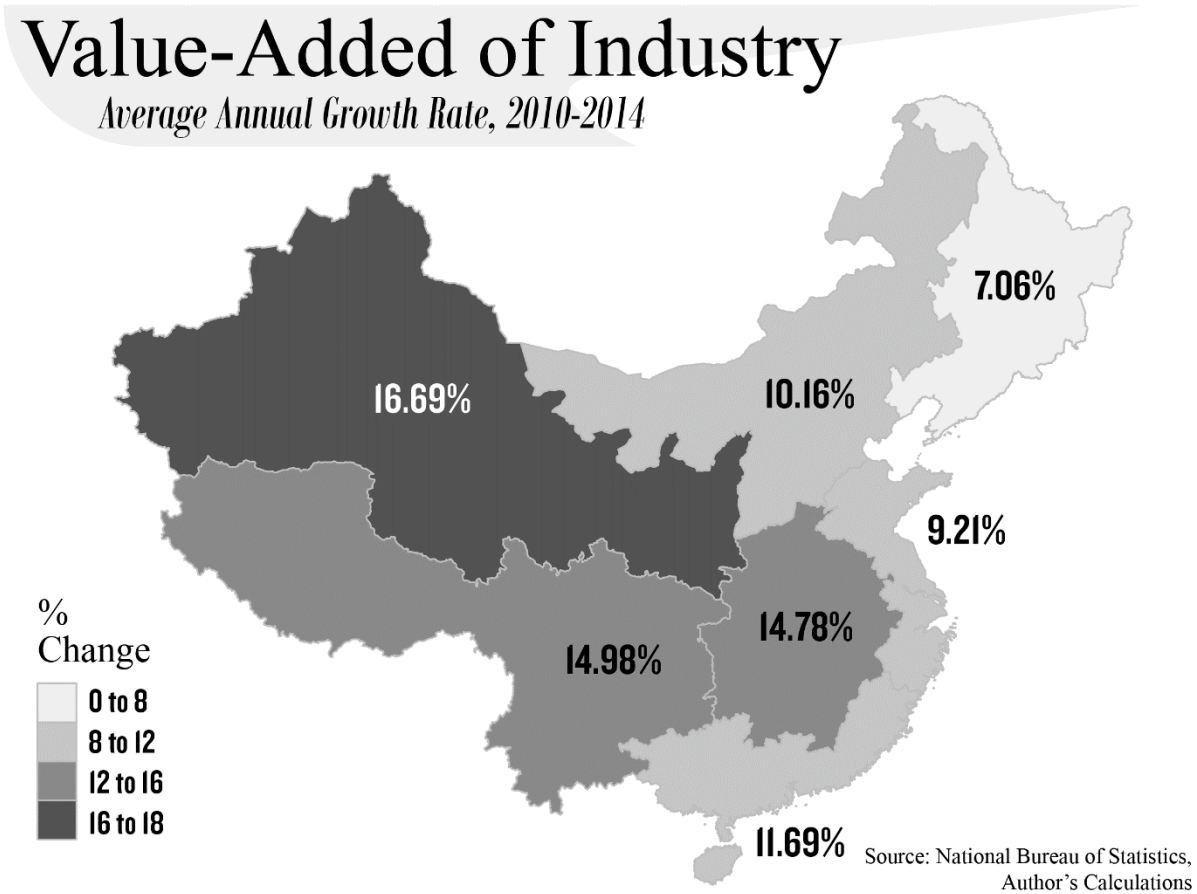
[54] Provincial breakdowns for growth rates of several of the major variables in all the 5-year periods are visible in Appendix 2, with a few of these tables reprinted below for particular regions.

years were a period of slow growth in the rate of return, with an average of just over two percent. In Ningxia, growth was relatively strong at eight percent and in Gansu an extremely large increase of forty percent essentially cancels out Qinghai's negative 29.15 percent over the same period—both are due to the low overall rate of return in these provinces. Meanwhile, In Shaanxi, growth was stagnant, though slightly negative (-.53%). Strong negative growth was also evident in the Northeast (-7.38%) and East Coast (-6.94%). In the Northeast, growth was negative in both Heilongjiang (-10.63%) and Jilin (-4.81%) and mild but positive in Liaoning (2.63%). In the East Coast, the downward trend was driven by Shandong (-15.69%) and Jiangsu (-8.18%), with Shanghai showing mildly negative (-2.56%) growth and Zhejiang basically stagnant, though positive (.41%). In the North, the mild negative growth (-3.80%) for the region as a whole essentially matched the declines seen in all provinces (with the largest decline in Tianjin, at -7.75%) save Inner Mongolia, where growth was stagnant at .29 percent and Shanxi, where relatively minor changes in the small absolute value of the rate of return amounted to a large percentage increase of 32.69.

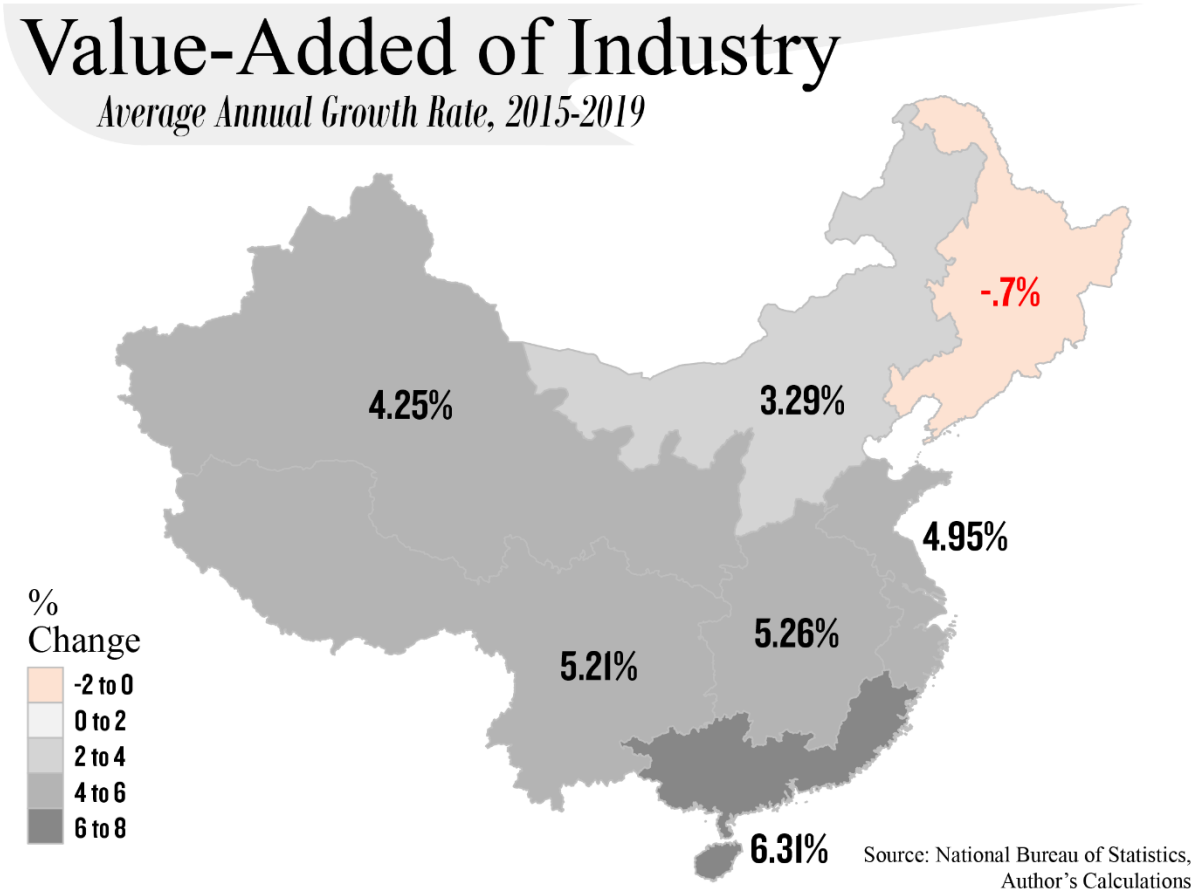
The decline of -4.02 percent in the Central region was led by strong negative growth in the return on assets in Jiangxi (-7.75%) and Henan (-6.03%), a moderate decline in Anhui (-2.57%) and relatively stagnant growth in Hubei (-.25%) and Hunan (.23%). By contrast, the South Coast saw mixed trends, with Guangdong (-3.55%) and Guanxi (-6.17%) both seeing negative movements in their return on assets, while Fujian (5.69%) and Hainan (3.20%) both saw positive movements. The result is that, on average, the region was able to avoid the sharp negative rates seen elsewhere, but only because it stagnated overall (-.45%). The same was true in the Southwest, where overall stagnation (-.14%) was caused by a substantial divergence between rapid positive growth in return on assets in Yunnan (18.68%) alongside moderate

growth in Guizhou (2.28%) and Sichuan (1.55%) as compared to the strong decline in Chongqing (-6.08%) and the mild to moderate decline in Tibet (-2.23%). Overall, the basic outcome of the trends seen in the growth rates of total enterprises and the rate of return over the course of the 2010s can be observed in the growth rate of industrial value-added within each region.

As above, the relatively high growth rates of the early 2010s leads to a different scaling between these two maps:




*Map 4-15*




*Map 4-16*

Paying attention to the changing classification breaks in the legend, the most distinct change between the early half and latter half of the decade is the sharp decline in industrial output overall. Regionally, however, what is apparent here is that output did indeed grow fastest in the Central and Western regions, with much slower rates of growth observed in the Coastal, Northern and Northeastern regions. In this period, even the comparatively slower growth seen in the Northeast was still substantial, at around seven percent, which was still faster growth than any region would see in the latter half of the decade. After 2015, output growth slows across the board and turns slightly negative (-.75%) in the Northeast. While the East Coast (4.95%) sees a

mild recovery relative to the interior provinces, the Central (5.26%) and Southwest (5.21%) regions still see faster growth in value-added in this period and the Northwest (4.25%) doesn't trail far behind. The more substantial recovery of position is seen in the South Coast, where 6.31 percent growth places it in the lead of all regions.

**Summarizing the Trends**

The trends are best compared back-to-back in tabular form. Summaries of the annual average growth rates of total enterprises and electricity consumption are visible in Tables 4.1 and 4.2 above. The growth rate trends in total enterprises (the same data as Table 4.1), return on assets, and value-added mapped in the previous section can also be visualized back-to-back in tabular form:

**Average Annual Growth Rates**

2000-2019, Industry

| | Enterprises | Return on Assets | Value-Added | Net Profits |
|---|---|---|---|---|
| Central | | | | |
| 2000-2004 | 5.07 | 25.09 | 14.45 | 35.51 |
| 2005-2009 | 14.94 | 15.65 | 21.55 | 39.48 |
| 2010-2014 | 9.22[1] | 3.33 | 14.78 | 19.76 |
| 2015-2019 | 1.12 | -4.02 | 5.26 | 0.90 |
| East Coast | | | | |
| 2000-2004 | 24.50 | 9.15 | 16.67 | 29.47 |
| 2005-2009 | 9.25 | 2.41 | 15.46 | 21.71 |
| 2010-2014 | 3.79[1] | 2.64 | 9.21 | 15.69 |
| 2015-2019 | -1.77 | -6.94 | 4.95 | -4.85 |
| North | | | | |
| 2000-2004 | 8.88 | 20.36 | 15.81 | 36.61 |
| 2005-2009 | 4.39 | 3.38 | 15.74 | 21.77 |
| 2010-2014 | 4.05[1] | -0.21 | 10.16 | 15.18 |
| 2015-2019 | -2.52 | -3.80 | 3.29 | -0.26 |
| Northeast | | | | |
| 2000-2004 | 12.46 | 6.50 | 6.90 | 15.58 |
| 2005-2009 | 14.97 | 0.23 | 15.09 | 16.08 |
| 2010-2014 | 0.63[1] | 0.48 | 7.06 | 12.95 |
| 2015-2019 | -10.74 | -7.38 | -0.70 | -5.87 |
| Northwest | | | | |
| 2000-2004 | -0.71 | 26.62 | 17.07 | 39.47 |
| 2005-2009 | 6.07 | 5.68 | 19.73 | 26.91 |
| 2010-2014 | 11.07[1] | -4.11 | 16.69 | 16.17 |
| 2015-2019 | 4.02 | -7.73 | 4.25 | 5.01 |
| South Coast | | | | |
| 2000-2004 | 15.25 | 20.62 | 16.00 | 29.83 |
| 2005-2009 | 8.77 | 7.69 | 16.85 | 24.50 |
| 2010-2014 | 3.53[1] | 0.45 | 11.69 | 14.75 |
| 2015-2019 | 4.86 | -0.45 | 6.31 | 6.82 |
| Southwest | | | | |
| 2000-2004 | 8.62 | 26.39 | 13.80 | 36.70 |
| 2005-2009 | 11.97 | 9.55 | 19.43 | 29.43 |
| 2010-2014 | 6.88[1] | 0.58 | 14.98 | 19.06 |
| 2015-2019 | 2.33 | -0.14 | 5.21 | 6.33 |

[1] Excluding 2011 due to distortion from statistical redefinition

Source: NBS, Author's Calculations

*Table 4-3*

In general, these tables show the relative changes over time in each province more clearly and allow for better comparison between trends in different variables over time. The only variable shown in the table but not visualized on the maps above is the annual average growth rate of net profits, which diverges from the trend in value-added in two important respects: First, in comparing the first and second half of the 2000s, five of seven regions (Southwest, Northwest, North, and the South and East Coasts) show more rapid net profit growth in the 2000-2004 period than in 2005-2009. Second, the decline in net profits in the latter half of the 2010s tends to be more extreme than the decline in value-added in the same years. This is true in all regions except for the Southwest and South Coast.

There is no simple one-to-one correlation between the growth rates of total enterprises and the rate of return, value-added or net profits. This is to be expected, since there is no reason to assume that the relationship would be this clear-cut. New entrants into an industry will obviously benefit from exploiting regional differences in labor costs and are relatively unburdened by sitting capital. These are the cases where the "window of locational opportunity" discussed by Storper and Walker is at its widest. It is also true that, on average, older firms will tend to relocate to areas where they can capture a greater share of profits by exploiting lower costs of production. But in these cases, relocation is often a final option pursued only after other methods of improving the firm's balance sheet have been exhausted. This is because it entails the loss of lucrative local aggregation effects and often also demands an enormous up-front investment in new plant and equipment—potentially at a time when the old plant and equipment from the last cycle of investment are not even fully paid down. The alternatives are essentially shrinking profit margins until bankruptcy or, for some firms, successful diversification into other

lines of production (even in these cases, the original product lines may be spun off as subsidiaries and relocated if they are not abandoned outright).

When making decisions about relocation, numerous factors other than the prospective rate of return enter the equation. But the expectation of a higher rate of return remains the minimum baseline requirement, regardless of any other considerations. This expectation may be short-term, in the case of smaller private firms that will need to pay down the costs of the move more rapidly; or it may be long-term, as in the case of larger monopoly firms, many of them state-owned, where access to easy credit and subsidies extend the horizon of profitability well into the future. This adds an important qualitative dimension to China's changing industrial geography, explored below. But it also means that the relationship between movements in the rate of return in an area and its ability to attract new investment will almost certainly exhibit a time lag, since the owners of firms seeking relocation can only base their expectations of higher returns in a new region on that region's trend *in the years preceding relocation*. Short of wide-ranging interviews with factory owners, there is no rigorous way to narrow down the ideal number of years to use in measuring this lag. Nor is there any expectation that the methods used to estimate potential returns in a new location would be the same or even similar. But again, on average, factory owners are certainly making some sort of estimate about whether or not returns would be higher in new locations and, in comparing potential sites in different regions, are almost certainly taking into account not only the prevailing rates of return in their product line but also the *trend* in the rate of return, since this is the baseline forecast of potential returns in the future.

For my purposes here, I am only seeking the most general outlines of this relationship. Using the average annual growth rates over 5-year intervals, this means that each period's trend

in the growth of total enterprises should be compared to the *preceding* period's trend in the rate of return.[55] Similarly, the overall movement of the growth in total enterprises should be expected to loosely mirror the overall movement of the growth in the rate of return *offset by one interval*. In other words: if the rate of return in period A increases, then decreases (or, more often, grows more slowly) in period B but recovers in period C, the rate of growth of total enterprises should increase in period B, decrease (or, more often, grow more slowly) in period C and then recover in period D. A roughly similar relationship should hold between growth in rates of return and growth in value-added. There are several important caveats. First, since growth in all variables tends to be slower in the 2010s, the general trend will be downward everywhere. Thus, attention must also be paid to the ranking of rates of return between regions in each period and the proportionality of the declines. The regions with the highest growth in rates of return in period A should see some of the highest rates of growth in total enterprises in period B and vice versa—though for the middle values the relationship should be less clean. Similarly, particularly sharp drops in growth in a region's rate of return between periods A and B should be matched by particularly sharp drops in growth in that region's total enterprises between periods C and D. Second, over time, the increasing scale of production should see enterprise numbers grow more slowly, meaning that the proportionality of the relationship should decrease somewhat over time. Third, stagnation in rates of return should equate to *declines* in total enterprise growth rather than stagnation, since stagnation is, in practical terms, a negative forecast for future profitability.

These relationships are visualized in Table 4-3 using three types of line. The prevailing lagged relationship between growth in the rate of return and growth in the total enterprises is

[55] The low number of total observations makes any more detailed attempt to find a correlation utilizing such a lag no more informative than the method used here. Similarly, any non-inferential econometric method simply reproduces the results easily visible with the naked eye and summarized here.

visualized using red arrows. It is only shown for the first region, for simplicity, but *it prevails in eighteen of the twenty-one total rows that are comparable* (out of twenty-eight total rows, since the other nine are overhangs into the past or the future with no affiliated lag or lead variables). In other words, this relationship seems to exist for eighty-five percent of the cases, at least when the data is summarized at the regional level. In a very general sense, this relationship also tends to preserve a fair amount of its proportionality. For instance, in the Central region, the growth in the rate of return in 2000-2004 is 25.09 percent, dropping to 15.65 percent in 2005-2009, meaning that growth in the latter period was 62 percent as rapid as growth in the former period. Similarly, the growth in total enterprises in 2005-2009 was 14.94 percent and, in 2010-2014, 9.22 percent, meaning that growth in the latter period was 61.7 percent of the former—an almost perfectly proportional decline. The proportions are not necessarily so precise elsewhere and tend to become less precise the lower growth is and the later in the series (again, since growth in enterprise numbers declines with increasing scale). Nonetheless, the general movements tend to match.

There are two marginal cases where the relationship technically holds, but its proportionality does not match. Both are in the North region and are marked with red dotted lines. The three cases where the relationship does not prevail at all are shown with blue dotted lines. Of these, two are in the Northwest and one is in the South Coast. To better understand these cases, the same trends can be broken down by province. First, the Northwest:

| Northwest<br>2000-2019, Average Growth Rates | | | | |
|---|---|---|---|---|
| | Enterprises | Return on Assets | Value-Added | Net Profits |
| Gansu | | | | |
| 2000-2004 | -7.74 | 56.85 | 13.92 | 70.24 |
| 2005-2009 | 0.79 | 17.97 | 17.70 | 37.89 |
| 2010-2014 | 11.75[1] | -6.93 | 14.82 | 9.07 |
| 2015-2019 | -2.59 | 40.03 | -0.10 | 39.94 |
| Ningxia | | | | |
| 2000-2004 | 15.24 | 22.56 | 18.66 | 40.80 |
| 2005-2009 | 8.19 | 19.14 | 21.21 | 48.69 |
| 2010-2014 | 11.65[1] | -5.88 | 13.07 | 13.90 |
| 2015-2019 | 0.55 | 8.32 | 7.88 | 17.34 |
| Qinghai | | | | |
| 2000-2004 | 1.48 | 185.92 | 17.99 | 216.72 |
| 2005-2009 | 2.82 | 12.87 | 23.32 | 35.45 |
| 2010-2014 | 11.98[1] | -7.63 | 13.38 | 7.94 |
| 2015-2019 | 0.63 | -29.15 | 10.38 | -25.96 |
| Shaanxi | | | | |
| 2000-2004 | 4.64 | 27.31 | 19.29 | 44.25 |
| 2005-2009 | 8.54 | 6.71 | 21.60 | 30.22 |
| 2010-2014 | 9.00[1] | 2.81 | 18.33 | 20.50 |
| 2015-2019 | 6.87 | -0.53 | 4.64 | 6.83 |
| Xinjiang | | | | |
| 2000-2004 | -0.05 | 19.74 | 15.60 | 32.38 |
| 2005-2009 | 7.29 | 2.03 | 17.39 | 23.07 |
| 2010-2014 | 14.95[1] | -7.17 | 16.34 | 12.59 |
| 2015-2019 | 5.17 | 2.33 | 4.36 | 8.48 |
| Total Region | | | | |
| 2000-2004 | -0.71 | 26.62 | 17.07 | 39.47 |
| 2005-2009 | 6.07 | 5.68 | 19.73 | 26.91 |
| 2010-2014 | 11.07[1] | -4.11 | 16.69 | 16.17 |
| 2015-2019 | 4.02 | -7.73 | 4.25 | 5.01 |

[1] Excluding 2011 due to distortion from statistical redefinition

Source: NBS, Author's Calculations

*Table 4-4*

Here, some of the divergence can be attributed to the relatively high variability in each province due to the lower numbers of total enterprises and rates of return, leading to more wild movements in the growth rate. In the latest years, comparing movements in rates of return between 2005-2009 and 2010-2014 to movements in total enterprises between 2010-2014 and 2015-2019, every province shows the prevailing lagged relationship, exhibiting linked declines that appear loosely proportional. But the relationship does not hold in early years. While high rates of return (derived from extremely low absolute numbers of enterprises) in 2000-2004 are associated with growth in the number of enterprises in 2005-2009 everywhere save Ningxia, the extremely high growth in rates of return (caused by low absolute values) are not matched by similarly extreme growth in total enterprises. Instead, the fastest increase in total enterprises occurs in the stimulus years, 2010-2014 and *is not at all* associated with accelerating growth of the rate of return between 2000-2004 and 2005-2009, since the rate of return decreases substantially in all provinces in these years (though the decrease is proportionally a bit less in Ningxia). Moreover, the two largest slowdowns in the growth of the rate of return between 2000-2004 and 2005-2009 occur in Qinghai, Gansu and Xinjiang, all three of which actually see the largest increases in the growth of total enterprises.

This hints that the qualitative character of industrial change is essential to understanding the regional trends. Essentially, the impact of large-scale monopoly investment and stimulus spending, all linked to the state, will not be even geographically. The unevenness of the stimulus itself, its subsequent credit boom and the reconcentration of state industry in the 2010s means that there should be a divergence in the expected *short-term* relationship between rates of return and enterprise relocation. On the one hand, following Lardy (2019) and Batson (2020), the lower

or even negative rates of return in the Western provinces matched by relatively high rates of growth of total enterprises in the 2010s makes sense given that these are the provinces that have been more exclusively dependent on state investment and large infrastructure projects, which tend to have low initial returns (or just low returns in general). Shanxi should exhibit trends more closely linked to stimulus spending as well, even though it is classed in the North. On the other hand, both enterprises and rates of return should move together, albeit very loosely, over the course of both decades in the Central provinces as well as in the "near" West (i.e. Chongqing and Guizhou, as well as possibly Sichuan and Yunnan), all of which are clearly "interior" provinces that have seen greater growth of manufacturing rather than continued dependence on mining or related natural resource sectors over time and, while beneficiaries of the stimulus, have not been as dependent on this spending as "far" Western provinces like Xinjiang, Qinghai and Tibet. Since these areas have had more of a mixture of state and private investment, they should therefore see relocations dictated more by the short-term, rather than long-term, horizon of profitability.

The Northeast should see more or less uniform decline, and it does. But a few further issues are raised by the relative changes in the three smaller (in areal units) centrally administered cities: Beijing and Tianjin in the North and Shanghai in the East Coast. In these areas, enterprises may more frequently be induced to relocate to immediately peripheral locations beyond the city borders. As early as 2008, for instance, Beijing encouraged the "temporary" closure of factories in the surrounding area to ensure clean air for the weeks in which it hosted the Olympics. Over time, such closures were encouraged on a more permanent basis, with many firms moving to neighboring Tianjin or Hebei (Liu et al 2020). Meanwhile, the North region also includes Shanxi, with an industrial base in primary resources (and, on average,

less modernized mining practices than Inner Mongolia) and a lower-than-average GDP per capita, all of which place it more in line with the neighboring northwestern region. Altogether, these features likely account for the marginal status of the North with regard to the overall trends: when disaggregated by province (see Appendix 2), Beijing sees the same relationship between growth in rates of return and total enterprises that prevails in most cases, but its total number of enterprises drops early and then accelerates. Since many of the enterprises shed from Beijing (at least initially) relocated to neighboring Hebei, the province saw a sharper than expected increase in enterprise growth between 2005-2009 and 2010-2014. Meanwhile, changing registrations may have been part of the cause of suddenly rising rates of return in Tianjin in the same interval. Overall, the combination of these divergent internal trends results in the relationship between growth in rates of return and total enterprises being loose at the regional level, though still technically in line with expectations.

While the same doesn't seem to be true of Shanghai in the East Coast, it is likely that the declines in neighboring Jiangsu and Zhejiang have been softened by enterprise relocations out of Shanghai. Similarly, since better-performing firms have had less incentive to relocate, this may have accounted for the brief uptick in growth in the rate of return in Shanghai in 2010-2014—though this could also be related to the stimulus. Overall, the East Coast does not go against the trend. The same is not true of the South Coast, at least in the final comparable years. While the growth in the rate of return declines substantially between 2005-2009 and 2010-2014, the total number of enterprises grows a bit more quickly in 2015-2019 than in 2010-2014. To understand which provinces might be driving the trend, it will help to disaggregate it in the same way as above:

| South Coast<br>2000-2019, Average Growth Rates | | | | |
|---|---|---|---|---|
| | Enterprises | Return on Assets | Value-Added | Net Profits |
| Fujian | | | | |
| 2000-2004 | 18.92 | 19.94 | 14.25 | 38.73 |
| 2005-2009 | 8.84 | 6.45 | 16.67 | 25.30 |
| 2010-2014 | 5.88[1] | 1.52 | 15.55 | 18.09 |
| 2015-2019 | 1.89 | 5.69 | 8.02 | 13.34 |
| Guangdong | | | | |
| 2000-2004 | 15.96 | 11.78 | 17.01 | 27.31 |
| 2005-2009 | 8.94 | 7.02 | 16.73 | 24.28 |
| 2010-2014 | 2.46[1] | -0.16 | 10.34 | 12.49 |
| 2015-2019 | 6.32 | -3.55 | 5.83 | 5.62 |
| Guangxi | | | | |
| 2000-2004 | 5.35 | 28.60 | 11.87 | 42.08 |
| 2005-2009 | 8.96 | 3.64 | 18.90 | 24.84 |
| 2010-2014 | 5.94[1] | 14.91 | 12.89 | 35.23 |
| 2015-2019 | 2.60 | -6.17 | 5.49 | -1.07 |
| Hainan | | | | |
| 2000-2004 | -0.28 | 44.89 | 20.75 | 61.99 |
| 2005-2009 | -2.84 | 15.24 | 15.20 | 31.65 |
| 2010-2014 | 1.82[1] | -10.12 | 11.35 | 2.48 |
| 2015-2019 | 2.09 | 3.20 | 4.34 | 9.42 |
| Total Region | | | | |
| 2000-2004 | 15.25 | 20.62 | 16.00 | 29.83 |
| 2005-2009 | 8.77 | 7.69 | 16.85 | 24.50 |
| 2010-2014 | 3.53[1] | 0.45 | 11.69 | 14.75 |
| 2015-2019 | 4.86 | -0.45 | 6.31 | 6.82 |

[1] Excluding 2011 due to distortion from statistical redefinition

Source: NBS, Author's Calculations

*Table 4-5*

Of the South Coast provinces, only Fujian clearly follows the prevailing trend seen across most time periods in most regions. In Guangdong, the trend is initially followed in the early years, but recovering growth in total enterprises in 2015-2019 occurs after stagnation in the rate of return in 2010-2014. It is difficult to say with precision what the cause of this might be, though the centrality of Guangdong within national efforts to increase rates of automation and climb the value chain in production is one likely reason, since some of these efforts have been accompanied by direct government subsidies (He 2015 and Asia Times 2018). These subsidies were also not made use of across all regions but saw a particular concentration in Guangdong, as demonstrated in Cheng et. al. (2019), where the authors conducted a comparative survey that found much more intensive use of robots in production in Guangdong as compared to Hubei in the interior.

At the same time, Guangxi and Hainan both diverge from the trend as well. In Guangxi, the rate of return increases between 2005-2009 and 2010-2014 and, while it might have prevented enterprise growth from declining further, it is not associated with any increase in the growth rate of total enterprises. By the subsequent period, however, the relationship is restored. In Hainan, by contrast, the relationship is almost perfectly inverted in the later years. Declines in the rate of return are met with relatively stable, though low, growth in total enterprises. In Hainan, this is almost certainly due to low absolute numbers of industrial enterprises and rapid changes in the province's industrial base. It is an outlier in the maps above as well. In Guangxi, low total numbers may be part of the cause of the divergence, but the stimulus and its aftereffects are a more likely source of the marked increase in the rate of return in 2010-2014 which was driven by a boom in net profits in the province that began in 2010 and persisted through much of the decade before declining after 2016. Ultimately, however, the exact cause remains unclear.

Enterprise relocations should also be related to inter-regional differences in the total rate of return as well as the growth trend in a single region. In other words: while it is important that the Central region has positive growth in the rate of return because this acts as a forecast of potential rates of return in the future, it is also significant that it consistently sits at the top of the ranking in rate of return by region after the Great Recession. These facts combined are what seem to cause the Central region to hew most consistently to the trend, with all provinces more closely following the same pattern that is visible in general across regions:

| Central<br>2000-2019, Average Growth Rates | | | | |
|---|---|---|---|---|
| | Enterprises | Return on Assets | Value-Added | Net Profits |
| Anhui | | | | |
| 2000-2004 | 6.87 | 40.67 | 13.27 | 53.92 |
| 2005-2009 | 24.56 | 8.18 | 22.19 | 33.34 |
| 2010-2014 | 13.32[1] | 1.40 | 18.54 | 21.78 |
| 2015-2019 | 0.18 | -2.57 | 5.50 | 3.22 |
| Henan | | | | |
| 2000-2004 | 4.40 | 17.64 | 15.46 | 32.00 |
| 2005-2009 | 10.60 | 22.59 | 21.05 | 46.30 |
| 2010-2014 | 6.39[1] | -4.05 | 9.55 | 15.88 |
| 2015-2019 | -1.93 | -6.03 | 4.75 | -3.72 |
| Hubei | | | | |
| 2000-2004 | -0.20 | 14.49 | 12.04 | 26.75 |
| 2005-2009 | 17.94 | 9.55 | 21.69 | 32.50 |
| 2010-2014 | 14.62[1] | 6.47 | 16.87 | 18.45 |
| 2015-2019 | -0.48 | -0.25 | 7.07 | 5.03 |
| Hunan | | | | |
| 2000-2004 | 12.15 | 31.56 | 12.88 | 45.92 |
| 2005-2009 | 12.21 | 16.11 | 21.04 | 39.13 |
| 2010-2014 | 3.44[1] | 3.47 | 17.02 | 22.43 |
| 2015-2019 | 3.85 | 0.23 | 4.08 | 6.79 |
| Jiangxi | | | | |
| 2000-2004 | 4.28 | 46.30 | 20.86 | 60.56 |
| 2005-2009 | 13.65 | 23.97 | 23.20 | 51.68 |
| 2010-2014 | 9.89[1] | 12.35 | 16.99 | 32.96 |
| 2015-2019 | 7.77 | -7.75 | 4.86 | 1.54 |
| Total Region | | | | |
| 2000-2004 | 5.07 | 25.09 | 14.45 | 35.51 |
| 2005-2009 | 14.94 | 15.65 | 21.55 | 39.48 |
| 2010-2014 | 9.22[1] | 3.33 | 14.78 | 19.76 |
| 2015-2019 | 1.12 | -4.02 | 5.26 | 0.90 |

[1] Excluding 2011 due to distortion from statistical redefinition

Source: NBS, Author's Calculations

*Table 4-6*

Overall, then, any explanation of the Central region's more robust growth in the number of total enterprises over the course of the 2010s should include both the time-lagged effect of growth in the rate of return and the fact that, after 2008, the rate of return in the region consistently lay above the rate elsewhere. But these are not mechanical relationships. Similarly, while true at the larger regional scale, they tend to decompose as the geographic units of analysis become more granular. Enterprises cannot simply flow to any area with higher returns because the much of the capital embodied in the firm is not truly liquid. In part, this is because they are literally fixed in place. Each territorial production complex is, after all, an accretion of multiple waves of investment in plant and equipment. But there are also other factors to consider, such as the reality that the rate of return may simply be high in a given region because of a concentration of a particular production line that tends to gather super-profits. This is especially going to be true of trends in the Northeast, North and Northwest, all of which have, throughout the period in question, had a much higher dependence on primary commodity sectors that are most subject to the movements of the commodity super-cycle, for instance.

To conclude, then, it will help to close with a simple table showing 5-year summaries by region of the average rate of return on assets—note that this is an average of the actual rate of return in each 5-year period, *not* an average annual rate of growth:

**Average Rate of Return on Assets**

2000-2019, Industry

| Time | Central | East Coast | North | Northeast | Northwest | South Coast | Southwest |
|---|---|---|---|---|---|---|---|
| 2000-2004 | 2.67 | 5.42 | 3.47 | 6.15 | 2.64 | 3.81 | 2.44 |
| 2005-2009 | 6.83 | 7.00 | 6.23 | 9.24 | 7.03 | 6.53 | 5.47 |
| 2010-2014 | 10.07 | 8.36 | 7.03 | 8.61 | 5.61 | 8.33 | 6.14 |
| 2015-2019 | 7.76 | 6.97 | 4.46 | 4.02 | 2.28 | 7.15 | 5.35 |

Source: NBS, Author's Calculations

*Table 4-7*

This is essentially a tabular summary of the same trends visible in Figure 4-9 above, but with the relative relationships simplified. One potential problem is that, in this form, the inflection point of the Great Recession is not perfectly captured, since the 2005-2009 period includes both pre- and post-crisis trends even though Figure 4-9 shows that the last two years in this period saw some significant changes in the relative position of each region—with the Central region ascending to the top of the ranking and the Northwest declining to the bottom after having risen to second place in the first few years of the period.[56]

When this table is compared to the changing patterns of industrial location visible in the maps above, a few important features stand out. The first is the fact that, while profitability has fallen in the final half of the 2010s, it still generally lies above where it sat in the first half of the 2000s. The two exceptions are the Northeast, where the return on assets in the late 2010s lay two

[56] If this is corrected for by shifting the years, the pre- and post-crisis trends become more clear, but then each bin has an uneven number of years. This is a problem for averages computed across so few observations and comes a bit too close to tipping the scales in favor of what we might want to see. Meanwhile, it would ensure that the table was not comparable to every other series shown above.

percentage points below where it sat in the early 2000s, and the Northwest, where it has fallen to a similar rate as in this earlier period. But, whereas the rate of growth in the number of total enterprises in the Northeast has declined in line with the return on assets, the opposite has been true in the Northwest, where the growth in total enterprises was just above four percent in 2015-2019, but was slightly negative in the early 2000s even though both periods had similar rates of return. Again, this reflects the basic fact that the relationship is not mechanical. The relative influence of large, state-directed infrastructural investments are abundantly clear in the case of the Northwest, where enterprise growth was robust despite low rates of return. This also clearly shows that two otherwise similar regions initially dependent on the production and processing of primary commodities can see a strong divergence in their trajectories caused both by high-level industrial policy guiding investment and by differences in the inherited burdens of their pre-existing industrial complexes. While much of the Northwest was a minimally developed greenfield site for new firms, the Northeast had not successfully shed the weight of obsolete industrial infrastructure that marks it out as a rust belt.

Ultimately, the prospects of different regions can be very loosely forecast using the methods laid out above, with the caveat that short-term responsiveness to the rates of return can be desensitized through reliable manufacturing subsidies (such as robots in Guangdong) and the intentional funneling of investment against short-term gains in favor of securing long-term prospects for profitability (as in the Northwest). Similarly, the rising scale and increasing capital-intensity of production will tend to place downward pressure on most growth trends. But, short of industrial policy exerting an opposite force, it seems likely that the Central region will see some decline in its enterprise growth over the following five years or even see the total number of enterprises begin to shrink. The trend should be the same in the East Coast, but this will

depend, in part, on whether robotization drives are accelerated in the Yangtze River Delta in the early 2020s, as they were in the Pearl River Delta over the latter half of the 2010s. The negative growth rates of total enterprises seen in the North and Northeast will likely continue into the next five-year period, but there's a slight chance that subsidies targeting high-tech development in the Beijing-Tianjin-Shijiazhuang Hi-Tech Industrial Belt (京津石高新技术产业带) might lead to an uptick in enterprises like that seen in Guangdong.

In the Northwest, prospects are still dependent on state investment, though less so than in the past. It is difficult to predict exactly what will happen here, since the boom in state-subsidized projects depressed the rate of returns over the course of the 2010s even while it increased the number of enterprises. Given the low absolute value of the return on assets in the Northwest by the end of the decade, it is likely that it will recover in the early 2020s and this recovery might be accompanied by modest growth in the number of enterprises. In the South Coast, Fujian will likely see growth in the number of enterprises matching its rising rates of return at the end of the decade. Guangxi will see a decline unless it is targeted for further attention in central industrial policies. Hainan is too much of an outlier to make any prediction for since most of its economy is still agricultural and much of the remainder is now service based.

In the Southwest, Chongqing will almost certainly see a decline in its total number of industrial enterprises in the next five years unless targeted industrial policies prevent this. Growth in the number of enterprises should slow in Guizhou and Sichuan, under the same conditions. Tibet, like Hainan, is enough of an outlier that little prediction can be made. It is also more subject to subsidized investment drives, similar to those in the Northwest. Finally, Yunnan poses itself as a curious case where the rate of return rose sharply in the latter half of the 2010s,

helping lead the brief recovery in the region as a whole—where the return on assets in 2019 had nearly converged with that of the East Coast, with the two essentially tied for third place. As in the case of Guangxi, it's unclear exactly what caused this uptick, but, since it lies in the 2015-2019 period rather than 2010-2014, it seems unlikely to be related directly to the stimulus. If it represents a real increase in the return on assets, then the number of industrial enterprises in Yunnan should increase in the early 2020s despite declines elsewhere.

# Section 2
# –
# Trends in Outbound Investment

In certain sectors, offshoring became more common than relocating internally. Similarly, once the initial stimulus had built out the capacity of China's road, rail and utilities networks, many firms involved in such construction began to seek international contracts. This was often a simple result of market saturation and, in many cases, it preceded the stimulus. One example is the hydropower sector. Since the number of suitable locations for large dams is finite, China's domestic buildout of hydropower potential had begun to run up against limits well before the outbreak of the Great Recession. Thus, Chinese hydropower construction firms—primarily Sinohydro, one of the world's largest transnational hydropower corporations—began to seek contracts overseas fairly early, in the first decade of the new millennium (McDonald et al 2009). Since most of the world's untapped hydropower potential lies in the Himalayas, Southeast Asia and, to a far lesser extent, Sub-Saharan Africa, this led to large, early investments in countries

like Nepal (Galen et al 2016) and Cambodia (Siciliano et al 2016) as well as later contracts in various African countries (Tang and Sheng 2020). But the stimulus created similar conditions across the construction and heavy industrial sectors more broadly, triggering a sharp increase in the number of construction contracts secured by Chinese firms overseas.

Early on, then, Chinese outbound investment tended to be dominated by large monopoly firms (usually state-owned) operating in the construction, utilities and mining industries. But the general glut of global manufactures documented by Brenner and Benanav also meant that Chinese industrial firms were already experiencing overcapacity and facing intensified competition. The stimulus was merely a temporary credit boom designed to make use of this overcapacity, doing nothing to solve its underlying cause in intensifying competition. Over the course of the 2010s, rising costs and receding stimulus funds meant that many firms now found it cheaper to relocate overseas. Since rising labor costs were a major source of downward pressure on profitability, outbound relocation of manufacturers was more common in certain labor-intensive sectors like textiles than in capital-intensive sectors like steel. But since overcapacity was building across all industries, it became increasingly lucrative to relocate even higher-tech production lines. At the same time, the true scale and scope of Chinese outbound FDI has been exaggerated in the media, largely due to unethical reporting practices that have intentionally overstated Chinese presence overseas and especially in Africa (as documented in Brautigam 2011 and 2015). There remains a need for a more systematic process of sifting through the existing data, conducting wide-ranging field surveys of Chinese firms operating in the world's ascendant territorial industrial complexes, and interviewing Chinese factory owners about the motivation behind relocation.

While the entirety of such a project is beyond the scope of this dissertation, I hope to offer the outlines of what it might entail. Above, I offer the theoretical framework within which we might understand industrial-territorial development and its relation to crisis and data looking at the relevant industrial trends within China itself. In this section, I will give a very broad framing of available data and research on Chinese outbound investment, which will contextualize the field work presented in the subsequent chapter. In that chapter, several of these topics will be returned to with more focus and several of the major sources mentioned here will be analyzed in more detail. In this section, I will give an overview of the major sources of data on Chinese outbound investment and then, using these sources, explain some of the large-scale trends visible in this data. Since my focus is on the prospects of the economy's core, productive firms—which, as argued by Benanav, Brenner, Smith and Mattick, are the real engines of economic growth and also, as argued by Storper and Walker, industrial-territorial development—the primary focus here will be on foreign direct investment, especially in manufacturing. I will give secondary attention to other forms of investment and aid, such as construction contracts, since these are related insofar as they form the early infrastructural framework necessary for successful industrial relocation but they are also already widely studied, tending to be the focus of most surveys and field studies over the course of the last decade. By contrast, the most significant aspects of global industrial competition that will remain more or less unexamined here are the changing trade dependencies that have accompanied the period of "deglobalization" (Herrero 2020) and Chinese manufacturers' increasing emphasis on production for export to poorer countries, rather than the world's wealthiest nations. This has created new trade dependencies abundantly evident in countries like Tanzania, which are mentioned in passing in the next chapter but not systematically examined here.

**The Major Data Sources on Chinese Outbound Investment**

There are four major sources of data on Chinese outbound investment that explicitly include information on FDI rather than just financial information on loans, aid or large infrastructural contracts. The first is the Chinese Ministry of Commerce (MOFCOM), the official Chinese state institution tasked with recording data on outflows of capital, which it reports in its annual Statistical Bulletin of China's Outward Foreign Direct Investment (中国对外直接投资统计公报). One important subset of MOFCOM data is that same data as recorded and cross-referenced by the China-Africa Research Initiative (CARI) of the School of Advanced International Studies at Johns Hopkins. CARI also gathers independent data on Chinese loans, construction contracts and agricultural investments in Africa. But the institute's FDI series comes more or less straight from MOFCOM, though it is cross-referenced against similar data from national statistical authorities in African countries where possible. Since CARI is the only organization to have systematically digitized MOFCOM data—albeit just for Africa—their dataset is used repeatedly in this section and in the next chapter.

The data derived from MOFCOM is not ideal, but it is gathered by an official statistical authority with unparalleled access to the primary sources necessary to systematically record such capital flows. Individual problems with each dataset will be addressed in more detail in the next chapter. Data from MOFCOM, like all national statistics, should be expected to at least distantly reflect the interests and biases of the nation funding their collection and, as with US FDI statistics, inaccuracies can arise from changing national policies that incentivize or disincentivize the formal registration of overseas activity. For example, in 2005 and 2018, changing tax policies

in the US encouraged firms to repatriate their earnings from their foreign affiliates, causing negative dips in FDI data that did not proportionally reflect any real declines in investment (OECD 2018). Similarly, MOFCOM data has always been distorted by the special administrative status of Hong Kong, which has acted as a financial nexus for Chinese capital flowing out of China as well as capital being recycled back into the country. This means that Hong Kong is consistently listed as the major recipient of outbound FDI, even though a large portion of this is actually domestic reinvestment. In 2019, for instance, Hong Kong accounted for fifty-eight percent of all Chinese outbound FDI stock (MOFCOM 2020, p.152).

Hong Kong's own Census and Statistics Department provides some data on inward and outward investment, but its role as a global financial hub with links to offshore banking centers ensures that the actual character of these investment flows quickly grows murky. The single largest source of FDI stock in Hong Kong in 2018, for example, was not the mainland but instead the British Virgin Islands, at 31.9 percent. By comparison, the mainland accounted for some 26.8 percent in the same year, followed by the UK, the Cayman Islands and Bermuda. In any given year, however, the mainland does often compose the largest source of FDI flows (Census and Statistics Department 2019, pp14-16). By comparison, the mainland was the major site for Hong Kong outward FDI, with 40.6. percent of all Hong Kong outward FDI stock held there in 2018, most of which lay in neighboring Guangdong province. Beyond this, almost no accurate tracing of the remaining FDI stock can be performed, since the next largest locations are, again, the British Virgin Islands (33.8%) and other offshore tax havens (ibid, pp.21-22). Overall, however, there seems to be no reason to believe that the final destination of those funds funneled through Hong Kong but not recycled back into the mainland would systematically differ from the final destinations of direct investment as recorded by MOFCOM. At most, we

might suspect that the preponderance of financial tax havens in Hong Kong's outward FDI position would suggest that portfolio investment may be a larger share of these flows, in contrast to direct investment in production. Either way, the ultimate effect is simply that the *total* figure reported by MOFCOM exaggerates overall FDI as compared to other countries. But the *relative* shares of each FDI destination are likely unaffected.

The second major source is the China Global Investment Tracker (CGIT) developed by Derek Scissors for the American Enterprise Institute (AEI) and the Heritage Foundation. Though widely used and cited even by some of the most reputable economists studying China (as in Naughton 2018, pp.440-442), Scissors' project is openly aligned with US defense interests and demonstrates a clear bias that overstates the relative size of Chinese investment in several respects. Scissors himself is a conservative economist who has worked in corporate intelligence and for the Department of Defense (AEI 2020). The next chapter will focus in more detail on the biases built into CGIT and within Scissors' work more specifically, including a case-by-case analysis of the CGIT data in Tanzania, where it is found that as many as half of Scissors' reported investment projects are either unidentifiable or have not come to fruition (whether indefinitely stalled or outright cancelled). Overall, though, this is due to the fact that Scissors relies in part on media reports of large-scale investment agreements between China and other countries and seems not to accurately or systematically remove all projects that fail to break ground or stall long before all these announced funds are actually transferred.

Part of this structural bias is likely due to the fact that the CGIT is a project of the AEI and Heritage Foundation, both of which are well-known conservative think tanks funded in part by rightwing American industrialists and extremely influential in the formation of the Trump cabinet at the height of recent trade tensions with China (Brulle 2014 and Mahler 2018). Thus,

the CGIT seems have the same explicit bias built into its project as that found in other initiatives funded by these think tanks, which are well known for rejecting scientific consensus in fields such as climate science (Brulle 2014). It is not surprising, then, that the investment tracker explicitly clothes its project in the language of national security and draws actively from trade war and "New Cold War" discourse, all while failing to scale Chinese FDI against other major sources of FDI, such as US-origin investment (as is done by CARI). At the same time, the CGIT data can still be made use of if one understands the nature of its built-in bias.

As is mentioned above, the largest source of this bias is its partial reliance on public announcements of investment, including in news reports. This means that the total figure recorded by the CGIT is simply not accurate. But there is no reason to believe that it diverges that strongly from the overall trend over time averaged across all countries—and, in fact, it tends to match the trend visible in MOFCOM data and as recorded by various national statistical authorities in the host countries at least when averaged at this level. As will be seen in the next chapter, the CGIT data is far less reliable when focusing on certain countries. But for all years prior to 2019, the trends in the CGIT database and the MOFCOM database are identical when averaging across all countries. Beginning in 2019, the clear decline in large-scale investments as compared to smaller-scale investments began to mark a clear divergence in the trend (Scissors 2020a, Tables 1 and 2). Overall, though, the exact source of the CGIT remains unclear. It is simply stated that the CGIT "uses corporate sources, sometimes partners but often the Chinese participants" and that "transactions are often disclosed by these firms then revised, requiring CGIT revisions, which occur biannually" (ibid, p.2). This means that the most recent years will likely be the least accurate. The scope of the data stretches from 2005 to 2019 (at writing), and "contains more than 1,600 investments worth $1.2 trillion" and "1,700 construction projects

worth more than $800 billion" (ibid). But these summary values are not true capital stock figures, since they contain no measure of depreciation (and little confirmation that all of the initially announced investment actually took place).

But there are a few other important sources of bias to keep in mind here. The first is that the CGIT database is bound to overreport the relative influence of larger companies and thereby overstate the role of state-owned enterprises (which tend to be larger) in the overall composition of China-origin FDI. Especially in recent years, it should be considered "only a subset of total Chinese investment overseas" (ibid, p.3). This is because "the CGIT contains investment and construction transactions worth $100 million (rounded) or more" (Scissors 2020, p. 2) but does not record investments that fall below this scale. Since many areas see significant shares of inbound FDI from smaller, predominantly private enterprises (Sun et al 2017), the CGIT data does not accurately represent the composition of Chinese FDI for these areas. This is partly offset by the fact that the CGIT database separates investment from construction contracts, since construction contracts tend to be secured by larger firms, many state-owned. Nonetheless, this often makes the investment data for individual countries extremely unrepresentative, often including only a few mega-projects, many of which may not have broken ground. Meanwhile, if the trend of smaller-scale, private investment diverges from the trend of larger-scale investment through state-owned firms, then the CGIT data will cease to be representative of the overall trend. This appears to be the case from late 2018 onward.

The basic distribution of the CGIT data hints at this positive skew, while also suggesting that more investments likely sit below the cutoff that artificially slices off the left tail of the distribution. This is visible in histograms for CGIT records of both FDI and construction contracts:

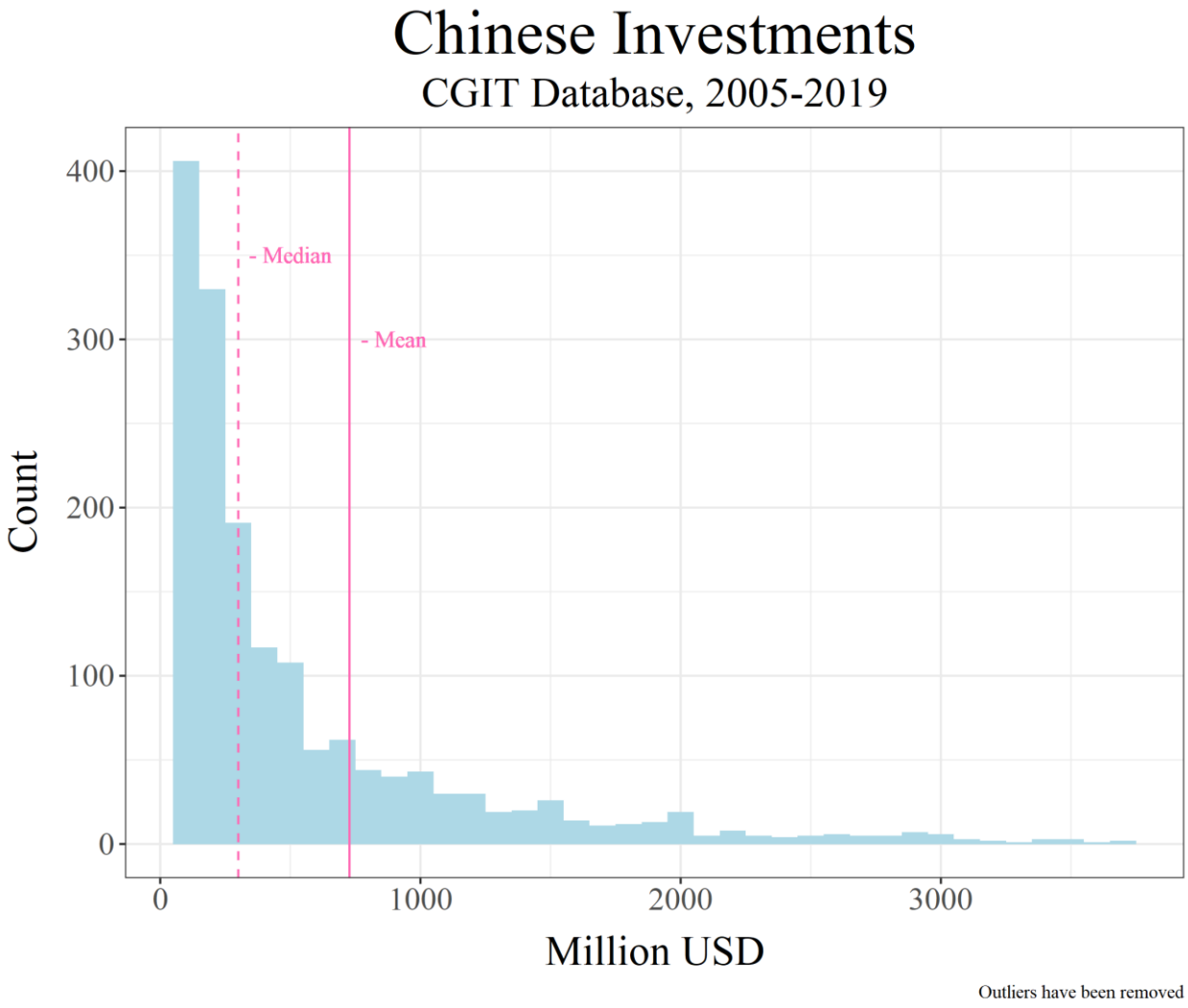

Chinese Investments
CGIT Database, 2005-2019
400
300
200
100
0
Count
- Median
- Mean
0
1000
2000
3000
Million USD
Outliers have been removed

*Figure 4-13*

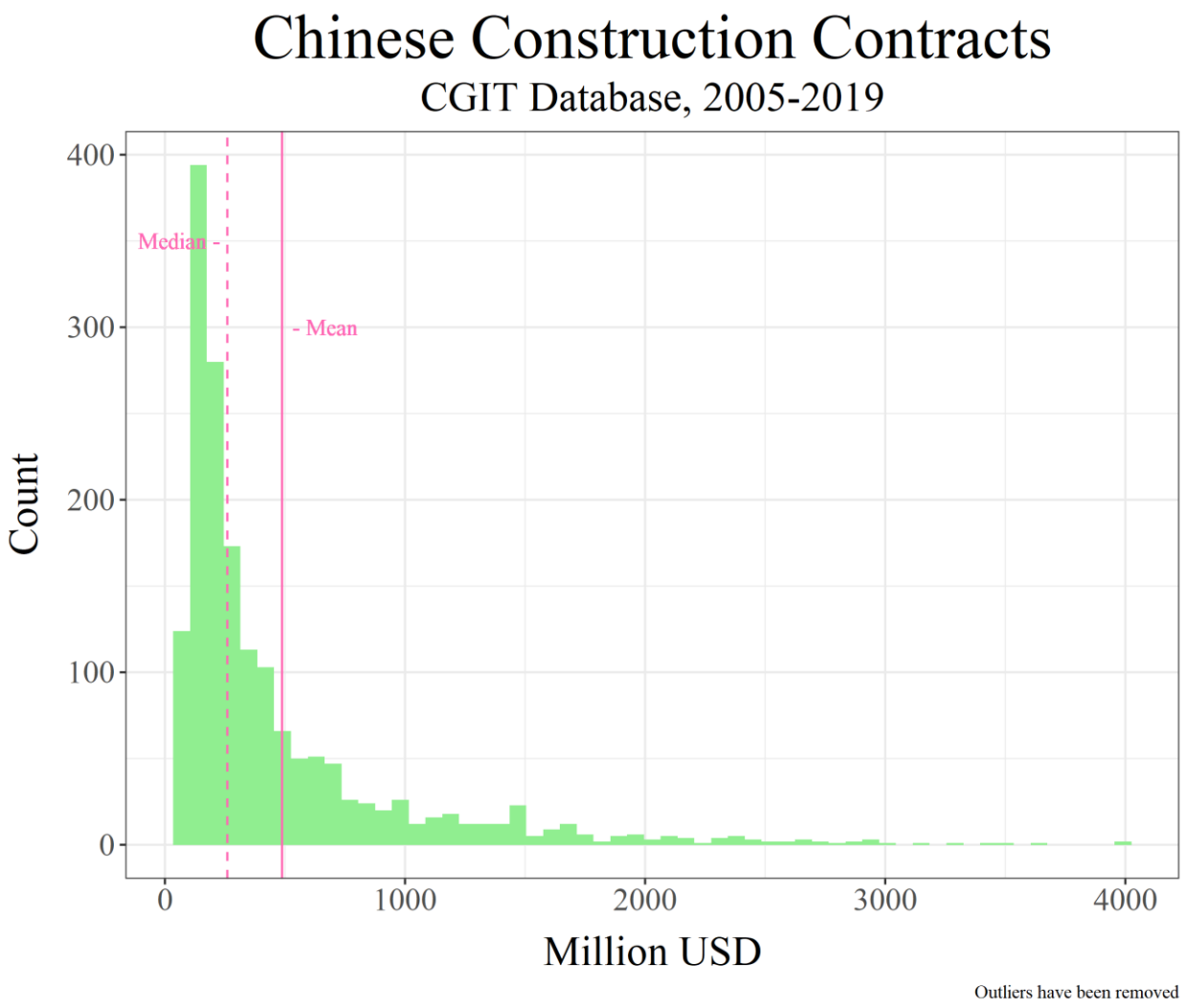



*Figure 4-14*

The distribution in both series has a fairly strong positive skew even when outliers are excluded for the sake of scale (the most extreme of which are all major mergers and acquisitions, not greenfield investments). The FDI distribution is mesokurtic or mildly leptokurtic and the distribution of construction contracts is highly leptokurtic. If investments and contracts below the one hundred million cutoff were included, it is clear that the distribution would still be positively skewed. But in the case of FDI, it seems that this cutoff likely excludes far more total projects,

since the distribution is rising continually as the size of investment falls. By contrast, construction contracts are presumably larger on average and the distribution shows a clear decline in the lower range of the x-axis.

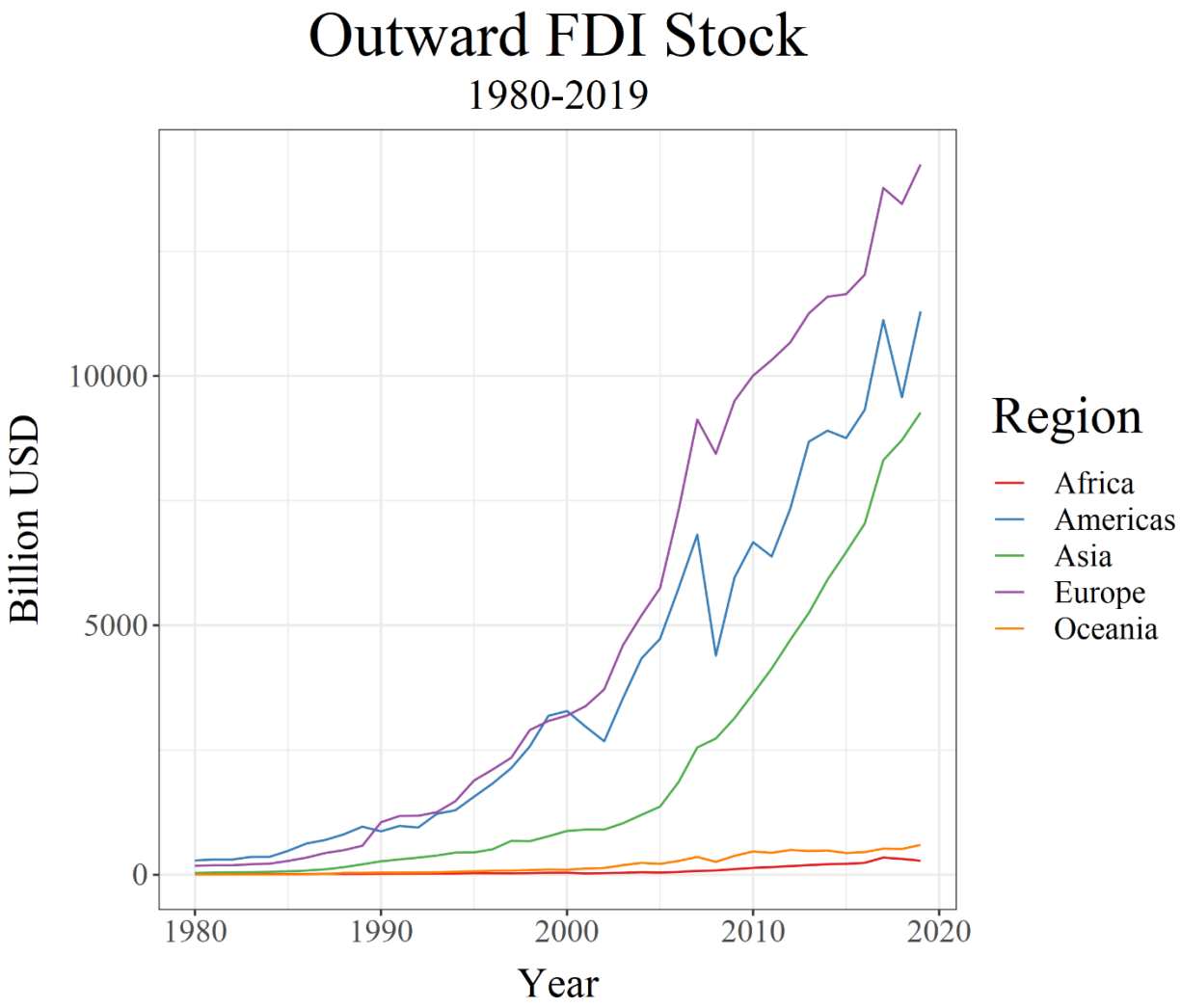
Aside from MOFCOM and CGIT, a third category of data is found in the records of inbound FDI kept by certain countries national statistics agencies. On average, these statistics are most accurate in the wealthiest countries, which tend to have greater ability to fund such data-gathering and already have robust state institutions in place to record inbound capital flows, since such data is necessary for fiscal management and more general economic policy making. But this also means that national data on FDI will be weakest in the poorest countries where that FDI would have a larger relative impact. In some cases, as in Kazakhstan and Thailand, fairly consistent series can be found recording inbound FDI flow and stock by country. In others, such as Kenya, less consistent data is recorded in periodic Foreign Investment Surveys, which sometimes show unaccountable inconsistencies when compared between years or contrasted to alternate datasets. In still other locations, such as Tanzania and Laos, there seems to be very few accurate, accessible local records of investment. As will be explained in more detail in the next chapter, while the Tanzania Investment Center is technically tasked with recording such data, their actual records diverge extensively from the reality on the ground (Xia 2019a). One final subset of this data are the United Nations Conference on Trade and Development (UNCTAD) bilateral investment series, which recorded inbound and outbound FDI data by country from the early 2000s through the early 2010s.

A final source of data is the broad array of more local surveys conducted by scholars and think tanks, often focusing on one region or one country. Sometimes these surveys are more oriented toward the collection of quantitative data (as in Leke et al 2014 and Sun et al 2017).

More often, they are ethnographic studies focusing on the particular social nexus of an individual investment project or cluster of such projects in a single country. Here, CARI is another important source of data through its Working Paper series, which often publishes field surveys of individual sectors within individual countries (such as Xia 2019a and 2019b, Tang K. 2019 and Tang X. 2019) and/or summary reports of certain sectors across the continent (such as Brautigam et. al 2018). Unfortunately, the CARI Working Papers only focus on Africa. Elsewhere, the best resources are found through simple searches of scholarly articles in relevant social science journals on a country-by-country basis. These searches almost always return ethnographic reports on particular development projects in particular locations of these countries. For instance, Murton et al (2016) analyzes a hydropower project in Nepal, Altenburg et al (2020) examines Chinese investment in the textile industries in Ethiopia and Madagascar, Andrews-Speed et al (2016) surveys mineral investments across Southeast Asia, Kiik 2016 focuses on the Myitsone Dam project in Myanmar, Bennett 2020 gives the results of both remote sensing analysis and field work ground-truthing of the same data along the Kazakh border, Alff 2016 looks at commerce in Kyrgystan, Siciliano et al 2016 examines the effects of dam projects in Cambodia, Jackson and Dear 2016 looks at Chinese presence in extractive industries in Mongolia—there are more studies of this sort than could possibly be cited here. I will therefore cite relevant instances as they come up in the text below.

**Major Trends in Chinese Outward Investment**

The overall trend of Chinese outbound foreign direct investment is upward. This is visible in UNCTAD, MOFCOM and CGIT data[57] and has been confirmed in various field studies. China has rapidly ascended into the upper ranks of global contributors to FDI, holding the second or third place in measures of FDI flow since 2012 and of FDI stock since 2017 (MOFCOM 2020, p.93, Table 2). China is now the main driver of a much longer-run increase in global FDI originating from East Asia, visible in both raw numbers and as a share of total global FDI stock:




[57] Controlling for the fact that CGIT diverges downward in recent years due to its exclusion of smaller firms.

*Figure 4-15*

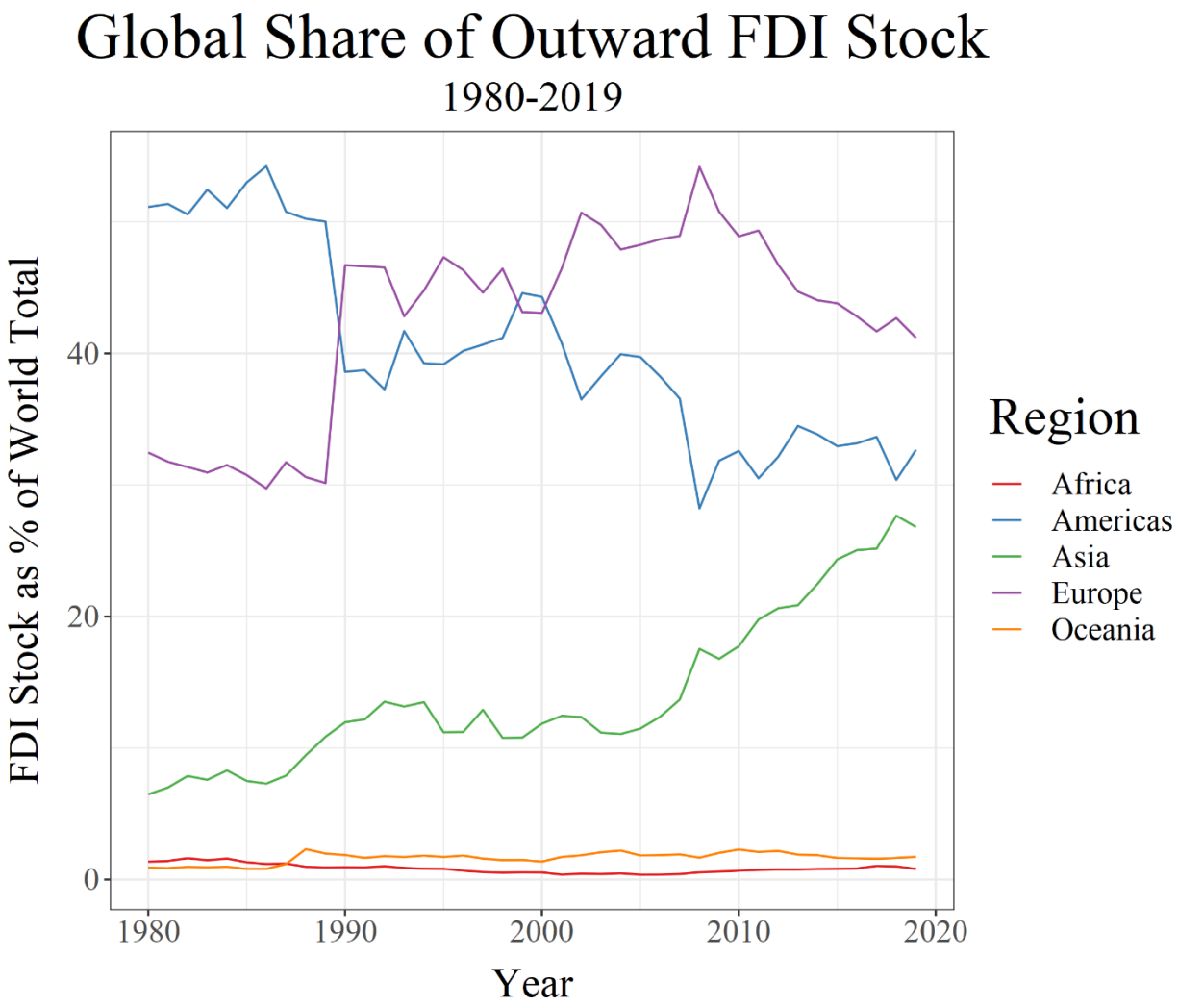



*Figure 4-16*

There are two key periods in the growth of East Asian contributions to FDI visualized in the charts above. The first is the contribution of Japan throughout the 1980s and into the 1990s, at which point it was accompanied by similar increases in Hong Kong and Singapore and, later,

Taiwan and South Korea. The second is the rapid acceleration of East Asia's share after the mid-2000s, almost entirely driven by the rise of Chinese-origin FDI, though again accompanied by linked increases in Hong Kong and Singapore (as important financial centers).

After the collapse of the Soviet Union, the European share overtook the share from the Americas (the bulk of which is the US, though Canada and offshore tax havens in the Caribbean also make a contribution). The two moved in unison over the 1990s, converging at the end of the decade. But then the European share grew again over the 2000s, while the American share fell. In the 2010s, the European share fell and the American share stagnated, even while both were increasing in absolute terms—meaning that the cause of the decline in the European share and the stagnation of the American share was largely the rapid (and more consistent) rise of the Asian share throughout the period. But the European and American shares are still far from another convergence. The European ascent in the 2000s was mostly driven by the reconfiguration of the UK and the Netherlands into financial centers (for an analysis of this process, see Norfield 2016) and the rise of the German export industry. Trends in the Americas were almost entirely due to the influence of the US.

The relative positions are made clearer when the shares are disaggregated. Since there are too many countries and separately-recorded administrative areas (such as Hong Kong and Macao) to clearly visualize, I will simply highlight a few of the most important cases for comparison:

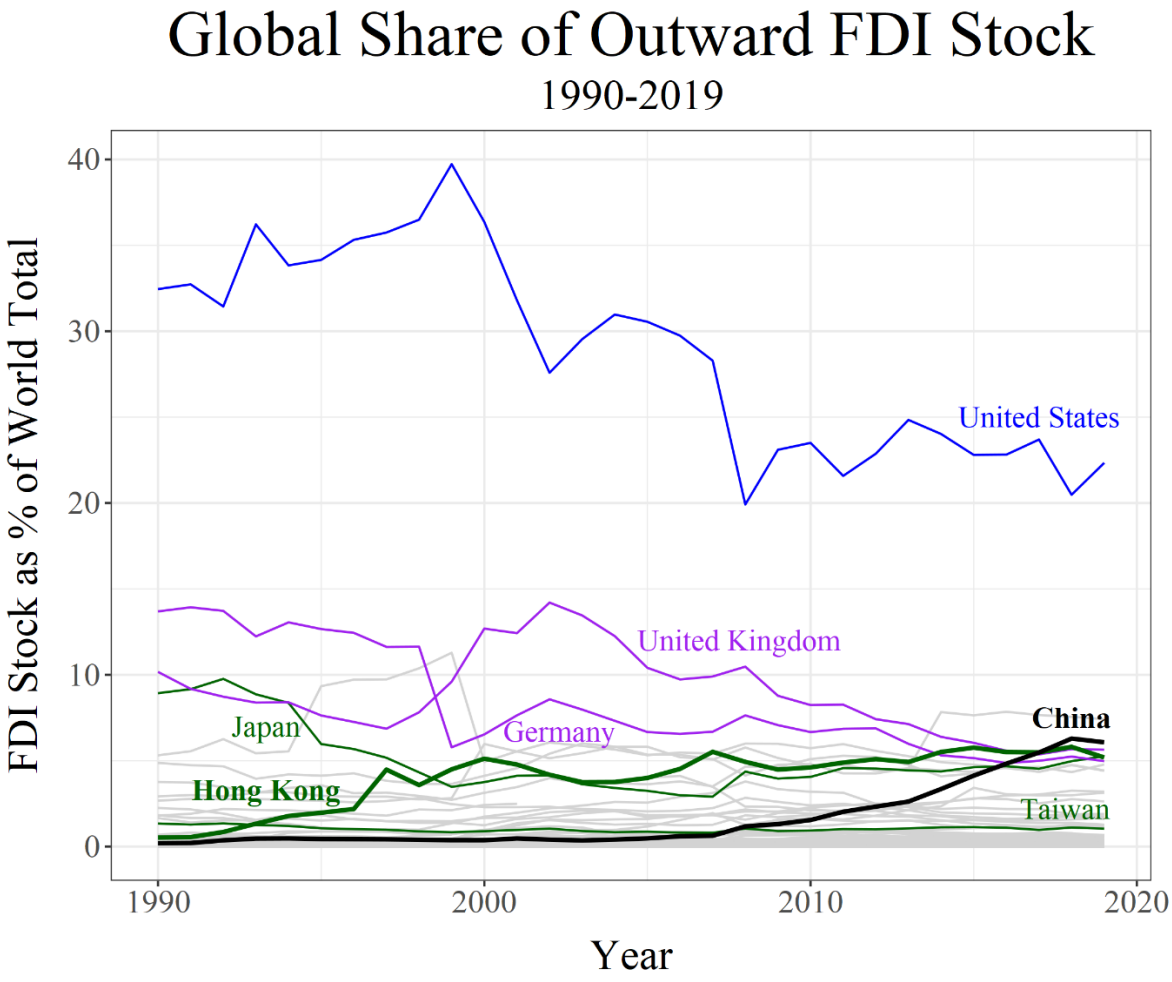



*Figure 4-17*

The most distinct feature here is the rise of Hong Kong after the late 1990s and the rise of mainland China after the late 2000s. As of 2019, China holds third place in ownership of overall FDI stock, far below the US and just below the Netherlands, having overtaken Japan, Germany and the UK (as well as several other nations and tax havens) in the late 2010s. If it were combined with Hong Kong, its share would increase substantially, but this would at least in part be because of double-counting investment recycled into the mainland. That said, the share has stagnated in Hong Kong since the late 2000s and has begun to stagnate in mainland China in the

most recent years, even declining slightly. Meanwhile, Taiwan's share remains extremely low, despite the apparent increase in recent years of high-profile investments outside of mainland China made by large Taiwanese conglomerates such as Foxconn.

The decline of Japan's relative influence after the 1990s is also visible, though its share of FDI has slowly begun to climb upwards again after the late 2000s. Meanwhile, even if the individual national shares of the UK, Germany and other European nations have tended to decline or been recently overtaken by China, it's important to remember that, when its component nations are combined, the EU still outranks China substantially. The result is that European and American nations account for some seventy percent of overall FDI stock, and the US alone controls more than twenty percent, even if this has declined from a peak of nearly forty percent in the late 1990s. If Chinese capital is aggregated with other Asian nations, there is slightly more parity, with the Asian share lying just above twenty-five percent in the most recent years. While such an aggregation makes sense in part due to the closer integration between sources of capital across the "bamboo network" (formed of influential Chinese investors located across Southeast Asia) and the fact that Hong Kong is administered by China, the region does not have a level of administrative or monetary integration comparable to that of the EU.

While the above figures make the rise in Chinese outbound investment appear to be more or less continuous, the CGIT data can be used to explore the shape of the trend in more detail:

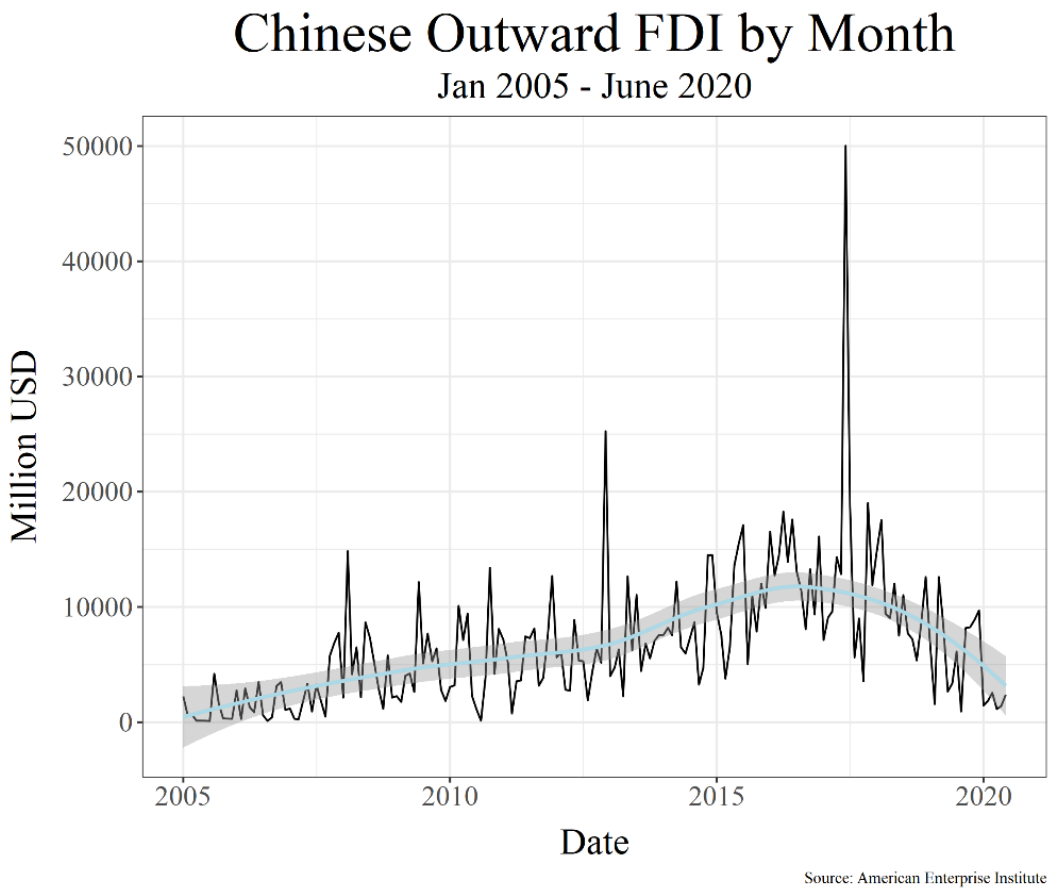

Chinese Outward FDI by Month
Jan 2005 - June 2020
Million USD
50000
40000
30000
20000
10000
0
2005
2010
2015
2020
Date
Source: American Enterprise Institute

*Figure 4-18*

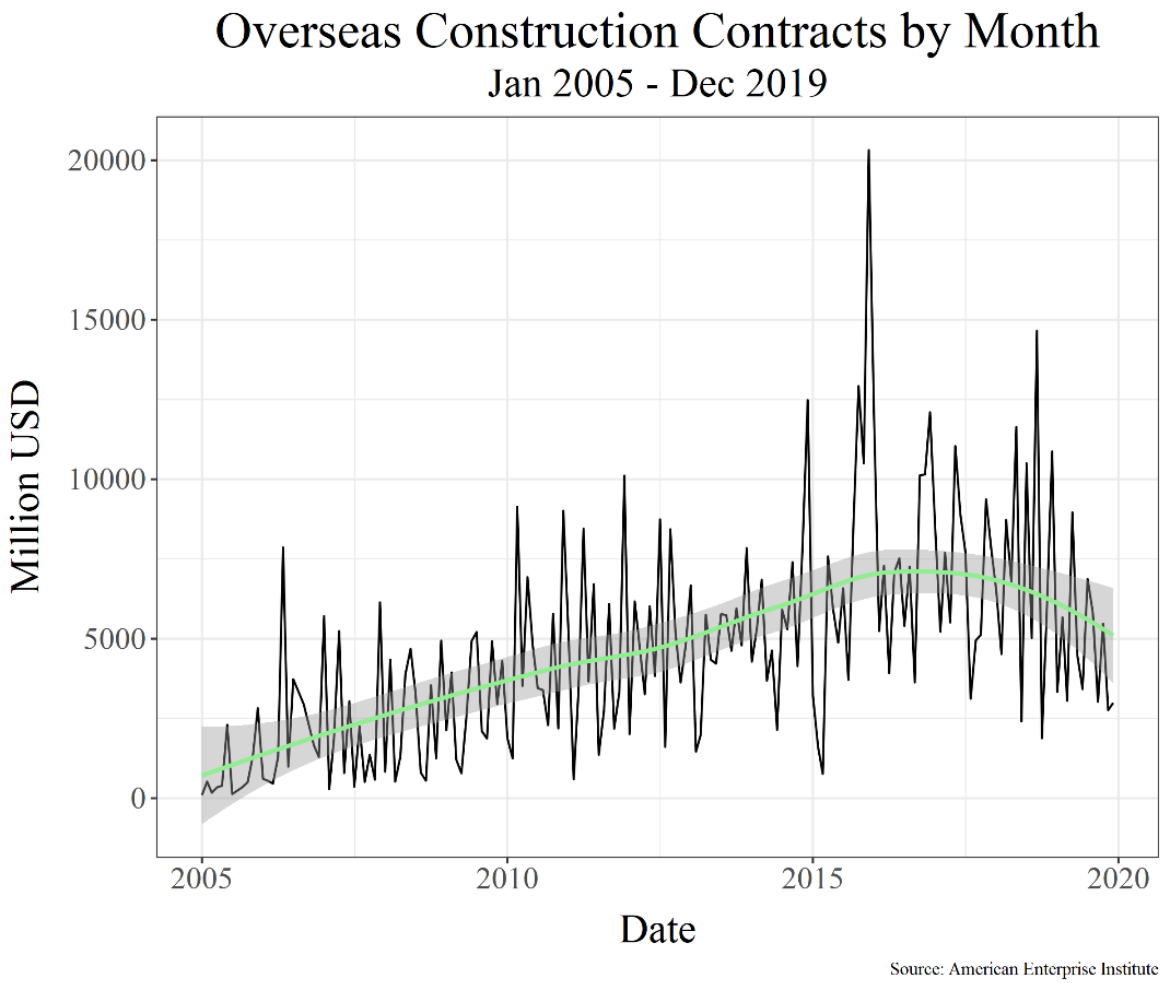



*Figure 4-19*

For both FDI and construction contracts, the increase begins in the late 2000s, accelerates in the mid-2010s and then declines after peaking sometime around 2016 and 2017. By the beginning of 2020 (preceding the outbreak of the coronavirus pandemic), the trend had fallen back to where it sat before the acceleration in the middle of the decades. It is this bubbling of FDI flow and construction contracts that accounts for the particularly rapid rise of Chinese FDI stock recorded in the UNCTAD database throughout the 2010s followed by stagnation or even slight decline in the Chinese share of overall FDI stock after 2018 or so. This bubble in investment is at least partially related to the last commodity supercycle driven by development in the BRICs, with

high prices of primary commodities leading to a boom in Chinese investments in the mining, oil and natural gas sectors globally, even as Chinese urbanization was, at least in part, driving the supercycle. In part, it's also likely that the CGIT data overstates the decline somewhat, since the MOFCOM data shows a rising influence of smaller firms in the latest years (Scissors 2020a).

But the investment bubble was also driven by the credit and real estate bubble and was associated with increases in global asset purchases on the part of Chinese capitalists as well as high-profile mergers and acquisitions within high-income countries. Overall, Chinese-origin FDI stock in developed economies accounted for 11.4 percent of the total and high-income countries such as the US, Australia, Canada and several EU member nations all held the highest rankings as destinations for outbound FDI (after Hong Kong, of course, and alongside numerous tax havens). Taken together, developing countries host 87.3 percent of Chinese-origin FDI stock, but it is not nearly as concentrated in any individual nations as it is among developed countries. Indonesia, at 9$^{th}$ place (with 15.13 billion USD or .7% of overall Chinese-origin FDI stock), was the highest-ranking developing country in 2019, followed by Laos, at 17$^{th}$ place (8.25 billion USD or .4% of overall FDI stock), Malaysia, at 18$^{th}$ place (7.92 billion USD or .4% of overall FDI stock), and Kazakhstan, at 20$^{th}$ place (7.24 billion USD or .3% of overall FDI stock). The vast majority of developing countries saw far slimmer shares, contrasting sharply with the full 3.5 percent share of FDI stock located in the United States or even the 1.7 percent located in Australia (MOFCOM 2020, pp.113-114, Figure 16, Table 13).

A large portion of Chinese outbound FDI is not industrial investment in any sense of the word. Instead, the sectoral breakdown is dominated by Leasing and Business Services, which includes "foreign investment activities focusing on investment holding" and is concentrated in the high-income countries, Hong Kong, Singapore and various offshore tax havens (ibid, p.115).

Altogether, this sector accounted for 734.08 billion USD or 33.4 percent of China's total outward FDI stock of 2,198.88 billion USD in 2019. It was followed by the wholesale and retail industry (295.54 billion USD or 13.5%), the financial sector (254.53 billion USD or 11.6%), the information technology services sector (202.21 billion or 9.2%). Alongside the lower shares held by Real Estate (77.61 billion USD or 3.5%), Scientific Research and Technical Services (46.01 billion USD or 2.1%) and a few other minor sectors, the high concentration of FDI devoted to producer services and portfolio investment resulted in 79.6 percent of China's total outward FDI stock lying in the tertiary sector in 2019—though the 76.53 billion USD (3.5% of total) in transportation, storage and postal services is also counted as tertiary, despite including many logistics firms that might otherwise be conceptualized as industrial (ibid pp.116-117, 120, Figure 18, Figure 20).

Secondary industry, by contrast composed only 19.9 percent of the total outward FDI stock, while primary industry composed a mere .5 percent, largely because Chinese statistics place mining in the secondary sector, leaving only the main productive activities in agriculture, forestry, animal husbandry and fisheries to make up primary industry. Outward FDI stock in secondary industry was dominated by Manufacturing in 2019, which made up 200.14 billion USD or 9.1 percent of the total. This was followed by mining, at 175.4 billion USD or 8 percent of the total and construction, at 42.23 billion USD or 1.9 percent of the total (ibid). However, these proportions have changed in important ways over the course of the 2010s. Over the course of the decade, the share of overall FDI stock in secondary industry has been declining slightly against that devoted to tertiary industry. In 2011, secondary industry made up 24.02 percent of the total, and in 2015, 22.59 percent. The exact proportions of secondary industry's component sectors have also shifted. In 2011, Mining composed the largest share of total outbound FDI

stock at 15.77 percent, followed by Manufacturing with 6.53 percent and Construction at 1.9 percent. In 2015, Mining's overall share had fallen to 12.97 percent, Manufacturing's had risen to 7.15 percent and Construction's to 2.47 percent. Manufacturing finally overtook Mining in 2018, while Construction's share began to fall again (ibid, p.159, Annex Table 4).

But these are numbers showing only overall investment. The proportions change substantially when looking at the number of overseas enterprises—which are firms set up by Chinese domestic investors in other countries. These amount to a total of 43,994 enterprises in a total of 188 countries. Of these overseas enterprises, 13,659 are in the secondary sector, composing 31 percent of the total, with the vast majority, 8,630, in Manufacturing, which composed the second largest single category at 19.7 percent of the total, followed by Construction, which had 3,628 (8.3% of the total) enterprises and Mining, which had 1401 (3.2%) in Mining. That compares to 377,815 total industrial enterprises above the designated size (which excludes construction) and 103,805 construction enterprises listed by the NBS in the same year. The single largest category of overseas enterprises was in Wholesale and Retail Trade, with 11,993, or 27.3 percent of the total (ibid, p.144, Table 27). Most of these (86.4%) were linked to local firms in China, while a minority (13.6%) were linked to centrally-administered enterprises (ibid, p.145). Overall, it seems that even while China's FDI flows have declined somewhat and its overall share of FDI stock has begun to stagnate, the relative weight of manufacturing in its foreign investment position has risen to prominence in the most recent years.

Within China, nonfinancial FDI originates in essentially all provinces and is owned by both private and state-owned enterprises. Of investment not linked to centrally-administered enterprises, Guangdong is the largest single source of FDI stock, at 178.38 billion USD or 27.67

percent of the total local outward FDI stock, of which Shenzhen alone contributed 119.26 billion USD or 66.9 percent of Guangdong's overall contribution. Shanghai exceeded Shenzhen's figure, at 130.33 billion USD, or 20.22 percent of the total. It was followed by Beijing (73.69 billion USD, 11.4%), Zhejiang (65.9 billion USD, 10.2%), Shandong (62.4 billion USD, 9.68%) and Jiangsu (54.50, 8.46%), more or less mirroring the general dominance of the East Coast and Guangdong seen in China's own industrial geography (ibid, pp.122-123, Table 15). Notably, these are also the areas that saw declining returns on assets and (with the exception of Guangdong) declining numbers of total enterprises in the latter 2010s. In terms of firm registration, the relative weight of state-owned enterprises has declined in all but one two years, falling from 81 percent of all outward FDI stock in 2006 to 50.1 percent in 2019 (up slightly from an all-time low of 48 percent in 2018). This matches trends in the sectoral composition of FDI, since state-owned enterprises have tended to be more concentrated in natural resource sectors (Luo et al 2017) and the shares devoted to mining and primary industry have fallen over the years (MOFCOM 2020, p.159. Unlike the NBS data, MOFCOM does not exclude firms below the designated size and it does record self-employment (个体户). The majority of non-state enterprises were LLCs (15.3% of the total), followed by Joint-Stock Companies (9.1%), Private Enterprises (7.6%) and Self-Employed (6.9%), with this final figure hinting at the rising importance of small firms.

In terms of geographical distribution, the dominance of Hong Kong, the Cayman Islands and the British Virgin Islands distort the overall picture substantially, especially in Asia and Latin America. If we subtract the outward FDI stock held in these locations from the total, the 2019 figure falls to 505,501.61 million USD, or twenty-three percent of the total. It is this smaller figure that will be used to calculate the relative proportions of each region in order to get

more adequate shares. Other potential tax havens, such as Luxembourg, the Netherlands, Bermuda and Singapore remain in the dataset since their relative influence is significantly smaller. The data here is drawn from MOFCOM's 2019 report, Annex Table 2:

**China's Outward FDI Stock by Region, 2019**

| REGION | FDI STOCK (Mil. USD) | SHARE OF TOTAL |
|---|---|---|
| Asia (-HK) | $184,866.38 | 36.57% |
| Africa | $44,390.22 | 8.78% |
| Europe | $114,383.86 | 22.63% |
| Latin America (-Tax Havens) | $18,023 | 3.56% |
| North America | $100,225.53 | 19.82% |
| Oceania | $43612.55 | 8.62% |

*Table 4-8*

Even excluding Hong Kong, Asia makes up a substantial portion of overall Chinese FDI stock in 2019, followed by Europe and North America. In Europe, aside from tax havens such as the Netherlands, Luxembourg and Switzerland, investment is highest in the UK (3.4% of total outward FDI stock), Germany (2.8%) and Russia (2.5%), where the figure has been declining since 2015. In North America, a small portion of FDI stock is held in Bermuda (1.6% of the total) and a slightly larger portion in Canada (2.8%), but the bulk is in the United Sates (15.39%). In Latin America, Brazil (.88%) and Venezuela (.67%) are the major recipients. In Oceania, the vast majority of FDI stock is held in Australia (7.5%).

Africa will be explored in more detail below, but since Asia is the main site of investment, it will help to break down the region's top recipients in some more detail here. Hong Kong, Macao, the UAE and Singapore are excluded from the list below, though Singapore

accounts for the single largest location for outbound FDI after Hong Kong in Asia (52,636.56 million USD, 10.41% of the total). The total used to calculate the shares is, as above, the total outward FDI stock in 2019 minus Hong Kong, the Cayman Islands and the British Virgin Islands:

**China's Outward FDI Stock in Asia, 2019**

| COUNTRY | SUBREGION | FDI STOCK (Mil. USD) | SHARE OF TOTAL | GDP Ratio (FDI / GDP) |
|---|---|---|---|---|
| Indonesia | SE Asia | $15,132.55 | 2.99% | 1.35% |
| Laos | SE Asia | $8,249.59 | 1.63% | 45.39% |
| Malaysia | SE Asia | $7,923.69 | 1.57% | 2.17% |
| Kazakhstan | Central Asia | $7,254.13 | 1.44% | 3.99% |
| Thailand | SE Asia | $7,185.85 | 1.42% | 1.32% |
| Vietnam | SE Asia | $7,073.71 | 1.4% | 2.7% |
| South Korea | East Asia | $6,673.40 | 1.32% | .4% |
| Cambodia | SE Asia | $6,463.70 | 1.28% | 23.86% |
| Pakistan | South Asia | $4,797.98 | .95% | 1.72% |
| Myanmar | SE Asia | $4,134.45 | .82% | 5.43% |

*Table 4-9*

Overall, nations across Southeast Asia are host to some of the largest shares of Chinese outward FDI stock, with Indonesia leading others by a substantial margin. However, considering population size, the per capita weight of investment in Laos is substantially higher, since the country's population is estimated by the World Bank to be only barely more than seven million, compared to two hundred seventy million in Indonesia. The same disproportion can be seen

when Chinese-origin FDI stock is divided by each country's GDP (as recorded by the World Bank). In Laos, Chinese-origin FDI stock is an astounding 45.39 percent of GDP. In Cambodia, it is also strikingly high, at 23.86 percent. For comparison: during China's own export-led development, FDI peaked at six percent of GDP in the early nineties and declined thereafter (Naughton 2018, p.425, Figure 17.1).

Overall, it is clear that much of the Chinese outbound FDI within developing countries has been concentrated in Southeast Asia and, to a lesser extent, Kazakhstan and Pakistan, both of which share a border with China in the West. Part of the boom in state-led development in Northwest China visible in the growing number of total enterprises and rising shares of electricity consumption (despite falling rates of return) is clearly related to the build-up of FDI stock in these two neighboring countries. Both see major oil and gas pipelines as well as road and rail networks running through Xinjiang, where the upward trends in enterprises and electricity consumption have been the strongest. These have also been major focal points for large infrastructural development, with Kazakhstan host to major "new silk road" projects (Bennett 2020) and Pakistan host to a range of developmental projects grouped together as the construction of a "China-Pakistan Economic Corridor" (Garlick 2018). But investment has not boomed across Central or South Asia. In Kyrgyzstan, Tajikistan, Nepal, Bhutan and India, Chinese FDI stock remains comparatively low, well under that of Mongolia, a neighboring country that has played only a small part in the media branding of the "belt and road initiative" (BRI), defined by widely-reported diplomatic signing events for various memorandums of cooperation and/or announcements of prospective investment that often fail to pan out (see below). Overall, when investments recorded in both the MOFCOM and CGIT database are compared to whether or not individual nations have signed a BRI memorandum, there is

absolutely no correlation between FDI (measured as flows or stock) and "membership" in the BRI—in other words, countries that have signed such a memorandum have not, on average, received more investment than countries that have not (Scissors 2020a).

The relative prominence of Southeast Asia as a site for outbound FDI stock is a case in point. While many nations are technically part of the "maritime silk road," it seems that the development of new territorial industrial complexes in places like Vietnam simply has far more to do with some combination of more traditional concerns of industrial location and the relative ease of doing business in a region with embedded cultural, linguistic and family capital networks, all of which have influenced Chinese developmental trends since the earliest years of the reform and opening (Folk and Jomo 2003). It therefore makes perfect sense that, despite prominent advertising to the contrary, the bulk of Chinese outbound investment in developing countries seems to be concentrated mostly in countries that lie in close proximity to China itself, though seemingly influenced by factors such as relative labor costs and size of the available working population as well as by pre-existing capital and cultural integration, all of which have tended to pull the concentration of Chinese outward FDI definitively into Southeast Asia above all other regions, on average. Certain other border countries, such as Kazakhstan and Pakistan, have also clearly benefited from this proximity effect, but it remains unclear precisely how much of the infrastructural networks developed in these locations actually prefigure potential industrialization in the future. At the moment, they seem mostly geared toward the transit of freight goods and/or the development of oil and gas pipelines (Alff 2016, Bennett 2020), with much of their affiliated economic activity happening in relatively small hubs for warehousing and wholesale trade at the border or at the final destinations, in new port sites or coastal facilities for natural gas extraction.

Even Laos seems to follow this pattern, with its small population less important as a potential source of labor than its geographic position is as a site for overland links to mainland Southeast Asia and a potential site for upstream hydropower development (Kyophilavong et al 2017). A useful comparison is with the Philippines, which has a large potential labor force and low wages but does not sit on a similar geographical crux and sees the lowest Chinese FDI stock of any nation in Southeast Asia aside from Timor-Leste and Brunei. Meanwhile, much of the early development across Southeast Asia promoted infrastructure development, but often in connection with pre-existing production hubs, as in Malaysia (Zhang et al 2020), rather than as the speculative development of entirely new deep sea ports, as at Gwadar in Pakistan, Lamu in Kenya and (the now definitively cancelled) Bagamoyo in Tanzania. This is represented by the fact that Manufacturing composes some 24.2 percent of all Chinese outward FDI to ASEAN nations in 2019, trailed by infrastructural investment as represented in the 8.6 percent share devoted to Production and Supply of Electricity/Heat/Gas and Water and the 7.2 share in construction. In stark contrast with many countries in Latin America and Africa (see below), Mining accounted for only seven percent of Chinese outward FDI in the same year (MOFCOM 2020, p.129, Table 18). This is the key difference: in Southeast Asia, Chinese FDI is being drawn to several pre-existing industrial-territorial complexes, all of which have developed rapidly but very few of which began at the same low level of development as exists in Dar es Salaam, surveyed below, or other sites where investment is forced to take on far more upfront greenfield costs, as in Central Asia and Latin America. Similarly, FDI flows from China across Southeast Asia have been facilitated by the pre-existing financial hubs in the region, the most important of which is Singapore, which alone saw a Chinese FDI stock of 52,636.56 or 10.41 percent of the total (minus HK and tax havens as calculated above), much of this financing then

being funneled into other Southeast Asian nations (MOFCOM 2020, Annex Table 2). This also means that Chinese investment in the region is, in fact, understated.

Unfortunately, there is no space here to truly delve into the details of Chinese investment in Southeast Asia or to examine the new industrial-territorial complexes (strongly linked to China) that have seen rapid development in areas such as North Vietnam. Such a project is worthy of its own study and a few surveys of Chinese involvement in the entire region have seen publication in recent years (Yeh 2019, Strangio 2020, Hiebert 2020, Shambaugh 2020). Similarly, other developing regions remain underexamined here, including much of South Asia, Central Asia and Latin America. In part, this is simply due to constraints of time and space. But it is also because these areas have seen far less Chinese investment on average, have not been as prominent in media portrayals of Chinese economic influence and, especially in Latin America, have tended to see that investment more closely linked to trends in the commodity supercycle, concentrated in mining (as in Chile and Peru), oil (Venezuela) or agricultural goods (as in Brazil). These trends have also been thoroughly documented on their own in several book-length studies (the most rigorous being: Myers and Wise 2017 and Peters 2019). Only Brazil, Venezuela, Uzbekistan, Mongolia and India host total China-origin FDI stocks comparable to those of the *lowest* rung of major Southeast Asian nations (except for the Philippines).

**Chinese Outward FDI Trends in Africa**

By contrast, Africa seems to be a unique case, insofar as reporting on Chinese investment across the continent (but particularly in Sub-Saharan Africa) has occupied a central position in the general media image of Chinese investment overseas (as documented by Brautigam 2015) even

while the total FDI stock remains low in absolute terms. As a whole, Africa is host to significantly more investment than Latin America (excluding tax havens, see Table 4-8 above). But nation by nation, in 2019 only South Africa (6,500.84 million USD, 1.29% of the total calculated as above) and the Democratic Republic of the Congo (5,596.6 million USD, 1.11%) host more Chinese-origin FDI stock than countries like Brazil and Venezuela. Even oil-rich Angola, more or less dependent on trade with China, only has a Chinese FDI stock of 2,890.73 million USD in 2019, which is a mere .57% of China's total outbound FDI, minus Hong Kong and tax havens, and less than any major Southeast Asian nation aside from the Philippines (MOFCOM 2020, Annex Table 2).

Yet even major research institutions (Sun et al 2017), academic accounts (Sun 2017) and official policy recommendations emphasizing "south-south cooperation" (Lin and Wang 2017) have tended to emphasize the potential of the "African lions" taking on the mantle of the East Asian Tiger economies of yesteryear, fueled not only by Chinese infrastructural investment but also direct investment in manufacturing. At its most extreme, this amounts to the claim that Chinese investment is remaking Africa into the next potential "Factory of the World (Sun 2017). At least part of this can be attributed to increasing competition between the US and China which, in recent years, has taken on the character of a "New Cold War" discourse. In this context, the key fact documented by CARI (but entirely ignored in the CGIT database) is that traditional sources of capital from Europe and the US remain important across Africa but the overall FDI flows originating from the US in particular have fallen precipitously over the past decade. This is

particularly stark in the most recent years, where every year from 2015 shows negative US-origin FDI flows, except for a very slight recovery just above zero in 2017.[58]

In absolute terms, aside from South Africa, the DRC and Angola, Chinese FDI stock in Sub-Saharan Africa in 2019 was largest in Zambia (2,863.79 million USD), Ethiopia (2,558.87 million USD) and Nigeria (2,194 million USD), followed by Ghana (1,831.29 million USD), Kenya (1,624.23 million USD), Tanzania (1,335.54 million USD), Sudan (1,203.09 million USD) and Mozambique (1,146.75 million USD), according to MOFCOM (2019, Annex Table 2). The small island nation of Mauritius, with an established Chinese population dating back to the colonial era, shows a particularly large outside FDI stock (1,2981.68 million USD) given its size, as it has sought to position itself as a sort of Singapore for Sub-Saharan Africa. But this is also the region where MOFCOM and CGIT data both are most unreliable. The next chapter focuses on this question in much more detail and attempts a more systematic "ground-truthing" (Bennett 2020) of available econometric datasets via a field survey of most major industrial zones in Dar es Salaam, Tanzania. Prior to this, however, it will at least be useful to overview the existing data for Africa, derived from MOFCOM but cleaned and compiled by CARI.[59]

Even though my focus here is on investment in the productive sector, the media portrayal of Chinese investment in Africa in recent years has placed enormous emphasis on loans and the specter of "debt trap diplomacy." This myth has since been systematically debunked by Deborah Brautigam and her team at CARI (as documented in Brautigam 2020), in part through the

[58] This is visible in CARI's Overview of its FDI dataset, located here: CARI (2020) "Data: Chinese Investment in Africa", *China-Africa Research Initiative, School of Advanced International Studies*, Johns Hopkins. < http://www.sais-cari.org/chinese-investment-in-africa>

[59] Unfortunately, the CARI data used below was obtained and processed just prior to their release of the numbers for 2019. Time constraints prevent it being systematically updated here, though I will periodically compare it to the MOFCOM numbers. This does not change anything substantial about the trends, however, since 2018 was the peak of investment in most African countries to date.

database on loans compiled by her research team. Using this database, it is clear that dependence on Chinese loans among African nations peaked as a share of all outside debt (as measured by the World Bank) as early as 2012 at 4.99 percent and has fallen precipitously since, to a mere .92 percent in 2017:

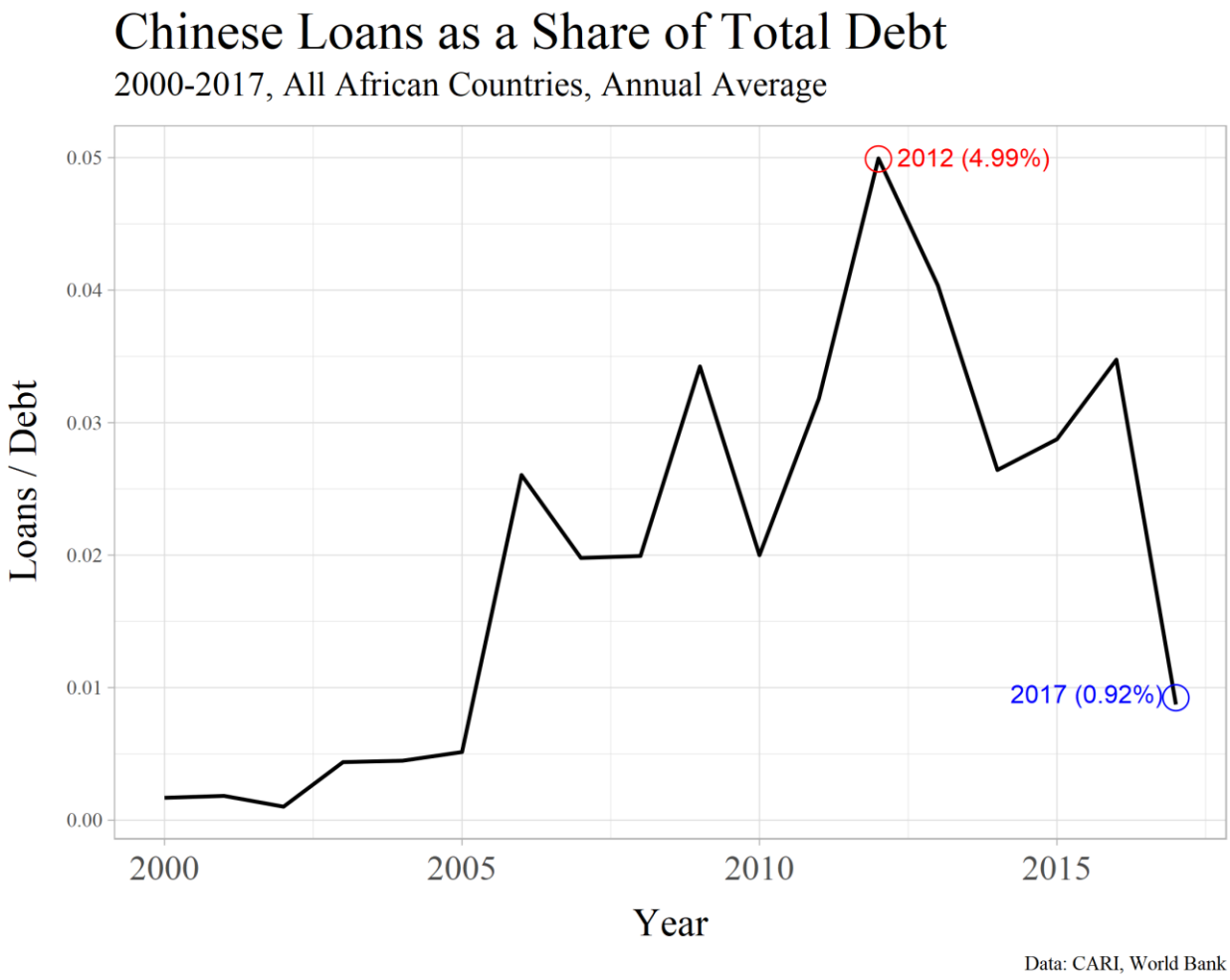



*Figure 4-20*

Clearly, African nations are, on average, far more dependent on loans sourced from elsewhere. More importantly, most Chinese loans have been concentrated in Angola, where they've been linked to developing basic infrastructure for the oil industry, as is shown when calculated cumulatively for the entire 2000-2017 period:

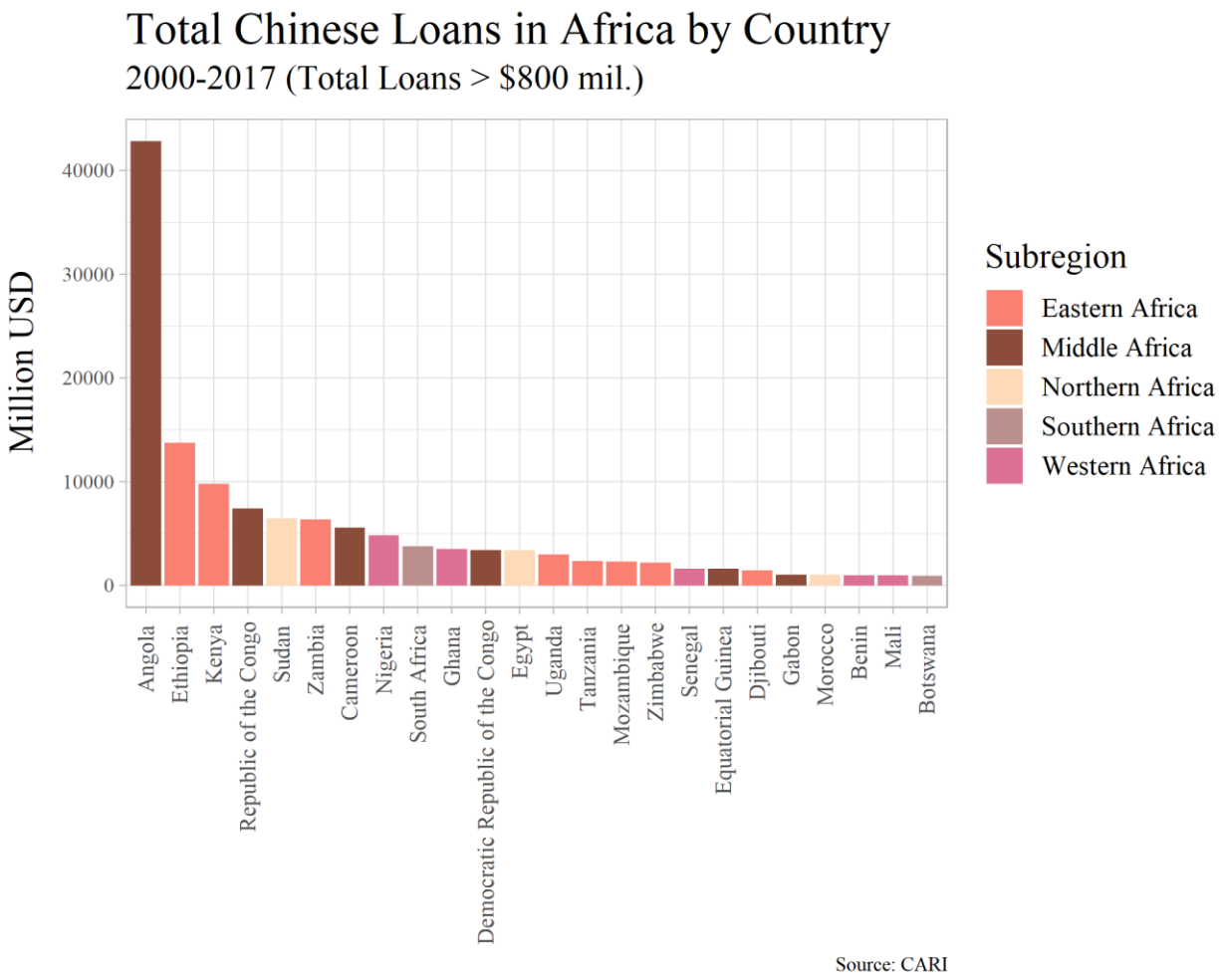



*Figure 4-21*

The vast majority of loans have been affiliated with basic infrastructural development of transport and utilities network, alongside resource extraction:

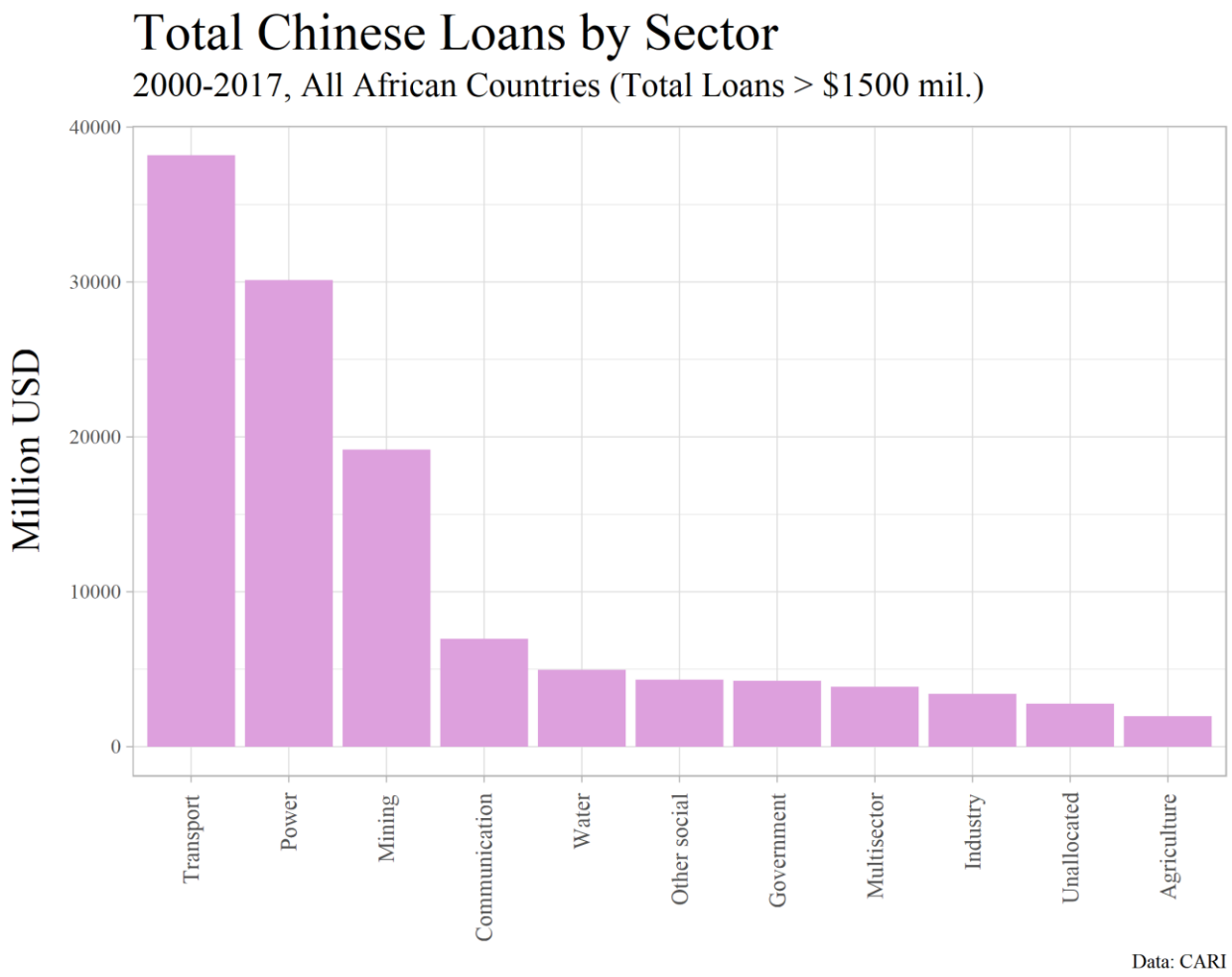



*Figure 4-22*

Very little of this loan money has been funneled directly into industry and almost none has funded the supposed "land grabs" that were previous focus on China-scare journalism (and again systematically debunked by Brautigam 2015). But loans do compose a larger share of total debt (though still an extremely small share overall) in Ethiopia, which has been a major site of Chinese manufacturing investment (Attenburg et al 2020), preceded and accompanied by substantial funding for infrastructure projects:

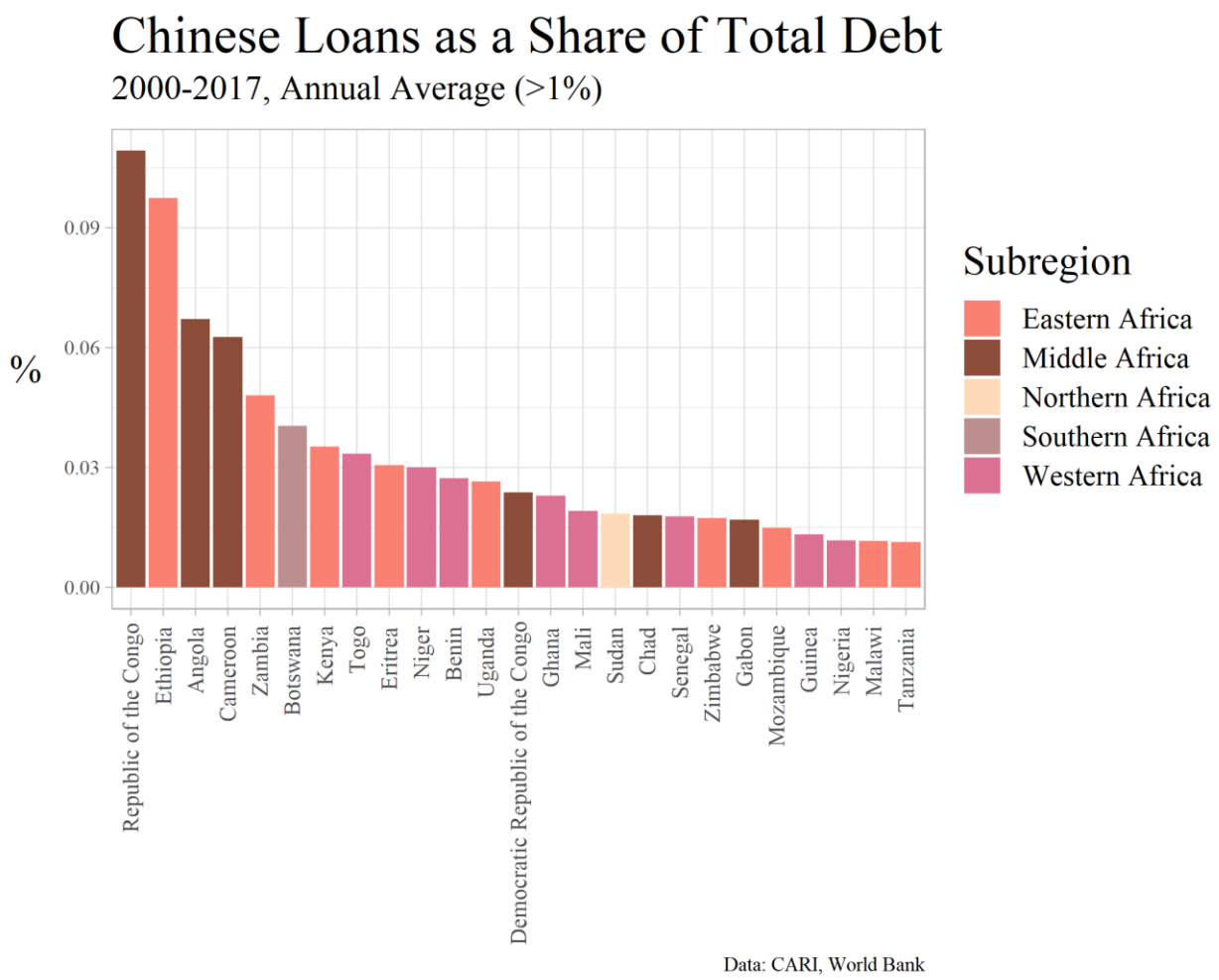



*Figure 4-23*

These infrastructural projects are better visualized through construction contracts secured by Chinese firms. Similar to the CGIT, CARI also records data on these contracts, but tends to use a more robust method and cross-references more actively with local sources. When cumulative gross revenue from 2000 to 2018 is calculated by country, the relative importance of countries that see most investment in natural resource sectors (i.e. Angola, Algeria, Zambia) is scaled more clearly alongside countries that have seen more investment in manufacturing and logistics (i.e. Ethiopia, Nigeria, Kenya, Tanzania), as documented in CARI working papers and the scholarly literature more generally:

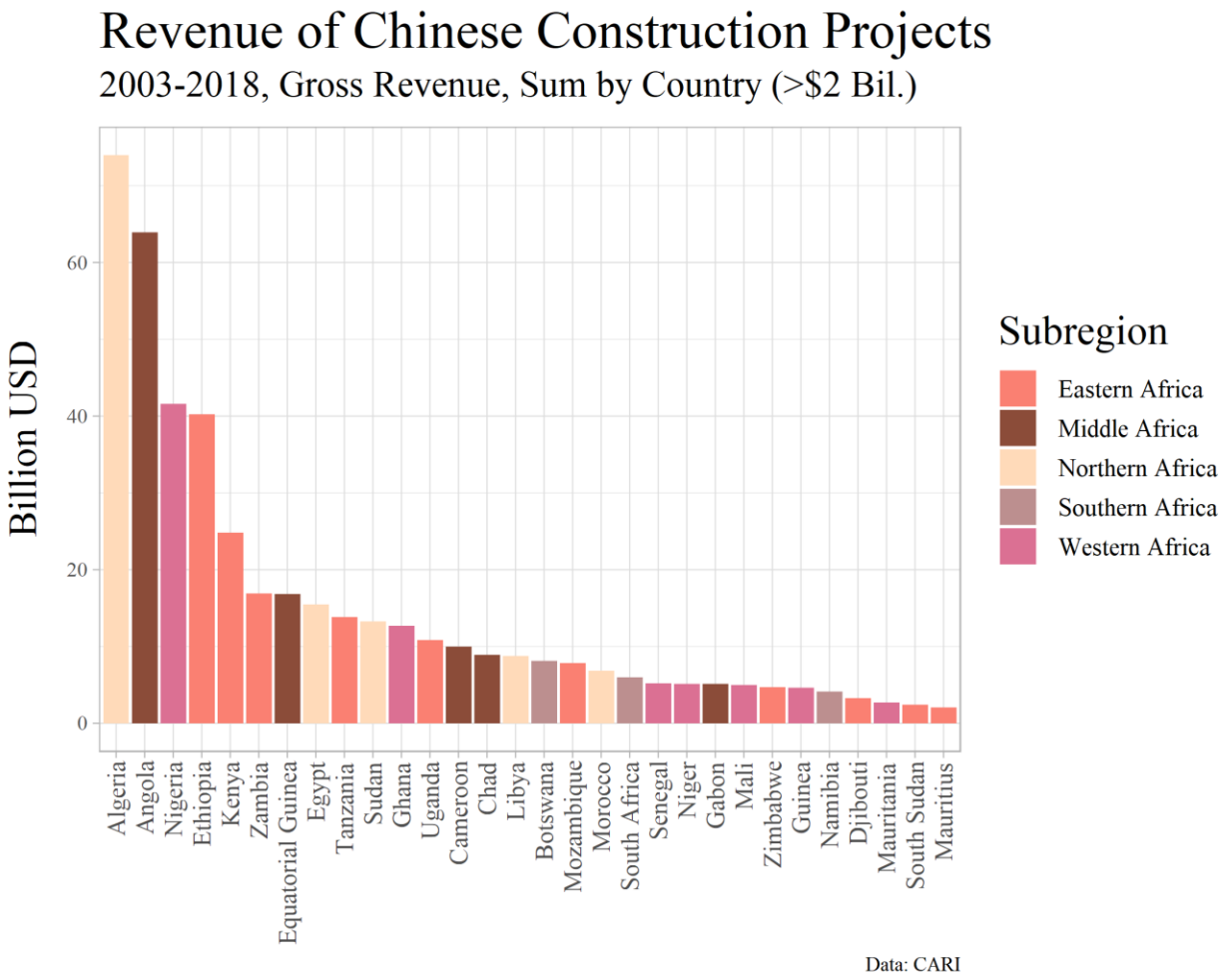



*Figure 4-24*

Aggregated by region, the relative importance of East Africa in Chinese-funded construction becomes even more prominent:

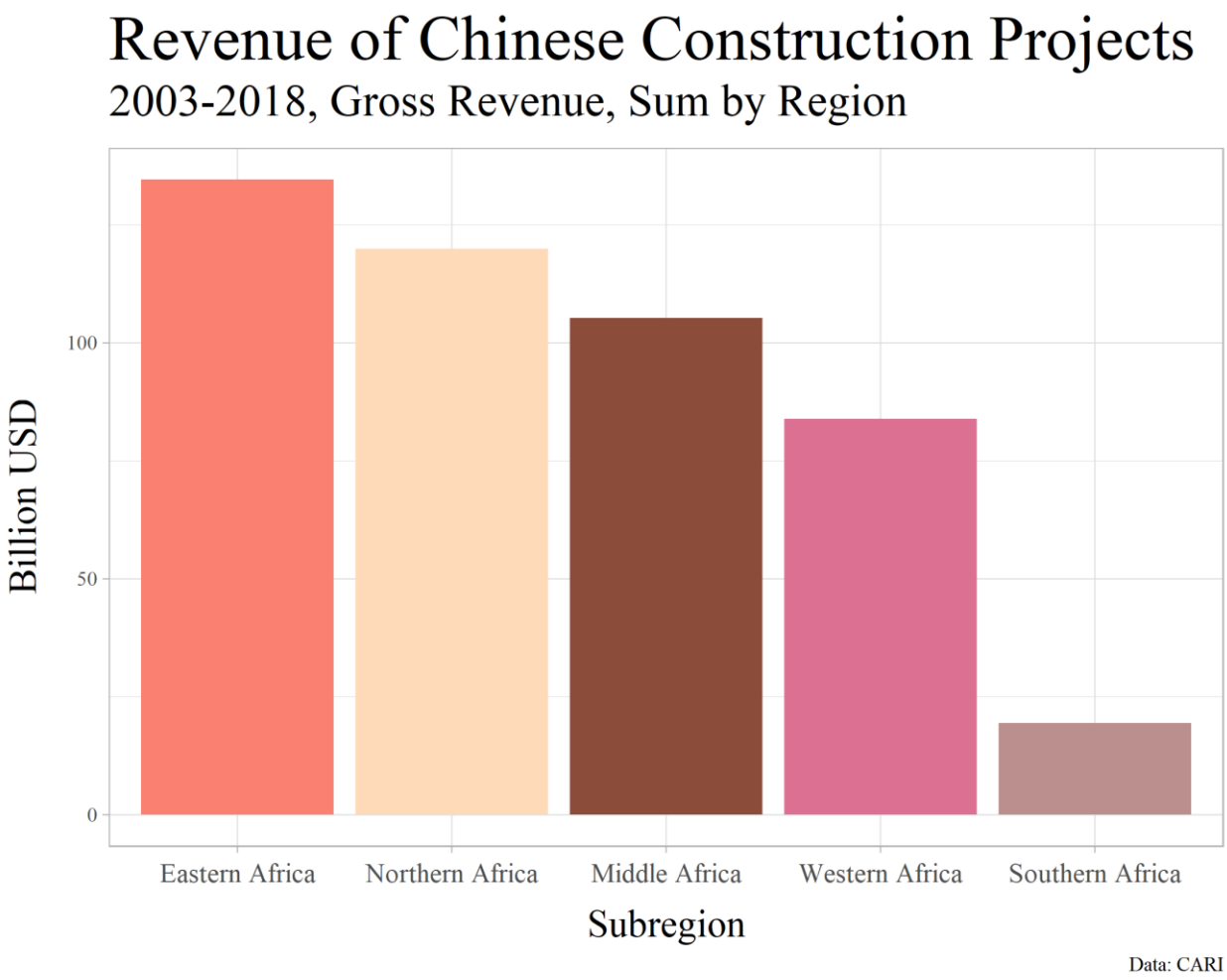



*Figure 4-25*

Similar trends are visible for Chinese-origin FDI stock across the continent. These can be visualized as raw numbers, but the relative weights are better captured when looking at the share of all Chinese-origin FDI stock in Africa held in each subregion in 2018:

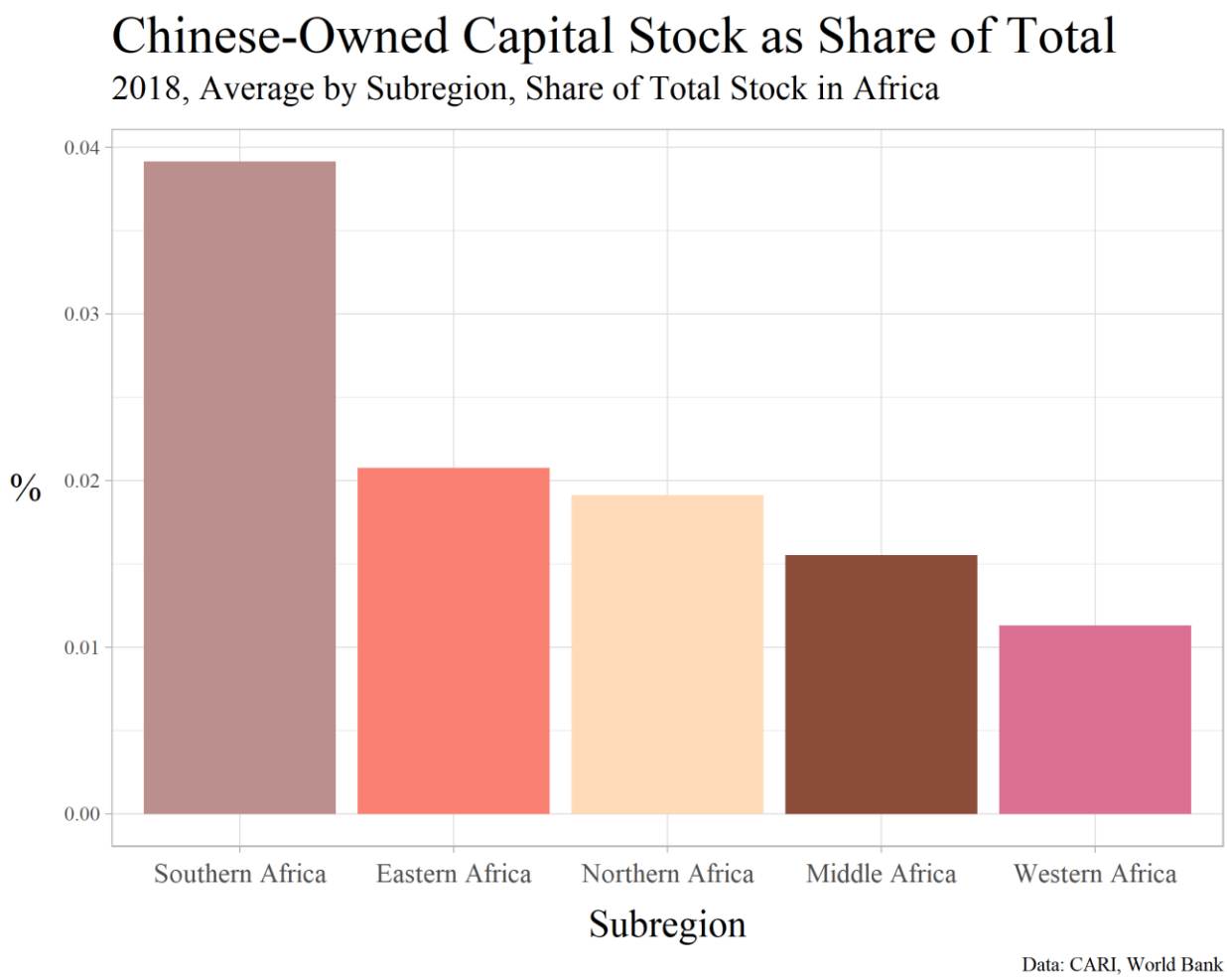



*Figure 4-26*

The same data visualized by country demonstrates the outsize influence of South Africa in the entire Southern subregion. If it were removed, the highest shares of regional FDI stock are concentrated in Zambia, Ethiopia, Nigeria, Angola and Algeria, but the next highest values see an outsize presence of East African nations (alongside Ghana):

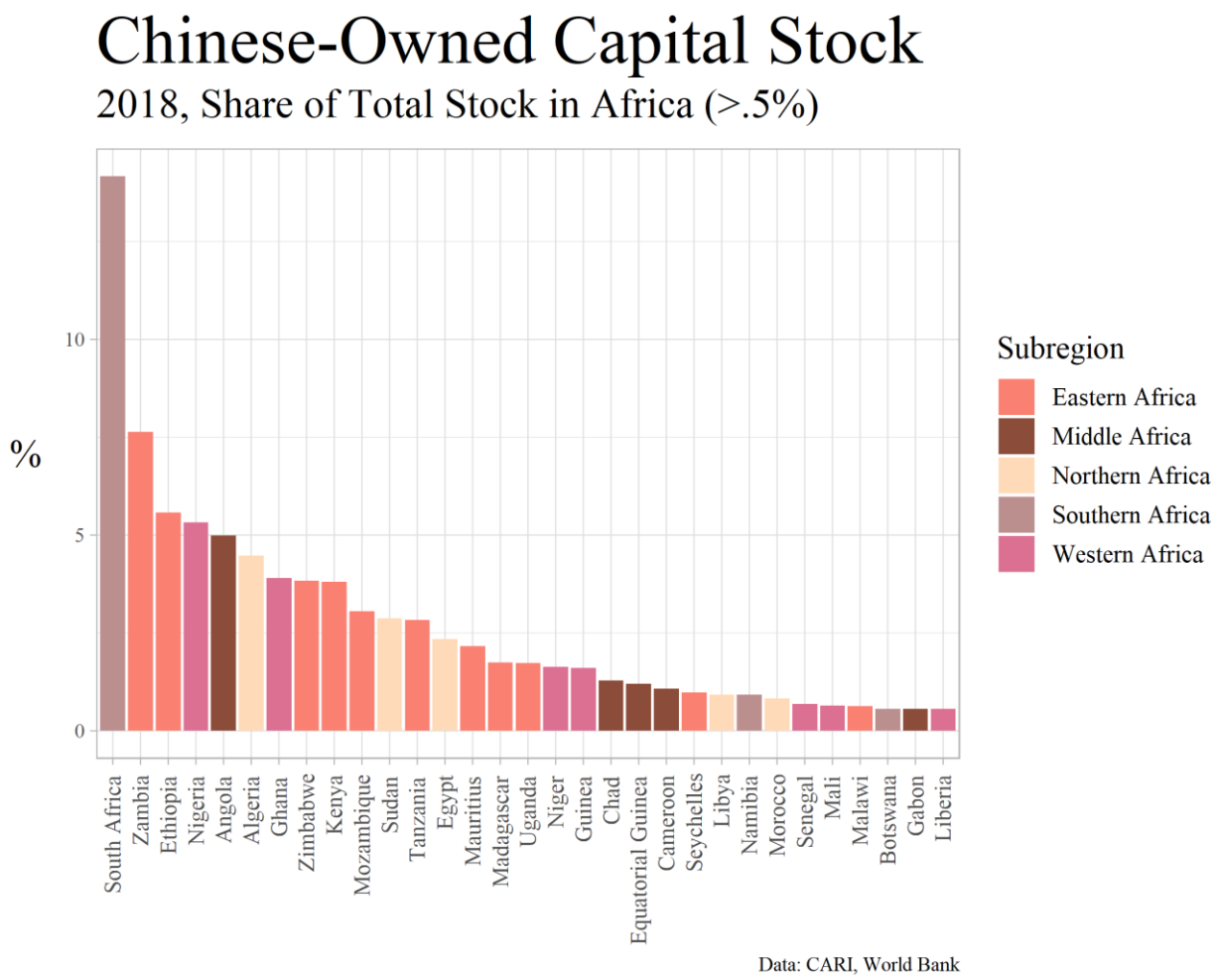



*Figure 4-27*

As above, its apparent that East Africa as a whole plays a particularly important role with regard to Chinese FDI, even if several other sites are important on the country-by-country level, such as resource centers located in other subregions like Angola and Algeria, or more mixed investment sites such as Nigeria and Ghana in West Africa. Another important caveat to note here is that CARI excludes data for the DRC, considering it unreliable. MOFCOM, however, shows it as the second most important site for FDI after South Africa. Overall, it seems that the MOFCOM data reflects the reality of active Chinese involvement in the DRC's natural resources sectors, even if the precise numbers are unreliable. If it were included, the importance of Middle Africa would

likely ascend to the lead here, but it would be driven entirely by resource extraction in Angola and the DRC.

Though explored more extensively in the next chapter, it is important to note that the East African subregion is itself subdivided between countries where more FDI has flowed into manufacturing (as documented in Sun et al 2017), which tend to be coastal or near-coastal countries in the region's north, and countries where more FDI has flowed into mining or other natural resource sectors, which tend to be in the region's south. This is especially important insofar as it cautions against treating studies of large Chinese mining conglomerates as representations of "Chinese investment in Africa" as such. This is the method used by Lee (2017) in Zambia, which overgeneralizes the insights of a narrow (though deep) field ethnography of a few mining companies into both sweeping claims about the nature of "state capital" versus private and unrepresentative claims about the nature of Chinese investment across the entire continent. At the other end of the TAZARA rail line connecting the Zambian copper belt to the Swahili Coast, however, my own field survey confirms earlier surveys by Sun et al (2017) and Ying (2019), which find substantially different trends than those observed by Lee in the copper belt. This should caution any scholar against posing the results of their own limited fieldwork in a single African nation as representative of the continent as a whole, especially when conducted in a sector that has been declining in importance as a share of overall FDI stock for years.

Finally, the same trends can be compared for all countries (minus South Africa, to retain the scale) in all subregions and these African nations can at least be loosely compared to trends through 2012 in Southeast Asia (derived from UNCTAD's bilateral series) and trends through 2018 in Thailand, derived from Thailand's national statistics bureau. The increase throughout the

time period is clear, but it's also evident that the end of the series for African nations host to the largest quantities of Chinese-owned FDI stock lies more or less at the level of most Southeast Asian nations almost a decade ago:

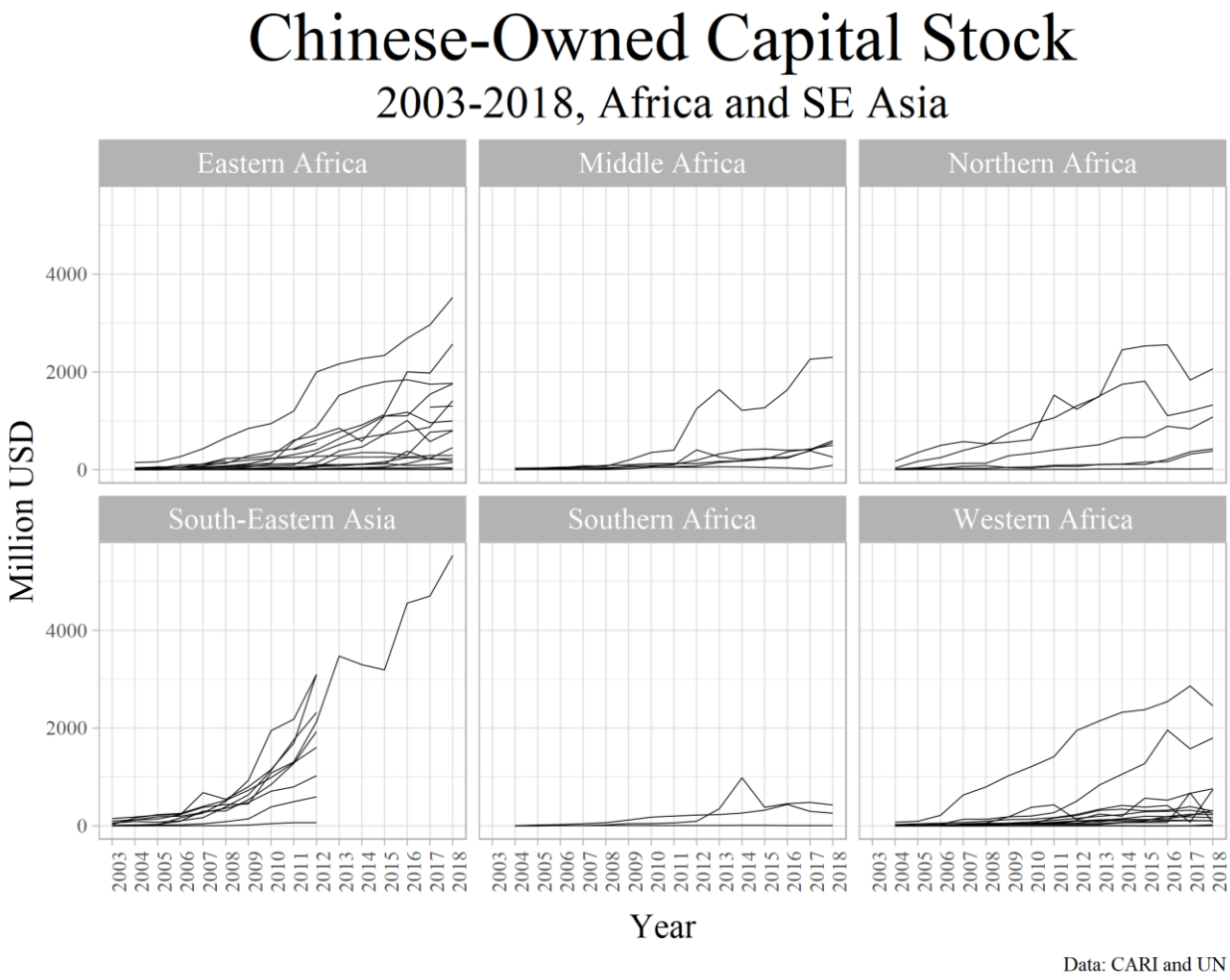



*Figure 4-28*

Moreover, referring to Table 4-9 above, it's clear that today basically every major country in Southeast Asia except for the Philippines is host to substantially more Chinese-owned FDI stock than any country in Africa (except for, in a few instances, South Africa, which has been excluded

here).[60] Unfortunately, overall Chinese FDI stock held in Africa is such a small share of the total that MOFCOM provides no sectoral breakdown. A more detailed breakdown of Chinese FDI into East Africa specifically will be offered in the next chapter, summarizing the results of existing surveys and ethnographies in the region.

**Global Demographics**

Although Africa currently composes a small share of overall Chinese outward FDI stock, the theoretical considerations presented in earlier chapters hint at the continent's potential importance for the future of capitalist development. This is almost entirely due to demographic factors, with Africa and, to a lesser extent, South Asia, set to become the last regions of the globe that will see rapid population growth and urbanization. Even if capitalism tends, over time, to produce a surplus population (as the necessary units of labor input per unit of output decrease), absolute demand for labor increases nonetheless and the general social reproduction of the system requires, as a baseline condition, that dependence on the wage be universally maintained and continually extended. Thus, surplus population is generated alongside surplus capital and capitalist development continually requires access to large, cheap pools of potential laborers even as the number of workers employed per unit of manufactured output declines over time. These are the essential dimensions of the "capitalist law of population" as identified by Itoh, based on Marx's "general law of capitalist accumulation."

[60] It is possible to append the MOFCOM data onto this series for a more thorough comparison, but MOFCOM does not provide their data in an easy to access digital form. Instead, the numbers are only available in print and pdfs formatted such that they are resistant to text mining. While I did not have time here to fully digitize these records by hand, I will be doing so in the near future.

China was the last country where a sufficiently large workforce existed that was not yet incorporated into global capitalism through dependence on the wage. While, contra Harvey and Luxemburg, the incorporation of areas that lie "outside" capitalism in some sense is *not* necessary for the "expanded reproduction" of the economy (i.e., growth), nor is it the source of accumulation, the expansion of the system *driven by its internal dynamics of accumulation* (rather than exogenous non-capitalist inputs or "dispossession by accumulation") is accompanied by both geographic expansion and by an intensification of its own endogenous mechanisms for social reproduction. The first of these entails the "traditional" proletarianization of any remaining global peasant or semi-peasant population able to subsist (at least in large part) off local production, as observed by Lewis in his model of growing industrial employment, and as part of a more general process of dispossession mistaken by Luxemburg for the font of capitalist accumulation itself. This has mostly been completed today, in a process best described as a general global "depeasantization" (Araghi 1995). The second entails the reinforcing of the wage relation and the general policing of the private property system in order to maintain the baseline conditions for accumulation even as the stability of this process is threatened by an increasing in the surplus population, where proletarians have only a tenuous connection to the wage. As observed by Ruth Wilson Gilmore, the rise of ever-larger policing and carceral infrastructures is one important endogenous mechanism for social reproduction in such conditions, alongside rising rent-taking, dependence on unwaged reproductive labor and a general increase in the informal sector, self-employment or just generally "flexible" work, as documented by Smith and Benanav. These forms of social reproduction are mistaken by Harvey for forms of "accumulation by dispossession" necessary to expanded reproduction in the same fashion that the plunder of the non-capitalist world was for Luxemburg. Gilmore, by contrast, is more accurate in identifying

these as related to the production of surplus population, with carceral and policing activities geared toward baseline social reproduction rather than profitable dispossession for the sake of economic growth (as in Harvey).

Limits to either of these processes threaten continuing accumulation. China's absorption into the market has been the major engine of economic growth globally for the past thirty years and for most of these years Chinese labor costs have been, if not the absolute bottom, at least the average minimum global wage helping to determine (alongside mechanization) the baseline prices for global manufactured goods. As Chinese growth now begins to slow in the context of a general depeasantization, the result is that the remaining exogenous subsistence population will become even more important to the prospects of the system even as it disintegrates into an informalized, already-proletarianized population without steady access to the wage, concentrated in rapidly growing slum cities across the global south—but especially in South Asia and Sub-Saharan Africa, since this is where the bulk of population growth is predicted to occur over the next eighty years. The next chapter will explore more on-the-ground details of what we might call superfluous or *a priori* surplus proletarianization in relation to a rising industrial hub in one of the world's fastest growing cities. But the systemic importance of such a population is already evident in simple demographic data.

According to the UN's 2019 World Population Prospects report, Sub-Saharan Africa was projected to have a population of just over 1.09 billion people by 2020. That number was predicted to increase to 1.37 billion by 2030, 2.12 billion by 2050, 3.04 billion by 2075 and 3.77 billion by 2100. The bulk of this growth will be in the working-age population, which is projected to grow continuously throughout the entire projected period. The large share of children in the total population results in a higher starting dependency ratio (185.4 in 2020),

which is set to decline over the course of the century as the working-age population grows (UN 2019, p.85). Other estimates argue that even these already-high population growth estimates might be exceeded (Ezeh et. al 2020). By contrast, even though Southeast Asia is today the site of some of the most rapidly growing industrial complexes in the world, the region's long-term demographic projections strongly suggest that this rate of growth will not be sustainable over multiple decades, as was the case in China. The UN estimates that Southeast Asia had a population of 668.9 million in 2020 and that this was projected to grow by a mere 58.67 million to 727.29 million by 2030 and then peak at 794 million in 2050, dropping to 744.22 million by 2100. Of this, the working age population will peak sometime between 2030 (for the youngest cohort, 15-to-24) and 2050 (for the older cohort, 25-to-64) and the dependency ratio is estimated to decline from 94.9 in 2020 to hit its trough at 92.5 in 2030 before rising again to 94.4 by 2050 and, by 2075, passing the 100 threshold sometime in the 2070s (UN 2019, p.137).

For comparison, China's population in 1970 was around 827.6 million, booming to 1.18 billion by 1990, when children aged 0-15 made up 28.6 percent of the total population, those aged 15-24 made up 21.7 percent and those aged 25-64 made up 44.1 percent. In the same year, those 65 and older composed a mere 5.6 percent of the population. As population increased to 1.29 billion in 2000, 1.37 billion in 2010 and 1.44 billion in 2020, the share of elderly grew to account for 12 percent of the population (by 2020), while the share of children had shrank to a mere 17.7 percent. Meanwhile, the bulk of the population (58.5%) now lay in the older working-age cohort. All projections show the elderly population rising rapidly as this cohort ages out of the workforce. Those 65+ are projected to compose 16.9 percent of the population as early as 2030, rising to 26.1 by 2050 and 30.1 by 2075. Population as a whole may have already plateaued, with only slight growth to 1.46 billion by 2030, followed by a decline to 1.4 billion by

2050 and just barely above 1 billion by 2100. The dependency ratio already saw its trough in 2015 (at 68.4) and had begun to rise again by 2020 (79.3). It is projected to surpass 100 by 2050 (ibid, p.388).

Forty years of rapid growth in China were undergirded by the addition of some 100 million people to the total population every decade, on average, followed by their slow filtering into the labor force. At most, Southeast Asia may add about 60 million people to its population by 2030, and then another 67 million by 2050—on average, this is a mere 42 million per decade for three decades (most of this concentrated in the 2020-2030 period), compared to China's 100 million plus per decade for four decades straight. There quite literally will simply never be enough workers in the entire region to successfully relocate the bulk of global industrial capacity currently concentrated in China into Southeast Asia regardless of how low the wages are or how high the profit rate is. At most, Southeast Asia should see success securing the relocation of portions of particular production lines once concentrated in China, such as textiles. But, at this point, it is not even clear how feasible it even is to relocate much of the work that would represent a true ascent up the value chain (like more advanced electrical assembly) to countries like Vietnam or Malaysia.

By contrast, Sub-Saharan Africa at least offers population growth in the long run exceeding that of China during its growth boom, adding, on average, 330 million people every decade between now and 2100, with this growth more evenly spread out over the entire period, though slowing substantially after 2075. Between 2020 and 2100, Tanzania alone is projected to see its population grow from roughly 60 million to around 285 million, with the population doubling between 2020 and 2050. The vast majority of this will be in the working-age population, with the dependency ratio already declining from its 1990 peak and set to decline

continuously for the next eighty years. Nigeria, the largest single country in Africa by population, is projected to add as many people to the global population as all of Southeast Asia combined over the next decade, growing from 206.1 million in 2020 to 262.97 million in 2030. Afterwards, growth accelerates, with just over 400 million projected for 2050 and an astounding 732.9 million by 2100. As in Tanzania and Sub-Saharan Africa more generally, the vast majority of the population throughout the entire period will be working age population and the dependency ratio is set to fall continuously. Thus, a *single nation* in Africa could absorb all the potential jobs created or relocated to the entire region of Southeast Asia in the coming decades. The prospects for the entire East African region (where trends are all more or less identical to those in Tanzania) are salient, at least when considering the most baseline demographic potential.

In reality, however, the population problem is more complex than this. Part of the benefit offered by the Chinese working population in the course of the reform and opening was its extremely high level of literacy, excellent public health measures given its per capita GDP, and widespread training in technical skills and low-level engineering linked to the autarkic development of the country's industry during the Cold War and administrative mechanisms (namely the *hukou*) that could be used to control population movement and discipline labor—all features inherited from the socialist era. A similar inheritance in Vietnam seems to be at least one contributing factor in that country's prominent role as an offshoring site for Chinese industry. But aside from this one instance, almost no other country in the world has measures of health, education and technical training or inherited administrative and industrial structures that match those of China at the end of the socialist era. Baseline demographics are simply not enough, on their own. This is at least part of the reason that new investment in greenfield sites globally

seems to require more upfront infrastructural development that is also broader in scope (i.e., accompanied by aid directed toward public health, literacy, etc.) than those required in China in, say, the 1990s.

In some countries, these qualitative demographic features are more developed. In the middle-income nations of Latin America and Eastern Europe, for instance, literacy rates, technical training and public health measures tend to be closer to those in China at the time of its opening, but baseline demographic conditions are essentially the opposite. In Eastern Europe, the population pyramid is already inverted and total population has been declining more or less continuously since the fall of the Soviet Union (ibid, p.185). In Latin America and the Caribbean, population growth is set to crawl upward slightly, with 100 million people added to the region over the next thirty years (~33 mil per decade) at which point population will peak (in 2050) and then decline. Most of the growth in the young population has already occurred, with the number of those under 15 and those 15-24 having peaked in 2020, and the 24-65 cohort set to peak in 2050. Thus, the dependency ratio is estimated to reach its trough in 2030, rising thereafter (ibid, p.141). Central Asia also inherited relatively higher literacy rates, levels of technical training and better performance on public health metrics from its socialist-era history, but the major limit in the region is simply that overall population numbers are strikingly low: a mere 74 million in 2020, less than the population of Vietnam alone. While population is projected to grow continuously in the region for the next eighty years, it will peak at barely more than 111 million in 2100, with the dependency ratio oscillating around 100 for the whole period. Even if it might play a central logistical role in linking China to Europe, there are simply not

enough potential workers[61] in Central Asia for much industry to relocate there—even if the other geographic frictions could be overcome through infrastructural investment (which is not at all clear), such as the fact that coastal locations are, in economic terms, "closer" to one another than these deeply landlocked regions. Either population would need to be moved to these locations or something significant would have to occur within the global geo-economic structure to encourage a re-continentalization of commerce, which is only conceivable in fairly extreme conditions of colonial expansion and/or regroupment around extremely local trade blocs.

South Asia is only other region of the world that has anything like the demographic potential of Sub-Saharan Africa. But even here, total population growth is projected to be both slower and lower in absolute terms than that across Africa. In 2020, the South Asian population was estimated to be 1.94 billion, of which children under fifteen those between the ages of 24 and 65 made up the largest shares, 27.5 and 48.3 percent, respectively. But the relatively larger share of the older working age cohort means that growth will be more subdued: 2.14 billion is projected for 2030, and 2.4 billion by 2050 climbing very minimally to a peak of 2.41 billion by 2075 before declining to 2.21 billion by 2100. The dependency ratio is expected to trough in 2050 and rise thereafter, the same year that the working age population peaks (ibid, p.125). Demographic factors likely account, in part, for the boom of Chinese interest and investment in neighboring Pakistan, which had a population of 220.89 million in 2020 and is projected to see continuous growth to 403 million by 2100 (adding roughly 22 million per decade). This trend is driven by the growth in the under 15 and working age populations, manifesting in the decline of

[61] It should be mentioned that absolute population size is not the sole issue here, since it would still be technically feasible to relocate more capital-intensive industries to regions with low overall population. An equally fundamental limit is simply that individual countries with their own currencies will tend to see relative wage costs increase faster if their working population is smaller, simply because demand for labor will more quickly outstrip supply.

the dependency ratio seen since 1990 and projected to continue through the end of the century (ibid, p.829). But the rest of the region's population growth is centered in India (with 1.38 billion pop in 2020 expected to peak at 1.639 billion in 2050, ibid. p.635), where geopolitical rivalry has precluded more substantial Chinese investment.

At this point, it seems far more likely that South Asian development will be split according to the prospects of greater integration with China. In other words: after or alongside the development of Southeast Asia via the outflow of capital from (or at least still subcontracted through) China, those nations in South Asia most open to Chinese investment (and with larger populations and more locational amenities, such as a seaport or a lucrative border position, etc.) will see the most rapid industrialization. Essentially, this means that, short of some major rapprochement between India and China, Pakistan and Bangladesh will likely lead the entire region in economic growth over the next decade and may supersede Southeast Asia as the major site for Chinese outbound FDI once wages in Southeast Asia rise enough. This initial trend should be true regardless of whether deglobalization and the new regionalization around distinctive trade and currency blocs predicted by Howell (2020) accelerates or reverses. Either way, it seems that global industrial development will begin to definitively pivot away from the Pacific Ocean and toward the Indian Ocean. The long movement of Asian littoral industrialization that began in the Tokyo-Nagoya complex, spread north to South Korea and south to Taiwan, Hong Kong and Singapore then jumped to the coastline of mainland China and is today shifting southward across the South China Sea will finally cross the Straits of Malacca and come full circle, returning to one of the earliest sites of pre-capitalist "globalization" (as documented by Banaji 2020 and Sheriff 1987). It is in this context that the particular importance of East Africa becomes apparent, since future decades will prove it to be the single largest and

fastest growing population pool on the Indian Ocean, with strong pre-existing cultural ties to South Asia (not to mention the Middle East). Thus, long-run global demographic trends combine with physical geographic amenities to raise the prospect of an emergent string of territorial-industrial complexes across the Indian Ocean, with China linked to Sub-Saharan Africa, the world's last remaining population hub, through links between ports in East Africa and those in Pakistan and/or Myanmar or Bangladesh.

Such a potential is only in the earliest stages of gestation. Rising geopolitical tensions between China, India, the US and Europe also all but guarantee extremely unpredictable new conflicts and attendant shifts in global flows of trade and investment. What has been identified here is the most baseline forecast for global development within a capitalist system constantly grappling with the twin problems of profit and population. These two factors can be imagined to set the conditions of the system's inertial movement, even while they also lead to new, more dynamic and conflictual forms of propulsion that will always displace this movement off any path determined solely by these initial conditions—but only rarely so far off this path that the system's inertia is stilled or its general direction reversed. For instance: even though mass migration offsets some of the locational importance of population growth, migration flows driven by industrial employment (which are the only flows large enough to have any real effect on development's geographic trajectory) will tend to gravitate toward zones of pre-existing accumulation and active industrialization and thereby just as often reinforce the initial trend. While it is impossible to make any deterministic prediction of which African nations (if any) will become central to any future Indian Ocean industrial complex, it is reasonable to make probabilistic forecasts based on the factors outlined above. Ultimately, if industrialization in places like the Pearl River Delta is any example, the reality is that much more contingent factors

of cultural, linguistic and historical linkages will be equally important in determining precisely which areas will be ascendant even in the context of general regional economic growth.

More insight, then, will be gained by an on-the-ground investigation of the industrial zones in Dar es Salaam, Tanzania, one of the world's fastest growing cities in a country set to see rapid population growth over the next century, with intricate historical and cultural ties to the Middle East (dating back to the middle ages), South Asia (similarly old, but reinforced under British colonialism) and even China itself (during the postcolonial Ujamaa period, which saw major developmental partnerships between the two countries). Today, Tanzania has also been one of the major African sites for Chinese outward FDI in manufacturing (as documented by Sun et. al 2017 and Xia 2019a and 2019c). Poor data, however, means that the details of this development remain murky. The next chapter will attempt to clarify conditions in this small but growing industrial hub, still in the earliest stages of its gestation, through a field survey of almost all the major industrial areas in the city and the Chinese firms already operating within them. In so doing, I make no hard claim that this particular site will be the entrepot for the "next factory of the world" (Sun 2017) in fifty some years. As has been demonstrated above, capitalism is an inherently conflictual system ever-driven toward breakdown and shaped by constant crisis that often manifests, in the last instance, as inter-imperial war and has been attended throughout its history by earth-rending ecological devastation. If the system is somehow able to prevail over (or more likely through) these tendencies in the next eighty years, then Dar es Salaam may just play this role. If not, a more likely future is inundation from rising sea levels or reduction to rubble in some coming war.

# Chapter 5

## A New Territorial Industrial Complex?

### The Question of Chinese Investment in Dar es Salaam

**Introduction**

The classic quantitative field survey, so integral to early geography, seems to have become less and less common in the literature, slowly superseded by either more intensive ethnographic inquiries on the one hand or more extensive use of pre-existing official data, on the other. But the field survey was itself once envisioned to be the very basis of quantitative geography, with its purpose "to supply an adequate, systematized body of significant data out of which generalizations may shape themselves with increasing diversity as the source material grows in amount" (Sauer 1924, p.20). While the use of official statistics was important, it was nonetheless considered secondary, since "the intelligent selection of these data for cartographic representation and the interpretation thereof […] presuppose a considerable amount of direct observation in the field" (ibid). In short, quantitative studies require a constant attentiveness to their source material since they inherently reduce the real complexity of that material while also implying some foreknowledge of that complexity in the selection and construction of relevant variables. If performed correctly, this process of scientific reduction is ultimately productive, illuminating new features of the phenomena in question which would not be observable by the naked eye. Nonetheless, as data becomes distant from its source it often grows more opaque. It becomes less clear how, exactly, it was measured and what, exactly, the object of measurement

was. At the same time, such data often takes on a false objectivity through its very repetition, even while that process of propagation and reuse also amplifies existing errors or misrepresentations.

The field survey is thus the reflexive nexus between the intuitive, direct experience of the space being studied and the reduction of that space into useful and informative quantitative data. This nexus is an ethical and political one since it is where key decisions are made by the researcher regarding the transformation and representation of the inherently inexhaustible experience of any given phenomena in space in order to make broader theoretical arguments, all of which are inherently political and all of which entail an ethical duty, for practitioners of geographic science, to not engage in willing and forceful distortions of this data to serve a given theory against evidence to the contrary. Overall, the fundamental utility of the field survey lies in the fact that real understanding of the phenomena in question requires continual and recurring attentiveness to how those phenomena take shape in space. Only with this condition met can the geographer then accurately decide how to productively reduce this experience into a quantitative form and how to select from other pre-existing masses of official statistical data the most relevant and comparable information. This process then acts as the scaffolding for the construction of geographic theory.

This is not merely a contrast between an ethnographic, qualitative and local dimension and a statistical, quantitative and global one. Such an opposition is false since each always inheres in the other. But this contrast is wrong at a more fundamental level as well, because the qualitative experience of the field survey is of an entirely different character than an ethnographic one—and may, in fact, require little to no directly anthropological dimension whatsoever, as in a field survey focusing on questions of geomorphology or ecology—while also

not being inherently limited to a single locale. The qualitative dimension, maybe better described as the way in which real phenomena in space will always exceed their reduction into quantities, is defined not by direct ethnographic experience (though this may be a part of any given survey) but instead by direct experience of the space as such. Even in geographic studies that have an anthropological dimension, then, the attention of the geographer is offset from that of the anthropologist. The focus is on the place and the spatial organization of the phenomenon in question, understood at whatever scale is relevant to that phenomenon. Thus, the importance of the field survey is to be found in both of its terms: not just a systematic survey of respondents, as might be conducted by long-distance polling, but also a literal, embodied exploration of the "field" itself. The direct, on-the-ground experience of space is an essential precondition for its proper theorization.

Below, I offer the results of one such field survey, recording basic data on Chinese industrial firms operating in the city of Dar es Salaam, Tanzania, as well as an overview of the data problems it was designed to address. The aim of this field survey was breadth of coverage rather than depth, so only rudimentary data on firms was gathered, namely: name, sector and product, years in operation, number of workers and the exact geographic coordinates of the firm. Frequently, more data was added in the form of additional notes gathered from conversations with respondents, including information and speculation on the split between local and Chinese workers, the home city or home province of any Chinese employees, the observed level of activity and the general knowledge of locals with regard to the firm including any key contrasts in respondents' claims about the firm. The first section of this article explains the existing lack in the data that this field survey was responding to and frames the theoretical stakes of the project. Emphasis is placed on the examination of Dar es Salaam's industrial districts as one locality

within a larger territorial productive complex taking shape across the region. The second section places the data into its qualitative and methodological context, using a narrative approach to give the reader some sense of how the spaces in question where experienced on foot and how this embodied immersion in the industrial territory led to the use of particular modes of inquiry and prioritized certain methods over others. The third section summarizes the quantified results, compares these results to other existing data on Chinese firms operating in the city and concludes with some thoughts on how these survey results inform the larger theoretical questions about Chinese investment in Tanzania and elsewhere in sub-Saharan Africa.

# Section 1 – The Financial Mirage

The bulk of existing data on Chinese-origin investments outside of China is of questionable reliability. This problem is exacerbated in countries with low capacity to accurately gather their own economic statistics. In many places, the gap in official statistics is paired with official announcements of prospective investment, the opening of lines of credit with China or the initiation of negotiations for large infrastructure projects. This frequently leads to a misreporting of such phenomena as "investments" in the media even though no ground has been broken on any project and no funds have changed hands. Such stories then take on a life of their own as they are repeated in new forms in the academic literature and in the policy world—an easy transition in the US in particular, where growing economic tensions with China ensure that policymakers are especially receptive toward any narrative that casts the Chinese as a threat. The basic problem is identified in a recent Congressional Research Report:

> Figures cited in news articles, think-tank reports, and academic studies may not be entirely accurate and should be interpreted with caution. For instance, many publicly and privately available unofficial "trackers"—from which these data are often sourced—are based on initial public announcements of Chinese overseas projects, which may differ significantly from actual capital flows because such projects may evolve or may never come to fruition (Schwarzenberg 2020, p. i)

This is one of the many reasons that field surveys are essential in such cases, since they re-anchor the data to the reality of what is ostensibly being measured. The process of "ground-truthing" that Bennet (2020) argues is essential to the critical use of remote sensing data applies equally here to global economic statistics.

Media myths of Chinese intrusion have been particularly prominent in the African case, especially considering that many of the African countries with the highest shares of Chinese debt (as measured against their GDP) are the same countries that suffered most under the IMF-led lending regime in the later 20$^{th}$ century, which resulted in the third world debt crisis (see: Horn et al. 2019). In the past, these myths tended to focus more on the most spectacular scenarios of Chinese land-grabbing and even the supposed (and entirely fictional) mass settlement of Chinese ruralites in sub-Saharan Africa (Brautigam 2015). Today, they tend to focus on the idea of "debt-trap diplomacy," which centers more on Chinese capitalization of large infrastructure projects and the use of collateralized loans as a standard lending practice (Brautigam 2020). On reason for this shift from the agricultural to the infrastructural sphere is the fact that attentive scholars systematically went in search of the alleged "land-grab" agricultural investments, only to find that many deals were never deals in the first place but instead over-inflated pronouncements based on misreporting of Chinese policymakers' own statements or, at most, that they were real

investments of a much smaller scale or cases in which negotiation over some potential investment was opened but ultimately fell through (Schoneveld 2011 and Brautigam 2015). Once these stories were so decisively disproven, the same media and policy interests had to turn to other "evidence" to make the same case. As Chinese infrastructural investment ballooned alongside the commodity bubble, scholars began to claim that this situation was homologous with the build-up to the Third World Debt Crisis (Horn et. al. 2019), only now, because of the common use of concessional loans, China was positioned to threaten countries with the turnover of key infrastructural assets (Parker and Chefitz 2018). This latest round of China-scare journalism is again being treated to systematic academic attention and again being rigorously disproven (see: Brautigam 2020 and Brautigam and Kidane 2020). Nonetheless, it is likely that, as Chinese manufacturing firms increase their presence globally (and in Africa specifically), similar narratives will arise with stories of Chinese-owned factories at their center. Thus, early field surveys clarifying the situation on the ground are even more essential.

Despite the recent shift to questions of debt and diplomacy, the basis of these myths remains the same: a financial mirage generated by the particular way that the media and, subsequently, academics and policymakers, receive and interpret news of potential investment. Announcing wide-ranging infrastructure projects or other big-ticket investments acts as a form of political capital for local governments as well as Chinese global diplomacy. On the local side, it shows that a given administration is bringing potential jobs, ensuring that roads will get built or simply delivering on campaign promises to revive the economy in particular regions (this was the case in the now famous example of Hambantota Port in Sri Lanka, see: Sautman and Yan 2019). On the Chinese side, it helps to demonstrate the "win-win" potentials of Chinese investment and to promote key government campaigns, such as the "Belt and Road Initiative"

which, like most heavily promoted policy efforts in China, is better understood as an exercise in branding and advertising draped over disparate and often fundamentally uncoordinated investment projects and government policies. Regardless of which perspective the investment is viewed from, there is a clear structural incentive to both affirm its reality and to exaggerate its impact. Nonetheless, the same problem that plagued stories of Chinese land grabs a decade ago now afflict similar stories of ballooning debt to China. As in the earlier instance, "surprisingly few of the Chinese investments headlined in media stories have actually taken place" (Brautigam 2015, p.2). This is because what the media is often reporting are *prospective* investments, possibly agreed to at the diplomatic level, or simply the opening of sources of credit from Chinese investment institutions. Reporting on these as if they represent actual investment and actual debt would be equivalent to imagining your bank account inflating and your credit score plummeting because your mailbox is full of letters saying you've been "pre-approved" for new credit cards. In the case of Chinese investment, however, the reality is even more tentative, since even when projects are agreed to by both parties, they often never break ground and funds are never dispersed.

**Dice in the Sand**

Since this field survey was conducted in Dar es Salaam, it will be helpful to begin with a review of how this financial mirage has taken shape in Tanzania specifically. Even though the focus of this survey is on industrial investment in manufacturing and warehousing, I will briefly outline the nature of Chinese involvement in the country's infrastructural development and its overall urbanization, since industrialization is linked to both and Dar es Salaam remains Tanzania's primate city, its economic capital and the area where the majority of industrial production aside

from resource extraction and agricultural processing takes place. Throughout the mid-2010s, Tanzania was by no means exempt from the flurry of media reports announcing a tidal wave of Chinese investment pouring into the African continent. President Jakaya Kikwete signed several prominent investment deals with China throughout the duration of his administration and in particular in the final years of his second term, which coincided with the height of the global commodity boom and the peak of Chinese outward investment in the 2010s. On a single trip to Beijing in 2014, Kikwete finalized five such deals, allegedly worth more than 1.7 billion USD. These included the construction of a satellite city outside Dar es Salaam, a new financial hub in the city and a large rural electrification project (Reuters Staff 2014). They followed similar announcements for a new port at Bagamoyo (BBC News 2016) and the early construction of the Mtwara-Dar es Salaam Natural Gas Pipeline which would be completed in 2015, at the end of Kikwete's administration (Mwakyusa 2019).

At the same time, because of its own socialist history in the postcolonial era, Tanzania had already been host to two of what were once the largest and most well-known of Chinese investments on the continent: the Uhuru/TAZARA railway, built with Chinese assistance between 1970 and 1975 (Monson 2011), and the Urafiki textile factory, founded in the late 1960s and initially specializing in the production of *kanga* worn throughout East Africa (Ryan 2016). TAZARA is still in operation today, though its freight capacity is far diminished and its formerly central position as the main railway linking the Zambian copper belt to international markets was long ago lost to alternate routes. Meanwhile, Urafiki's facilities are still located in the Ubungo neighborhood of Dar es Salaam, having been restructured into a joint-venture with the Changzhou Textile Industry Bureau in 1996 and recapitalized in the same year with a new concessional loan from China EximBank. But, faced with increasing competition beginning in

the 2000s, the firm hemorrhaged business, scrapped machinery and dwindled to a fraction of its former workforce before finally ceasing operation in 2018, ostensibly to restructure and ultimately return to business (Xia 2019a). As of January 2020, Urafiki was still non-operational. In informal interviews conducted by the author, the facility's security guards and several neighborhood residents confirmed that the company was not in business, though people were still visibly living on the compound.

Meanwhile, beneath the series of enormous investments announced in the final years of the Kikwete administration, a wave of smaller and/or more informal projects either funded by Chinese capital or constructed by Chinese subcontractors were already reshaping the country's infrastructure in significant ways. Among the most important of these were road and real estate projects, the former distributed throughout the country and latter concentrated in Dar es Salaam. In the case of roads, financing was either sourced through the Tanzanian National Roads Agency (TANROADS) or from multilateral agencies like the African Development Bank or the World Bank's International Development Association. In these relationships, (different) Chinese firms frequently played a role in both financing and contracting, since Chinese capital also funded the multilateral development agencies themselves. In fact, many projects that are formally funded by such agencies or even by the Tanzanian government or its ministries are often actually financed through chains of global liquidity that can be hard to trace to their source. But it was in the role of contractors that Chinese involvement would become most visible. Ten large TANROADS projects resulting in a total of 1069.6 km of upgraded or rehabilitated road were completed between 2000 and 2016. Of these, eight were built by Chinese contractors (the remaining two were awarded to firms from Kuwait and Japan). Similarly, of the nineteen projects financed by

multilateral aid agencies, Chinese firms won the construction contracts for thirteen (Flores 2017, pp.17-18, Table 2).

In terms of real estate, Chinese firms played a similar role, but with the effect of the investment much more visible. This was because real estate investments were mostly concentrated in a burst of urban construction in Dar es Salaam, reshaping the city's skyline in the space of several years. This process was buoyed by rapid urbanization, with the city's population skyrocketing from just over 3 million in 2010 to an estimated 6 million in 2019, making it one of the world's fastest growing urban centers (Rosen 2019). Though well on the path to megacity status, much of this development has been in informal peri-urban settlements. The rise of new skyscrapers and smaller residential towers in and around the urban core signal the uneven nature of the city's development, with the vast majority of the population unable to afford units in these buildings even while demand for housing remains high (Murphy and Carmody 2019). At the same time, under the Kikwete administration a series of real estate projects of unprecedented size and scope were either announced or initiated, with the vast majority composed of higher-end developments not at all geared toward the reality of the city's peri-urban agglomeration of informal settlements. The largest of these was the Kigamboni New City, a proposed greenfield project located in the more sparsely populated peninsula across the water from Dar's central business district. Not only was this among the biggest of the proposed real estate investments, but the idea "came from the president himself" and was led by "a new central government body—the Kigamboni development agency" created specifically to oversee the project (Lindell et al 2016, p.20). While the Kigamboni New City was the largest and most favored of such initiatives, several similar satellite city projects were also formulated in these same years: Dege Eko Village in the far southwestern corner of Dar, and Kawe 711 in the city's north (Rosen

2019). All were intended as modern suburban commuter cities, modelled on greenfield city projects in East Asia (namely Malaysia and Hong Kong).

The Kigamboni project floundered from the start, unable to find any workable means to deal with the displacement of the 97,000 existing residents in the huge area required for development despite securing funding from the National Social Security Fund in addition to the deal Kikwete had signed with China (Lindelle et al 2016, p.). Nonetheless, it did result in the construction of over a thousand houses in the area, left in various states of construction (Tairo 2019). The other projects cleared land and began to build but all ultimately collapsed amid the corruption scandals that plagued the country throughout the 2015 elections. The election of president John Magufuli on a populist, anti-corruption platform in that same year saw the centralization of planning authority for urban development and the cancellation of a number of the investment projects agreed to by Kikwete and numerous local officials (Rosen 2019). By the time of this field survey in January 2020, Kigamboni New City had essentially been cancelled while Dege Eko Village, Kawe 711 and similar projects sat in limbo, their bare, empty concrete facades staring out over the coastal plain and the Indian ocean beyond it. With its many buildings moldering on a prime strip of beach right near a major thoroughfare, Kawe 711 was a particularly frequent reference in interviews and random conversations with people describing the general economic conditions of Dar's urban development in recent years. Almost every taxi driver on any trip north of the city would point to it as we passed, praising the crackdown on corruption but bemoaning the eyesore.




***Image 5-1***

*Public signboard for the New Selander Bridge project, located at the construction site.*
*Photo by author, January 2020.*

In the midst of this blitz of speculative urbanization a number of major real estate ventures financed or constructed by Chinese firms were initiated. In some cases, Chinese firms may have served in a minor capacity, supplying materials or acting as sub-contractor on one small part of construction, while the main contract was handled by a non-Chinese firm. This was the case in the construction of the PSPF Towers, contracted to the Kenyan firm Estim Construction but with portions of the work handled by Chinese firms. In many cases, financing derived from the Tanzanian state or other outside sources and Chinese firms acted in a more visible capacity, even if they were not the main contractor. For example, the New Selander Bridge project in the heart of the city appears to be a "Chinese" venture—and is reported was reported as such by many respondents—due to the visible presence of Chinese construction workers, but in reality the financing comes from the Korean Economic Development fund and the Government of Tanzania, with the main contractor listed as GS Engineering and Construction, a South Korean company. Nonetheless, much of the actual on-the-ground work was sub-contracted to China Railway Seventh Group Co, Ltd. Image 5-1 above visualizes the complex reality of such investment, showing the sign boards that are legally required to be posted outside such construction projects that list the various contractors and sources of financing.

Ironically, the New Selander Bridge is set to replace the older Selander Bridge, which was also funded by East Asian capital (the Japanese International Cooperation agency) in the 1980s, when Japan was experiencing a real estate and construction bubble similar to that of China today. It is, therefore, often impossible to entirely separate out "Chinese" investment from broader chains of capital, especially those originating in East Asia more generally. Similarly, foreign and especially Asian influence on infrastructural development in Tanzania (and across

Africa) has its own history, preceding the more recent influx of Chinese capital and construction firms.[62] Meanwhile, these complex chains of financing and sub-contracting mirror the reality of infrastructural development in Tanzania more broadly, where urbanization is not necessarily driven by purely domestic trends in industrialization and development but is better understood relative to "the deployment of global infrastructure" (Wiig and Silver 2019, p.912), serving global needs, not to mention the global demands of portfolio investors fueling the real estate boom. The past decade has certainly seen Chinese firms playing an increasing role as both construction contractors and as sources of capital in Tanzania. However, this role is far better characterized in relationship to a growing influence of Asian capital more generally. This includes renewed investment from East Asian countries, with major financing from Japan and South Korea accompanying that of China. But it also includes both renewed and much older investment ties to South Asia and the Middle East, with roots in the region's colonial history.

There are plenty of cases where the role of Chinese firms has been central. Examples of this can be seen in large mixed-use projects like Uhuru Heights and Mwalimu Nyerere Foundation Square, where the same Chinese firm acted as primary construction contractor, though not the main financer—both were built by CRJE (East Africa) Ltd., a subsidiary of the China Railway Construction Engineering Group dating back to the construction of TAZARA (The Citizen 2020). But while Uhuru Heights and the Mwalimu Nyerere Foundation Square represent relatively successful projects that reached completion, another project constructed by a different Chinese contractor offers a stark contrast. The Mzizima Towers were projected to be

[62] Anecdotally, while living outside Arusha, Tanzania in 2018, I witnessed a similar pairing: a new Chinese-constructed road project leading from the city to the suburb of Usa River, built between two small bridges that, according to an elderly local gardener who'd witnessed their construction firsthand, had been constructed by the Japanese in the 1980s.

the tallest skyscrapers in East Africa (at 32 and 30 floors) when construction began in 2013 and they technically reached this goal when the buildings were topped out in 2017. At this point, however, the project stalled. Construction of the towers was contracted to the China Chongqing International Construction Corporation (CICO) and its Group Six International LTD local subsidiary, and the project was financed by the National Social Security Foundation—in a pattern that was repeated across the real estate market in the early 2010s, where public pension and housing money was funneled into speculative real estate ventures (Tairo 2019). As of January 2020, the buildings remained standing as largely empty concrete frames, topped out three years prior but with little apparent work done to complete the project—at the time of the survey, no glass had been added to the exterior and it appeared that the interior remained almost entirely unfinished. A small team of local construction workers was visible on site but appeared to be living on the ground floor without much work to do. Locals in the surrounding area hadn't seen any substantial deliveries to the site or other major activity in years.

An even more stark example, however, could be found in the mega-projects that were announced, lauded in the media and then simply never materialized. If the real estate projects were speculative ventures gambling on future property prices, these mega-projects were bets of a different scale entirely. The Bagamoyo Port was by far the most significant of these, since it was poised to be Sub-Saharan Africa's largest deep-water port were it to have been completed (BBC News 2016). Financing was to have come from China Merchant Holdings, an Omani sovereign wealth fund and the Tanzanian government, with the actual management of the port undertaken by the Chinese firm, to which the land was to be leased for 99 years. Further, the Kikwete administration's original contract had included terms guaranteeing an extended tax holiday for the port facilities and discounted rates of electricity and water (Mittal 2020). A gas pipeline was

supposed to link to the project and a large special economic zone was established, almost as large as the current town (BBC News 2016). The agreement was criticized by many at the time and, once Magufuli became president, the project was halted for an extensive review. Although it initially seemed like work would begin again in 2018 under new terms, the project quickly stalled again before finally being cancelled in 2020.

Instead, the government decided to refurbish the existing port at Dar es Salaam, signing a contract with the China Harbour Engineering Company to expand its capacity, funded by a World Bank loan, alongside several similar contracts funded by the UK Department for International Development and TradeMark East Africa (Reuters Staff 2017, Hönke and Cuesta-Fernandez 2017). In another twist of irony betraying the more complex integration that has long existed between East Asia and East Africa, the container port of Dar es Salaam is itself already owned by Tanzania Container Terminal Services Ltd, a joint venture with Hutchinson Port Holdings which is a subsidiary of Hong Kong firm CK Hutchinson Holdings (formerly Hutchinson Whampoa). Meanwhile, bulk cargo (i.e. non-containerized goods) were still managed by the state through the Tanzania Port Authority, which also owns or controls most of the facility's sheds and warehouses, and the Tanzanian Revenue Authority retained all of its jurisdictional powers (Hönke and Cuesta-Fernandez 2017). Overall, the port saga gives a succinct image of all the key capital relationships: Chinese construction contracting combined with partial financing, the involvement of capital from both the Middle East and the UK (both with colonial histories in the region) and the intermediation of other East Asian firms closely linked to, but nonetheless distinct from, the Chinese construction contractors.

Throughout the entire Bagamoyo debacle, then, the port in Dar es Salaam was bustling, albeit continually constrained by the massive congestion caused by unregulated urbanization. If a

single pairing could be chosen to symbolize the financial mirage of much of the data on Chinese investment in Tanzania or Africa more generally, the contrast between Bagamoyo and Dar es Salaam might be the most telling. On the one hand: a mega-project that never came to fruition, overwhelmingly branded as Chinese (despite Omani and Tanzanian investment), initiated by a previous administration, wrapped in scandals from its very inauguration and then agonizingly kicked through years of limbo until it was finally cancelled almost a decade later. On the other hand: a series of build-outs of existing capacity and replacement of existing infrastructure at the port in Tanzania's long-standing economic capital, with its own inertia and history, all managed through a much more complex process of joint investment, contracting and management spanning circuits of global finance, with key roles played by the Tanzanian government, Chinese firms, firms from other advanced countries in Asia and financing from former colonial powers. Meanwhile, all of this was taking place in the midst of a massive urban boom that encircled the port facilities in the congestion of massive informal settlements, which thereby required further build-outs of road, bridge, rail and pipeline infrastructure.

Taking all these forms of investment into account—from the financial mirage to the real buildout of roads and real estate—Dar es Salaam has been marked by a deep spatial and temporal disjunction. Urbanization was advancing rapidly, a fact etched in the changing skyline. Meanwhile, a number of projects were being completed and going to market, including several smaller residential buildings constructed by the Aviation Industry Corporation of China (AVIC). Demand for housing was real throughout this period, but even these smaller residential units were not geared toward the bulk of the population. After their completion, many would sit empty, their windows mostly dark on any given night. While all this formal construction was occurring, however, most of the city's actual urbanization was taking shape in peri-urban

informal settlements, where upwards of 70 percent of Dar es Salaam's population resided throughout the 2010s (Rasmussen 2013, Limbumba and Ngware 2016). Such settlements soon surrounded the expanding port, strangling its flows of traffic and encouraging a series of new state-led redevelopment plans (Hönke and Cuesta-Fernandez 2017). Meanwhile, the skyline is to this day pockmarked by the concrete frames of eternally stalled projects like Mzizima Towers and Kawe 711, moldering in the humid heat. As some projects advance rapidly, others fall into limbo. Sometimes, this results in more open conflict: In 2019, four Chinese contractors were arrested due to "slow progress" on their construction of a canal and a segment of road in the city, thereafter being held at the police station at night and released to oversee the completion of the contracts during the day (Agence France-Press 2019).

The speculative dimension of urbanization in the city is stark, each of the high-rises a gamble funded by excess capital sloshing against the shoreline from overseas. But the informal settlements were also their own sort of gamble, with higher stakes: a bet that work could be found, that the seasonal floods wouldn't wash the makeshift houses away, that the claim would be acknowledged by local officials. Overall, the result is a deeply uneven experience of accelerated development and an equally uneven urban geography. Achille Mbembe argues that this is the general condition of development in Africa more broadly, which sees

> a kind of capitalism that is mostly disjointed, almost galactic in the sense that consists of a seemingly random collection of disconnected enclaves. These enclaves are incongruously linked together in a contrived form that cannot be easily grasped within the conventional analytical paradigms. It is a capitalism of multiple nodal points, of scattered patterns, of spatial growth combined with neglect and decline (Mbembe 2016).

This was the basic shape taken by Dar es Salaam at the time of the survey, with investment projects scattered across the city in various states of prosperity, stagnation and collapse, like so many dice scattered into the sand.

# Section 2 – The Existing Data

Keeping these facts in mind, it is now possible to explore the data as it currently exists. First, I will use Bureau Van Dijk's Orbis Database—one of the largest databases of private firms, with 300 million records worldwide—to illustrate the basic composition of the large-scale Tanzanian corporate sector and to explore the existing data on international ownership within this sector. Second, I will look at Chinese investment in Tanzania as recorded by the China Global Investment Tracker (CGIT), introduced in the chapter above. In this section, I will measure which of the CGIT-recorded projects for Tanzania have actually been completed or are likely to be in the near future and which have been cancelled, stalled or are unlikely to be completed. This helps to frame a more critical appraisal of one of the most commonly used data sources on Chinese investment. Third, I will place Tanzania into regional context using the existing statistics on outward FDI by location as recorded by the Chinese Ministry of Commerce and aggregated by the China-Africa Research Initiative (CARI) at Johns Hopkins, both also introduced in the previous chapter. Finally, I will look at two additional surveys of Chinese investment in Tanzania and surrounding countries: one general survey from McKinsey & Company, conducted in 2016, and a linked pair of scoping surveys on manufacturing investment conducted by Xia Ying in 2018 and published as CARI Working Papers in 2019—one for

Tanzania and one for Kenya. These provide the most direct point of comparison for my own survey. While Xia's approach was top-down (via formal contact with Tanzanian investment officials and surveys of Chinese businesspeople) and includes much more financial detail but almost no geographic information, my own was bottom-up, emphasizing geographic information, neighborhood knowledge and interviews with workers, but including little financial detail. Altogether, these pre-existing surveys will provide basic context and points of contrast for the results of my own survey.

**The Informal Norm**

Since this project is primarily concerned with measuring Chinese-origin FDI in manufacturing and affiliated sectors such as warehousing, it will be helpful to first place manufacturing in proper context relative to other sectors in the Tanzanian economy. While this field survey and all data reviewed here focuses its specific data gathering (for the most part) on medium- to large-scale enterprises[63] operating in the formal sector, it is important to remember that the vast majority of day-to-day manufacturing activity in Tanzania is conducted by small- and microscale firms operating at least partially within what is often recorded as the "informal" sector. This manufacturing activity is composed of what is essentially handicraft-style production in small workshops or even households. "Handicrafts" is used here in both the more technical sense of craft or artisanal production as well as in the colloquial sense, with Tanzanian respondents consistently categorizing such activities as *kazi ya mikono*, or "work of the hands" and referring

[63] Here, "medium-scale" is relatively small in absolute terms, referring to enterprises that, on average, may only have 10 to 50 workers. While several enterprises operating with fewer employees are also captured here, none have the character of truly small workshops, family businesses or the import-export ventures in the clothing market that employ only a handful of people, including the owner.

to the locale of such production as *karakana* (garage/workshop), *duka* (shop) or simply with reference to the craft producers themselves (i.e. *ma/fundi*, particularly used when referring to textile craft locations) and almost never as *kiwanda* (factory), though periodically the term *ghala* (warehouse) is used for smaller and less formal storehouses as well as larger, more formal facilities. Some of these distinctions will be explained in more detail below, with reference to associated distinctions in the built environment.

These handicraft firms provide not only the conceptual context for an examination of Chinese FDI projects but also define the literal landscape of the field, with expansive clusters of *karakana*-based artisanal production encircling almost every one of Dar es Salaam's more formal industrial districts and melting seamlessly into the residential settlements beyond. In the sites surveyed here, handicraft production was particularly common for basic wood and metal materials, usually with an open, shed-like *karakana* (or similar tarp-covered spaces) facing the street that acted as both shopfront, workspace and product exhibit, with work and storage sometimes spilling out into (often shared) yards behind or, more frequently, onto the street and sidewalk. In sites more distant from the city centers, these forms of production often take place in long strips along major thoroughfares, with finished products exhibited to passing drivers in the open air. Such handicraft production not only dominates entire industrial sectors (such as furniture manufacturing, as documented in Murphy 2013) but also forms the relatively weak *de facto* industrial base for the city's urbanization (as documented in Murphy and Carmody 2019) and has thus far not been prone to economic upgrading or more formal industrialization. This is consistent with other work showing the "premature deindustrialization" (Rodrik 2015) of poor countries, defined in Africa especially by rapid urbanization that has often not been accompanied

by a symmetrical industrialization in the formal sense (Obeng-Odoom 2010, Turok and McGranahan 2013, UNECA 2017).

Instead, the “informal” sector in Dar es Salaam dominates in manufacturing as it does in the economy of Tanzania and sub-Saharan Africa at large (IMF 2017, pp.49-57). The dominance of the informal sector in manufacturing has meant that labor productivity in the country has either stagnated or declined, despite increases in output (Diao et al 2021). At the macroeconomic level, however, the informal sector is better captured in the rising share of service jobs. This is visualized in Figure 5-1, which the sectoral breakdown of employment.

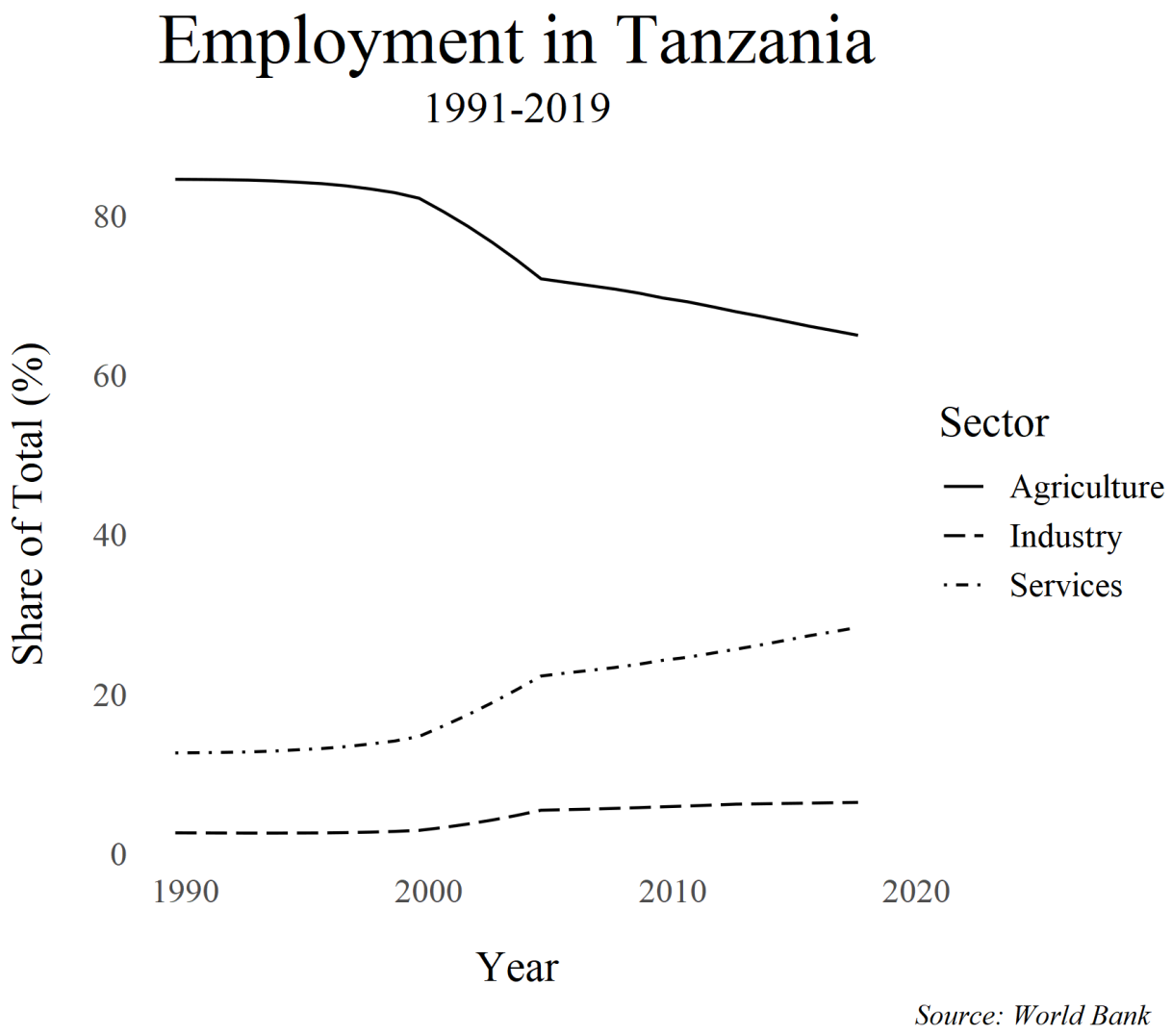



*Figure 5-1*

While both the Lewis Model and Three Sector Model, which form the orthodoxy of development economics, argue that industrial employment should be rising rapidly as agricultural employment decreases, Tanzania exhibits all the signs of "premature deindustrialization" (as identified by Rodrik 2015, and Diao et al 2019 and 2021), with the structural shift to services taking place earlier and at an accelerated rate. Between 2000 and 2019, agricultural employment has

plummeted from 83 percent of the total to 65.1, a decrease of 17.9 percentage points. But manufacturing employment has grown only slightly, from 2.83 percent to 6.47 percent, or only 3.64 percentage points. It follows that the vast bulk of the decline in agricultural employment has been made up for by growth in services, not industry. The service sector, which sat at a mere 14.2 percent of total employment in 2000, had doubled to just under a third (28.4%) by 2019.

Industrial districts in the city have the character, then, of islands and archipelagos of larger, more formal firms floating in a sea of smaller, less formal firms (here including microscale self-employed producers and households) which both encircle formal spaces of production and often seep into the interstices between factory compounds. While there is abundant ambiguity about the exact line past which "informal" or "craft" labor becomes formal manufacturing work, the spatial distinction between the two is a bit clearer than the conceptual one. In the Tanzanian industrial districts surveyed here, more formal firms tend to operate in enclosed spaces within which even production or storage yards are literally walled off from their surroundings. In some cases, such as the two major Special Economic Zones in the city or various warehousing sites, multiple firms enclosed from one another are further sequestered within a larger walled compound. By contrast, informal production clusters tend to be less self-contained, with manufacturing activity taking place in tarp-covered clearings or in sheet-metal sheds or garages often enclosed by only three walls and often spilling out into mixed-use yards, onto the sidewalk or even into the street itself. Thus, the conceptually indistinct categorization of formal/informal takes on something of a vernacular character, since respondents almost universally point to enclosed compounds and concrete buildings when asked about the location of *viwanda* (factories) and tend to refer to informal clusters as *karakana* (workshops/garages) or simply *mafundi* (referring to the artisans themselves). The term *ghala*, indicating a warehouse

space, is slightly more general but tends to refer to the same enclosed compounds and concrete structures as *viwanda*. A similar, though slightly less clear, distinction exists when asking about workers in such industries. While the term *wafanyakazi* (workers) is more inclusive and was loosely applied to both employees at various factories and to those directly engaged in handicraft production, the category of *mafundi* could only be applied to the latter. This survey, then, focused almost exclusively[64] on facilities that could be categorized as *viwanda* or *ghala* and is best contextualized in comparison to other medium- to large-scale firms recorded in the formal corporate sector.

**Formal Industry**

Due to the conceptual difficulty in differentiating between formal and informal as well as the small average size of even formal firms in Tanzania, the exact sectoral distribution of manufacturing among small- and medium-scale firms is poorly captured. That said, a minority of larger-scale firms tends to dominate in many industries—particularly those that are not easily translated into handicraft-style production—and these companies tend to be the ones most consistently recorded in national statistics, proprietary databases and in various other industrial surveys. Of these data sources, the Orbis database, constructed by the Belgian analytics firm Bureau van Dijk, contains the largest set of records on corporations globally, made comparable across economies. The database includes "information on more than 375 million companies and

[64] The most informal firm captured in the survey was a garage in Mikocheni owned and run by a Chinese man who had lived in Tanzania for almost 20 years. This garage was run much more like an informal local firm, with an open yard and visible workspace and engaged in the repair of vehicles, the warehousing of vehicles and parts, as well as the (re-)manufacturing of all sorts of informal vehicles and machinery from these spare parts. Since its primary activity was the service of repairing cars and the warehousing of vehicles and parts, it is listed as a warehousing, not manufacturing, firm in the final database.

entities across the globe," often with detailed financial information recording revenue, debt, and the intricacies of corporate structure and financial flow, including all major shareholders and the "global ultimate owner" of a given firm. The "companies" and "entities" include industrial firms as well as financial ones (not only banks, but also insurance companies and various funds or other non-bank financial institutions) as well as sole proprietorships (81 million) and even marine vessels. Though most detailed for Europe, the database has information on 18 million companies across Africa (Orbis 2020). The data is robust and reliable enough that it has been used to reveal the details of illicit financial flows, the location of offshore tax havens and the more general shape of the global corporate ownership network (Garcia-Bernardo et al 2017).

For all available companies in Tanzania, I extracted information on their industrial sector, main product, revenue, profits, number of employees, location (city and region) and higher-level organizational structure (i.e. main shareholders, global ultimate owners and similar information about these firms, such as sector, revenue and home country)—all recorded for the last available year, which tended to be between 2016 and 2019. In general, the most relevant information recording sector, product, location and number of empoyees, tended to be complete (with only a small handful of NA values that were excluded from further analysis), while balance sheet and shareholder information was less complete. It appears that Orbis data, however, is particularly sparse for Tanzania, with only 278 firms recorded for the entire country, most of which seem to be large-scale companies that would more easily appear in the national economic census and on international registrations. Of these, only a single firm is recorded as having a global ultimate owner or even a significant shareholder located in China, and this firm is unlikely to have much Chinese presence on the ground since its ownership status is the product of a high-level merger. The firm is Swissport Tanzania PLC, which has the contract for the airport's ground and cargo

handling services in Dar es Salaam. The reason this is recorded as a Chinese firm is because Swissport itself was acquired in 2015 by the Chinese HNA Group (海航集团), a large conglomerate originally founded around Hainan Airlines but today better known for its many international acquisitions and equity investments.[65] The Orbis database, then, will not provide much insight into Chinese-origin FDI in Tanzania and seems to be a poor source of information for FDI in poor countries in general. The character of the 278 firms recorded in the database will, however, be useful to illustrate the general composition of the formal corporate sector.

[65] After 2017, HNA's acquisitions slowed substantially, as the company was targeted by the central government for its practice of borrowing at low interest rates from the state-owned banks to fund risky overseas investments. HNA was held up alongside other private firms Wanda and Anbang Insurance as an example of unsustainable corporate debt, fueled by speculation in overseas assets. This crackdown came around the same time as the end of the commodity super-cycle and the announcement of a "new normal" of lower GDP growth in China, all of which contribute to the plateauing of Chinese-origin FDI in Tanzania and elsewhere beginning in the second half of the 2010s.

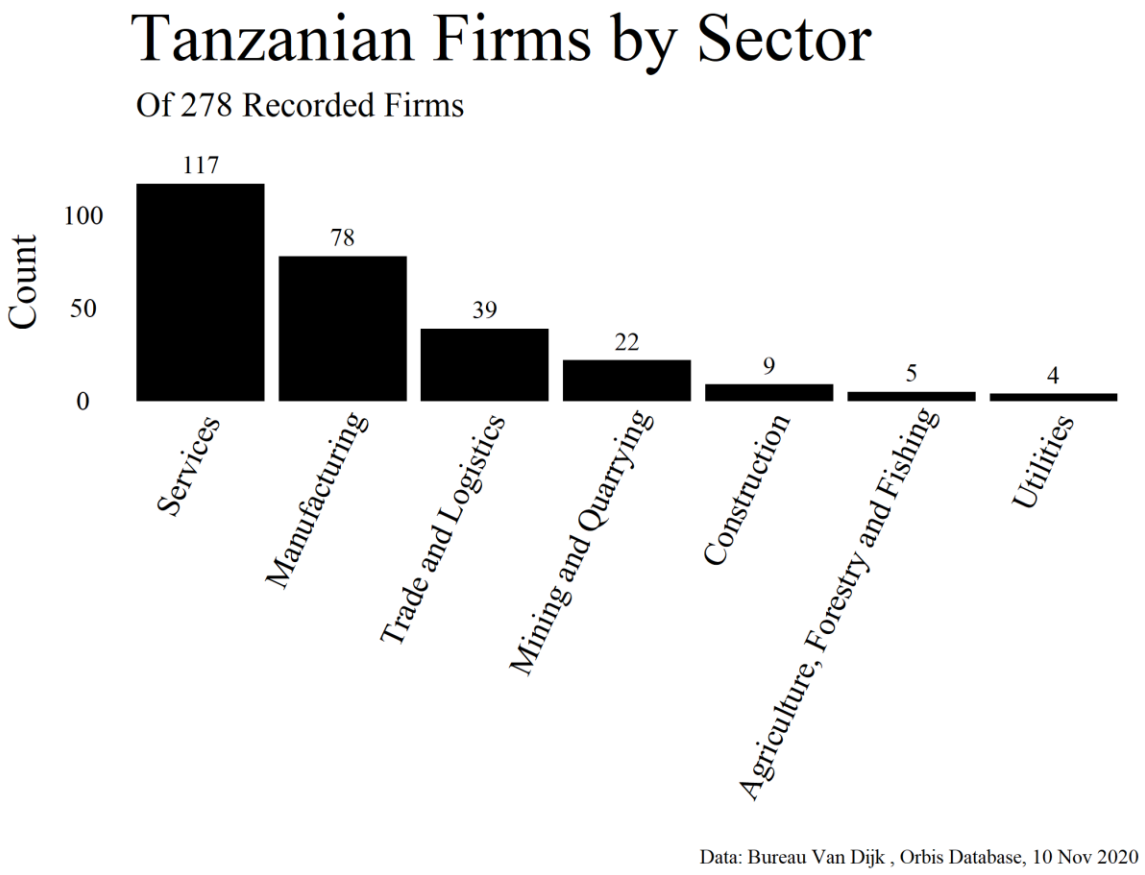



*Figure 5-2*

Of these 278 firms, the majority (117) are within the service sector, followed by manufacturing (78), trade and logistics (39) and mining and quarrying (22). Of the remaining 18 firms, half are in construction and the rest in agriculture, forestry and fishing or utilities. The full breakdown is visualized in Figure 5-2, above. The low share of firms involved in agriculture (5, in total) hints that the database is biased toward urban production and is particularly poor at recording rural businesses linked to agriculture (of which there are many, both formal and informal). Nonetheless, since it includes just as many manufacturing or trade and logistics firms

as service sector firms, it will act as a good summary. More importantly, the geographic distribution of the firms clearly shows Dar es Salaam's continued role as the country's economic capital—and specifically as the capital of formally recorded corporate activity. In Figure 5-3, it becomes apparent that the vast majority (212) of the 278 recorded firms are located in Dar es Salaam.[66] It is followed by Arusha (14) and Mwanza (12), the next two largest cities. All other cities and regions have fewer than 10 firms recorded in the database. This appears to confirm the initial logic of conducting the survey in Dar es Salaam, where the bulk of such firms are most likely to be concentrated.

[66] Here and elsewhere, I will be making use of the formal administrative boundaries of the city. But Dar es Salaam city (*mji*) also acts as an *mkoa* ("region" or "province") in the Tanzanian administrative hierarchy. The city's borders are coextensive with those of the *mkoa*, but the built-up area today only covers the core of this metro-regional administrative area.

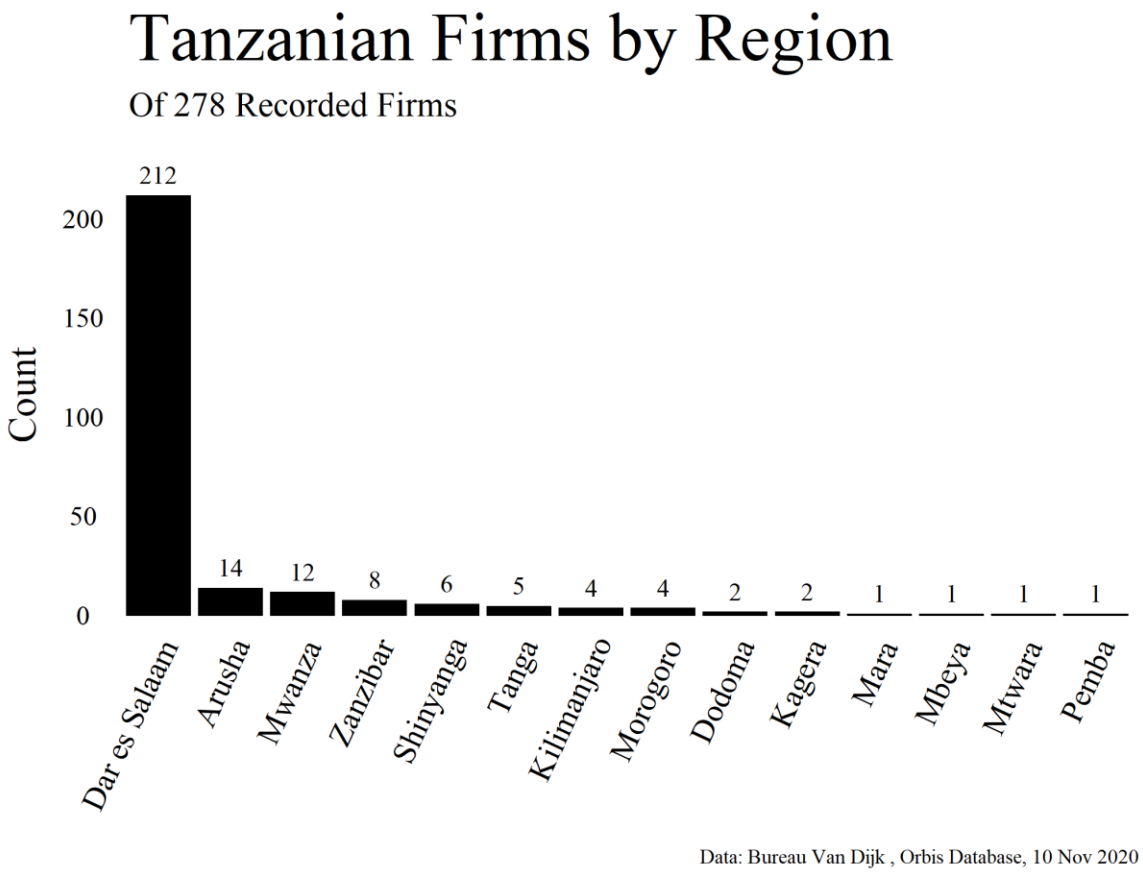

Tanzanian Firms by Region
Of 278 Recorded Firms
Count
212
14
12
8
6
5
4
4
2
2
1
1
1
1
0
50
100
150
200
Dar es Salaam
Arusha
Mwanza
Zanzibar
Shinyanga
Tanga
Kilimanjaro
Morogoro
Dodoma
Kagera
Mara
Mbeya
Mtwara
Pemba
Data: Bureau Van Dijk , Orbis Database, 10 Nov 2020

*Figure 5-3*

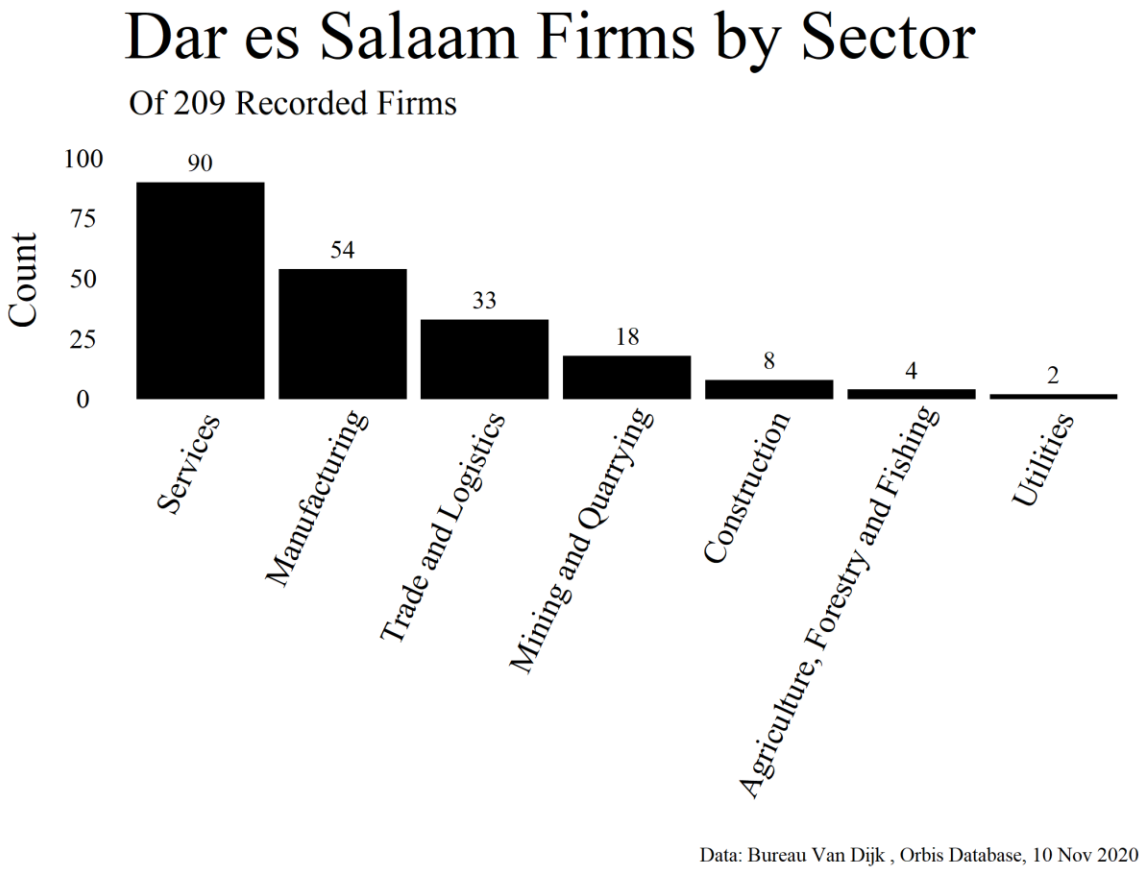



*Figure 5-4*

This also means that it will make more sense to look at the specific concentration of firms within Dar es Salaam itself. The sectoral breakdown (Figure 5-4) for all Dar es Salaam firms (technically 209 of the 212 total Dar firms, since three had no sector recorded) essentially matches that seen at the national level, which services dominating the other sectors. Even mining firms appear here, though their operations might be elsewhere, since they are large international conglomerates that have local headquarters in the city. In order to capture a rough breakdown of international ownership, Figure 5 shows the recorded home countries of the firm's "ultimate

owner" as recorded by Orbis. This covers 105 of Dar's 212 firms, with the remaining 107 presumably Tanzanian (though for scale and readability, they're not added to the TZ column here). Of these 105, almost half (48) have ultimate owners registered in Tanzania. The most prominent foreign owners are registered in Great Britain (9 firms), Kenya (8) and South Africa (8), with a small handful registered in India (4), the US (3) and the Virgin Islands (3) as well. Essentially, this shows the persistent influence of both older colonial geographies and newer circuits of international finance: on the one hand, the prominence of British, Kenyan, Indian and even South African investors all trace out the same basic nexus of capital that existed in the colonial era, which established integral links between Britain and its colonies. On the other, the contemporary structure of global finance is visible in the role played by various tax havens which, if combined, account for as many firms (9) as Great Britain.[67]

[67] Here, Mauritius and Togo, two less common tax havens, are included in the measure alongside the Virgin Islands, the Netherlands and Luxembourg.

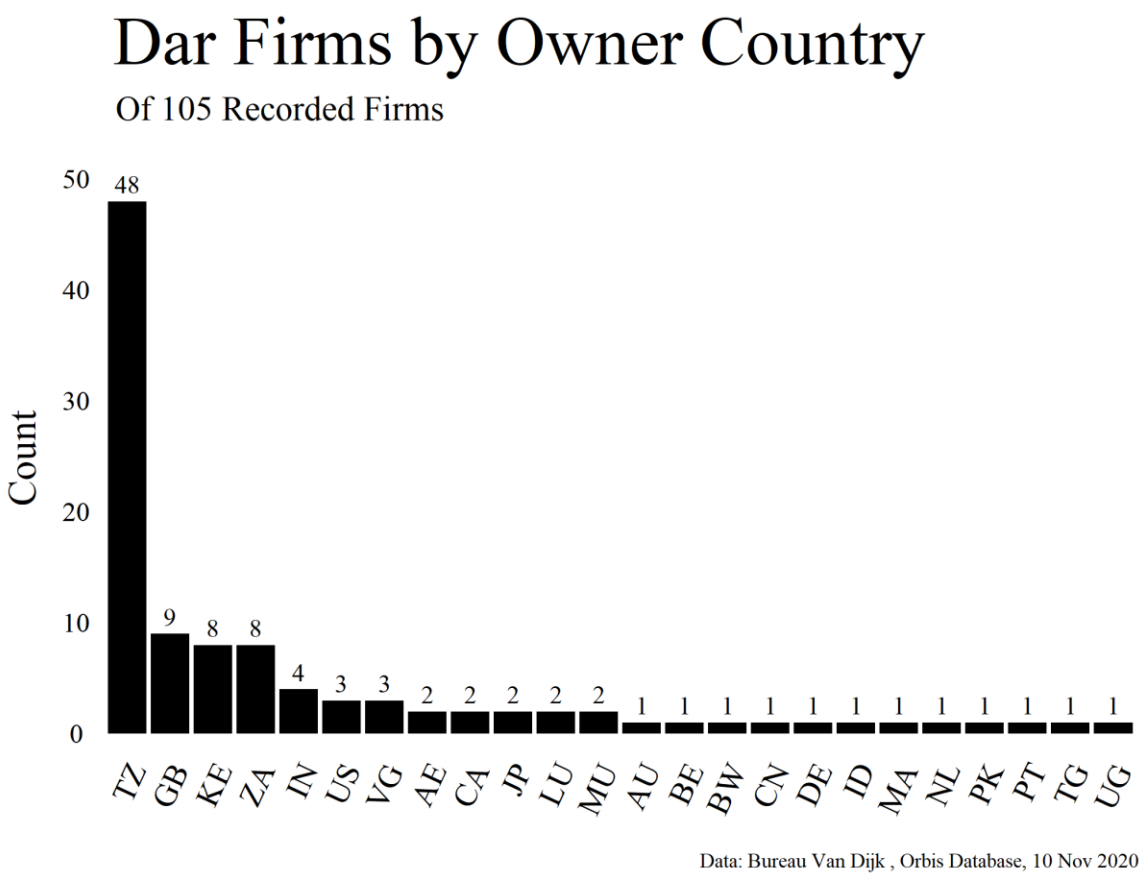



*Figure 5-5*

Only one Chinese firm is recorded here, allowing for little direct comparison between the survey results and the Chinese investment visible in the Orbis database. Thus, it will be most useful to focus instead on the 54 Manufacturing and 33 Trade and Logistics firms altogether, regardless of their final owners. Since basic information such as main product and number of employees are recorded in the field survey, it will be helpful to explore the same categories for this subset of non-Chinese firms, the bulk of which are owned by Tanzanian capitalists. In Figure 5-6 we can see the breakdown by main product (warehousing here, however, is simply recorded as

"transport, freight and storage" with no further detail), the most common being food and tobacco (19 firms), followed by chemicals and various petroleum products (11). Fewer than 10 firms are recorded as operating in all other product lines, though "Leather, Stone, Clay & Glass" as well as "Metals and Metal Products" together account for 12 firms. The most distinct divergence between the formal activity recorded here and the real economic activity on the ground in such industrial districts is clearly the low number of firms in printing, textiles, clothing, wood products and furniture, all of which are common industries for less formal *mafundi*. A roughly similar pattern is visible in Figure 5-7, showing the sum of employees in all firms operating in different product lines. Food & Tobacco Manufacturing dominate, with 21,020 employees, followed by the entire warehousing sector, with 9,656. The position of warehousing and the chemical and petroleum products line have, however, flipped, with the latter mobilizing more labor in absolute terms.

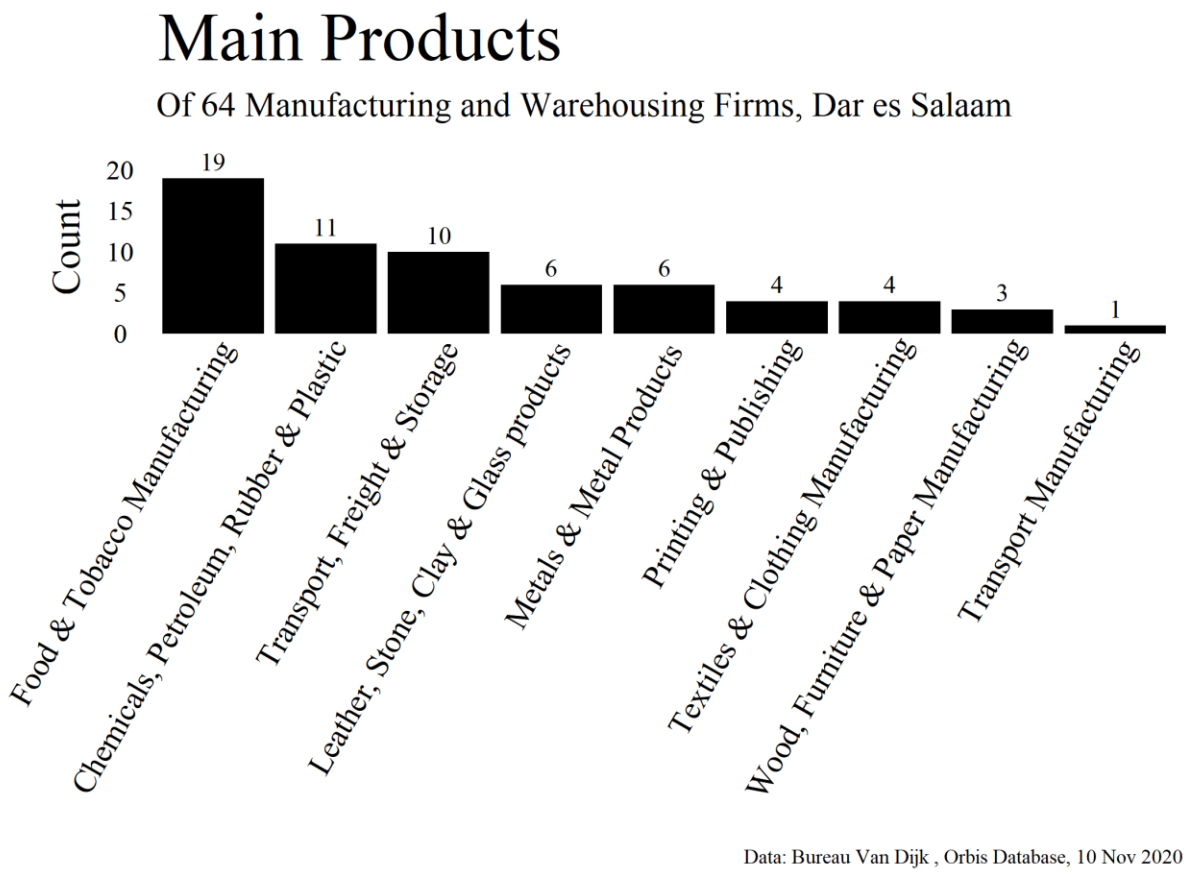

Main Products
Of 64 Manufacturing and Warehousing Firms, Dar es Salaam
Count
20
15
10
5
0
19
11
10
6
6
4
4
3
1
Food & Tobacco Manufacturing
Chemicals, Petroleum, Rubber & Plastic
Transport, Freight & Storage
Leather, Stone, Clay & Glass products
Metals & Metal Products
Printing & Publishing
Textiles & Clothing Manufacturing
Wood, Furniture & Paper Manufacturing
Transport Manufacturing
Data: Bureau Van Dijk , Orbis Database, 10 Nov 2020

*Figure 5-6*

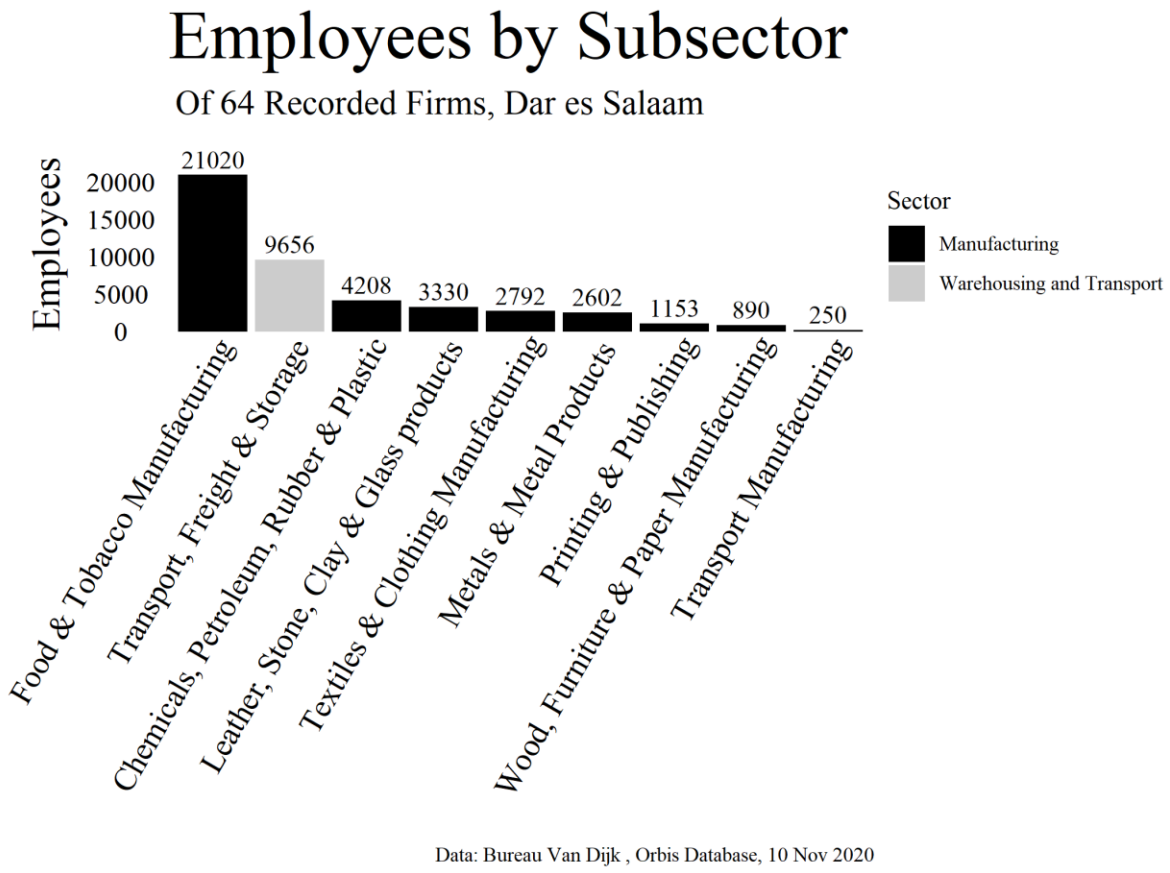



*Figure 5-7*

Finally, a loose sense of labor deployed per firm can be obtained by dividing the total employees in each product line by the total firms in that line, as visible in Figure 5-8. With so few firms in many lines, this is a somewhat volatile measure. Nonetheless, it serves for better comparison with the results of the field survey, which attempts to record the number of employees at individual production sites. The largest value here is the smallest in the measure of firms: 250 employees are recorded as working at the sole Transport Manufacturing company, Superdoll, which produces trailers. After this, we see that the "Textiles & Clothing" line employs the most

people per firm (174.5) and that "Chemicals, Petroleum, Rubber & Plastic" employs the fewest (34.78). Meanwhile, "Food & Tobacco Manufacturing," with the highest number of employees in absolute terms, also has a high number of total firms and thus sees a low ratio between the two (58.23).

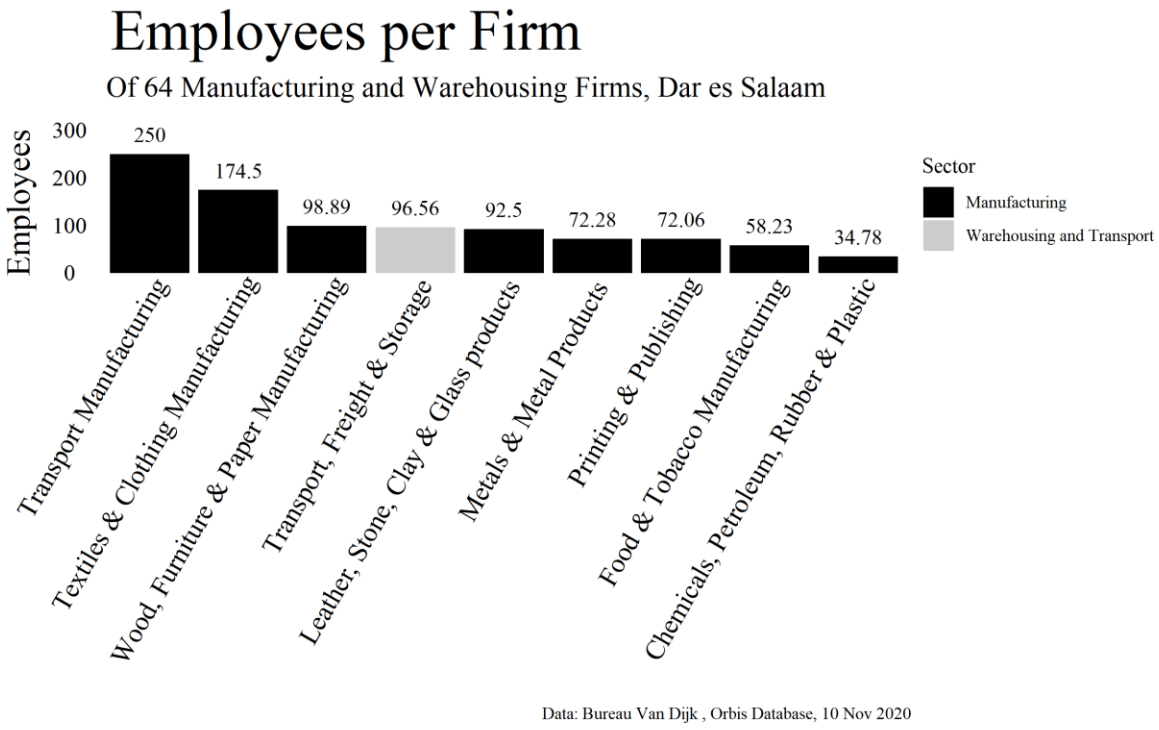



*Figure 5-8*

Overall, these figures give some insight into which sectors might be expected to employ more workers, while also further emphasizing that the Orbis data appears to only capture large firms. There is good reason to believe, then, that these patterns might change once the scale shifts to

focus on the small- or medium-sized enterprises found in the survey. The Orbis data will be useful as a point of comparison precisely because of these differences, demonstrating the deeper divergence between firms that tend to be measured by official statistics (here the best available formal data on firms in general) and those that can be identified on the ground. Maybe equally significant is the general lack of Chinese firms recorded in the database in the first place and the complete lack of any records for Chinese manufacturing firms operating in the country. This omission will be a recurring phenomenon.

**Chinese Investment and Construction Contracts**

By far one of the most frequently cited sources of data on Chinese FDI, acquisitions and construction contracts globally is the China Global Investment Tracker (CGIT), produced by Derek Scissors with funding from the American Enterprise Institute and Heritage Foundation. Both of these are conservative think tanks with very clear political interests. The American Enterprise Institute receives funding from right-wing industrialists such as the Koch brothers and its many affiliates work closely with US defense interests, including the CIA (Brulle 2013). The Heritage Foundation is more openly conservative and has long been an influential force in American politics, most recently giving recommendations that were used to staff the Trump administration (Kopan 2017). Formerly the senior Asia research fellow at the Heritage Foundation, Scissors himself is a conservative economist who has previously worked in corporate intelligence and for the Department of Defense (AEI 2020). He has long been a China skeptic, authoring papers predicting economic collapse or stagnation and the end of pro-market reforms, all of which have been consistently proven wrong by subsequent events. This means that he is of the school that treats Chinese economic statistics as fundamentally incorrect, despite

substantial evidence to the contrary for many measures, especially in recent years (for an overview, see: Orlik 2014). More recently, also saw him join other rightwing conspiracy theorists to stoke the myth of a large-scale cover-up of coronavirus-related deaths in China (Scissors 2020b and 2020c).

In most regards, this makes the CGIT database a deeply unreliable source of data since it creates a structural incentive for the database to exaggerate the influence and magnitude of Chinese investment in order to justify the New Cold War discourse popular among the conservative interests that fund the project. At the level of presentation, the data is already clearly intended for this purpose since it fails to scale Chinese influence relative to similar numbers for other countries—there is no comparison, for instance, to the magnitude of American investment in the same host economies, even though such comparison is standard in other data sources (such as that of the China-Africa Research Initiative, explored below). Similarly, many of the reports released by Scissors are not peer reviewed papers nor even working papers being prepared for peer reviewed publication. They are for the most part little more than blog entries written for the American Enterprise Institute, with Scissors as the sole author. Thus, the claims made in them are not subject to professional scrutiny. Regardless, these sources of funding and institutional support from the American defense establishment have ensured that CGIT data have proliferated widely in the news media and even been cited widely by well-known China scholars (as in Naughton 2018). The CGIT is among the most cited sources of data on Chinese outward investment and was, for this reason, included alongside UNCTAD, OECD, MOFCOM and BEA databases as one of the major sources of such data in a recent Congressional Research Service report on the topic (Schwarzenberg 2020). But while UNCTAD and the OECD only record gross outward investment and the BEA only records inbound investment from China within the US,

MOFCOM and CGIT have been the only sources consistently recording outbound investment at the bilateral level.

Thus, the existing CGIT data on Chinese investment in Tanzania will be a necessary point of comparison, but must be used critically. The basic problem with the database is to be found in the way that the data is gathered: it is based at least in part on reports of prospective contracts (rather than completed projects) and is therefore particularly prone to inflation through the same financial mirage effect identified above—although there does appear to be some attempt to retroactively update the dataset once projects are formally cancelled or substantially modified. Overall, the ultimate source of the data is opaque. As described by Scissors, the CGIT "uses corporate sources, sometimes partners but often the Chinese participants. Transactions are often disclosed by these firms then revised, requiring CGIT revisions, which occur biannually" (Scissors 2020a, p.2). This means that the most recent data may be the least accurate, especially when numbers are low on average. Keeping this in mind, according to Scissor's own most recent report "the CGIT contains investment and construction transactions worth $100 million (rounded) or more. There are no trade, lending or bond transactions" and investment is specifically "measured as gross outlay, thereby matching the practice of China's Ministry of Commerce" (ibid). This means that the dataset is both a) not capturing the smallest-scale investments, which tend to be un- or under-reported anyways, and b) not counting purely financial relations or trade transfers. The dataset divides "investment" from "construction contracts and offers a third series on "troubled transactions" which notes "when sales incur losses" (ibid) and will not be used here.

The scope of the data currently stretches from 2005 to 2019 and it "contains more than 1,600 investments worth $1.2 trillion [and] 1,700 construction projects worth more than $800

billion" (ibid). In addition to the year (and month) and estimates of the amount invested, each entry in the dataset includes information on the host country, the Chinese firm(s) involved, the sector and subsector of the investment, whether or not it was a "greenfield" investment, whether or not the host country has signed on to the "Belt and Road" initiative and, sometimes, the name of the local partner and the share owned by the Chinese firm. In the years for which comparable data is available from MOFCOM, the two datasets seem to move in unison even if the exact numbers differ—with CGIT data exceeding MOFCOM records of "base" investment (i.e. with reinvestment stripped out) in most years (ibid, Tables 1 and 2). More significant is the fact that the CGIT database at least attempts to trace investment through to its final source, thereby reducing the measurement problem posed by the role of Hong Kong and other financial centers. In contrast to MOFCOM methodology, in the CGIT dataset "Hong Kong is not treated as an external customs port" (ibid, p.4). This is significant because "Hong Kong is said by MOFCOM to receive well over half of Chinese outbound spending" even though these "funds actually flow through Hong Kong to final destinations" (ibid). Overall, the CGIT data begins to diverge more substantially after 2018, since "the average size of transactions recorded in the CGIT has been dropping since late 2018," likely because "more transactions are falling below the $100 million threshold and not being measured" and thus "from late 2018 through late 2019, the CGIT is only a subset of total Chinese investment overseas" (ibid, p.4).

For Tanzania, the CGIT data is particularly sparse. Only two projects are included in the "investments" table, with 24 in the "construction contracts" category, making for 26 total records. In order to fact check these, I cross-referenced the dates, quantities, sector and subsector as well as the transaction parties (Chinese and local) with news reports, Tanzanian investment records (held by the Tanzanian Investment Center), MOFCOM reports, the listed projects of

Tanzanian utilities and roads agencies, as well as evidence on the ground in Dar es Salaam. Since the CGIT data does not include a name or any detailed notation for its individual entries, this was often a guessing game where a given project was triangulated using multiple sources and at times could not be identified at all. That said, the small number and high publicity of such projects in any given year frequently made them easy to identify and to trace through time. This allowed me to produce a rough ranking of these projects, categorizing them into one of five categories based on their progress, with one category (Unknown) reserved for projects that I was unable to identify:

> **Cancelled** – The project has clearly been cancelled and no work was ever done (or the contract was given to a non-Chinese firm, but usually in those cases it either isn't completed or is not even listed)
>
> **Unlikely** – No work has been done yet and it seems unlikely that the project will ever begin
>
> **Indefinitely Stalled** – Some work has been done but it has now been stalled for at least a year
>
> **Likely** – Reasonable to assume this project will at least start or work has already begun.
>
> **Completed** – Project has been completed
>
> **Unknown** – Unclear what this project is and unclear of the status

The projects can then be broken down by rank, sector and year, as is visible in Figures 5-9 through 5-11.

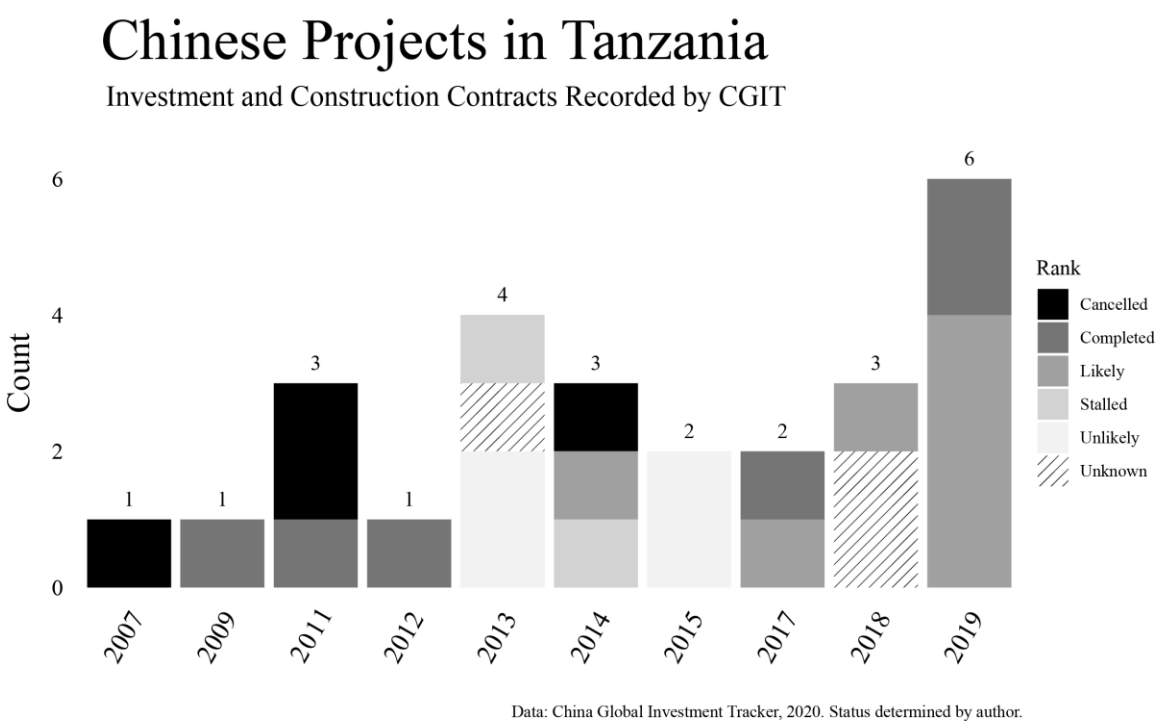

Chinese Projects in Tanzania
Investment and Construction Contracts Recorded by CGIT
Count
0
2
4
6
1
1
3
1
4
3
2
2
3
6
2007
2009
2011
2012
2013
2014
2015
2017
2018
2019
Rank
Cancelled
Completed
Likely
Stalled
Unlikely
Unknown
Data: China Global Investment Tracker, 2020. Status determined by author.

*Figure 5-9*

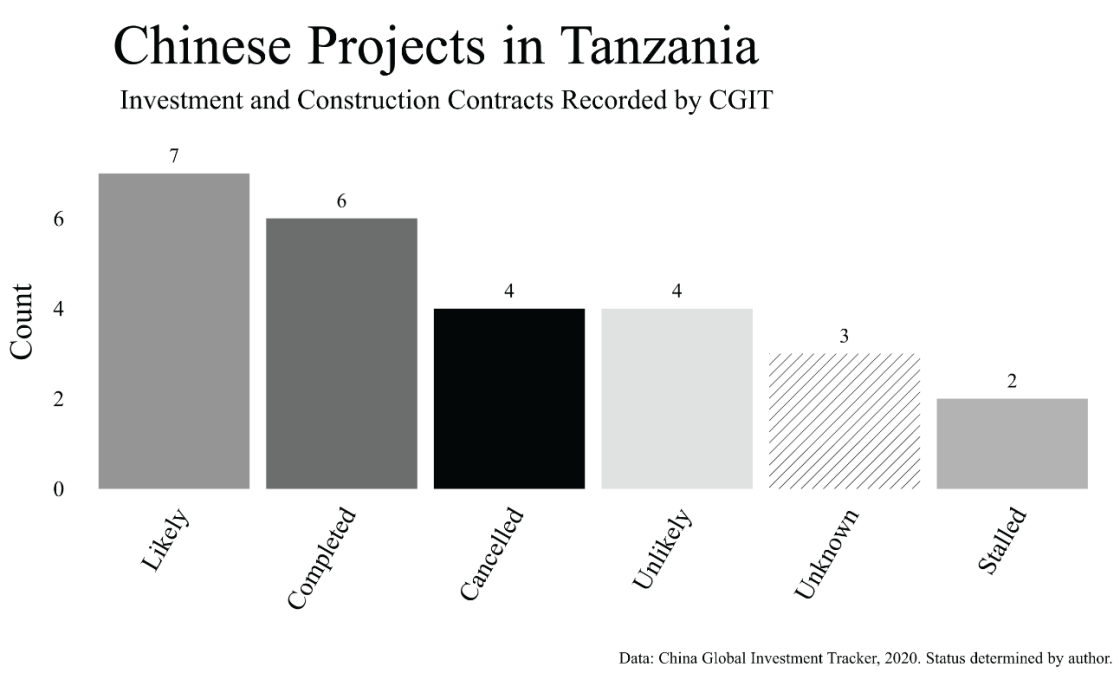

Chinese Projects in Tanzania
Investment and Construction Contracts Recorded by CGIT
Count
0
2
4
6
7
6
4
4
3
2
Likely
Completed
Cancelled
Unlikely
Unknown
Stalled
Data: China Global Investment Tracker, 2020. Status determined by author.

*Figure 5-10*

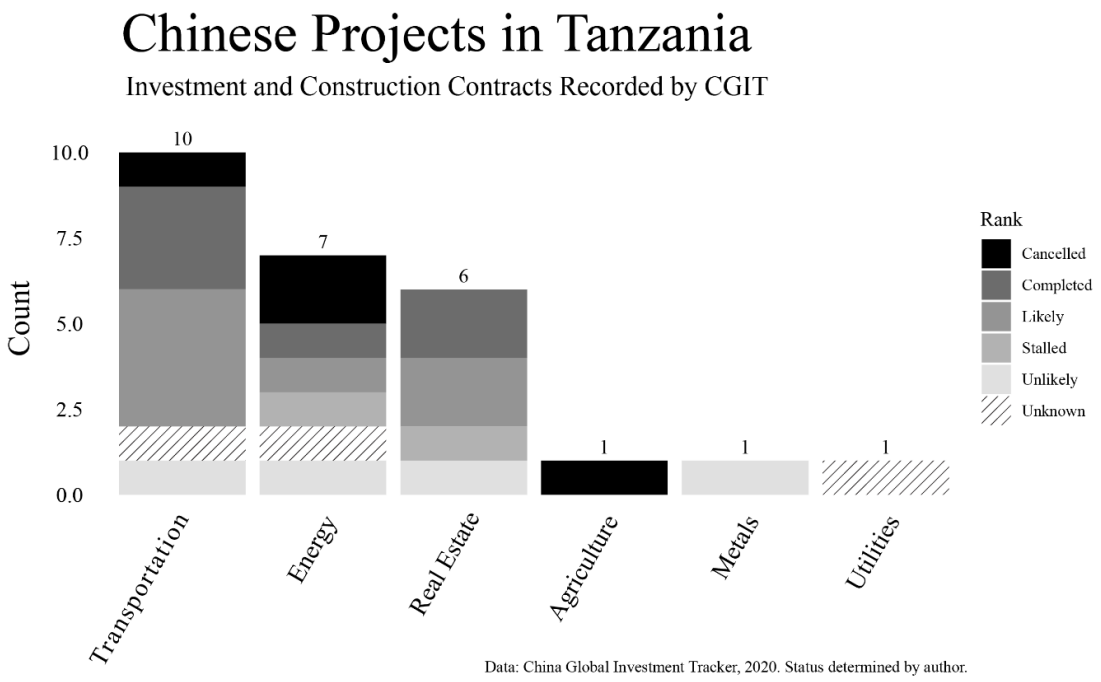



*Figure 5-11*

In total, cancelled, unlikely and stalled investments compose 10 of the 26 total records, while likely or completed projects make up 13, with the remaining 3 unidentified. Fewer projects are found the farther back we go in the database and many in these early years have been since been cancelled (4 total, all from 2014 and earlier) or completed (3 completed between 2009 and 2012). Interestingly, 2019 shows a substantial number of projects and runs against the more general trend in the CGIT dataset overall, which saw falling numbers in 2018 and 2019. More importantly, all projects listed for 2019 are either already completed or are likely to be completed. This could hint that records are improving, at least for Tanzania. But it is more likely

that this tracks a real decrease of speculation, since both China and Tanzania saw prominent anti-corruption campaigns begin to escalate in 2014 and 2015 leading to widespread cancellations of outstanding projects and several thorough corporate restructurings. In the years since, it seems that there has been more emphasis from both sides on selecting projects that will actually move forward.

Through all years, the vast majority of the projects recorded by CGIT are in Transport (10) Energy (7) and Real Estate (6) and, of these, further research showed that road and bridge building are the most common Transport projects, with Real Estate mixed evenly between commercial and residential use. Energy is more volatile, with a one key projects completed and one likely, but far more cancelled, stalled or unlikely. In other words, the bulk of Chinese investment activity in Tanzania recorded by CGIT is associated with the construction of basic infrastructure, and transportation and real estate projects tend to be the ones that get completed. That said, the two energy projects either completed or likely to be completed will have an outsize influence compared to other infrastructure. Probably the most significant single Chinese-contracted infrastructure project completed to date was one of these two Energy projects: the Mtwara-Dar es Salaam natural gas pipeline, announced in 2011 and completed in 2015, despite protests against it in 2013 (Reed 2020, GEM 2020a). The pipeline was funded by an Exim Bank loan and built by CNPC. Its affiliated power plant project, also recorded in the CGIT database, was ultimately cancelled—this was to be a joint venture between Sinomach and Siemens, also funded by Exim Bank, but seems to have never gotten past the planning stages (Bofin et al 2020 and Ng'wanakilala 2011). Nonetheless, this was a key project in Tanzania's energy infrastructure and it was essential to the further electrification of Dar es Salaam in the midst of breakneck urban growth.

The second energy project is even more integral to Tanzania's ongoing electrification and even more controversial: this is the Rufiji / Julius Nyerere Hydropower Station, set to be the largest power station in East Africa upon completion. The contract for the project was awarded to two Egyptian firms (Arab Contractors and Elseweddy Electric Company) who then subcontracted major portions of the work to PowerChina's subsidiary, Sinohydro. Funding is intended to come entirely from the national budget, though the Egyptian contractors themselves have taken out additional loans from the African Exim Bank, headquartered in Cairo (Zhang 2019). Whereas the Mtwara-Dar es Salam gas pipeline saw local protests against its planned distribution of income (the bulk of which would go to wealthier Dar, with a small fraction left in much poorer Mtwara), the Rufiji Dam project has met with international opposition due to its location in the middle of the Selous Game Reserve, a World Heritage site which risks losing that status due to the project. Meanwhile, construction began prior to the completion of the environmental impact studies (IUCN 2019). Nonetheless, the project is seen as a cornerstone for the Magufuli administration (which one its second 5-year term in 2020) and is central to the administration's goal of widespread electrification. Work has already begun, and the project is likely be carried through.

In terms of manufacturing, only two projects appear as investments in the CGIT data, though a third listed as a construction contract should be included here as well. The first is a 2015 project associated with Sichuan Hanlong, listed at 2.7 billion USD in the Metals sector. This is almost certainly the Mchuchuma Power Station, Mchuchuma Coal Mine and Liganga Metallurgical complex. Originally proposed in 2011, this was to be a joint venture project in the country's south between Sichuan Hongda (a subsidiary of Sichuan Hanlong) and the Tanzanian government. Work was supposed to begin in 2013, with the project completed in 2015 (this is

likely why it's listed as a 2015 project in the database). Negotiations went forward but no work seems to have actually been done on this project and even the feasibility studies remain unfinished (GEM 2020b, Machira 2020 and PPRA 2020). The original proposal came on the eve of a massive corruption scandal for Sichuan Hanlong, which resulted in the arrest (in 2014) and ultimate execution of the firm's chairman, Liu Han (in 2015), and which left a number of the firm's planned investments stalled indefinitely (Murray 2015). A similar series of corruption scandals rocked Tanzania in the same years, resulting in the ascent of current president John Magufuli on an anti-corruption platform which included wide ranging review and cancellation of proposed projects. Even though the Mchuchuma complex has not been formally cancelled it seems unlikely to go forward. That said, there are other ventures in the same region designed to exploit the same coal and iron ore reserves that have moved forward, such as the Mbeya Coal to Power project, a joint venture between the Irish firm Kibo Mining and Chinese SEPCO3, which has not begun work but has at least been approved for a license (Kibo 2020). Despite SEPCO3 being awarded the contract for the power plant and power transmission lines, this investment remains unreported in the CGIT dataset.[68]

The second recorded investment is actually a simple acquisition: Huaxin cement's 2019 purchase of Maweni Limestone (located in Tanga) from the heavily indebted Kenyan cement firm ARM. This was mostly just a straightforward acquisition of an existing plant. Initially there were plans for a substantial expansion of the current facilities, but these seem to have been

[68] It seems likely that a contract entailing the construction of an entire power plant and its transmission line would exceed the 100 million USD cutoff for the CGIT data. It might be excluded because work has not begun yet—though plenty of other investments were recorded prior to breaking ground in the past—or it could be listed elsewhere. The most natural assumption is that the project would be recorded in Ireland, since it is a subcontracting arrangement with an Irish firm. However, of the 6 total records for investment (there are none for construction) for Ireland, this project is nowhere to be found. Meanwhile, it seems that no contracts for SEPCO3 have been recorded in the database in any year or any country.

scaled back. As of July 2020, the new owners began trial production. A similar but far larger investment recorded in the dataset for 2019 (seemingly misrecorded, since it's listed in the Real Estate sector and as a mere construction contract) is another cement factory that's been approved by the Tanzanian government and is in the early stages of land acquisition—but has not yet been built. This factory is also planned for Tanga, however, which has an existing agglomeration of cement firms and associated infrastructure. The facility (which includes a small, linked power plant) will be constructed and owned by Sinoma Cement and its subsidiary Hengya Cement. Set to be the largest cement factory in the country, production will easily overspill domestic demand and the facility is thus not intended to produce primarily for the local market (an estimated 30% of its output) but instead for export to Kenya, Somalia and Mozambique (an estimated 70% of output). Sinoma is itself a subsidiary of China National Building Materials (CNBM), the biggest cement and gypsum producer in China. CNBM already has an established relationship in Tanzania and, in 2018, opened a large office headquarters and modern building materials warehouse in Dar es Salaam through another subsidiary, Beijing National Building Materials (BNBM). This warehouse is picked up in the field survey.[69] Given CNBM's established status, its preexisting warehousing and wholesale infrastructure and the formal approval of the cement factory, it seems likely that the planned Tanga factory will move forward.

A third cement firm unaffiliated with either Huaxin or CNBM was picked up in the field survey (Juye Cement Company — 巨业商混公司), with is production facilities located in Mikocheni in Dar es Salaam. Altogether, Juye's new facilities in Dar, alongside the Huaxin

[69] It's also worth noting that the BNBM warehouse was the most modernized of all the Chinese investments found in the survey, marked by brand new construction of three different enclosed concrete buildings (in a single compound complete with a basketball court): an office building, a dormitory and the warehouse itself. In its level of modernization, it was followed by the Ju Ye Concrete Company, which had installed several large freshly-painted storage tanks and had a fleet of new cement trucks, though its office facilities were simple, single-story buildings.

acquisition and CNBM's planned factory, both in Tanga, are significant because all are part of a larger trend of Chinese expansion in the production of cement in Africa helping to shift the nature of dependence in the product line away from reliance on imports from China and toward dependence on local production that is nonetheless Chinese-owned. In an interview for CCTV-4 (Chinese International Channel),[70] the head of CNBM's East African investments, Xiao Yuenan, explains the basic pull factors for the building materials industry in general in Tanzania. Prior to the entry of CNBM (in 2006) into the Tanzanian market, Xiao explains, the building materials sector was controlled by Indian and Pakistani firms and remained relatively undeveloped (every ton of steel materials turned a profit of only 300 to 500 USD, according to Xiao) while the cement industry was monopolized by a single German firm:

> Do you know to what degree cement was in short supply back then? At that time, there were maybe only two cement factories in Tanzania. They were built by Heidelberg Cement Group. Do you know how powerful their position was? If a company needed cement for its project you would have to give their salesman something like a tip (像小费这样) so that they would deliver goods to you in the first place. Some of them even said: "Don't tip me now because no goods are available at the moment."[71]

[70] The video, produced by the "Belt and Road" production team, was broadcast on 27 October 2017 on CCTV-4 and is archived on the portion of the BNBM website showcasing their work in Tanzania: <http://www.bnbmg.com.cn/en/tanzania.html>

[71] The translation here is primarily the pre-existing translation offered by CCTV-4, visible in the video. A few slight corrections have been made by the author to better capture the original Chinese or fix simple grammar mistakes. The term "tip" is accurately translated in the original, with the Chinese term being 小费, used to denote tips to waiters or service staff, but the CCTV translation loses the implication of the original, since Xiao doesn't say that these *were* tips but instead that they were "something like a tip" (像小费这样). The implication, of course, is that these were bribes. Xiao is using a polite phrasing here, however, rather than the formal (贿赂) or pejorative/colloquial (红包) terms for bribe.

Such conditions created a strong incentive for the establishment of an import infrastructure for building materials since rapid urbanization and the more general development of the road and energy systems had begun to stimulate local demand. Tanzania is, in fact, one of the few parts of the world with strong demand despite general global overcapacity (Perilli 2020b). This induced CNBM's entry into the market, which began in its role as an importer, via its subsidiary BNBM. It is in this capacity that the BNBM warehouse in Dar es Salaam is recorded in the field survey below.

But this is also occurring amid severe overcapacity in the cement industry as a whole and in the Chinese market specifically. While the pull factors for producers in Tanzania and elsewhere in sub-Saharan Africa are salient, the push factor of overcapacity in China itself is likely the real driving force behind the new agglomeration of Chinese-funded cement production in Tanzania. It is a widely cited statistic that China used more cement in just 3 years of its infrastructure-focused stimulus program (6.6 gigatons between 2011-2013) than the US did in the entire hundred years from 1901-2000 (McCarthy 2014). But the unspoken consequence of this is that Chinese cement producers quickly hit a saturation point in their local market and, in part because the Chinese market composes such a large share of the global market (50% in 2016), this led to extreme overcapacity in the industry worldwide (Cui 2016). For CNBM, this led to a massive 83% slump in profit in 2015, with their capacity utilization rate a mere 63% in 2017 (Global Cement 2019). Within China, these conditions created an impetus for further automation[72] and a focus on production quality rather than sheer quantity, with the expectation

[72] This is within an already heavily automated industry. In the 1980s, a typical cement production line (at a firm producing about 4,000 tons per day) had about 3,000 employees. By 2015, that number had fallen to a mere 300, with many firms already implementing new innovations that enabled them to operate with as few as 100 employees (Cui 2016).

that the years 2015-2035 would see the shuttering of uncompetitive firms accompanied by further monopolization of the remainder (with the 4200 cement-related enterprises operating in China in 2015 reduced to something like 1200 by 2035) and a general reduction in capacity (Cui 2016).

These expectations have, thus far, largely been realized. The domestic cement sector has seen wide-ranging closures and acquisitions, the most significant of the latter being the 2018 CNBM-Sinoma merger, which is what enabled the massive new plant planned for Tanga. At the same time, the shuttering of obsolete plants and the further monopolization of the industry has also enabled the remaining firms to turn their attention outward. Faced with new competition in the global cement market (namely from Vietnam, which filled the void left by the decline in Chinese exports) and persistent stagnation in domestic demand, Chinese firms have had to direct their attention to the areas of the globe that have few local competitors but much room for prospective growth (Edwards 2020, Perilli 2020b). In late 2017, CNBM announced its intention to "build 100 new plants in 50 countries by 2021" (Global Cement 2019). Including joint ventures, Chinese companies have been involved in 49 investment projects in the cement sector worldwide since 2018, with the majority located in sub-Saharan Africa (8) and Central Asia (7), followed by South Asia, Southeast Asia and North Africa, with 6 each (ibid). For Tanzanian industry, this phenomenon is significant because it demonstrates one of the first real cases of Chinese firms shifting from an intermediary role as importers to a more direct role as manufacturers. This has not only occurred in cement, but in building materials more generally. Similar expansions have been proposed (by CNBM and Twyford Ceramics/Sunda Group) in the ceramics industry, for instance, and the field survey returned abundant examples of the same trend among sheet metal producers.

Overall, however, it is clear that the CGIT dataset only gives a distant view of these trends. At the same time, the high number of false positives in the data seem to hint that, at least for countries such as Tanzania, the dataset is only representative of real trends in the most general sense—and is particularly inaccurate in more volatile sectors such as energy. Dominated by larger infrastructure construction contracts, the dataset offers less insight into the composition of Chinese manufacturing in Tanzania. It captures only three manufacturing investments at all, one of which has since been stalled so long as to have been effectively cancelled, one of which is misreported as a construction contract in the real estate sector and one of which was an acquisition of a pre-existing facility. The two investments in the cement sector offer useful insights, but CGIT's opaque data conventions meant that these insights were only possible after extensive additional research, aided by the fact that the local BNBM facility (and a third unaffiliated concrete firm) were already recorded in the author's own field survey.

**MOFCOM and The China Africa Research Initiative**

In terms of general FDI flow and stock data that is comparable to that recorded in aggregate by UNCTAD or in detail by national statistics agencies in other countries (such as the US BEA), the primary source for data is not CGIT but instead the Chinese Ministry of Commerce (MOFCOM) and its annual Statistical Bulletin of China's Outward Foreign Direct Investment (中国对外直接投资统计公报). The MOFCOM data has a number of problems and tends to suffer from some similar over-reporting issues seen in other sources (alongside additional over-reporting due to the inclusion of reinvestment in its measures) as well as a particular under-reporting problem, since it consistently records the bulk of outbound FDI as bound for Hong Kong, despite the fact that

the bulk of this is then reinvested elsewhere (MOFCOM 2020).[73] That said, it remains the most intact source and likely tracks relative change over time with some accuracy, even if the absolute amount of investment in any given country may be wrong.

The China-Africa Research Initiative (CARI) at the John Hopkins School of Advanced International Studies (SAIS) is probably the most comprehensive single source of data on economic relations between China and Africa. Not only do they collate (and periodically correct) data on Chinese trade with African nations (derived from UNCTAD) and Chinese investment (derived from MOFCOM and cross-referenced with national statistics when available) but they also pair these datasets with their own records of Chinese loans (more rigorously fact-checked than the CGIT data, see: Brautigam et al 2020), as well as agricultural investments. In addition to this, they release a stream of working papers and policy briefs focusing on particular topics or countries. The most important of these for the case of Dar es Salaam have been a series of Scoping Studies conducted in Kenya and Tanzania by Ying Xia (reviewed below). Before moving on to the results of these more on-the-ground surveys, however, I will review the general investment data for Africa and East Africa specifically, as recorded by MOFCOM and compiled by CARI. This will help to place Tanzania's importance into regional context.

First, to properly scale Chinese FDI into Tanzania the MOFCOM numbers will need to be shown relative to total FDI into the country (regardless of location) and compared to the numbers for other major investor economies. No single source exists for such data, so I have selected out a few key countries for comparison, each of which has, at some point, been a leading

[73] To make the relationship even more confusing, according to Hong Kong's own statistics, aside from investment bound for the mainland much of its outbound FDI then routes through the UK (likely linking to City of London financial networks) or Singapore or tax havens such as the Virgin Islands, Cayman Islands, Bermuda and the Netherlands. See: Census and Statistics Department 2019, Table 4, p.22.

investor in Tanzania: the US, UK, Kenya and South Africa.[74] For global data, I use the FDI flow measure recorded by the World Bank[75] and the FDI stock measure recorded in the UNCTAD World Investment Report (2020). For stock and flow from the US, I used the "country detail" tables on US Direct Investment Abroad from the Bureau of Economic Analysis, which has entries for Tanzania for the years 2000-2019, though these entries are periodically broken by missing years and data suppressed for confidentiality reasons—usually meaning that the year only included one or two investments, easily identifiable (BEA 2020). For stock and flow from the UK, I used data from the OECD (OECD 2020). For stock and flow from Kenya, I used two different sources: first was data from the UNCTAD Bilateral FDI series, which was used for stock data from 2001-2011.[76] For flows, and for stock after 2011, I use the 2018, 2016 and 2013 Foreign Investment Surveys, alongside the 2015 "Preliminary" Foreign Investment Survey, collected by the Kenyan National Bureau of Statistics (KNBS 2018, 2016, 2015 and 2013). The KNBS data are converted from Kenyan Shillings to USD using their respective years' average USD-KEN exchange rate. Since a disjunction appears between the stock recorded by UNCTAD and that recorded by KNBS, I used overlapping years to compute a ratio between the divergent values and used the most conservative version of this ratio to weight the KNBS stock figures, presuming the UNCTAD methodology to be the more accurate and standardized of the two.[77]

[74] I am excluding investment from tax havens here, both because they rarely offer clear data on FDI flows and because there's no particular reason to believe that the relative weights of each country's FDI into Tanzania would change. On the one hand, tax haven investment doesn't appear particularly important in an economy where FDI is so sparse in the first place. On the other, even where it does occur, there's no reason to believe that China would be more or less prone to using tax havens than the US or the UK, though the choice of haven might differ.
[75] Indicator BX.KLT.DINV.CD.WD, downloaded from their API via the wbstats library in R.
[76] This series has since been taken down from its original location on the UNCTAD website but is still available in various secondary databases such as Knoema. The data used here was download in PDF form from the original webpage: <https://unctad.org/en/Pages/DIAE/FDI%20Statistics/FDI-Statistics-Bilateral.aspx>
[77] The weighting caused no major shifts in trend, it just aligned the two disjointed series. The large jump visible between 2015 and 2016 exists in the original KNBS records, recorded in shillings.

For South Africa, I used the same bilateral UNCTAD data for the years 2001-2012 but found no comparable data for the years after. The results are visible in Figures 5-12 and 5-13.

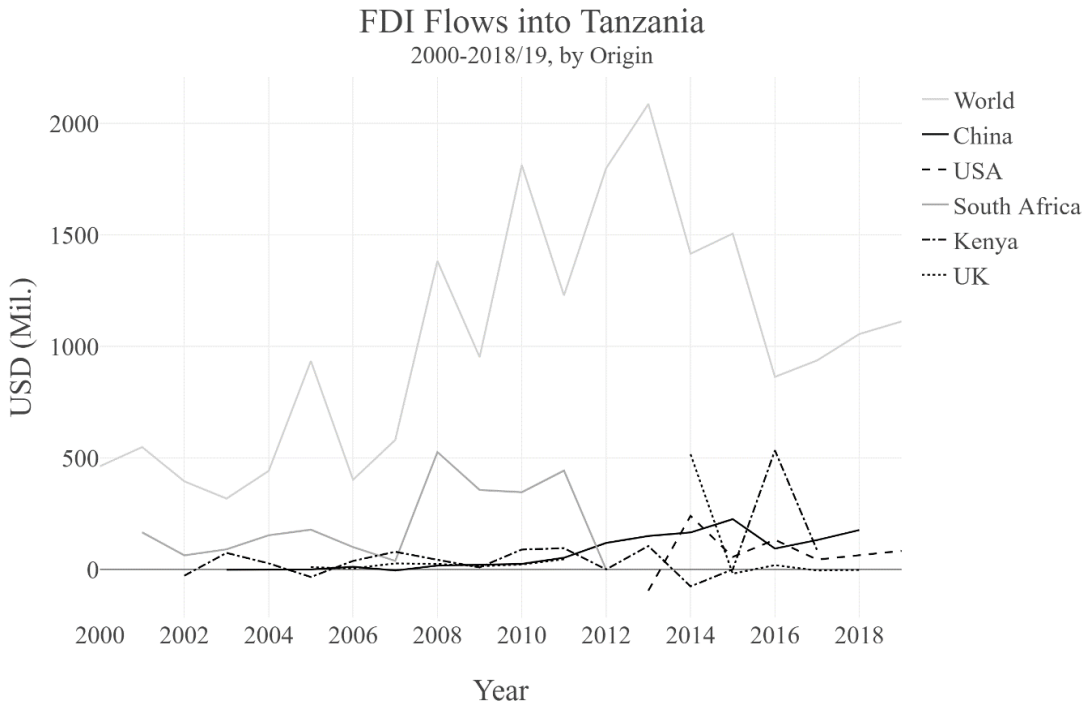



*Figure 5-12*

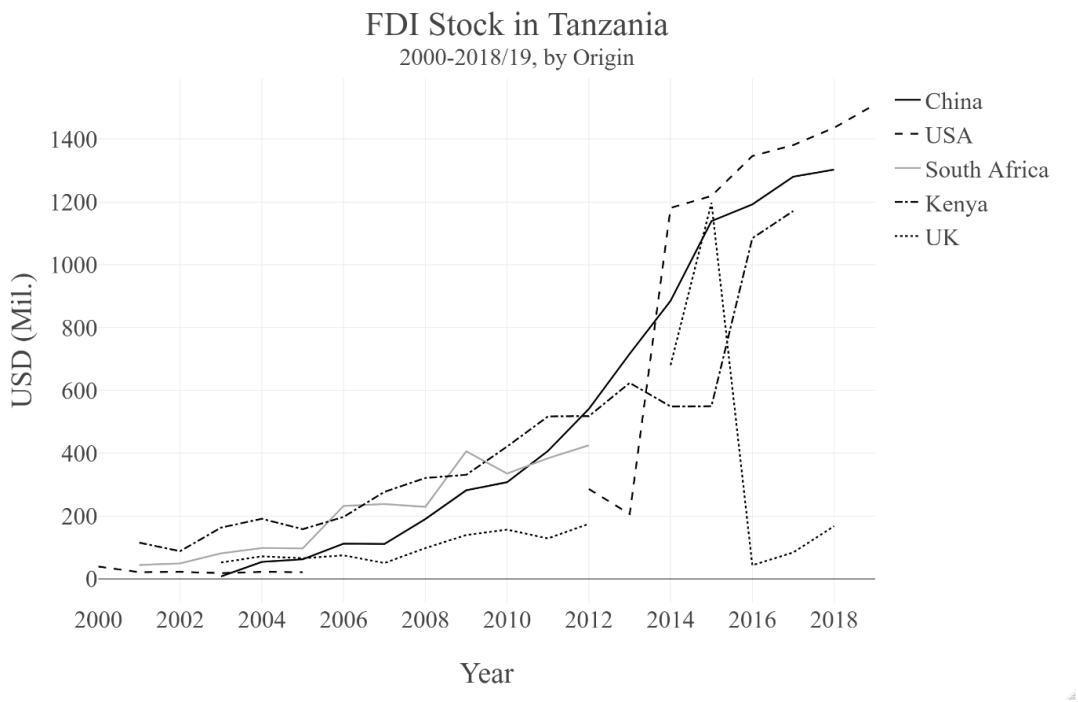



*Figure 5-13*

Flows have been visualized here alongside the sum of inbound flows from the entire world since all are visible on the same scale. Prior to the 2010s, Chinese FDI was negligible and regularly exceeded by FDI from Kenya, the UK and South Africa. In particular, the position of South Africa stands out in these years, with flows spiking in the early 2000s and then again between 2008 and 2011, before dropping to almost zero in 2012 (after which no data is available). Throughout the 1990s and into the early 2000s, South Africa was the predominant investor in much of sub-Saharan Africa, as firms sequestered within a saturated home market during apartheid were suddenly allowed (and in fact incentivized) to direct their attention outward. Within the thirteen member countries of the Southern African Development Community, South African FDI "averaged over $800 million per year between 1994 and 2004,

accounting for roughly a quarter of all FDI into the sub-region over this period" (Schroeder 2012, p.42). In Tanzania specifically, the sudden rise of South African investment had an impact that seems to exceed that of Chinese investment today—in part because these were same years in which Tanzanian parastatals were first being privatized and offered to international investors:

> In little more than a decade, South Africans acquired controlling interests in Tanzania's largest banking chain, the national airline, and the national brewery. They purchased or built hotels, gold and gemstone mines, and hunting and photographic safari companies. They were awarded a contract to run the national electric utility, and they set up the country's largest cellular telephone and satellite television networks. They bought factories producing plastics, cement, sugar, and fertilizer. And they quintupled their exports to Tanzania by marketing a wide variety of middle-class consumer items ranging from furniture and ceramic tiles to clothing and fine wines. (ibid. p.50)

While the data on South African investment grows more sparse after 2012, other sources seem to confirm that the country's relative influence plateaued in the 2000s and declined in the 2010s—even though South African firms still remain important in the formal corporate sector, as visible in the Orbis data. But the South Africans were not simply displaced by the Chinese. As is clear from the figures above, Kenya retained about the same level of ownership throughout the 2000s as South Africa and Kenyan investment began to increase markedly in the mid-2010s. US FDI increased in the same years—though it was dominated by a few big-ticket investments in the mining sector. The UK also grew in importance, but its sudden spike in FDI flow and especially in FDI stock hints that this investment was also dominated by some large acquisition.

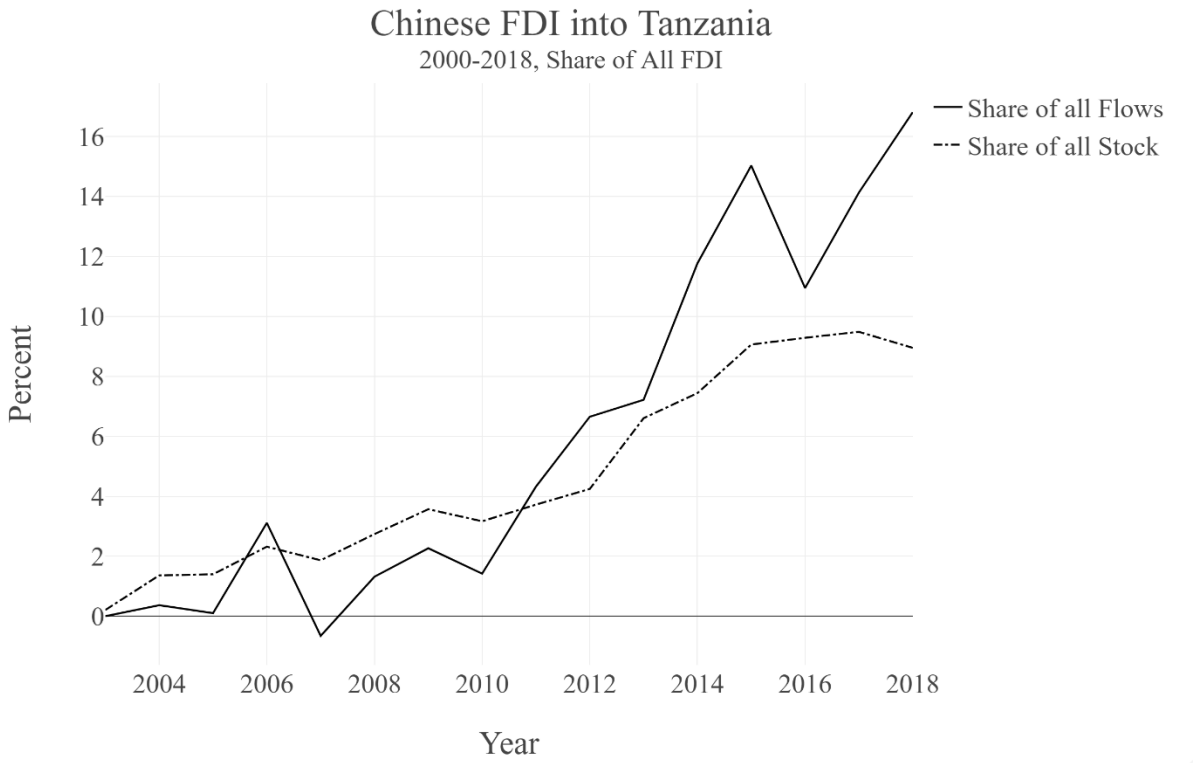

Chinese FDI into Tanzania
2000-2018, Share of All FDI
Share of all Flows
Share of all Stock
Percent
16
14
12
10
8
6
4
2
0
2004
2006
2008
2010
2012
2014
2016
2018
Year

*Figure 5-14*

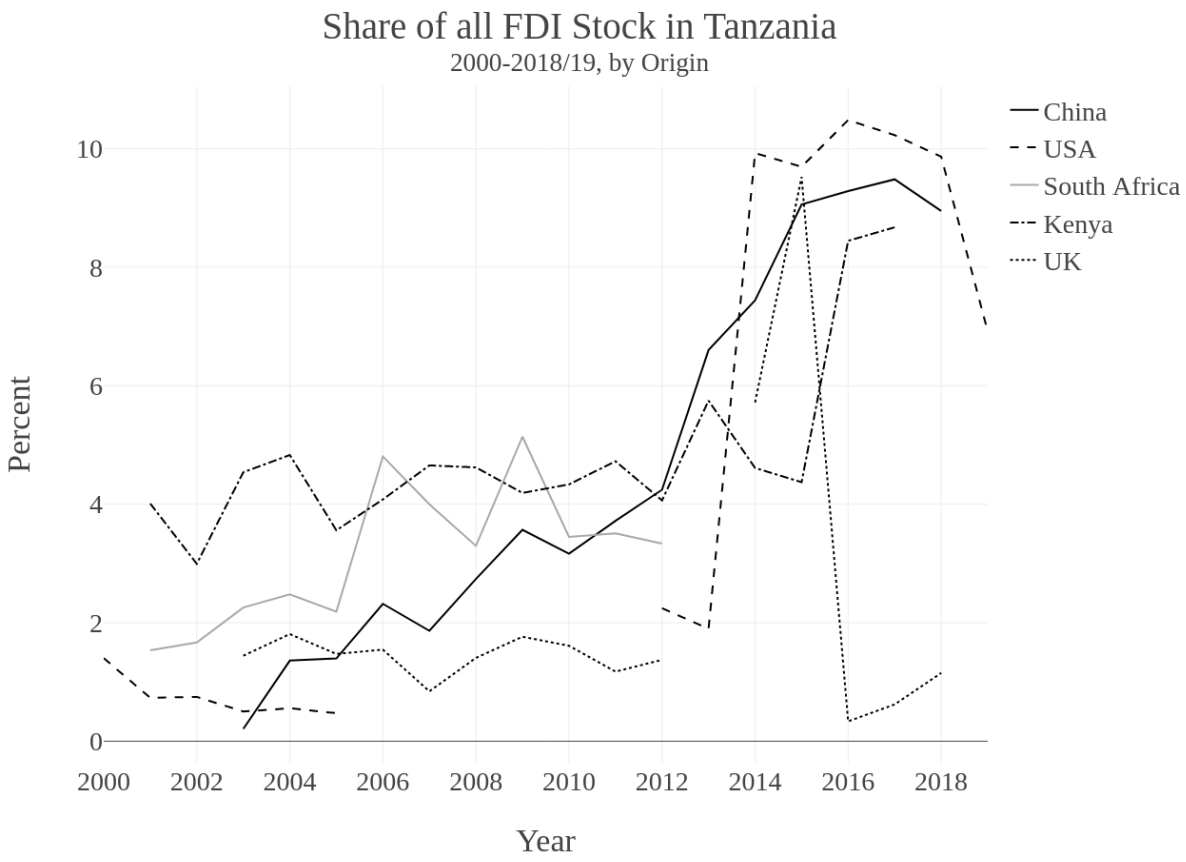



*Figure 5-15*

By contrast, Chinese flows increased steadily after about 2010, peaking in 2015 and falling thereafter. This produces a gradual plateauing in the officially recorded Chinese capital stock in the latter 2010s. In order to better capture the relative influence of different sources of investment, Figure 5-14 proceeds to scale Chinese FDI stock and flow as a share of all inbound FDI in Tanzania and Figure 5-15 shows the same stock share compared to shares for other countries, as recorded by UNCTAD and the World Bank, drawing on data from national

statistics agencies.[78] Figure 5-14 shows Chinese FDI flows ascending after 2010, peaking at around fifteen percent of all inbound FDI in 2015, declining somewhat in 2016, and then increasing again thereafter to sit at just about sixteen percent in 2018. Similarly, Chinese FDI stock as a share of all FDI stock in Tanzania rises slowly throughout the 2000s, and then grows more quickly after 2012 or so, lagging after the same trend in the flow data. By the later 2010s, however, the two series diverge, with the brief decline in flows manifesting as a multi-year plateau in stock. The most recent spike in flows visible here in the MOFCOM data as well as in the CGIT database seems to indicate that stock should rise again in the coming years, however.

Figure 5-15 compares this stock figure to an identical figure for other countries, giving a more accurate impression of China's relative weight, as compared to other major sources of investment. Early on, Chinese FDI stock lies at the bottom of the ranking, overtaking an equally low share held by the US in 2004 and the UK in 2006. By the 2010s, China begins to overtake those in the middle range, surpassing South Africa and Kenya in 2012 to lead the ranking. Even while it trails China, Kenyan investment also accelerates toward the end of the decade and more sporadic data from the UK shows high figures for 2014 and 2015, followed by a sharp decline. The biggest divergence in the later portion of the series is found in the US case, where a series of large mining investments between 2013 and 2014 sees China surpassed for all remaining years with available data. The decline in the US share in 2019, however, suggests that China is again at the forefront, especially considering the spike in Chinese FDI flows in 2019 and the number of accurately recorded projects in the CGIT database for the same year.

[78] A similar chart for FDI flow as a share of total was produced, but its movements were overly volatile and failed to show any new trends not already visible when absolute values of FDI flows are charted alongside the global total, as in Figure 12.

Finally, it will be helpful to compare Chinese investment in Tanzania to Chinese investment in other countries in the region. Since new territorial industrial complexes at the urban and regional scale tend to both depend on and further develop global logistics infrastructures (which themselves tend to crystalize around particular features of physical geography such as oceanic rims), Dar es Salaam's prominence as a rising territorial industrial complex is best framed relative to other African countries linked to the same Indian Ocean industrial rim rather than relative to all other African countries. The last half century of industrial development along the Pacific Rim and, more recently, the South China Sea, has clearly demonstrated the continuing influence of basic factors of physical geography and economic clustering on the general spatial extension of the technosphere (via the fact that industrial development tends to have an inbuilt directionality determined, at root, by profitability).[79] This means that Tanzania and its Dar es Salaam industrial complex are not in equal competition with all other nations for integration into the Sinocentric global manufacturing chain. Even if its pool of potential labor might be large and affordable relative to other locations, it is still at a clear spatial disadvantage due to its distance and the lack of both developmental infrastructure and pre-existing industrial clusters. At the same time, it is in a far more advantageous position than similarly endowed African nations in less accessible parts of the continent (or those further in maritime distance from key Chinese ports). Thus, it makes the most sense to compare Tanzania to other African nations that lie along the Indian Ocean rim or within the various trade blocs that abut it.

[79] For the development of the Pacific Rim complex, see Glassman 2019 and Chuang 2019, for the concept of the technosphere see Haff 2013 and for the role played by profitability in the development of new industrial territorial complexes, see Storper and Walker 1991.

Figure 5-16 shows this comparison for capital stock measures, breaking the region up into four geographic clusters. The EAC group includes all the (full member) nations in the East African Community, which have preferential trade and immigration agreements approaching a common market (with intent to develop a shared currency and ultimately a unified political federation). Tanzania, Kenya and Uganda are the most populous nations in this group and have the largest GDPs. The Horn group includes Ethiopia, Sudan, Djibouti and Eritrea (no data is available for Somalia or Somaliland). Of these, Ethiopia is by far the largest in terms of population, but Sudan's GDP has historically been larger due to its oil reserves—but Sudanese GDP began to decline markedly in 2018, accompanying political turmoil, after which point Ethiopian GDP overtook it. The Southern group includes Zambia, Mozambique, Zimbabwe and Malawi, of which Mozambique has the largest population followed by Zambia, while Zimbabwe and Zambia have the largest GDP. The Island group includes Madagascar, Mauritius, the Seychelles and Comoros. Madagascar is by far the most populous, but the GDP of much smaller Mauritius exceeds it due to the island's role as an offshore finance center—attempting to position itself as something like the Singapore or Hong Kong of East Africa. At the same time, Mauritius also maintains an outsize export sector. In both finance and export-oriented production, Sino-Mauritians play an outsize role as facilitators via their connections to Hong Kong and mainland China, meaning that inbound Chinese capital can make ready use of pre-existing networks (Wan 2015). Thus, investment recorded as going to Mauritius should in fact be understood as only partially destined for Mauritius itself, the remainder being rerouted through the country's finance apparatus to final destinations, mostly (but not exclusively) in East Africa.

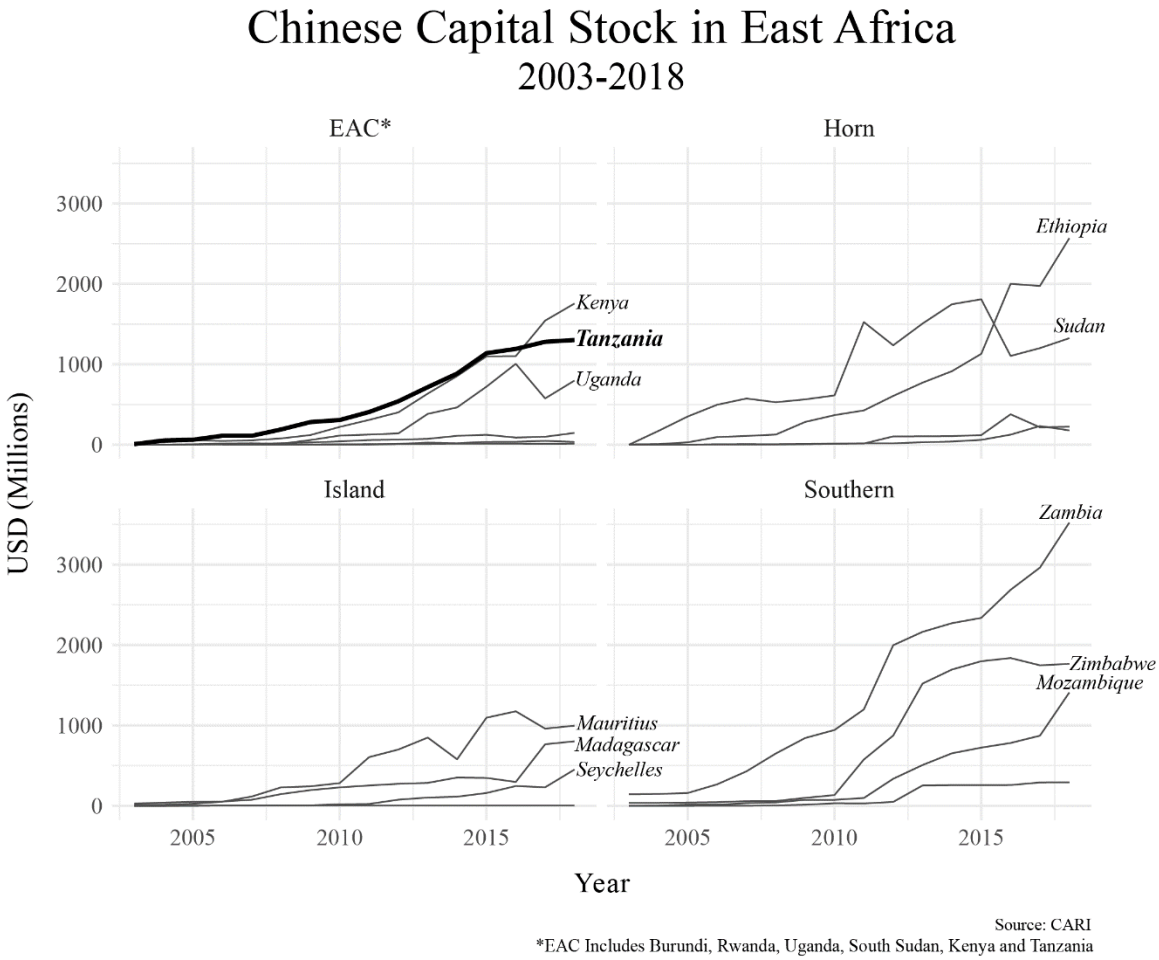



*Figure 5-16*

In Figure 5-16 we can see that Tanzania has a prominent position relative to other East African nations as a recipient of Chinese investment—but by no means the leading position. Within the EAC, the years after 2016 saw Tanzania overtaken by neighboring Kenya in the value of Chinese capital stock located in the country. The two countries are an important contrast since they're neighbors with a roughly similar population and GDP and both had competed in the early 2010s to attract Chinese investment, including massive infrastructure projects. In Tanzania, the

2015 election was followed by widespread cancellations or the indefinite stalling of major Chinese investments. In Kenya, though the rate of investment has slowed in recent years (as it has across the continent), many of the projects originally proposed in the earlier part of the decade ultimately came to fruition. The largest of these have been the Standard Gauge Rail project and the construction of a new container port at Lamu, now poised to be the largest port in East Africa if it is actually completed (capable of handling roughly 20 million TEU if completed, with its three existing berths currently capable of 1.2 million, compared to 2 million at Mombasa, just under 1 million at Djibouti and a mere half million at Dar es Salaam). The Lamu port project is a particularly telling example of the contrast between the two countries, since it was originally in competition with the even larger proposed port in Bagamoyo, discussed above. Since the cancellation of the Bagamoyo project, however, Lamu will now be unchallenged in its position as the maritime hub for trade in the region.

But both Kenya and Tanzania are outstripped by two other countries in the region: Zambia and Ethiopia. The Zambian case is explained almost entirely by major Chinese investments in the Zambian copper belt, alongside the sort of infrastructural projects awarded to Chinese firms across the continent. The same is more or less true of investment in Zimbabwe[80] and Mozambique, which are on par with Kenya and Tanzania in total Chinese-owned capital stock but appear to have much lower shares of manufacturing investment. These cases are significant, but (as in the case of oil investments in Sudan) they don't break with historical trends and have not spilled over into other sectors or climbed up the value chain in terms of processing

[80] In Zimbabwe, aside from the already well-documented dynamics of mining and infrastructural investment, the most notable dynamics have been financial, including both experiments with currency-swap schemes and a particular dependence on larger-scale Chinese loan financing—both conditioned by Zimbabwe's long political exclusion from similar resources offered by Europe and the US.

and production (for an extensive ethnographic investigation of Chinese investment in Zambia, see: Lee 2017). In contrast, investment in Ethiopia has included both massive infrastructure projects (namely the Addis Ababa-Djibouti railway and various efforts to build out the power grid) and an extremely high share of manufacturing investments. Ethiopia is one of the few countries in Africa that, at least in certain industries, seems to actually be moving up the value chain and successfully competing to host lines of production that have been shed from the mainland industrial complex due to rising wages. These include firms operating in the leather and leather products sector (Tang Xiaoyang 2019), general textiles production (Brautigam et. al. 2018) and even the production of smartphones, all concentrated in a series of new Special Economic Zones opened for the purpose (Keyi Tang 2019). Many factors have contributed to this, including unique institutional endowments, but likely the most relevant factor in terms of regional competition for Chinese manufacturing projects is the fact that labor in Ethiopia remains much cheaper than in Kenya or Tanzania, with a population double that in either competitor. In fact, Ethiopian wages (sitting at a legal monthly minimum of only 26 USD, compared to around 120 in Kenya) are by far the lowest of any country integrated into global garment supply chains (Barrett and Baumann-Pauly 2019). Thus, it makes sense that Chinese investment into the region is highest in Ethiopia, followed by Zambia.

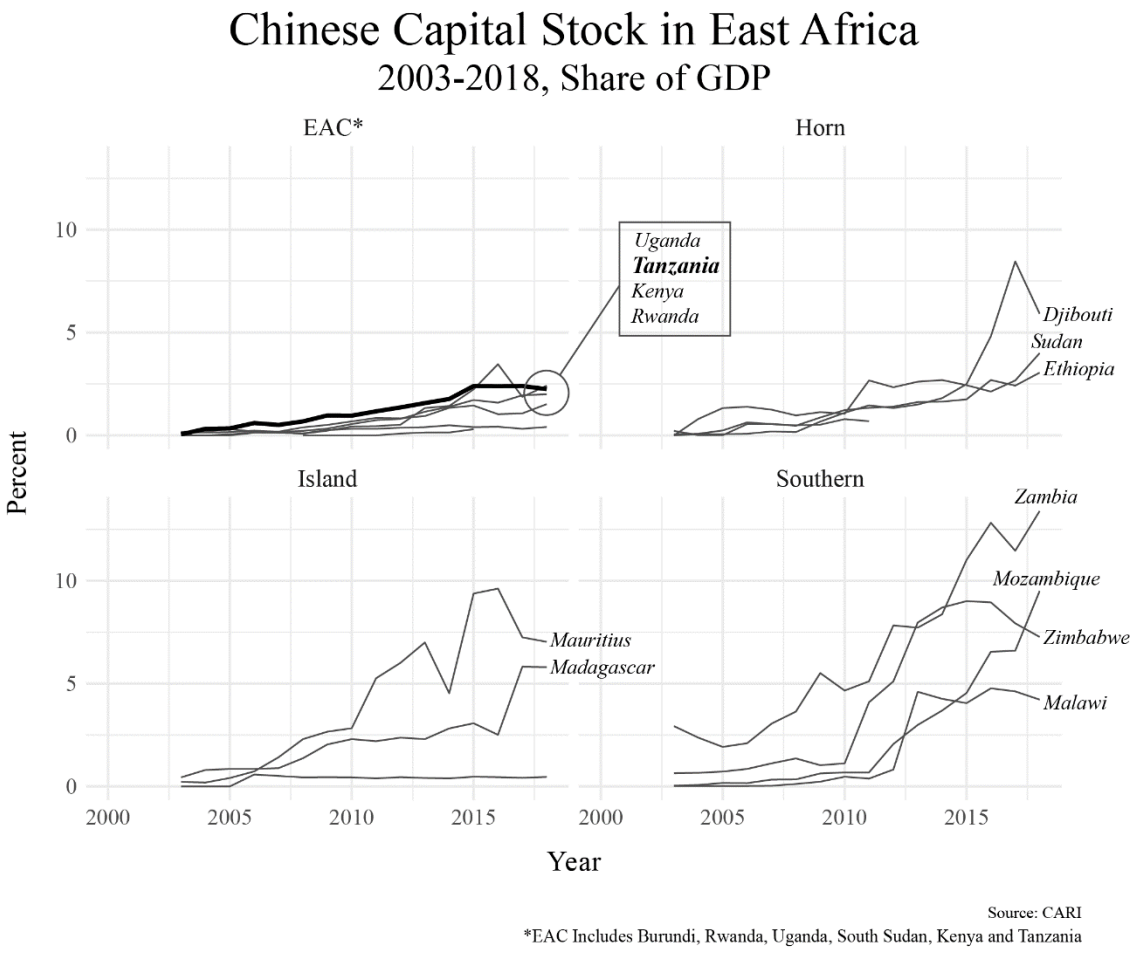
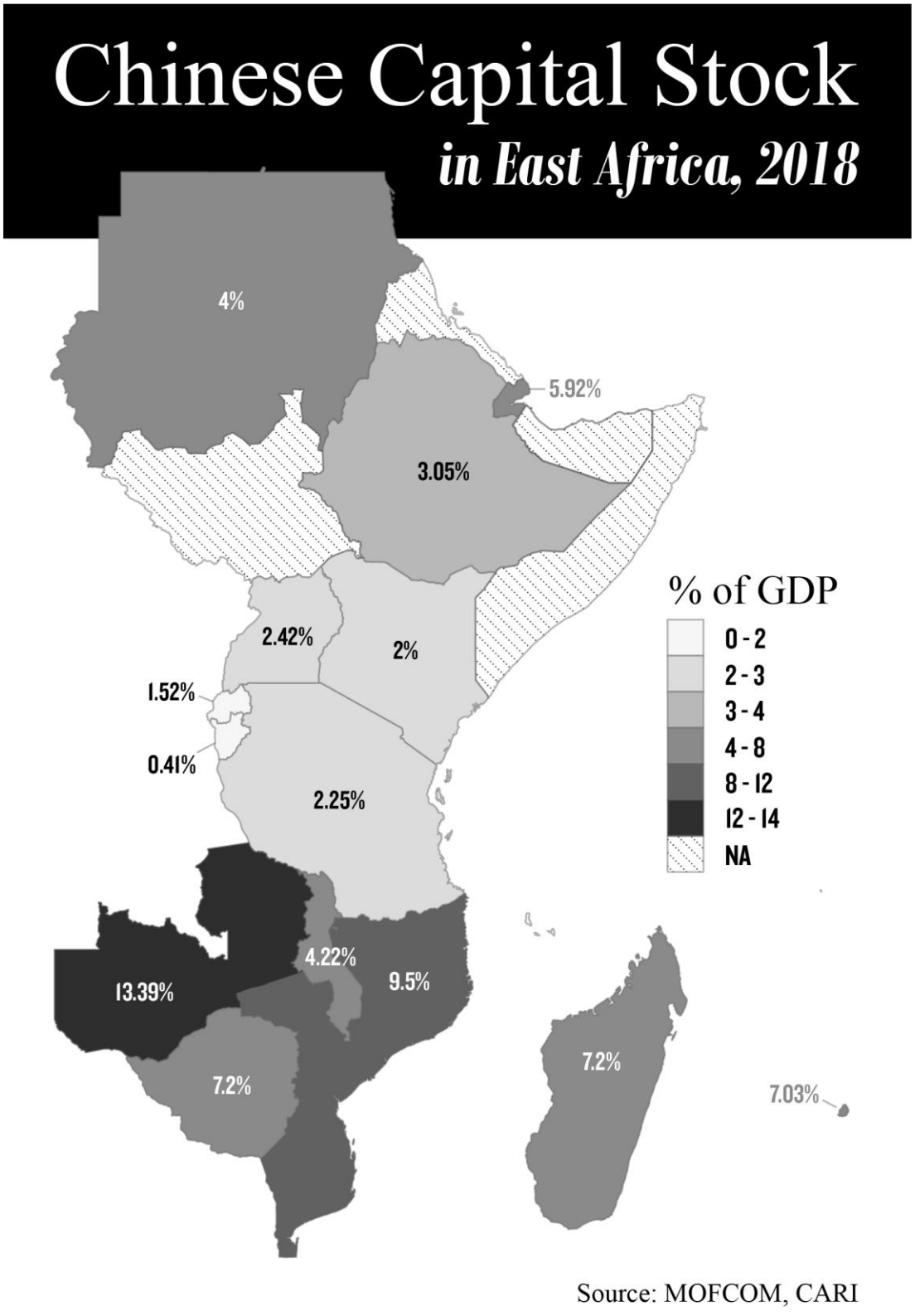

Chinese Capital Stock in East Africa
2003-2018, Share of GDP
EAC*
Horn
Island
Southern
Uganda
Tanzania
Kenya
Rwanda
Djibouti
Sudan
Ethiopia
Mauritius
Madagascar
Zambia
Mozambique
Zimbabwe
Malawi
Percent
Year
10
5
0
2000
2005
2010
2015
Source: CARI
*EAC Includes Burundi, Rwanda, Uganda, South Sudan, Kenya and Tanzania

*Figure 5-17*




Source: MOFCOM, CARI

Map 5-1

We can also scale the relative size of Chinese investment, as reported by MOFCOM, by each country's GDP. Figure 5-17 shows this scaling over several years and Map 5-1 shows it on the map for 2018.[81] Though this scaling is less relevant for understanding the absolute distribution of Chinese capital exports, it is a far better visualization for questions of relative impact and dependency. From Figure 5-17 it becomes apparent that despite substantial media attention, Chinese capital stock stays within 1 to 2.5 percent of GDP in the five years from 2013 to 2018 and appears to have plateaued. In Tanzania, Chinese capital stock as a share of GDP peaks in 2015 and again in 2017 at 2.4 percent. By 2018, it had fallen somewhat to 2.25 percent (just below Uganda, at 2.42% and above Kenya, at 2%, and Rwanda, at 1.52%). Even Ethiopia reaches only around 3 percent of GDP in 2018 (though its trend seems to be more clearly upward). Djibouti and Mauritius are each special cases due to their small size and important roles as an essential logistics hub and financial center, respectively. The most stark trend visible here is the fact that Chinese-owned capital stock is much larger relative to GDP in the Southern group and growing quickly, particularly in Zambia (13.39% in 2018), Mozambique (9.50%) and Zimbabwe (7.27% after peaking around 9% in 2015-2016). This clustering is particularly evident on the map in Figure 18. Sudan (4%), Malawi (4.22%) and Madagascar (5.8%) represent show a more mild but similar trend. Though the exact roots of this divergence are beyond the scope of this study, it hardly seems consequential that the nations with higher shares of manufacturing

[81] Both show all countries in the region minus the Seychelles, where the small GDP makes for a particularly high share, distorting the scale. Though the same is true to a different degree in Djibouti and Mauritius, these cases have been kept since they remain broadly in line with the others. Also note that no accurate GDP data exists after certain years for South Sudan or Eritrea, and data has consistently been absent for Somalia and Somaliland.

investment (Tanzania, Kenya and Ethiopia in particular) seem to be less dependent on a single source of foreign capital stock (at least understood relative to their overall economic capacity), while nations with higher shares of investment in infrastructure projects and mining seem to exhibit clear signs of such dependency. There is strong reason to believe, however, that large investments in infrastructure and mining helmed by major Chinese conglomerates are overreported in MOFCOM, relative to smaller investments in manufacturing which tend to be led by either private investors or much smaller state-owned enterprises (at the provincial or even city and county levels). This becomes evident when the MOFCOM figures are compared to the results of various on-the-ground field surveys.

**Field Surveys**

In regional comparison, Tanzania appears to be a fairly average case and one of the three best locations to study Chinese investment in manufacturing specifically (alongside Kenya and Ethiopia). But even though the above numbers from MOFCOM lay out important trends and points of comparison, they are also plagued by hidden biases and substantial misreporting. In order to determine the exact direction of error that exists in the MOFCOM data, it must be compared to the results of existing field surveys across the region. Three sources are of particular importance here. The first are a series of reports released by the economic research firm McKinsey and Company, derived from field interviews with Chinese companies in several sample nations across the African continent. The second are the CARI Working Papers and Policy Briefs, which include more focused inquiries in individual countries. Here, I'll mostly review the papers that discuss Tanzania, Kenya and Ethiopia, since these have been the major sites of manufacturing investment. Third and finally is the work of Irene Yuan Sun, who led the

McKinsey studies. Aside from the work produced for these reports, Sun wrote a book about Chinese investment in Africa (Sun 2017) which included the results of field surveys in Nigeria, Lesotho, Kenya and Ethiopia. Though there is no information on Tanzania in Sun's work, her material on Kenya and Ethiopia will be important points of comparison in the region.

McKinsey has produced several reports focusing on Chinese investment in Africa (Sun et. al 2017, Bughin et. al 2016, Roxburgh et al 2010), of which the most recent (Sun et. al 2017) provides both the most updated data and the results of a linked series of field surveys conducted in eight African countries between November 2016 and March 2017. East Africa was disproportionately represented in this survey, with Ethiopia, Tanzania, Kenya and Zambia all included. This represents the single largest effort in direct data collection on the topic to date. One of the most salient results of the survey was the discovery that there were many more Chinese enterprises operating in the surveyed countries than had previously been estimated and they do not signal the type of coordinated investment on behalf of large SOEs that seems to characterize the "China Inc" narrative popular in the media. Instead, "around 90 percent of the 10,000 or so Chinese businesses in Africa are privately owned" (ibid, p.28) and they reflect a diverse set of interests linking back to their home firms as well as different government agencies and financial institutions rather than a single, monolithic interest bloc. Part of what has generated the illusion of a monolithic, coordinated investment effort is precisely the Chinese state's way of talking about capital exports as if they were part of an intentional planning effort (the "Belt and Road"), further assisted by the fact that MOFCOM tends to only record the larger, more formal investments and fails to pick up myriad smaller ones.

This divergence between the MOFCOM data, national records of investment projects and the results of the field survey is one of the most immediately notable results in the report. In

every country surveyed, the number of firms discovered on the ground exceeded the official MOFCOM records and only Ethiopia had a national investment authority that came close to approximating the true number of Chinese firms in operation. On average, there were 3.7 times more firms operating on the ground than were reported. But in some countries this divergence was particularly extreme: in Kenya, the survey found 3 times more firms than MOFCOM; in Ethiopia and Zambia, 4.3 times more; but in Tanzania there were 5.2 times more. Specifically, MOFCOM had reported only 159 firms operating in Tanzania for these years, while the Tanzania Investment Center had reported 207. On the ground, however, the survey found 825 firms (ibid, Exhibit 5). In terms of ownership, the survey found that only 8 percent of Chinese firms operating in Tanzania were state-owned enterprises (SOEs), meaning that 92 percent were private (compared to 10% SOE in Ethiopia and Zambia and 20% in Kenya). In fact, the share of enterprises that were state-owned was low across the entire survey: "Even in Angola, where Chinese SOEs play a particularly prominent role in the local economy, our research shows that three-quarters of Chinese firms operating in the country are privately owned" (ibid, Exhibit 6, p.29). In addition, measured as share of all firms (rather than share of assets or profits), construction and real estate only compose about 15 percent of the total, exceeded by both services (25%) and manufacturing (31%). Thus, in terms of overall firm composition, it seems that large infrastructure projects or mining ventures are far less representative of the average Chinese investor (ibid).

The importance of manufacturing specifically is made evident in the survey: "we estimate that Chinese firms already handle 12 percent of Africa's industrial production" and this "reveals a relocation of some of China's manufacturing might to Africa" (ibid, p. 29). This is of course stimulated by the rapid increase in the rate of return on foreign investment across the African

continent, as compared to other regions of the world—growing from a mere 6.5 percent in 1995 (well under Asia's 17%) to 13 percent by 2012 (only barely surpassed by Asia's 13.7%, according to Leke et. al. 2014). This seems to be further confirmed by the healthy profits reported by the firms surveyed, with nearly one third reporting a 2015 profit margin greater than 20 percent. This number was even higher for manufacturing firms (where 34% reported profits above 20% and 24% reported profit margins between 10% and 20%) and for services (40% reporting profits above 20%, 21% between 10% and 20%), with the numbers a bit lower for trade and quite a bit lower for construction and real estate, where barely a fifth reported profit margins over 20% (Sun et. al, p.30). At this point, much of this production is still destined for African markets: "93 percent of the revenues of manufacturers we spoke to came from local or regional sales" (ibid, pp.29-30). Meanwhile, the product lines that the firms are operating in are extremely diverse, ranging from automobile manufacturing to small trading companies. Overall, investment funds also tend not to derive from the Chinese government or its affiliated financial institutions (such as policy banks). In fact, most of the Chinese firms (66%) self-financed via retained earnings, savings or personal loans. Only 13 percent relied on financing linked to government policy banks and 16 percent relied on loans from Chinese commercial banks, with another 6 percent deriving funding from either African commercial banks or other sources such as the World Bank (ibid, Exhibit 10, p.35).

For Tanzania specifically, roughly 40 percent of investments were in capital-intensives sectors like manufacturing, with a further 25 percent in capital-light investments such as services. Kenya had slightly more capital-intensive investments (44%) in the same year, while Ethiopia had by far the most of all countries surveyed, at 67 percent (ibid, Exhibit 11, p.36). In terms of linkage with global supply chains, most countries in the survey source about half their

inputs from China and half from local suppliers. Chinese firms in Tanzania follow this same pattern but tend to source slightly less from China than others (ibid, Exhibit 16, p.48). Though Tanzania is characterized (alongside Kenya and Nigeria) as a "solid partner" for Chinese firms, the report also demonstrates the ways in which Ethiopia far exceeds other countries in its share of Chinese manufacturing investment. Meanwhile, the survey singles out Tanzania specifically to show the "warnings signs" of an "investment shrinkage" driven by concerns over "stringent tax imposition, barriers to obtaining work permits, and the denial of some previously promised investment incentives" and even interviewing several investors who planned to leave the country in the subsequent years (ibid, pp.57-58). These results seem to have been borne out in subsequent years, with a plateauing of Chinese investment in the country visible in all data sources and equally evident on the ground. Unfortunately, the McKinsey survey provides almost no detail below these general summary statistics. There is not even a coherent breakdown showing the percentage of firms in each sector for each country. The result is that the McKinsey survey is not directly comparable to the results of the field survey reported below, even while it provides important contextual information.

A more detailed set of surveys are found in the CARI Working Papers series. Though these include detailed research on particular sectors in Ethiopia (Tang Xiaoyang 2019 and Keyi Tang 2019) and an equally important scoping study in Kenya (Xia 2019b), I will focus on the results of the Tanzanian survey (reported in Xia 2019a and 2019c) since this is the only existing survey that is directly comparable with my own. In both the Kenya and Tanzania studies—conducted by Xia Ying in 2016 (with follow-ups in 2017) as part of a single research project—there is a divergence between MOFCOM data, local FDI records and the results of the survey similar to that found in the McKinsey report. But, in contrast to the McKinsey report, these

scoping studies focus on manufacturing specifically and tend to have found *fewer* firms in operation than were officially reported. This could be due to the methodology used: these are more detailed surveys recording more information about the firms in question, pursued from a top-down snowballing interview method whereby Chinese businesspeople in the country of interest were identified and contacted through formal business federations or embassy organizations, interviewed about their firms and then asked if they knew of other Chinese firms operating in the area of interest. Thus, some breadth is sacrificed for depth. It's also likely that many of the smallest firms (including all firms in trade and services) were not picked up in Xia's surveys. The data gap is stark, however, even if it lies in the opposite direction:

> In Kenya, of the 23 active investors identified during our fieldwork, only two were registered with MOFCOM, and only seven had registered with KenInvest. In Tanzania, MOFCOM registrations showed 49 projects with the intention of investing in manufacturing or agro-processing, while TIC [Tanzania Investment Center] had records of 471 Chinese investments in the same industries; of these only 15 entries matched MOFCOM registration. Of the 70 active investors confirmed during fieldwork, fewer than one-fifth were registered with MOFCOM, and only about one-third with TIC. (Xia 2019a, pp. 2-3)

Specifically, 26 of the 70 confirmed investors in Tanzania had registered with TIC and were still in operation. Of the 15 that matched MOFCOM data, however, "only six are still in operation" (Xia 2019c, p.9). Meanwhile, the study showed that 3 firms in Kenya had invested but exited the market, while MOFCOM and TIC reported divestment by 15 and 29 firms, respectively (Xia 2019a, p.3).

Similar to the McKinsey surveys, Xia reports that the majority of Chinese firms operating in these sectors are privately owned and very rarely received any sort of financial incentive from the Chinese government or affiliated lending agencies (ibid p.4). Much of the logic of the investment, then, is determined by market imperatives. Since these firms tend to produce for local and regional markets in Africa, relocating manufacturing to these countries not only provides cheap sources of labor and raw materials but also escapes "increasingly tighter regulatory enforcement in China" and gets around "high import duties levied on manufactured products" in the host countries while also giving certain firms access to "duty exemption for exports to the US […] and Europe" via Africa-specific trade agreements (ibid, p.5). In Tanzania, the survey found that the bulk of manufacturing firms "are located in Dar es Salaam and neighboring areas, while agricultural investments in farming, timber processing, butchery, spinning, and other agro-processing activities are more dispersed geographically […]" (Xia 2019c, p.8), meaning that most manufacturing firms record in Xia's survey should be captured in my own—so long as they did not close between 2016 and 2020. Aside from the agro-business firms surveyed, Xia found the sub-sector of larger manufacturing firms to be dominated by building materials (15 projects) and plastic recycling and processing (12), with a further 6 in smelting and metal recycling and 2 in textiles and garments (ibid, Table 2, p.9). In addition to these, the survey also found a number of very small firms in other sub-sectors "including motorcycle assembly, furniture making, and manufacturing of daily commodities such as detergents, bottled water, and air filters for cars" (ibid, p.14). These results are broadly in line with my own survey, though of the two textile firms, one has since closed (Urafiki) and the other (Tooku Garments) saw its parent firm acquired by a Taiwanese company.

Compared to the McKinsey surveys and to my own fieldwork, Xia's survey provides much more detail on the character of individual firms and includes more extensive interviews with owners about exactly why and how they decided to relocate production to Tanzania. My own survey is, in part, an update of this information several years later. At the same time, Xia's top-down snowball interview approach tends to emphasize the ownership side of the equation. This is an excellent resource for discovering the reasoning behind factory relocation or identifying the interpersonal networks and histories of industrial clusters in particular sectors, but it includes essentially no geographic information, nor much insight into the experiences of workers and residents in these industrial zones. My own fieldwork follows an opposite approach: a bottom-up snowball method beginning with respondents in the neighborhood and focusing the bulk of attention on geographic information—both in the quantitative sense, including locational data and information on spatial clustering, as well as in the qualitative sense, such as locals' own opinions, attitude and ways of speaking about Chinese firms (or simply their own economic prospects) as well as more general firsthand experience of the character of such industrial zones. Regardless, information on many of the same firms is found and several new firms are identified which were not included in Xia's survey.

# Section 3
# –
# Dar es Salaam Field Survey
## Jan-Feb 2020

**Objectives and Coverage**

Dar es Salaam has five to six major industrial zones where factory production is concentrated, depending on the measure—as well as numerous smaller neighborhood workshop complexes which tend to aggregate around these main industrial zones, but also exist in many places (particularly along major thoroughfares) independent of the presence of larger industry. In their simplest form, these zones are small clusters of factory and warehouse buildings with affiliated industrial yards located along the city's larger paved roads and divided from the surrounding urban fabric by high concrete walls or fences of barred metal. As they develop, they tend to first lengthen out along the paved thoroughfare before splitting off and spawning new clusters as multiple industrial sites develop along the same stretch of road in a string-of-pearls pattern. The largest and most developed zones then compound in on themselves as they clear and convert surrounding land (whether built-up for residential or workshop use, devoted to intermittent subsistence agriculture or simply fallow land) into similar uses and thereby extend off the main, paved thoroughfares and into unpaved side-roads and alleyways.

Given the informality of development in the city, it seems that much of this process has been defined by relatively automatic mechanisms determined by the economic logic of agglomeration (Ottaviano and Thisse 2004). After a certain degree of concentration, such areas are then acknowledged as industrial centers by urban administrators and further investment is encouraged. In many cases, the existing industrial centers first agglomerated around key infrastructural nodes or areas where industry had been concentrated historically. This is particularly true of the industrial zone around the Port of Dar es Salaam and the one surrounding the terminus of the TAZARA railway, which extends to the Julius Nyerere International Airport. A smaller example of the same thing can be seen in the zone near the historic Urafiki textile plant in the Ubungo neighborhood. In recent years, a handful of export-processing zones have

also been founded in the city. Though they are spatially more closed off from the surrounding areas, they've also tended to be located in the general proximity of the smaller pre-existing clusters and will likely contribute to the further industrialization of these areas. In addition to this, there are some scattered industrial facilities in the city's more rural outskirts near and beyond the border with the Pwani Region. Many of these are agricultural processing facilities, such as Milkcom, a dairy processing center located in the city's far southeastern edge. Others are utilities stations, such as the Kinyerezi power plant near the city's southwestern border, or lone factories located out along major interprovincial highways, such as the Lake Steel factory far beyond the city on Morogoro road just before Mlandizi. In some cases, these lone facilities might prefigure industrial clusters to come, and relatively new clusters have formed in Mbagala (along Kilwa road at the southern border with Pwani) and may be extending to Vikundu and Vibura[82] (further along Kilwa road, within Pwani).

While the primary goal of the field survey was to identify Chinese firms operating in the area and record basic summary statistics and geodata about these firms, a secondary objective was to capture the more qualitative character of the new industrial territorial complex taking shape in one of the fastest growing cities in the world. In both respects, the focus of the on-foot field survey was necessarily concentrated in these industrial zones. With limited resources, it was not possible to attempt an exhaustive survey of all Chinese firms operating in the area, nor one that extended beyond the city to record information on industrial facilities lying along major highways.[83] Meanwhile, the focus on larger more formal facilities in manufacturing meant that

[82] Vibura has the larger industrial cluster of these two, with scattered factories stretching down to Mkuranga even further south. Online data shows at least one Chinese firm operating in the Vibura cluster: Zhong Fu International, which is listed in customs records as exporting "Pvc material (for making rubber sole" and other shoe components.
[83] This is why the Keda/Twyford Ceramics factory does not appear on the survey. While it is Chinese-owned, the facility is located somewhere out along Morogoro road near Chalinze, far from the city. When searched on google,

other clusters of handicraft workshops (in the furniture-making industry, for example) were not included in the survey, nor were the various small traders in Kariakoo, who often have some financing or connection to Chinese merchants. Technically these traders also "warehouse" goods in the small sales stands they rent in basements throughout the district, but these shops contain no more goods than a normal retail store. The closest the survey gets to the trade sector is its record of larger warehouses devoted to trade in *mitumba* (large bundles of second-hand clothes) imported from China, among which only one site (in Kariakoo) was located outside the main industrial zones.

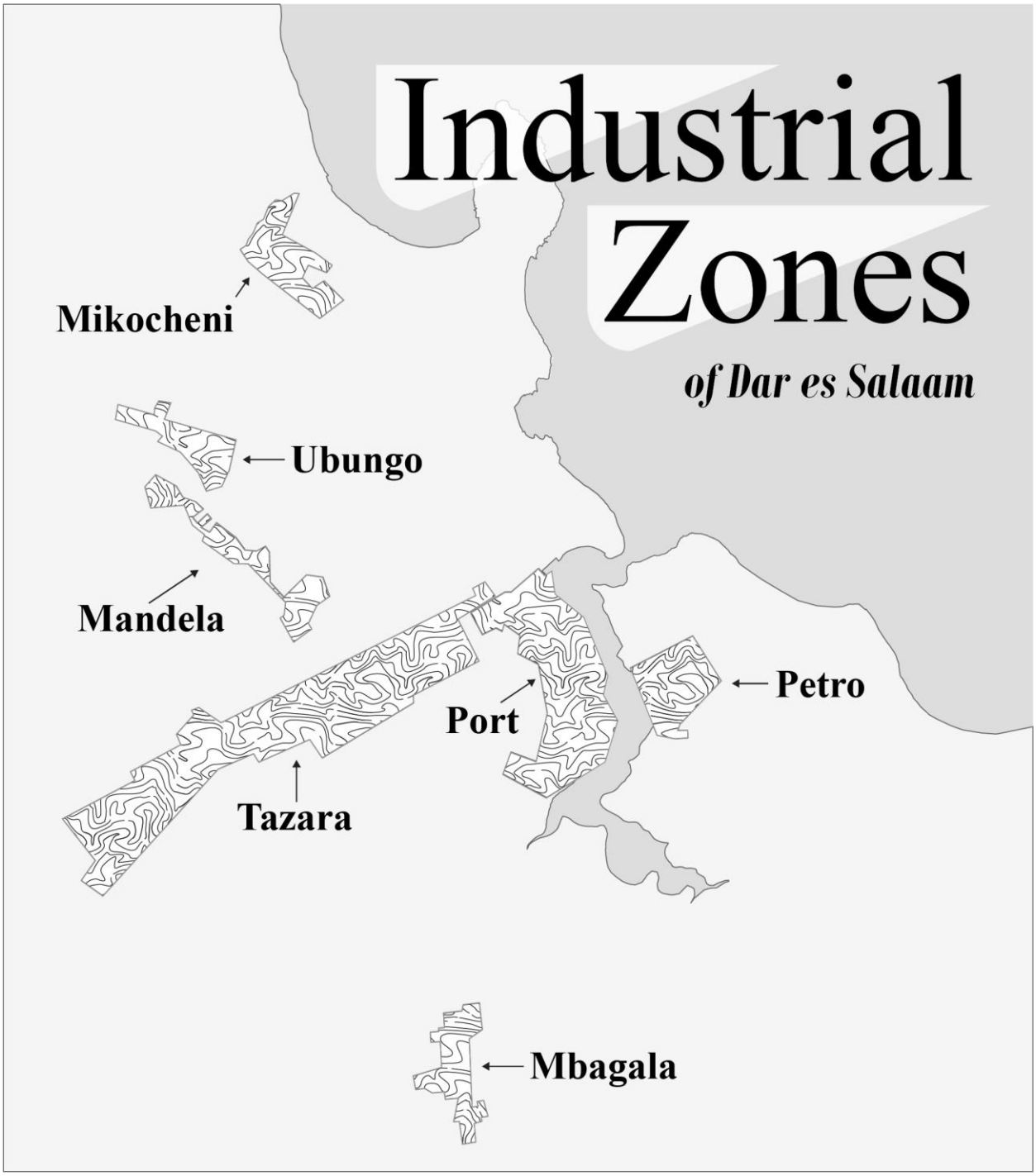
The first methodological task, then, was the identification of the industrial zones on which to focus the survey. These areas were determined in two main ways: First, through word-of-mouth, particularly in conversation with *pikipiki* (motorocycle) and *bajaji* drivers for hire, whose regular traversal of the city gave them an intimate familiarity with its layout. Second, through a scan of recent satellite imagery and slightly older data from the Ramani Huria community mapping project. The common building methods in Dar es Salaam mean that most residential areas outside the central urban high-rises are easily distinguishable from industrial zones with large, formal facilities. These industrial areas are dominated by large rectangular facilities with concrete walls and sheet metal roofing that lies at a modest pitch (mostly in shades of blue or blue-green, sometimes in red, and periodically in a different flat style using other materials in shades of white or brown). On satellite imagery, these areas are easily distinguishable from both the much smaller, much more tightly packed residential buildings, which predominantly have roofs of red-brown sheet metal (interspersed with some blues, green

Twyford does have a "factory" location listed in the Ubungo EPZ (Millennium Business Park), but this appears to be simply a showroom and possibly a small warehouse—it is this facility which appears on the survey, not the factory.

and whites) and surrounding workshops which are similar in size and roof color to residential buildings but tend to be oriented around more open yards, with room for loading and unloading. This remote sensing analysis was not automated but was produced manually by the author through continual cross-referencing between the areas as they were reported by respondents, as they existed on the ground during the survey and as they appeared on satellite images. Similarly, industrial or commercial usage was confirmed at the ward level on the infrastructure maps composed by the Ramani Huria community mapping project—though many of these were several years out of date and did not include many new facilities visible on foot or in satellite images.[84] Map 5-2 shows the city's major industrial zones, ranked by size. The names given in the map are not official designations[85] and do not necessarily match the wards and districts that the zones lie within but are instead descriptive names used for convenience.

[84] Ramani Huria is a largely opensource mapping project, with layers available from their website and on Openstreetmap. Their website is located here: <https://ramanihuria.org/en/>

[85] One exception would be the Mikocheni zone, which is officially designated the Mikocheni Light Industrial Area.


Industrial
Zones
of Dar es Salaam
Mikocheni
Ubungo
Mandela
Petro
Port
Tazara
Mbagala

*Map 5-2*

As can be seen above, by far the largest is the Tazara Zone (~13 sqkm), named after the terminus of the historic Chinese-built railway project located roughly midway between downtown and the airport. This is a fully developed industrial supercluster (see Neel 2016) that lies at the nexus of several major logistics hubs. It likely represents the fusion of several older industrial clusters that took shape along Julius K. Nyerere Rd. and the surrounding area, now extended into the shape of a single East-West industrial corridor stretching from the airport all the way to the outer edge of downtown. The precise border between the Tazara Zone and the Port Zone (~7 sqkm) seems somewhat arbitrary, if judged purely by satellite imagery—and it could be claimed that the two should be fused into a single, J-shaped industrial area emanating out in two directions from the port. On the ground, however, there is a fairly sharp qualitative break between the two, beginning at the stretch of residential housing that intercuts them at the intersection with Kiggo Rd, followed by a distinct southward change of traffic flow visible at the intersection with Mzimbazi St. (which turns into Gerezani St.). This narrow slice of shops and residential buildings extends south until meeting a key roundabout just before a bridge over a railyard. This bridge, railyard and roundabout mark the changing flow of traffic toward the port. This is the beginning of Bandari St. (*bandari* means port), with heavy industry immediately visible again as soon as this bridge is crossed. This is the distinction used in dividing the two into separate industrial zones. The Port zone acts as its own corridor supercluster, extending from the port and railyard hub southward along both Kilwa and Nelson Mandela roads.

Aside from these two superclusters which lie along the city's most crucial logistical arteries there are five other industrial zones. The most specialized of these is the Petro Zone on the Kigamboni side of the peninsula (~ 2.4 sqkm). Though technically linked to the energy infrastructure in the southernmost stretch of the Port zone across the Nyerere Bridge, both the

water and a long stretch of residential development (or fallow land) still separates the two. The Petro Zone is defined by the presence of a large, central petroleum refinery, with some related facilities clustered nearby. Over time, it will likely fuse with the Port Zone across the water as the newly-constructed Nyerere Bridge integrates traffic flow between the two. The most distant from the city itself is the Mbagala Zone (~2 sqkm) which exists in a single small cluster along Kilwa Rd., which appears to be in the early stages of lateral expansion away from the thoroughfare. It's unclear why, exactly, this cluster formed at this particular location along Kilwa Rd., as there don't appear to be any major logistical hubs in the area and a more detailed appraisal could not be obtained without on-the-ground investigation. It's most likely that the cluster formed simply through some confluence of incidental factors, such as an amenable local administration combined with some pre-existing industrial infrastructure and/or easy access to land.

The Ubungo Zone (~1.7 sqkm) is similar in shape but lies along Morogoro Road to the West, sitting at the edge of the city's intensively built-up area. Like Mbagala, it is a smaller single cluster that has seen limited lateral extension off the main thoroughfare. Unlike Mbagala, the reasons for its location are more clear: it has an older industrial inheritance as the host to the country's once-largest textile factory, Urafiki, originally established as a joint venture between the Chinese and Tanzanian governments and since privatized and then closed down. Meanwhile, it now lies at a key logistical nexus between Morogoro Road and Mandela Road, it sits on a spur of the growing commuter rail system, has a new bus commuter bus terminal as part of the Bus Rapid Transit (BRT) system linking the area to downtown and, most importantly, includes the Ubungo Thermal Power Station, which is the terminus of the Songas pipeline funneling natural gas to the city from Mtwara in the south. Similarly, it hosts the Millenium Business Park, also

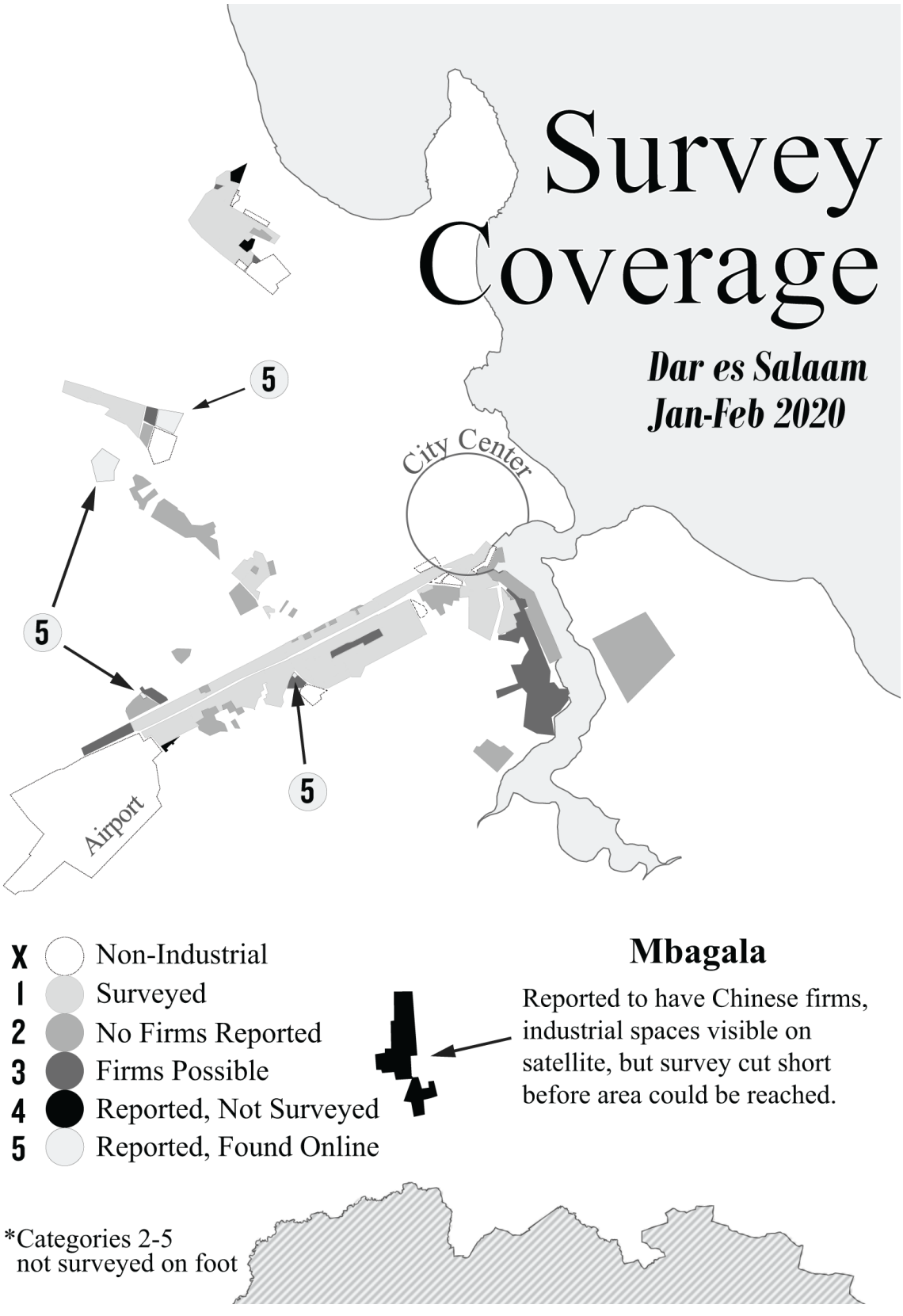
called the Ubungo EPZ. Just south of the Ubungo Zone is the Mandela Zone (~2.1 sqkm), named after Mandela Road, the main northbound thoroughfare leading out of the Tazara Zone. Here, 3-4 smaller clusters are aligned in a string-of-pearls pattern along the main paved street. The largest cluster (the southernmost) extends laterally in both directions. The middle two are essentially corridors with little lateral extension, and the northernmost cluster is the Benjamin Mkapa EPZ. Over time, the Mandela Zone could easily fuse with the Ubungo Zone to the north or the Tazara Zone to the south, though the current density of residential settlement in the area might slow this fusion.

Finally, the Mikocheni Zone (2.28 sqkm) is located at the northern limit of the city's build-up area, within an administrative zone that has officially been designed Mikocheni Light Industries. The large cluster here seems to have developed less around any major logistical endowments (there are none, other than the one highway leading out of the city) and more around an inherited complex of older industries, including steel mills, concrete plants and various light industrial activities like numerous bottling plants (such as the Coca-Cola Kwanza bottling plant located on Coca-Cola Road). This also means that the cluster has spread out laterally from the main road (solely north of it) far more than most of the other industrial zones, which have retained their character as productive-logistics corridors. This likely also has to do with the fact that Mikocheni is one of the areas with a clearer administrative acknowledgement of its industrial status, with its distance from the main city also likely allowing for more expansive development and easier access to land. In every case, the exact spatial nature of each zone's growth can only really be determined through a more focused investigation of all the incidental historical and administrative factors constraining the more basic economic logic of geographical agglomeration.

Using this map as a basis, the survey sought to cover the vast majority of this industrial space, street by street. Before detailing the exact process of data collection, however, limited resources determined that labor would have to be concentrated in certain areas first, to the potential exclusion of other areas toward the end of the survey period. Thus, weight was given to the largest areas (where density allowed for more efficient collection), the areas most consistently reported to host Chinese firms (this was a dynamic factor, changing in the course of the survey as more residents were interviewed), and the areas more accessible from the central city. Some areas were deprioritized because they were consistently reported to host few or no Chinese firms (confirmation was then sought by exploring the existing online data for these areas) or to only host warehousing facilities or a few large utilities projects. In a spare few instances, access was also difficult or prohibited—this was true of the port itself (which also hosts warehouses, apparently leased out by the port authority), a portion of the Vingunguti neighborhood where entry was guarded by a police checkpoint (one firm was reported here by respondents outside the area and found online later) and the Benjamin Mkapa Export Processing Zone, though guards and residents here also confirmed that the only Chinese firm operating in the zone (in fact, it is now a Taiwanese firm) was one already identified via online sources.

In particular, this led to three major limits to overall survey coverage: 1) systematic door-to-door surveys of facilities in the two EPZs were either not possible (Benjamin Mkapa EPZ) or limited (Ubungo Millennium Business Park), and thus a greater reliance was placed on secondary sources, including verbal confirmation of firms' presence but without visual confirmation; 2) The area in and around the port and the industrial facility across the water from it were not exhaustively surveyed on foot, since the port itself had an obvious function with many of the plots in this zone clearly transshipment centers (this was confirmed by interviews)

and others had pre-existing data online showing that they were decisively not Chinese firms—this includes the numerous gas and oil facilities in this area, such as the large Tanzanian and Italian Refining Co. refinery across the water from the port; 3) The visible industrial cluster in Mbagala (and smaller ones in Vikindu and Vibura, beyond the city border) was reported by several respondents midway into the survey as having several Chinese firms, but its distance from the urban core meant that it would not be reached until the survey's final weeks. Unfortunately, the outbreak of the novel coronavirus in China in January of 2020 accelerated the timetable of the survey's final stage and then cut it short before a trip to Mbagala could be arranged—while also disallowing an originally planned week of final clean-up, returning to the few individual blocks accidentally missed on foot or attempting access to the port facilities. Aside from these few locations, Mbagala is the only full industrial cluster that was entirely missed and therefore remains an important site for similar surveys in the future.


Survey Coverage
Dar es Salaam
Jan-Feb 2020
City Center
Airport
5
5
5
X Non-Industrial
1 Surveyed
2 No Firms Reported
3 Firms Possible
4 Reported, Not Surveyed
5 Reported, Found Online
Mbagala
Reported to have Chinese firms, industrial spaces visible on satellite, but survey cut short before area could be reached.
*Categories 2-5 not surveyed on foot

*Map 5-3*

The map above illustrates the survey's coverage in detail. Each polygon represents a manageable breakdown of the industrial zone into walkable units. These breakdowns are somewhat arbitrary, but they provided a useful means by which to schedule each day's survey work by first referring to satellite imagery of the area and then recording survey results by hand, given inconsistent cellular access. Different ranks are given to different zones, in accordance with how thoroughly they were surveyed. Rank 1 indicates areas that were systematically surveyed on foot. In these areas, essentially all buildings were identified as either hosting or not hosting Chinese firms based on neighborhood knowledge (see below for potential limits to this approach or what forms of ownership may have been missed). Ranks 2-4 indicate areas that were not surveyed on foot. Most of these areas were not surveyed because: a) their function was apparent and firms identifiable from other sources (as in the case of the port); b) they were consistently reported to not have Chinese firms operating in the area and drive-throughs seemed to confirm this; c) they were simply missed on foot or their entrance was barred at the time of survey (as in the EPZs and one part of Vingunguti); d) the early cut-off induced by the global pandemic meant they could not be reached in time (Mbagala). Each of these eras was originally intended to be returned to in the final weeks of the survey for clean-up, with the intent that areas accidentally missed or temporarily barred were returned to and captured in the data. This was also prevented by the outbreak of the pandemic.

Nonetheless, drive-throughs of most areas were paired with information from respondents elsewhere and online research on all areas not surveyed on foot to determine the likelihood of Chinese firms operating in the area. Rank 2 indicates areas where secondary

information and drive-throughs indicate that the presence of Chinese firms is unlikely. These are places where respondents consistently and actively said that there were no Chinese firms in operation and where surface-level features visible from the back of a motorbike seemed to clearly indicate only the presence of Tanzanian, Arab or Indian firms (the most common). Rank 3 indicates more ambiguous cases, where reports were inconsistent or indicated some possible presence. In one case, in the Port Zone, some indication was given that additional Chinese warehouses or transshipment yards for containers may have existed (or that Chinese companies were involved in the leasing of space from port authorities) but manufacturing firms were not reported. In another case, in the Tazara Zone, several respondents suggested that there was a factory making nails (*misumari*) but the site could not be located and different locations were suggested for it—yet other respondents had not heard of such a factory in the area.[86] Thus, it is unclear exactly the status of the firm, but if it does exist it is likely in one of the small areas in the Tazara Zone missed in the survey. Rank 4 indicates cases where firms were consistently reported, detail could not be found about them online and the on-foot survey missed them. The largest zone in this category is Mbagala, already mentioned above. But it also includes a few sub-sections of the Mikocheni Zone and one slice of the Tazara Zone. These are areas where respondents on the ground reported the presence of Chinese firms, but they could not be found and precise directions could not be given. If a type of firm was consistently reported but not discovered, that area was reviewed on satellite footage and the few buildings identifiable from satellite which were missed on-foot are presumed to be the most likely locations housing the

[86] The factory could also possibly be making marbles (*marumaru*).In part this was a problem of translation. Only two respondents reported this firm and each had a different regional accent. When asked to describe the product, their explanations were unclear or inconsistent, but one respondent seemed to strongly indicate that the product was nails, as he explained that it was something used in woodworking. It is entirely possible, as well, that a nearby sheet metal firm (recorded in the survey) produced some sort of nails on the side from waste metal.

reported factories. It is also somewhat possible that unclear ownership relations or confusing building layouts in the on-foot survey prevented these firms from being found (see below).

Two unique ranks also exist on the coverage map. Rank X indicates areas that are adjacent to large industrial zones and which appear to also be industrial facilities on satellite images but which are not industrial or are not relevant to the survey—the largest of these is the airport (technically a logistics hub, but not relevant), but also included are shopping malls, office buildings, school campuses and a few otherwise-identifiable technical facilities. Rank 5, meanwhile, shows areas (often narrowed down quite substantially, requiring arrows for indication) which were not surveyed on foot for the same reasons as in ranks 2-4, but where individual firms are known to be operating based on an abundance of secondary information. This includes information from online surveys and information from respondents who consistently identified a single firm, its name, and its rough location. Rank 5 areas, then, include firms listed in the survey, but these firms may have less available information (in particular, number of workers) since they were not identified on the ground. Rank 5 includes both EPZs, which each have one Chinese firm in operation (in fact, one is now a subsidiary of a Taiwanese firm), though less is known about the smaller Twyford showroom and warehouse located in the Ubungo EPZ/Millenium Business Park. Rank 5 also includes two large Sheet Metal (Mabati) producers: the Vingunguti Dragon Mabati factory (same company as Dragon Mabati in the Mandela Zone) and the Sunshare Mabati factory, both in the Tazara Zone.

**Methods**

A single survey methodology was utilized to serve both the primary goal of identifying and recording Chinese firms operating in the industrial zones of Dar es Salaam and the secondary goal of capturing more qualitative features of the new industrial territorial complex developing in the city. The basis of this survey, as already mentioned, was the systematic on-foot observation of these industrial zones. This allowed for immediate qualitative features of these zones to be recorded in more narrative form, while also allowing for the identification of firms through signage and interviews with locals. Footwork also allowed the more general dominance of Tanzanian, Indian and Arab firms (also evident in the Orbis data) to be generally observed without the time and labor necessary for data on these firms to be systematically recorded. Finally, the on-foot nature of the survey allowed for rapid spatial snowballing interviews to occur, where respondents who didn't have certain information could be asked if they knew anyone nearby who might know, after which new respondents could be identified and further information gathered.

The on-foot survey was preceded by more informal interviews with various drivers (mostly *pikipiki* and *bajaji* drivers, but some rideshare car drivers as well) familiar with large areas of the city as well as the many informal "tour guides" who attempt to sell informal historical or cultural tours on the streets of downtown. Here, the survey also benefited from informal advice given by other academic contacts who had worked in the city or insight from pre-existing Tanzanian and Chinese acquaintances who were familiar with the area. In one instance, a respondent volunteered to guide me to the location of one particularly hard-to-find factory, but the surveys were otherwise entirely unguided. Interviews were conducted by the author primarily in Swahili, with some use of English to clarify unclear terms. Mandarin was used when interviewing Chinese employees and managers. The general format of the survey was

to start at the day's un-surveyed segment of the industrial zone and proceed on foot along one side of the street. Factories were identified by signage. If they were obviously non-Chinese, they were disregarded (though this was almost always double-checked with locals) and if they were ambiguous or had no signage, any individuals in the immediate area were asked if they had any information on the factory. These individuals were usually shopkeepers tending small food and water stands, *pikipiki*, *bajaji* or taxi drivers awaiting a fare, workers on lunch breaks or simply people gathered under spots of shade awaiting the bus or chatting—frequently, these were informal gathering spaces, called *maskani*, where people in the area tend to congregate and which periodically take on semi-official capacities in local governance. Sometimes guards employed at the factory gates or freight drivers waiting to unload were interviewed as well.

Once a site was identified as hosting a Chinese company, interviewees were asked a series of questions about the firm. Redundancy was central to the data gathering process here, with the same questions repeated to multiple respondents in the same area to ensure that the information was robust (and, in particular, to better estimate the number of workers employed). At the site of every Chinese firm, effort was then made to find out whether any Chinese workers or managers were on site and to gain entry to interview them. At times this was successful and interviews were conducted with a wide range of Chinese workers present on site (see below). In other instances, Chinese managers were present but workers or guards explained that it would not be feasible to gain access or to ask the manager to come out (the implication being that they might get in trouble for bothering the boss or giving away information). And periodically facilities would be identified consistently by locals as operational Chinese firms but no guards, employees or managers could be accessed and less information could be obtained. All interviews were then usually completed by asking if the respondents knew of other Chinese factories or

warehouses operating in the area or in the city in general. If they did, they were asked whether or not they knew what product line these firms were engaged in, helping to narrow the search and to clarify firms that had already been visited versus those not yet recorded.

The interviews were by their nature informal but were nonetheless guided by a minimal script that centered on asking a few key questions. This script began with asking whether a factory was Chinese or not. If not, an attempt was made to determine the firm's ownership. If the firm was Chinese, the interview would then progress to questions about what products the firm produced, how many workers it had, how long it had been located here and if the same company had other locations in the city. These questions would then be repeated with other nearby respondents—this confirmation was extremely important, as there was often periodic confusion between Chinese firms and other East Asian companies (such as a Japanese-owned cigarette producer) which was only clarified through repeat interviews and secondary online research. The repetition of many questions also helped to better determine otherwise vague knowledge about the number of workers and the years that a site had been in operation. If Chinese employees were available to be interviewed, a similar script was followed asking the same questions, to which were added a few additional ones such as: where in China the firm was from, where the workers themselves were from, how long they'd been in Tanzania (or overseas more generally) and why the firm had relocated here. In a few instances, follow-up interviews with Chinese respondents were later conducted via WeChat.

The results of these interviews were recorded by hand in a notebook and the GPS coordinates of the factory were recorded by dropping a point roughly on the center of the firm's plot of land (evident in satellite imagery) on Google Maps—this was done while standing in front of the firm, using offline maps and the phone's built-in GPS, with the coordinates of the

point screenshotted and saved. These screenshots and hand-written notes were then transferred to an excel spreadsheet. At this point, further research into each individual firm was conducted online. In some instances, little to no additional information was found. In others, an abundance of information was available, providing confirmation and clarification of data obtained in the interviews. This also allowed for a more rigorous tracing of subsidiary firms' chain of ownership. It was at this stage that the handful of firms in Rank 5 (not surveyed on foot but visible online) were added. In addition to the formally coded information obtained from the interviews with regard to the individual firms, hand-written notes were also kept on the general tone of the interviews, some of the offhand comments that respondents would make when remarking on Chinese firms, the attitudes of Chinese workers towards locals and their perception of life in Tanzania or Africa more generally. Photos were taken within these industrial zones to record key characteristics of both the individual firms being surveyed and the general features of the industrial zones themselves. Further notes were recorded about the layout, traffic flow, overall composition and just the general feel of each zone—these notes become the basis for some of the narrative account offered below.

Variables were recorded for the name of the firm, whether it was a subsidiary or the primary owner and, if a subsidiary, what firm was the ultimate owner or primary financer, whether the firm was engaged in manufacturing or warehousing, what product line(s) it operated in, how many workers it had (often expressed as the range of numbers given by most respondents), when it was founded (sometimes listed as a year, sometimes with more qualitative descriptors such as "a long time"), the website of the company and/or its ultimate owner, the notes recorded on each firm, and its geographic coordinates. The number of workers and year founded were then reproduced in a simplified form allowing for easier data analysis: ranges were

converted to their midpoints and firms for which no precise year could be found for when they began operating were converted to NA values.[87] Thus, a firm with an estimated 300-500 workers which respondents claimed had been there a "long time," but with no knowledge of the actual founding year, would be listed as a firm with 400 workers, with an NA value in the variable recording when it was founded. The data was then managed and visualized using R and R Studio, with geoprocessing and mapping conducted through the simple features package.

**The General Character of Dar es Salaam's Industrial Zones**

Before presenting the survey data pared down to its final form, it will first be helpful to describe the territories within which Chinese firms are operating and the ways in which these firms' economic activity has helped to shape and re-shape these territories. This qualitative detail is essential to conceptualizing the statistical results since it is impossible to understand "industrial location" purely on the level of the individual firm. Similarly, questions of industrial location at the local or regional level will also exhibit the more general features of geographical industrialization as such, wherein the inherent disequilibrium of capitalist production constantly shapes and re-shapes territory according to its imperatives. The guiding logic here is the fact that "the sites of economic activity are produced in and through industrial growth, not selected after the fact" (Storper and Walker 1991, p.36). In other words, "industries produce regions" (ibid, p.71), not the other way around. One goal of this survey is to offer a case study of how one of the

[87] In addition, many respondents tended to gravitate toward the year 2015 when asked how long the Chinese company had been in operation. In part, this is likely because there was a real boom of investment in 2014/2015. But it also seemed to operate as a sort of default year—if explicitly asked to guess, respondents almost always guessed 2015. In the full version of the table, an asterisk marks the founding year when most respondents claimed that the firm began operations in 2015 but seemed uncertain or seemed to be simply listing this as the year that made the most sense. In the simplified version, this asterisk is removed.

fastest growing cities in the world is being integrated into the larger industrial-territorial region slowly developing in East Africa and along the Swahili Coast specifically. On the one hand, this process must be understood at a global scale, as part of a larger territory gestating across the Indian Ocean Rim. This is, moreover, a contingent process, currently dependent on development elsewhere and sustained mostly by these ties of trade. On the other hand, the production of such territories reshapes space at the most local level. The literal process of grounding oneself in such spaces, then, provides integral insights into the real characteristics of these territories, nullifying hyperbolic accounts extrapolated from outlier examples, journalistic fly-overs or misrepresentative secondary data.

Conducting the survey in early 2020 proved to be particularly fortuitous. Industry was still operating as usual prior to the outbreak of the pandemic, but the speculative boom in investment and real estate that hard marked the mid-2010s had decisively settled. Thus, industrial activity and urbanization were neither in the throes of unrepresentative rapid growth nor suffering from an unrepresentative depression induced by the effects of the pandemic on global supply chains. Both the built-up area of the city and its fringe of urban settlement had seen massive growth in recent years, but this growth had also been bifurcated. For every new office tower constructed or condominium sold, the city's building boom left behind an equivalent share of half-empty luxury apartments, half-finished demolitions and numerous abandoned or indefinitely stalled construction sites. One of the city's tallest building is still the skeleton of the unfinished Mzizima Towers looming over the very center of the city, a fitting contrast to the shining golds and blues of other recently completed skyscrapers overlooking the bay. The most striking feature of Dar es Salaam itself was therefore this feeling of simultaneous boom and bust. The two were coextensive, their equally productive and destructive potential weaving the urban

fabric into a tapestry of shining steel and glass embroidered with growing rings of slum and rubble.

In both the urban core and the outlying industrial zones the feeling was like a wave had emerged from across the Indian ocean, struck against the beach, flooded the marshlands and was now slowly pulling back from shore. This is a loose metaphor, but not so distant from what happened: a massive inflow of capital originating overseas had crashed into Swahili coast, flooding the port cities already open to the flow and from there coursing more selectively into the depths of the mainland along a few new corridors (Mombasa to Nairobi, Djibouti to Addis Ababa). In Dar, it reshaped the landscape and washed away any parts of the old city not firmly fixed in place, but it also had nowhere else to go. Dar is already the primate city of Tanzania, so it became the *de facto* catchment for much of this investment—with some small tributaries trickling out to distant investment sites in Mtwara, Tanga or Shinyanga. In Dar, the tidal wave was in many ways unprecedented, flooding far past the sea walls to saturate the soil far inland, with construction cranes frothing up at the vanguard of the wave. But this inflow had then begun to retract. As I walked through the city, its sand was still soaked with the leftovers of this excess capital, collected in small pools teeming with life but now distant from their source. Meanwhile, the most speculative flows were now exposed to the scorching Equatorial sun, drying rapidly, their projects abandoned to molder in the humid heat. All the while, the deeper nature of that wave remained unclear: would this be a new tide, rising and falling in cycles? Or was it a mere rogue wave? Something triggered by a special confluence of events, flooding the city with cash which then slowly trickled away?

Maybe the starkest sign of this fact could be found on the edge of the Tazara Zone, at Mkuki House, a large mall funded by international investors during the building boom. At first, I

had assumed that the mall was brand new and not yet populated with businesses. The building itself was largely empty aside from a well-lit grocery store operating on the first floor, a Pizza Hut on the plaza and a furniture shop across from it. Inside, most lights were kept off to save on electricity. The escalators were either frozen or only turned on momentarily. Upstairs, a few small shops clung to life, cradling the only flickering lights a the vast, dark interior where most retail space sat shuttered. Contrary to my assumption, however, the timeline was exactly the opposite. As I wondered through the dark hallways, a security guard eventually came up to me and asked what I was looking for. "There isn't anything open down there," she explained. When I asked about the history of the building, she said that the mall had been bustling just after construction was completed. The building was funded by foreign investment, but she didn't know where from. Most of the businesses that had rented space, however, were owned by Tanzanian shopkeepers.[88] At the time, cash was flowing freely enough—and especially within the upper echelons of the city's social scene—that conspicuous consumption was well-funded, embossing the image of a rising "African middle class" in international media, despite the reality being much more limited (see Melber 2016). I asked why the shops had closed and the security guard simply shrugged: it was not profitable. After the speculative boom ended, the easy money that had funded a boom in "middle-class" consumption had dried up. The result was that the few open shops I had seen were not the first in but instead the last left. Maybe business would come back, she speculated. For the time being, the remaining high-end consumption concentrated on one or two shopping centers. The rest of the population got most of their goods on low-end, open-air markets like that in nearby Kariakoo—and Kariakoo vendors certainly couldn't afford

[88] Here and elsewhere, it was common for respondents to describe the ownership of firms in terms of perceived ethnicity rather than nationality. Though the two mostly overlapped in the case of Chinese firms, other firms were often identified as being owned by Indians or Arabs—but often these individuals were Tanzanian nationals and would have been recorded by official statistics as Tanzanian firms.

the rents necessary to pay for electricity in air-conditioned malls. The image, then, was almost like a hyper-accelerated montage of the typical lifecycle of the American shopping mall, which boomed briefly in the postwar decades, reaching an apex alongside the dotcom boom before being slowly driven to bankruptcy and abandoned. Here, however, the entire cycle had played itself out in a mere handful of years.

Beyond the mall, the stretch of industry began: toward the port in one direction, and in the other toward the TAZARA terminus and the airport. The backbone of both zones were their central, paved thoroughfares: Bandari Rd (which splits into Kilwa Rd and Nelson Mandela Rd) for the Port Zone and Julius Nyerere Road (also intersected by Mandela Rd) for the Tazara Zone. These were the main bus routes through which workers could commute in from nearby neighborhoods. Meanwhile, the Tazara zone linked to the city's commuter rail system runs parallel with a new long distance standard gauge railroad being built by a Turkish firm. Similarly, the smaller clusters in the Mandela and Ubungo Zones were tightly beaded around their central thoroughfares. Paved side roads were only common in the Mikocheni Zone, allowing almost all industrial facilities to face a paved street, with a few exceptions. Elsewhere, it was far more common for the paved central thoroughfares to host the larger and most formal of the zone's industrial facilities, ensuring them easy access for shipping and deliveries. Unpaved spurs and side streets then led to other industrial sites, often less formal but by no means less well trafficked. The bulk of firms in the Tazara Zone, for instance, were located on unpaved side roads and it was common to walk past lines of parked container trucks, as drivers waited to load or unload or simply took a break. The amount of traffic combined ensured that these roads were often potholed and precarious. No only were they prone to extreme flooding in the rainy season—at which point many become essentially impassable—but also in the dry season, due to

poor drainage. Several intersections were permanently flooded and, judging by satellite imagery, appear to have been flooded for some time. Similarly, the poor conditions periodically put other infrastructure at risk. One instance, pictured below, shows a collapsed power line in the Tazara zone with traffic still passing underneath.




***Image 5-2***
*One representative road in the Tazara Zone*
*Photo by author, January 2020*



***Image 5-3***
*Fallen power lines in the Tazara Zone, with traffic passing beneath*
*Photo by author, January 2020*

Meanwhile, many zones abut informal slum settlements and are intercut with numerous types of small-scale production and retail activity. The most common are kitchens and shops, including many moving vendors. But in some places these more informal areas also agglomerate into their own hubs and corridors of workshop-style production. This is particularly true around and between the clusters of the Mandela Zone, at the intersection of Bagamoyo Road and Coca-Cola/Sam Nujoma Road in the Mikocheni Zone and along Mwakalinga Road near the railyard in the center of the Tazara Zone. Even where informal industry does not predominate, however, the industrial zones tend to be surrounded by working class residential areas—and many respondents

explained that workers in the zones live in these neighborhoods, most of which would be classed as urban slums. Here, the building boom had not resulted in the construction of many new apartment complexes. Only the Mikocheni Zone saw some new, higher-quality housing construction, due to its location nearer the city's wealthier outer wards to the north. Most of the new apartment construction was focused near the city center, on the northern peninsula in wealthy Masaki and Oyster Bay or even further north toward Mbezi Beach, a richer exurb that hosts several coastal resorts. In contrast, the settlements near the industrial zones tend to be places like Mburahati or Manzese. Colloquially known as *uswahilini*, these are dense, low-income proletarian settlements dominated by informal tin-roofed construction, prone to deadly flooding in the rainy season and often poorly integrated into utilities infrastructure. Their proximity to the industrial zones also makes them major risk areas for industrial waste exposure through air and water pollution.



***Image 5-4***
*Informal settlement located near the Ubungo power plant*
*Photo by author, January 2020*



***Image 5-5***

*Satellite Image of the area pictured in Image 5-4 above, displaying the settlement's proximity to the Ubungo power plant and the general layout of* Uswahilini *adjacent to the industrial zones.*

*Source: Google Maps, Maxar Technologies, 2021*



***Image 5-6***
*Pollution from a plastic recycling firm abutting a residential neighborhood at the edge of the Mikocheni Zone*
*Photo by author, February 2020*

In terms of their industrial composition, the zones are by no means dominated by Chinese firms. In fact, Chinese companies tend to be hard to find and do not advertise their name or business as openly as many other companies operating in the area. Large, modernized facilities

often have prominent signage advertising the name of the firm or its larger conglomerate. The vast majority of these firms are Tanzanian, though they are often drawing funding from across the Indian Ocean rim. This relationship is evident in the prominence of Indian, Omani and UAE funding in existing FDI statistics, but it's also visible on the ground. When asked about the ownership of such firms, respondents often referred not to the nationality of the owners and managers (or the registration of the company) but instead to their ethnicity. When asked if a particular factory was Chinese, a common response would be: no, it is owned by Indians / by Arabs. Further research showed, however, that many of these companies were Tanzanian and their managers and owners were Tanzanians of Indian or Arabic descent. This arrangement seems to be far more common than direct foreign ownership and is itself a direct inheritance of subsequent waves of colonization in the region, including its incorporation into the British Empire but also stretching back to the Omani sultanate in Zanzibar (Sherriff 1987, Coulson 2013). Such firms still draw on regional networks to gain financing and many trade networks (such as in pharmaceuticals) are oriented toward India rather than China for this reason.




***Image 5-7***
*A typical large industrial facility owned by Azam, a subsidiary of Bakhresa Group, Tanzania's largest conglomerate.*
*Photo by author, January 2020*

Chinese firms are often entirely unmarked. Others have signage solely in Swahili and English. Only a small handful are branded with the name of the Chinese corporation that owns the facility. Those that do advertise this linkage tend to be larger, more established firms, such as FAW, AVIC or CNBM. But most Chinese firms operating in the city are small and tend not to be subsidiaries of the largest conglomerates. In the case of one Mikocheni Zone factory assembling motorbikes, the exact activity of the facility at first seems completely anonymous, as passersby are met with nothing but a tall, bare white wall and enormous metal door (see Photo 5-8). At the time of the survey, the firm had been identified by locals at a nearby *maskani*. This case

emphasizes the importance of the grounded survey method, since such a firm would have been unidentifiable otherwise.[89] Its Chinese ownership was then confirmed by a lone guard on duty, invisible from the street, interviewed through a small, barred window cut into the wall near the door. In another case, a Mandela Zone factory making mattresses and tanks for compressed gas advertised primarily in Swahili and English, though it also listed a transliterated Chinese name (see Photo 5-9). Finally, there were a few unique cases such as a facility making plastic bags within a rented space on the edge of the Tazara Zone. The existence of the firm was completely invisible from the street and the facility itself did not at first appear to be industrial at all. Instead, it was listed as the Sophia Hotel, offering residential, retail, office and warehouse rentals. The existence of a Chinese-owned industrial firm was made clear, however, because the company's workers wore shirts branded with its logo in Chinese. Seeing these workers on their lunch break in the neighborhood and inquiring about the shirts led to discovering the actual site of the factory (see Photos 5-10 and 5-11).

[89] It could be identified, however, by similar ethnographic methods approaching it from the opposite direction: through Chinese business networks. This is the approach used in Xia 2019, which also records a *pikipiki* factory—presumably the same.



***Image 5-8***
*Chinese-owned pikipiki factory in Mikocheni Zone*
*Photo by author, February 2020*




***Image 5-9***

*Chinese-owned Ocean Kiss mattress and gas tank company in Mandela Zone*

*Photo by author, February 2020*




***Image 5-10***
*Chinese logo printed on workers' uniforms*
*Photo by author, January 2020*




***Image 5-11***
*The site of a plastic bag manufacturer in Tazara Zone, its existence indicated only by the uniform pictured above*
*Photo by author, January 2020*

At the time of the survey, traffic flows were heaviest in the northern end of the Port Zone and throughout Mandela Zone, but this likely had more to do with lagging road quality than comparative commercial activity. Overall, the impression was one of industrial development and urbanization overtaking infrastructural capacity, but certainly not at the breakneck pace observed decades ago in places like China's Pearl River Delta. New roads were being constructed in the most congested areas, with particular priority given to the port. Similarly, the terminus of the new standard gauge railway built by Turkish Firm Yapi Merkezi was nearing completion (and at

the time, its construction site was the cause of the worst traffic snarl heading into the city from the airport). But in places like the Tazara Zone or parts of the Mikocheni Zone, multi-lane thoroughfares and paved access roads ensured easier access and thereby showed the true level of industrial activity in a clearer light. The feeling in these areas was one of much more muted activity, especially as compared to industrial zones in China or the United States. In the few instances where interviews were conducted within factory and warehouse compounds, the feeling was almost universally that of a "slow day," with the majority of workers milling around with little to do, while a minority engaged in some productive activity in the background.

Overall, the impression here was similar to that seen in the city's stagnant, half-finished construction boom: Business was certainly bustling, and the largest firms were especially active, making use of new capacity installed in recent years. At the same time, it seemed that this capacity had overshot actual demand. Numerous respondents explained that, in previous years, there had actually been a significant crackdown on informal, low-skill, labor-intensive workshops owned by Chinese investors, forcing most out of business. The story went that these firms had been the first to go because they hadn't legally registered their activities, weren't paying taxes and were the most egregious violators of Tanzanian labor laws. They had survived through bribery and kickbacks to local officials, who were similarly targeted in Magufuli's anti-corruption campaign. When asked about the character of these facilities, respondents described something similar to the conditions still prevailing at the few plastic bag manufacturers found in the survey—though often smaller scale and more informal. At least to some degree, then, it seems like the speculative building boom was accompanied by a similar boom and bust cycle in industrial activity. While the situation was by no means as extreme as the case of the mostly empty Mkuki House mall, the trend was shaped by the same arc of foreign investment—which

had last peaked in 2018. Further research would be required to illustrate the exact mechanics involved or to demonstrate how sustainable existing industrial production is in a context of declining FDI inputs. Similarly, FDI might be only in a momentary trough, and even if Chinese investment continues to decline other sources might step in to make up for the difference.

**Results of the Survey**

Overall, 30 Chinese firms were identified in the survey.[90] Of these, 26 were surveyed directly on foot. Only 4 were found through secondary sources afterwards.[91] Most firms (~20) were identified by name. For the remainder, respondents either did not know the formal name of the firm or provided somewhat ambiguous answers (recorded in the tables below with a question mark). For most firms estimates of the number of workers (for ~23 firms) and year of founding (for ~19 firms) were provided. Often the number of workers was given as a range. The survey preserved the original range estimate, while also adding a simplified mean value for purposes of visualization. Similarly, respondents were often somewhat unclear about the number of years a firm had been in operation, giving colloquial responses such as "a long time." When asked if they knew the exact year, most guessed 2015. Thus, two variables for the year in which a firm

[90] There is only one instance of a single firm having two locations: Dragon Mabati. Since each site seems to conduct its own business, these are treated as separate "firms" in the survey. The number of workers and years of operation are recorded for each site, not for the whole firm. Technically, however, they're owned by the same company.

[91] Of these, two were within gated business parks. One was in an area of the Tazara Zone that had a temporary police checkpoint, and the third was on a block that was simply missed on foot. Since the survey was cut short, the latter two could not be returned to in time to gather more information. Regardless, more information was available on each of these firms online and many respondents offered detailed reports. In the case of the second Dragon Mabati site in Vingunguti (within the Tazara Zone), information on this facility was also gathered by interviewing employees at the city's other Dragon Mabati facility in the Mandela Zone. The two are of roughly equal size, though it appears that more production may occur at the Vungunguti site, with the other serving more as a large warehouse space (plus some final processing work, like cutting sheets down to size for the customer).

was founded are preserved in the full dataset: one preserving the colloquial answers and indicating where "2015" seemed to be a guess rather than a hard number; and one in which the value is simplified by converting colloquial answers to NA values and presuming answers of 2015 to be accurate. The result is that this variable is likely the least accurate. In addition, variables were gathered about whether the firm was engaged in manufacturing or warehousing, what its main product(s) were, whether it was a subsidiary of another firm and whether it had any further information posted online. Finally, the geographic coordinates for each firm were recorded[92] to allow for mapping. The final table including the non-simplified number of workers and year of founding is printed below.

[92] In WGS 84 (EPSG:4326)

**Chinese Firms**

Dar es Salaam, 2020

| Company Name | Financer | Zone | Industry | Product | Workers | Founded | Size |
|---|---|---|---|---|---|---|---|
| Yidai | Self | Ubungo | Manufacturing | Electrical Transformers | 100 | 2019 | Medium |
| Unknown | Unknown | Tazara | Manufacturing | Glass (Vioo) and Steel Framings | 100 | Long Time | Medium |
| 矩阵国际 - Matrix International (T) LTD | Unknown | Tazara | Manufacturing | Bags (Mifuko) | 300 | 2013 | Very Large |
| Unknown | Unknown | Tazara | Manufacturing | Sheet Metal (Mabati) | 300 | Long Time | Very Large |
| Goodone | Unknown | Tazara | Warehousing | Hardware Goods | Unknown | Unknown | Small |
| Goodone?[1] | Unknown | Tazara | Manufacturing | Tires | 300 to 500 | Long Time | Very Large |
| Unknown | Self | Tazara | Warehousing | Metal Door Frames | 15 to 20 | 2015[2] | Small |
| Unknown | Self | Tazara | Warehousing | Mitumba | 1 to 7 | 2015[2] | Small |
| Unknown | Self | Tazara | Warehousing | Mitumba | 1 to 7 | 2015[2] | Small |
| Hainan International | China Railway Construction Corp 20th Bureau | Mikocheni | Warehousing | Construction Materials | 1 to 5 | 1999 | Small |
| Unknown | Unknown | Mikocheni | Manufacturing | Motorcycles (Pikipiki) | 12 to 14 | 2015 | Small |
| Unknown | Unknown | Mikocheni | Manufacturing | Metal Gas Pipes | Unknown | 2015 | Small |
| Unknown | Unknown | Mikocheni | Manufacturing | Bags (Mifuko) | 10 to 20 | Long Time | Small |
| 中国春城 - Spring City Garage | Self | Mikocheni | Warehousing | Car Repair | 10 | 2000 | Small |
| Shun Tao Investment Co Ltd | Unknown | Mikocheni | Manufacturing | Recycled Plastic Material | 20 | 2013 | Small |
| Sunshine Card High Technology Ltd | Unknown | Mikocheni | Manufacturing | Magnetic Cards | Unknown | Unknown | Small |
| 巨业商混公司 - Ju Ye Concrete Company Ltd | Unknown | Mikocheni | Manufacturing | Concrete | 100 to 200 | 2018 | Large |
| Jia Yi Xing Clothing Trade Limited | Self | NA[3] | Warehousing | Mitumba | 1 to 7 | Unknown | Small |
| 第一汽车集团 - FAW Group | Self | Tazara | Warehousing | Trucks | 100 | Unknown | Large |
| 汽车人汽修 - Autobot Garage | Unknown | Tazara | Warehousing | Car Repair | Unknown | Unknown | Small |
| Avic-Shantui | Self | Tazara | Warehousing | Construction Vehicles | Unknown | 2015 | Small |
| Dragon Mabati | Self | Mandela | Manufacturing | Sheet Metal (Mabati) | 500 | 2017 | Very Large |
| Ocean Kiss | Chang Qing International | Mandela | Manufacturing | Mattresses, Sofas and Tanks for Compressed Gas | 50 to 100 | 2010 | Medium |
| Anbano International Investment Co | Self | Mandela | Manufacturing | Bags (Mifuko na Viroba) | 50 | 2010 | Medium |
| 北新集团 -- BNBM (Beijing New Building Materials) | CNBM - 中国建材股份 | Mandela | Warehousing | Construction Materials | 158 | 2008 | Large |
| Unknown | Unknown | Mandela | Manufacturing | Sheet Metal (Mabati) | 30 to 50 | Unknown | Medium |
| Dragon Mabati[4] | Self | Tazara | Manufacturing | Sheet Metal (Mabati) | 500 | 2010 | Very Large |
| Sunshare Mabati[4] | Unknown | Tazara | Manufacturing | Sheet Metal (Mabati) | Unknown | 2015 | Very Large |
| Tanzania Tooku Garments Co Ltd[4] | JD Group (Roohsing Group)[5] | Mandela | Manufacturing | Clothing | 2500-3000 | 2012 | Very Large |
| Twyford Ceramics[4] | Sunda International | Ubungo | Warehousing | Ceramic Tiles | Unknown | Unknown | Small |

[1] Many respondents said that this was a tire factory called Goodone, which is also the name of a prominent local hardware goods brand, but unable to confirm further connection.

[2] These dates are estimates given by interviewees who weren't certain

[3] Located in Kariakoo market area, not an industrial zone

[4] Not surveyed on foot, location obtained online and from interviews.

[5] The parent company of Tooku Garments (JD Group, from Changzhou) was acquired by a Taiwanese firm, Roohsing Group, in 2017. Included here since Tooku began as a subsidiary of a mainland firm.

Source: Field Work by Author, Jan-Feb 2020

*Table 5-1*

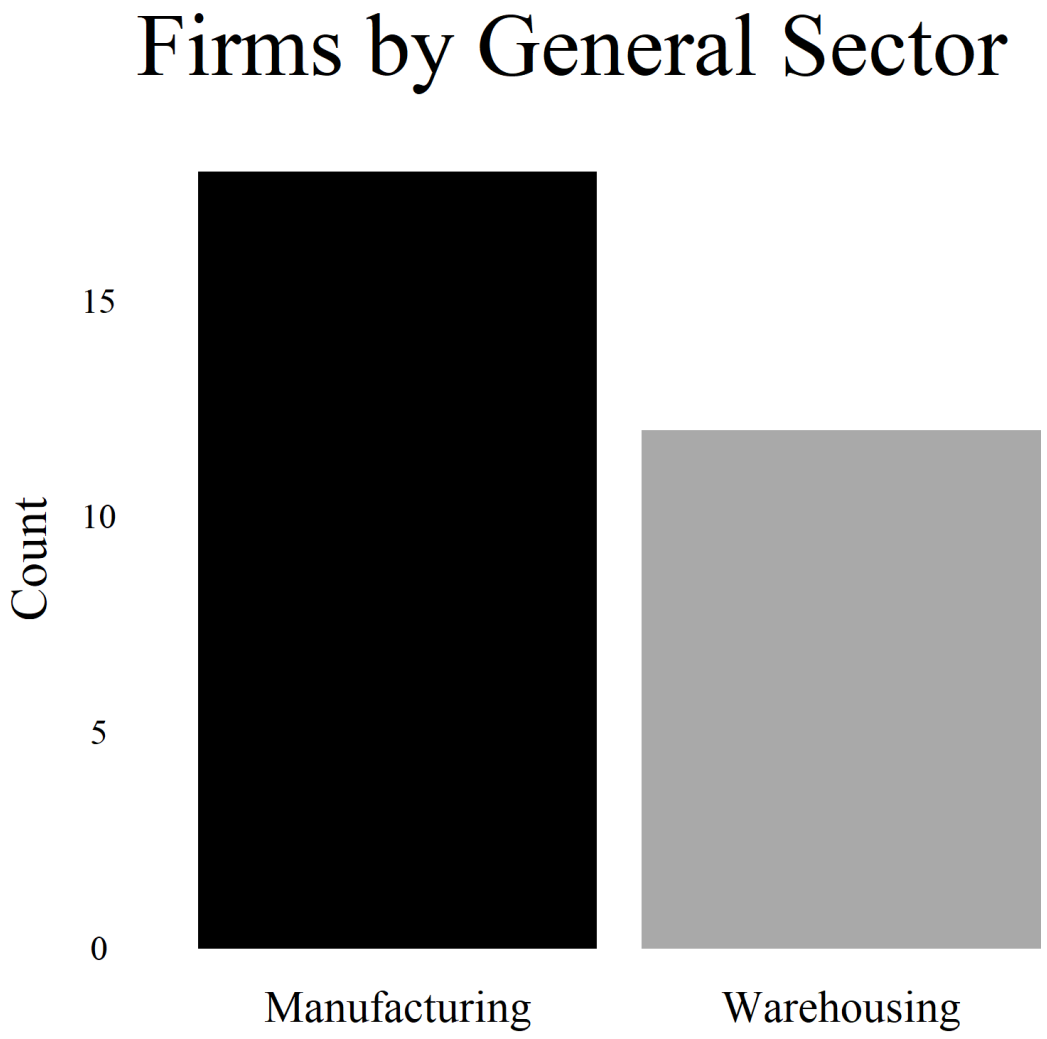



*Figure 5-18*

The majority of Chinese firms surveyed were in manufacturing (18), with the remainder in warehousing (12). That said, often these categorizations were somewhat ambiguous. Many firms appeared to be primarily warehouses or storage yards devoted to wholesale trade, but also engaged in some final processing (as in the case of one of the two Dragon Mabati sites). Others

might have been better categorized as services, but also warehoused goods and even engaged in what might be characterized as "artisanal" production—this was particularly true of the three auto shops captured in the survey, one of which (FAW Group) was simply a warehousing site for large trucks, one of which engaged in warehousing but made most of its profit from auto repair (Autobot Garage), and the final of which (Spring City Garage) acted as a warehouse for parts, an active garage for repair and constructed entirely new machines out of spare parts.[93] Nonetheless, since the manufacturing activity of the latter firm was extremely informal, it's being listed under Warehousing. A better idea of what sort of business was conducted at each location and what subsectors Chinese firms tend to populate is provided by Figure 24, showing the number of firms in each product line. From this, it's clear that Chinese manufacturing activity is not particularly concentrated in any one line, aside from possibly Sheet Metal (with 5 sites, 2 of which are owned by Dragon Mabati) and the processing of plastic products and production of plastic bags from the recycled material (3 firms produce bags, 1 recycled plastic material).[94] In some cases, firms also engaged in multiple lines of production, as at the Ocean Kiss factory, which manufactures mattresses, sofas and tanks for compressed gas. Older firms tended to be more informal, focusing more on (re)assembly work or operating in product lines with fewer capital costs. The motorcycle manufacturing site in the Mikocheni Zone, for instance, imports essentially all its parts from China and then assembles the finished products by hand.

[93] The diversity of activities reported here was not due to ambiguity in respondents' information. The Chinese owner of this firm was interviewed directly. His was one of the oldest businesses captured by the survey, founded 20 years prior—with the owner living in Tanzania that entire time and speaking fluent Swahili. Of all the Chinese enterprises in the survey, this one seemed to most closely match the prevailing character of the city's non-Chinese enterprises: it engaged in multiple lines of business, production was informal and the lines between manufacturing, warehousing and services were blurred to non-existence.

[94] In this case, it's also worth noting that one reported plastics recycling firm was never found on foot and that Xia 2019 reported far more plastics recyclers operating in the country at the time of her survey. It's possible that several have since shut down or that some were not located in Dar es Salaam (Ying's survey includes no geographic data on firm location).

The most recent firm, by contrast, was the electrical transformer factory in the Ubungo Zone. This site was still in the process of being set up and interviews were conducted with both the Tanzanian workers in training and the Chinese staff sent to install the machinery and train local laborers in its use. The Chinese workers lived in the factory itself, sleeping in makeshift dormitories set up in what would ultimately become the building's office space. It was clearly a capital-intensive facility, in the process of importing equipment from its parent company in Nanyang, Henan. When completed, it would employ roughly one hundred workers, mostly locals. The Chinese workers setting up the facility had all been employed at the company's various factories in Henan prior to taking this job. At the time of the survey, Tanzanian workers were being trained in welding and the operation of the few pieces of machinery already installed. At least one Tanzanian worker not on site at the time had worked as a student intern for the firm in China. This was the site where the most intensive interviews of the survey were conducted, both coincidentally—since these young Chinese workers seemed much more interested in talking and keeping in touch afterwards than the owners and managers interviewed elsewhere—and because the facility itself stood out as an example of a Chinese firm entering the local manufacturing market much higher in the value chain. Contact was maintained with two of the Chinese workers after the onsite interviews, who provided updates about the factory's progress and offered further information about their experience in Tanzania. The outbreak of the coronavirus pandemic led to a voluntary extension of their contract. They reported that the factory construction was completed several months after the survey and a second factory was being set up by the same company in Mlandizi, about 30km east of the city borders and 50km east of the first facility in Ubungo. By December of 2020, they had returned to China after successfully setting up the second factory.

**Chinese Firms by Product**

Dar es Salaam, 2020

| Firms | Product | Industry |
|---|---|---|
| 5 | Sheet Metal (Mabati) | Manufacturing |
| 3 | Bags (Mifuko) | Manufacturing |
| 3 | Used Clothes (Mitumba) | Warehousing |
| 2 | Car Repair | Warehousing |
| 2 | Construction Materials | Warehousing |
| 1 | Ceramic Tiles | Warehousing |
| 1 | Clothing | Manufacturing |
| 1 | Concrete | Manufacturing |
| 1 | Construction Vehicles | Warehousing |
| 1 | Electrical Transformers | Manufacturing |
| 1 | Glass (Vioo) and Steel Framings | Manufacturing |
| 1 | Magnetic Cards[1] | Manufacturing |
| 1 | Mattresses Sofas and Tanks for Compressed Gas | Manufacturing |
| 1 | Metal Door Frames | Warehousing |
| 1 | Metal Gas Pipes | Manufacturing |
| 1 | Motorcycles (Pikipiki) | Manufacturing |
| 1 | Recycled Plastic Material | Manufacturing |
| 1 | Tires | Manufacturing |
| 1 | Trucks | Warehousing |
| 1 | Various Hardware Goods | Warehousing |

[1] This firm was only identified by signage, followed by an online search to confirm its existence. They appear to be involved in the printing and encoding of magnetic cards, not the fabrication of the cards themselves.

Source: Field Work by Author, Jan-Feb 2020

*Table 5-2*

In warehousing, Mitumba (bundles of used clothes) are the most common product line. This makes sense, given the size of this trade. By far the bulk of firms in this sector are non-

Chinese. The three surveyed were outliers in an industry dominated by small warehouses owned and operated by Tanzanians.[95] Most warehouses captured by the survey were small and linked directly to manufacturers in China. A prototypical firm was one warehouse in the Tazara Zone linked to a manufacturer of unassembled metal doors and door frames in Guangzhou.[96] Here, Tanzanian workers loaded up trucks with wholesale orders bound for local shops or construction sites under the oversight of several young Chinese managers, all of whom had worked for the same company in Guangzhou. Warehousing activities were most common for product lines such as this one, related in some way to construction even if not specializing in core building materials. The same could be seen in the Goodone hardware warehouse in the Tazara Zone, which stored and sold various hardware goods to small retail centers across the city as well as the Avic-Shantui industrial yard in the Tazara Zone, where construction vehicles were stored and sold. Other warehouses were linked to Chinese firms that had were in the process of building up local manufacturing capacity in various product lines. The Twyford Ceramics site in the Ubungo Zone (within Millenium Business Park) is a small warehouse but is linked to a larger firm (Sunda International) in the process of constructing a large ceramics factory along Morogoro Road to the east of the city. Similarly, the BNBM warehouse in the Mandela Zone is linked to the CNBM conglomerate and has just completed a deal (mentioned above) to open an enormous cement production complex in Tanga. Finally, the Hainan International site in the Mikocheni Zone was essentially a dedicated storage yard for construction projects that had been contracted to Chinese

[95] One of the three was actually a Taiwanese firm, but it is listed here because it sat right next to a mainland firm (also recorded) with which it had some sort of business and financing relationship. The owner of the mainland firm was interviewed and explained that he had come to Tanzania because a Taiwanese friend (who he had met on the mainland and had since moved to Tanzania) had convinced him to come and replicate the same business model. Of these two warehouses sitting side by side, one seemed to source mostly from Taiwan, the other from mainland China.

[96] These are listed as metal door frames, but the facility also sold entire barred exterior doors (installed in front of the main wooden door in most apartment complexes, for security).

firms. The firm itself is a subsidiary of the 20th bureau of China Railway Construction Corporation and respondents reported that it did not seem to be engaged in general sales—though presumably excess materials would be liquidated on the local market.

The physical size, output and number of employees ranged wildly from firm to firm. That said, only one facility—the Taiwanese-owned Tooku Garment Co.—operated on the same scale as the old Urafiki Textile firm (since shut down), employing more than a thousand workers. Aside from this, the largest firms were the two Dragan Mabati facilities, one of the larger bag manufacturers (Matrix International), a tire factory and another sheet metal manufacturer—all in the Tazara Zone, save for one of the two Dragon Mabati sites. For all these firms, the estimated number of employees ranged from 300 to 500. Though exact numbers could not be obtained from a distance, the Sunshare Mabati firm located in the Tazara Zone seems to be of similar size. These also tended to be largest firms in terms of the actual land area occupied and the size of the buildings. If an ordinal ranking were to be applied to the firms based on some estimate of output, employment and physical size, these firms could be categorized alongside Tooku Garments as "very large." That said, a few facilities, such as the BNBM warehouse, the Ju Ye Concrete Company and the FAW Group auto yard were physically sprawling due to the nature of their business, though they employed fewer workers (around 100-200). These could be ranked as "large" firms. The electrical transformers factory (Yidai) discussed above, a glass and steel framings manufacturer located in the Tazara Zone, the Ocean Kiss factory, another bag manufacturer (Anbao International) and another sheet metal manufacturer (name unknown), all in the Mandela Zone, employed slightly less workers (40-100) and tended to be located in smaller buildings on more constrained plots of land. These could be categorized as "medium" firms. The majority, however, had fewer than 40 workers. By employment, many of these were

certainly “small” firms, with several essentially owner-operated micro enterprises that periodically take on more employees (as in the case of the Mitumba warehouses and the Hainan international construction yard). But at this scale the physical size of the facilities did not always shrink further: in the case of warehouses and construction yards, space was still needed even if there were few employees; in the case of smaller manufacturers, space was still required to set up production, store materials and give access to trucks for shipping out finished products.

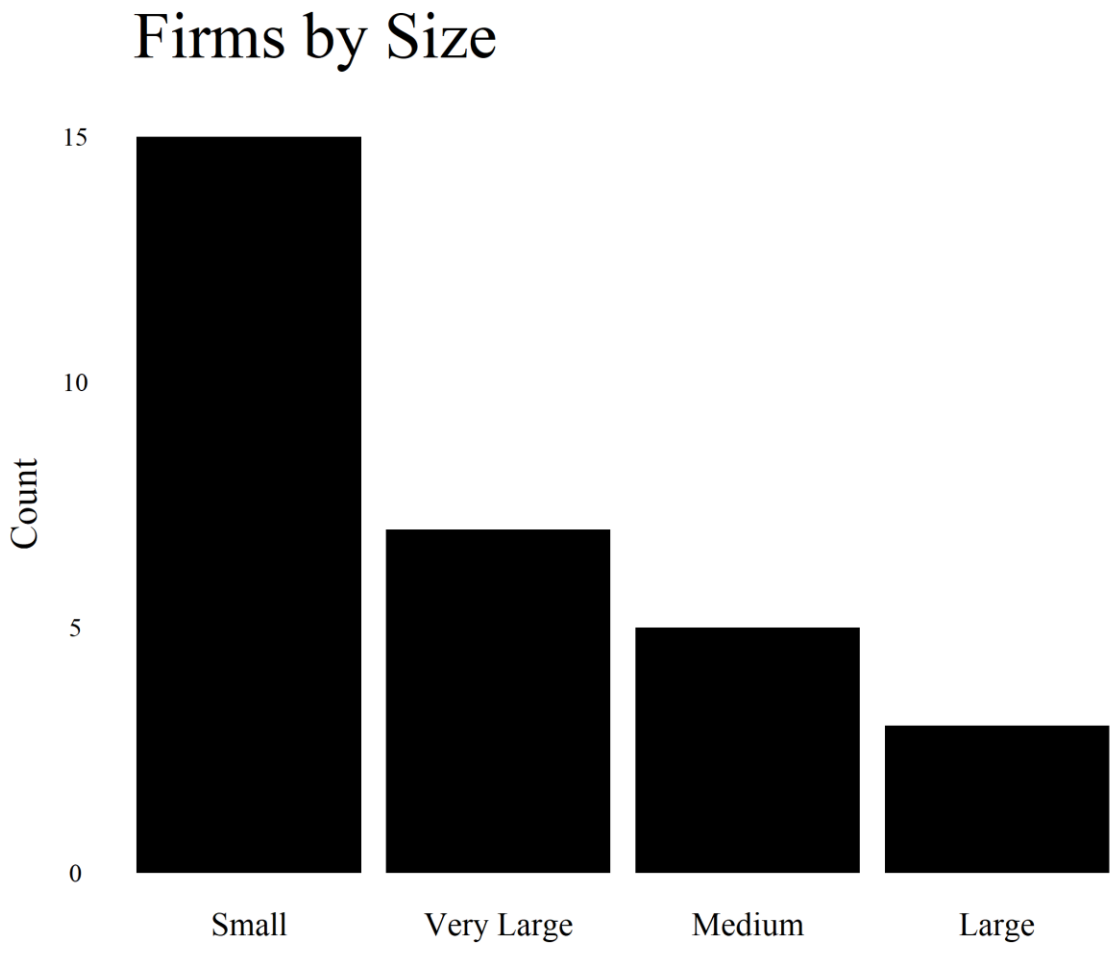



*Figure 5-19*

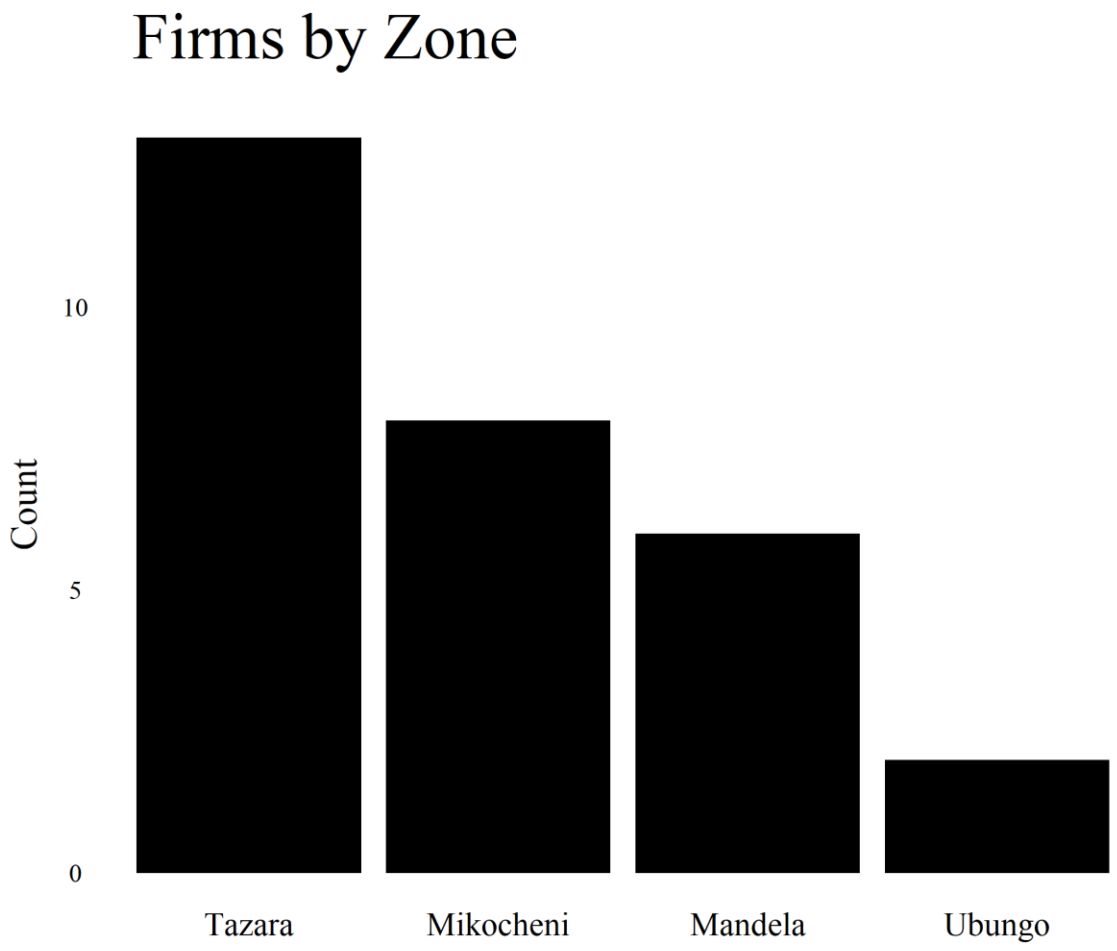



*Figure 5-20*

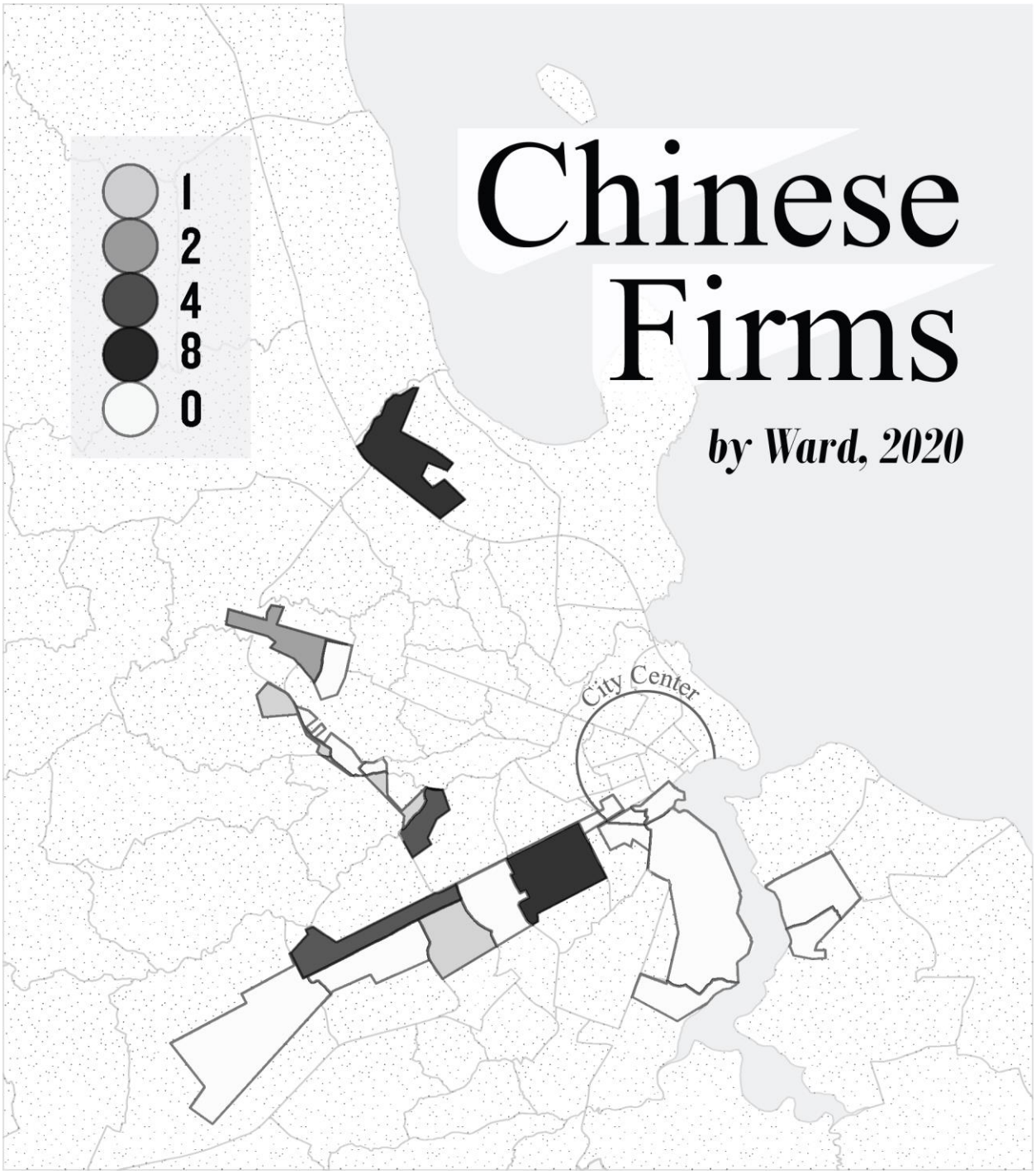
Among the most important contributions of this survey, however, is the fact that it also gathered geographic data on each firm's location. This allows for an examination not only of which industrial zones contain the greatest number of firms but also where smaller clusters of Chinese firms exist within individual zones. In general, Chinese firms were present in all industrial zones except two. Only the Port Zone and the Petro Zone did not contain any Chinese firms identified in the survey, but this was largely because their otherwise identifiable functions

(dominated by non-Chinese firms) took up the bulk of their industrial space. Within the other zones, strong internal clustering where a single block would contain multiple firms side by side (or nearly so) was rare, and only existed in two or three areas. With so few points, it is difficult to pursue any more rigorous statistical measure of clustering. At the level of the foot survey, there was only one cluster, in the southernmost portion of Mandela Zone, where an entire side street was taken up almost exclusively by Chinese firms. In this case, three key firms lie on the side street, neighboring one on the main thoroughfare and abutting another on a neighboring side street across a small river. The three key firms are all significantly older than others in the survey (two founded around 2010, one founded in 2008), and one (the BNBM warehouse) is particularly well-established, having recently opened a large, well-advertised warehouse for building materials on the street—its parent firm has also signed the largest single investment deal of any Chinese manufacturing firm in Tanzania, with its planned Tanga concrete complex. Meanwhile, the newest firm in the cluster is the Dragon Mabati facility on Mandela Road itself, one of the handful of "very large" firms recorded in the survey. Dragon Mabati may be the biggest sheet metal manufacturer and wholesaler in the city (it is certainly the largest Chinese one). This is one of two sites owned by the company, each roughly the same physical size and both employing around five hundred workers. The exact logic or economic mechanism leading to this clustering cannot be identified from the data here and no obvious relationships seem to exist between these firms, which are (almost) all in different industries.[97] Further research would be required to

[97] The firm abutting the cluster across the river is also a sheet metal manufacturer. It was much smaller, however, and very little information could be gathered on it other than the fact that it was a Chinese firm. Similarly, sheet metal is technically a building material, but it did not seem as if BNBM actually carried any of Dragon Mabati's sheet metal. Instead. Dragon Mabati appeared to store most of its stock on site. One employee explained that they sold directly to construction companies, smaller retailers and even individual customers. Aside from Dragon Mabati, the other sheet metal manufacturer and BNBM, the other two firms in this cluster are the Ocean Kiss facility producing mattresses, sofas and tanks for compressed gas and the Anbano International Investment Co, producing bags from recycled plastics.

determine if any of these clustered firms' owners have any relationships with one another or if they are part of a business network collectively able to negotiate with neighborhood authorities for land.

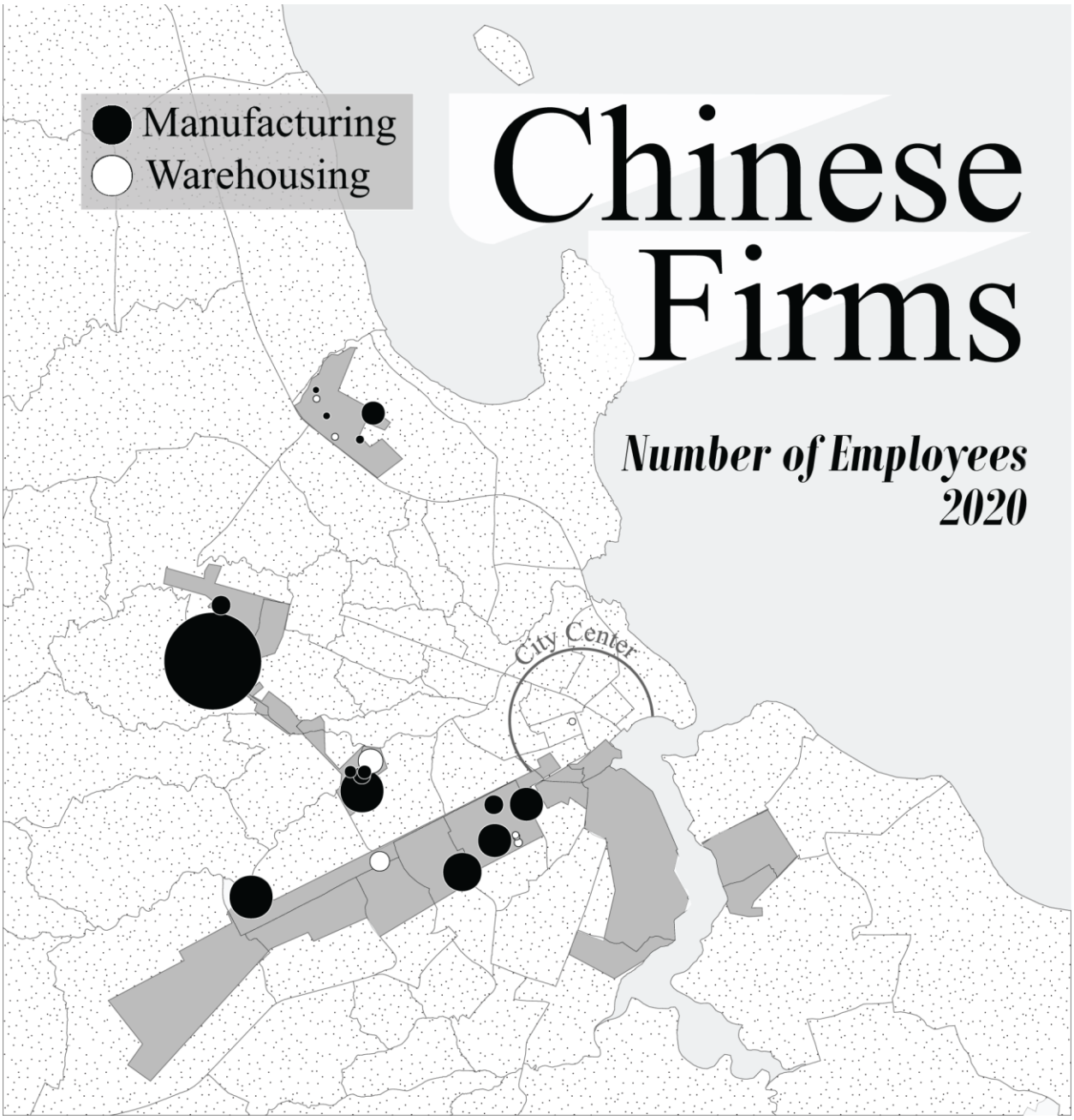
At the more general level, it is possible to cut the industrial zones down into smaller units to identify areas inside the zones that host more Chinese firms, on average. If all the industrial zones followed the string-of-pearls pattern of the Mandela Zone, they could be broken down into distinct spatial clusters along the main thoroughfare. Since the Mikocheni, Tazara and Port Zones are larger and more contiguous, however, it makes more sense to choose an arbitrary spatial designator already in use by urban planners: the official administrative wards (*kata*) of the city, which loosely correlate to local neighborhoods as colloquially understood. This also has the benefit of comparability with other statistics gathered for the city, since these are the official geographic units used by the Tanzanian Census and by the city's largest mapping project to date: the Ramani Huria project, which both utilizes and expands upon the official administrative divisions via its distributed community mapping approach. Geographic units below the ward level—the street (*mtaa*) in urban administration—were more volatile and unstandardized, given the area's rapid urban development. Below, Map 5-4 shows all the city's wards (divided between industrial and non-industrial, per the zones defined here) and the number of Chinese firms located in each industrial ward. Only one firm (one of the three *mitumba* warehouses) was located outside of any industrial area (in Kariakoo). It is not included below, since the goal of the map is to show clustering within industrial zones and this site is neither within an industrial zone nor within a cluster.



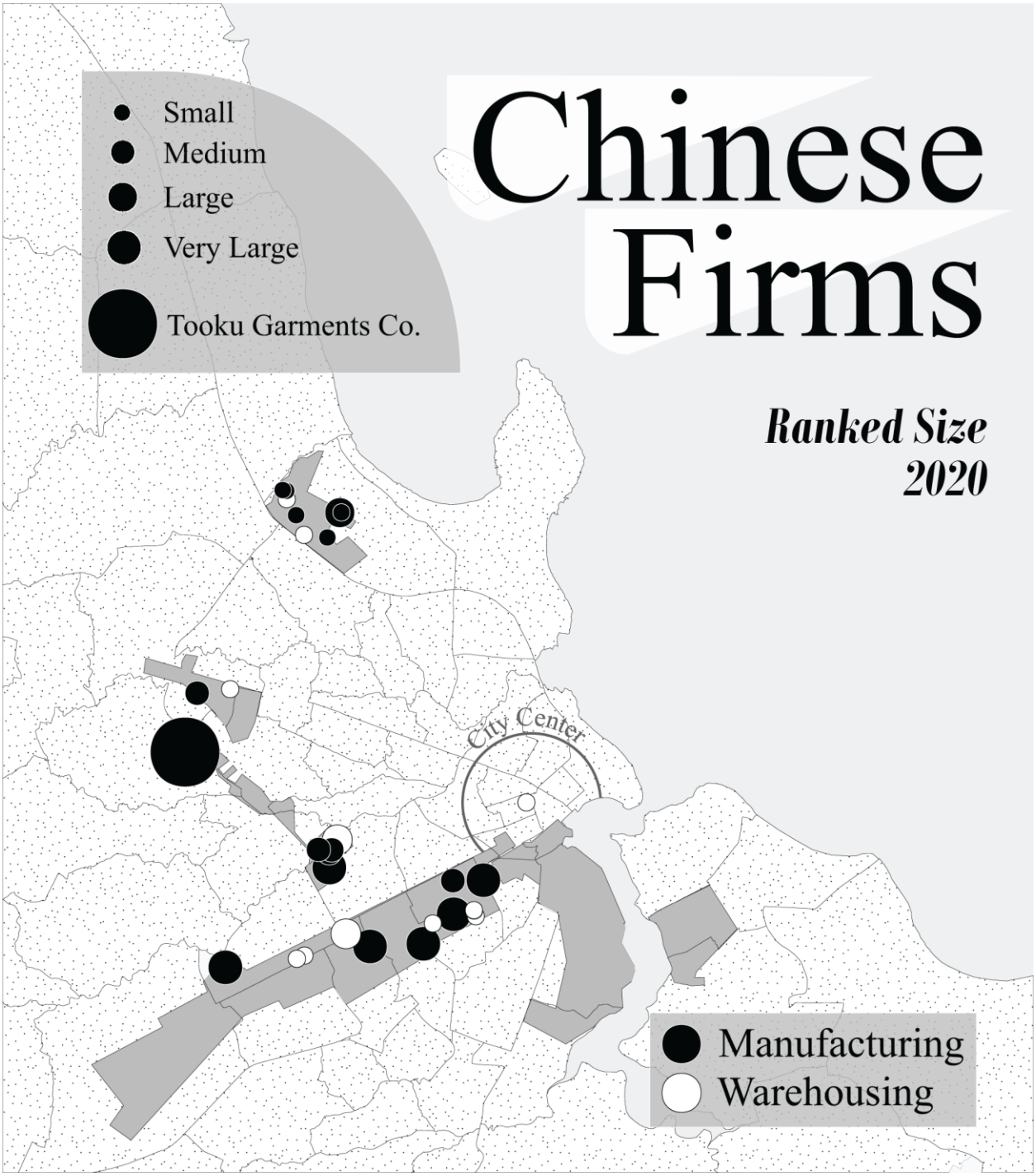

*Map 5-4*

From the latter map, two larger concentrations of firms are visible: one in Chang'ombe ward within the Tazara Zone, and the other in Mikocheni ward, which is a large ward containing the Mikocheni Zone. Each ward has a total of 8 Chinese firms. The tight-knit cluster in the Mandela Zone is recorded here as 4 firms located in the Buguruni ward and 1 firm located in the Tabata ward, since the river dividing the lone sheet metal firm from the others is also an administrative border. A similarly large concentration of firms is found in Vingunguti ward, distributed in a corridor along Nyerere road until the older Dragon Mabati facility.[98] While these polygon summaries are an accurate way to communicate the absolute number of firms, they show neither the scale nor their clustering in real space. This can be shown in two ways: first, by visualizing the mean number of workers reported by respondents for each firm; and, second, by visualizing the ranked size of firms. These are visible in Maps 5-5 and 5-6 below. Map 5-5 is limited by the fact that several firms did not have reliable enough information to record how many workers were employed there. It thus shows only 23 of the 30 total firms. Map 5-6 avoids this problem because it is a manually produced rank of the firm's size taking into account both quantitative and qualitative descriptions of the number of workers, as well as other factors such as apparent output and the physical size of plant and equipment. Thus, all firms are included. From these maps, we can more clearly see the tight clustering in Buguruni ward, as contrasted with the looser agglomerations in Chang'ombe and Mikocheni. Similarly, the significantly larger size of the JD Garments facility is visible in both.

[98] It's possible that other Chinese firms exist near this site, which could not be reached on foot due to a police checkpoint.


Manufacturing
Warehousing
Chinese Firms
Number of Employees
2020
City Center

*Map 5-5*


Chinese Firms
Ranked Size
2020
Small
Medium
Large
Very Large
Tooku Garments Co.
City Center
Manufacturing
Warehousing

*Map 5-6*

## Conclusion

Compared to the sort of reports on Chinese investment in Tanzania common in media portrayals and recorded in datasets like that of the CGIT, the results of the survey are underwhelming. But they are underwhelming in an informative way. This survey helps to both more accurately categorize the much more limited nature of the industrialization accompanying rapid urbanization in Dar es Salaam and properly rescale the overblown reporting on Chinese investment in Africa more generally. Not only are infrastructural mega-projects in Tanzania by no means exclusively Chinese (as the cases of South Korean, Japanese, Turkish, Egyptian and Kenyan construction projects attest) but Chinese direct investment in warehousing and manufacturing is relatively subdued—with only a single firm (JD Garments, now a subsidiary of a Taiwanese conglomerate) operating at anything like the scale of the shuttered Urafiki plant. By contrast, Tanzanian firms continue to be both the largest and most common industrial enterprises, accompanied by foreign-owned firms linked to more capital networks established via subsequent waves of colonization (first Omani, and then British). Meanwhile, this survey confirms the basic results of other scoping studies conducted by McKinsey & Co. (Sun et al 2017) and CARI at Johns Hopkins (Xia 2019) and adds further detail. In particular, it adds geographic information about the city's industrial zones and the location of Chinese firms.

Like the results of the McKinsey study, this survey confirms that the vast majority of Chinese firms operating in Dar es Salaam are small scale firms. They are all well below the cutoff used by CGIT in identifying Chinese investment—calling into question the relevance of that methodology. Similarly, it also confirms that the vast majority are privately-owned firms rather than subsidiaries of the major state-owned conglomerates. Many firms observed in Xia's

survey, conducted in 2017, were also found in this one—though the two could not be compared at the level of individual observations, since data was not available in such detail. That said, fewer plastics recycling firms were visible in this survey than in Xia's. This is possibly due to plant closures, but at least some of the discrepancy is accounted for by the few firms reported by respondents but not found—the majority of which were in these industries. In addition, Xia's survey was far more geographically expansive, capturing firms in other cities. This accounts for the discrepancy between Xia's findings in the agro-processing sector, since most of these firms are located in Shinyanga, not Dar es Salaam. Similarly, several of the smelting and metal recycling firms identified by Xia, such as Hongyu and Kiluwa Steel, are located in Kibaha and Mlandizi, in the Pwani region that lies outside the city's borders. The same is true of the two large ceramics factories identified by Xia: Twyford and Goodwill. Had these firms been visited, little additional information could have been added to that already gathered. None are located in identifiable industrial zones, though they may be early anchors for such development.[99]

Future research can build off this survey in multiple ways. First, individual firms and industrial zones could be explored in depth. Better numbers on the exact number of workers employed and years in operation could be obtained, and new information on output, value of capital invested in plant and equipment, average wages, working conditions, the nature of financing relationships and the history of the firm or investor in China could be explored. This could be pursued in either a bottom-up fashion, starting with interviews of workers and neighborhood residents (as used here) or in a top-down fashion, starting with interviews of

[99] This possibility is hinted at by the fact that one of my Chinese respondents later reported being sent out to set up a second factory producing electrical equipment in Mlandizi, completed by the end of 2020. Similarly, Hong Yu Steel is identifiable on satellite imagery and there are already a small number of other industrial facilities in the immediate vicinity.

Chinese owners and managers (as in Xia 2019). Second, similar data could be gathered for comparative purposes on non-Chinese firms. Ideally, this might look something like the Rumani Huria community mapping project, but conducted for an economic census. Rumani Huria has already provided one of the best classifications of residential, industrial, commercial, agricultural, and public usage of space in the city—even though these classifications are now often a few years out of date and key areas have undergone rapid changes since their maps were produced. Given the incapacity of the Tanzanian state's more official economic census, the inaccuracy of the Tanzania Investment Center records on Chinese investment (as demonstrated by Xia 2019) and the informal character of much of Tanzanian production, a community economic census on the model of Ramani Huria would provide invaluable information on Dar es Salaam's urbanization while also offering insight into the intricacies of the broader economic transformations currently underway in Africa.

At the smaller scale, follow-up summaries of a similar style could be conducted in Mbagala and the areas covered by this survey could be returned to and more systematically cleaned, with particular attention given to the few areas with reported firms that could not be identified. Similarly, the Port Zone could be given a more in-depth survey, possibly geared more toward identifying trade dependencies with China rather than direct investment relationships. Additional areas in the abutting Pwani region that stand out as potentially important future survey sites are Vikindu and Vibura, south of the city, and Mlandizi, to the east. In addition, Arusha, Mwanza, Shinyanga, Tanga and Mbeya would all be useful sites of future surveys focusing on slightly different sectors. But Dar es Salaam remains the economic capital of Tanzania, the largest city in East Africa, 5$^{th}$ or 6$^{th}$ largest in all of Africa, and one of the fastest growing cities in the world. Similarly, it has long been an important component of Indian Ocean

Rim economic activity and seems poised to retain this position in the future. It should thus be a central point of focus for future studies of urbanization and industrial territory formation in general, regardless of any focus on the specific role of Chinese investment.

# Appendix 1

## Rate of Profit Tables

**Chinese Rate of Profit Measures**

1990-2019

| | | | Industry | |
|---|---|---|---|---|
| Year | All | National | All Industry | Net Profit |
| 1990 | 20.58 | 25.72 | 11.56 | 4.02 |
| 1991 | 21.29 | 27.39 | 10.62 | 4.13 |
| 1992 | 21.92 | 28.38 | 10.62 | 4.13 |
| 1993 | 22.15 | 29.16 | 9.88 | 4.51 |
| 1994 | 21.48 | 28.43 | 9.32 | 3.62 |
| 1995 | 21.30 | 28.59 | 8.54 | 2.52 |
| 1996 | 20.28 | 27.08 | 8.39 | 2.01 |
| 1997 | 20.16 | 26.98 | 8.23 | 2.01 |
| 1998 | 19.21 | 25.70 | 7.86 | 1.65 |
| 1999 | 19.07 | 25.20 | 8.35 | 2.42 |
| 2000 | 19.58 | 25.06 | 9.98 | 4.31 |
| 2001 | 19.43 | 24.77 | 10.10 | 4.32 |
| 2002 | 19.78 | 25.01 | 10.61 | 4.93 |
| 2003 | 20.48 | 25.49 | 11.71 | 6.31 |
| 2004 | 21.40 | 26.78 | 11.99 | 7.26 |
| 2005 | 20.96 | 25.76 | 12.56 | 7.76 |
| 2006 | 21.07 | 25.56 | 13.23 | 8.67 |
| 2007 | 21.93 | 26.28 | 14.32 | 10.12 |
| 2008 | 21.81 | 26.52 | 13.56 | 9.37 |
| 2009 | 18.62 | 21.77 | 13.10 | 9.40 |
| 2010 | 19.01 | 21.13 | 15.30 | 12.34 |
| 2011 | 19.08 | 20.94 | 15.82 | 12.78 |
| 2012 | 18.25 | 20.39 | 14.50 | 11.47 |
| 2013 | 17.57 | 19.56 | 14.07 | 11.33 |
| 2014 | 16.69 | 18.79 | 13.01 | 10.23 |
| 2015 | 16.05 | 18.30 | 12.10 | 9.38 |
| 2016 | 15.82 | 17.84 | 12.28 | 9.73 |
| 2017 | 15.95 | 17.71 | 12.86 | 10.00 |
| 2018 | 15.42 | 17.12 | 12.44 | 9.05 |
| 2019 | 14.79 | 16.62 | 11.59 | 7.96 |

Source: Author's Calculations

*Table A1-1*

**Chinese Rate of Profit Measures**

1990-2019

| | National | | Industry | | | |
|---|---|---|---|---|---|---|
| Year | PWT 10 | Herd 2020 | Value Added of Industry | Net Profits / Total Assets | Net Profits / Non-Current Assets | Net Profits / Fixed Assets |
| 1990 | 30.58 | 35.56 | 34.19 | 2.77 | NA | NA |
| 1991 | 31.35 | 36.48 | NA | NA | NA | NA |
| 1992 | 32.80 | 38.31 | NA | NA | NA | NA |
| 1993 | 33.24 | 39.34 | 26.00 | 2.92 | 5.33 | NA |
| 1994 | 33.47 | 39.77 | 26.41 | 2.43 | 4.06 | NA |
| 1995 | 33.34 | 39.58 | 26.61 | 1.74 | NA | NA |
| 1996 | 33.00 | 39.34 | 27.51 | 1.39 | NA | NA |
| 1997 | 33.08 | 39.01 | 26.90 | 1.39 | NA | NA |
| 1998 | 31.96 | 38.19 | 26.50 | 1.13 | 1.77 | NA |
| 1999 | 31.35 | 37.62 | 26.14 | 1.66 | 2.60 | NA |
| 2000 | 31.15 | 37.35 | 27.01 | 2.95 | 4.64 | 5.34 |
| 2001 | 30.66 | 36.87 | 27.41 | 2.96 | 4.63 | 5.38 |
| 2002 | 30.52 | 36.43 | 27.67 | 3.35 | 5.30 | 6.13 |
| 2003 | 30.35 | 36.04 | 27.89 | 4.20 | 6.82 | 7.92 |
| 2004 | 29.98 | 35.26 | 26.17 | 4.75 | 7.74 | 9.31 |
| 2005 | 29.48 | 34.63 | 26.97 | 5.12 | 8.31 | 9.85 |
| 2006 | 29.65 | 34.71 | 26.92 | 5.69 | 9.27 | 11.04 |
| 2007 | 30.19 | 34.96 | 26.93 | 6.55 | 10.80 | 13.03 |
| 2008 | 29.58 | 34.14 | 26.13 | 6.06 | 9.91 | 12.13 |
| 2009 | 28.71 | 32.71 | 24.23 | 6.06 | 9.96 | 12.18 |
| 2010 | 27.91 | 31.71 | 24.16 | 7.76 | 13.12 | 16.14 |
| 2011 | 27.21 | 30.63 | 24.92 | 7.84 | 13.48 | 17.03 |
| 2012 | 25.96 | 29.26 | 23.59 | 6.99 | 11.97 | 15.44 |
| 2013 | 25.35 | 28.01 | 22.28 | 6.85 | 11.70 | 15.43 |
| 2014 | 24.93 | 26.94 | 21.36 | 6.24 | 10.55 | 13.89 |
| 2015 | 24.34 | 26.01 | 20.25 | 5.70 | 9.57 | 12.86 |
| 2016 | 23.93 | 25.20 | 19.96 | 5.85 | 9.87 | 13.46 |
| 2017 | 23.35 | NA | 21.44 | 5.84 | 10.00 | 14.17 |
| 2018 | 22.46 | NA | 22.64 | 5.38 | 9.37 | NA |
| 2019 | 21.93 | NA | 22.46 | 4.74 | 8.25 | NA |

Source: Author's Calculations

*Table A1-2*

**Divergence from Industry-wide Return on Assets**

2001-2018, Industry

| | Type | | Size | | | Ownership | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Heavy | Light | Large | Medium | Small | Private | Private LLCs | Private JSCs | LLC | JSCs | State | State Sole-Funded | Domestic | Foreign |
| 2018 | NA | NA | -0.32 | -0.25 | -0.52 | 0.95 | NA | NA | -1.45 | NA | -3.43 | -3.31 | -0.76 | 1.98 |
| 2017 | -0.77 | 2.53 | -0.56 | 0.40 | 0.59 | 2.82 | 22.90 | 27.18 | -1.69 | 13.45 | -3.15 | -4.13 | -0.44 | 2.56 |
| 2016 | -1.02 | 3.27 | -1.35 | 0.87 | 1.45 | 4.02 | 23.17 | 24.19 | -2.00 | 8.79 | -4.03 | -4.53 | -0.40 | 2.21 |
| 2015 | -1.04 | 3.35 | -1.51 | 0.94 | 1.62 | 4.12 | 22.71 | 23.30 | -2.00 | 6.82 | -3.52 | -4.96 | -0.35 | 1.97 |
| 2014 | -0.87 | 2.84 | -1.27 | 0.65 | 1.53 | 3.93 | 23.06 | 19.68 | -2.17 | 9.45 | -3.08 | -4.97 | -0.32 | 1.77 |
| 2013 | -0.86 | 2.83 | -1.53 | 0.58 | 2.13 | 4.57 | 26.50 | 24.81 | -2.14 | 12.73 | -3.64 | -5.10 | -0.14 | 1.10 |
| 2012 | -0.83 | 2.79 | -1.43 | 0.28 | 2.40 | 5.18 | 26.86 | 29.00 | -1.87 | 14.59 | -4.25 | -4.73 | -0.01 | 0.44 |
| 2011 | -0.62 | 2.09 | -1.38 | 0.31 | 2.49 | 5.13 | 28.09 | 34.98 | -1.82 | 15.44 | -5.07 | -4.65 | -0.15 | 0.69 |
| 2010 | -0.59 | 1.95 | -1.49 | 0.13 | 1.98 | 3.97 | 23.45 | 29.45 | -1.82 | 13.58 | -4.81 | -4.55 | -0.39 | 1.36 |
| 2009 | -0.61 | 2.00 | -1.35 | 0.20 | 1.61 | 3.62 | 18.52 | 23.54 | -1.52 | 7.88 | -4.12 | -4.07 | -0.38 | 1.33 |
| 2008 | -0.35 | 1.13 | -0.68 | -0.41 | 1.35 | 3.86 | 19.72 | 25.44 | -0.86 | 8.70 | -3.31 | -3.23 | -0.09 | 0.13 |
| 2007 | 0.02 | -0.07 | 0.53 | -0.75 | 0.15 | 1.79 | 16.88 | 21.92 | -0.45 | 14.58 | -2.89 | -3.12 | -0.04 | 0.35 |
| 2006 | 0.16 | -0.47 | 0.86 | -0.86 | -0.17 | 1.18 | 13.15 | 17.80 | -0.22 | 11.25 | -2.59 | -1.73 | -0.10 | 0.50 |
| 2005 | 0.16 | -0.44 | 1.11 | -1.02 | -0.30 | 0.95 | 11.23 | 15.81 | -0.09 | 9.30 | -2.05 | -1.94 | -0.14 | 0.75 |
| 2004 | 0.31 | -0.84 | 1.49 | -0.70 | -1.05 | 0.49 | 9.79 | 12.93 | -0.55 | 11.90 | -3.13 | -2.16 | -0.50 | 2.02 |
| 2003 | 0.06 | -0.15 | 0.85 | -0.37 | -0.78 | 0.98 | 10.42 | 13.44 | -1.57 | 13.02 | -3.07 | -3.43 | -0.65 | 3.10 |
| 2002 | -0.06 | 0.14 | 0.12 | -0.96 | 0.20 | 1.64 | 10.50 | 10.31 | -1.43 | 9.64 | -2.56 | -3.15 | -0.55 | 2.60 |
| 2001 | 0.02 | -0.04 | 0.25 | -0.89 | -0.10 | 1.80 | 9.60 | 8.88 | -1.18 | 10.08 | -2.40 | -2.76 | -0.42 | 2.20 |

*Table A1-3*

# Appendix 2

## Regional Growth Rate Tables

| Central<br>2000-2019, Average Growth Rates | | | | |
|---|---|---|---|---|
| | Enterprises | Return on Assets | Value-Added | Net Profits |
| Anhui | | | | |
| 2000-2004 | 6.87 | 40.67 | 13.27 | 53.92 |
| 2005-2009 | 24.56 | 8.18 | 22.19 | 33.34 |
| 2010-2014 | 13.32[1] | 1.40 | 18.54 | 21.78 |
| 2015-2019 | 0.18 | -2.57 | 5.50 | 3.22 |
| Henan | | | | |
| 2000-2004 | 4.40 | 17.64 | 15.46 | 32.00 |
| 2005-2009 | 10.60 | 22.59 | 21.05 | 46.30 |
| 2010-2014 | 6.39[1] | -4.05 | 9.55 | 15.88 |
| 2015-2019 | -1.93 | -6.03 | 4.75 | -3.72 |
| Hubei | | | | |
| 2000-2004 | -0.20 | 14.49 | 12.04 | 26.75 |
| 2005-2009 | 17.94 | 9.55 | 21.69 | 32.50 |
| 2010-2014 | 14.62[1] | 6.47 | 16.87 | 18.45 |
| 2015-2019 | -0.48 | -0.25 | 7.07 | 5.03 |
| Hunan | | | | |
| 2000-2004 | 12.15 | 31.56 | 12.88 | 45.92 |
| 2005-2009 | 12.21 | 16.11 | 21.04 | 39.13 |
| 2010-2014 | 3.44[1] | 3.47 | 17.02 | 22.43 |
| 2015-2019 | 3.85 | 0.23 | 4.08 | 6.79 |
| Jiangxi | | | | |
| 2000-2004 | 4.28 | 46.30 | 20.86 | 60.56 |
| 2005-2009 | 13.65 | 23.97 | 23.20 | 51.68 |
| 2010-2014 | 9.89[1] | 12.35 | 16.99 | 32.96 |
| 2015-2019 | 7.77 | -7.75 | 4.86 | 1.54 |
| Total Region | | | | |
| 2000-2004 | 5.07 | 25.09 | 14.45 | 35.51 |
| 2005-2009 | 14.94 | 15.65 | 21.55 | 39.48 |
| 2010-2014 | 9.22[1] | 3.33 | 14.78 | 19.76 |
| 2015-2019 | 1.12 | -4.02 | 5.26 | 0.90 |

[1] Excluding 2011 due to distortion from statistical redefinition

Source: NBS, Author's Calculations

*Table A2-1*

## East Coast

2000-2019, Average Growth Rates

| | Enterprises | Return on Assets | Value-Added | Net Profits |
|---|---|---|---|---|
| Jiangsu | | | | |
| 2000-2004 | 24.73 | 10.90 | 17.91 | 32.65 |
| 2005-2009 | 11.24 | 9.15 | 17.60 | 30.50 |
| 2010-2014 | 4.36[1] | 3.49 | 10.48 | 18.06 |
| 2015-2019 | -1.07 | -8.18 | 6.55 | -4.73 |
| Shandong | | | | |
| 2000-2004 | 20.69 | 8.46 | 16.04 | 28.74 |
| 2005-2009 | 13.80 | 5.97 | 16.75 | 26.98 |
| 2010-2014 | 2.52[1] | -0.25 | 7.63 | 14.94 |
| 2015-2019 | -7.03 | -15.69 | 2.45 | -14.63 |
| Shanghai | | | | |
| 2000-2004 | 17.32 | 15.50 | 15.90 | 28.64 |
| 2005-2009 | 3.16 | -1.45 | 9.83 | 9.45 |
| 2010-2014 | -2.96[1] | 6.65 | 7.39 | 15.21 |
| 2015-2019 | -1.37 | -2.56 | 3.54 | 2.36 |
| Zhejiang | | | | |
| 2000-2004 | 30.99 | 3.97 | 16.47 | 29.70 |
| 2005-2009 | 7.94 | -0.94 | 14.32 | 17.12 |
| 2010-2014 | 6.04[1] | 2.65 | 10.21 | 13.50 |
| 2015-2019 | 2.40 | 0.41 | 5.85 | 6.29 |
| Total Region | | | | |
| 2000-2004 | 24.50 | 9.15 | 16.67 | 29.47 |
| 2005-2009 | 9.25 | 2.41 | 15.46 | 21.71 |
| 2010-2014 | 3.79[1] | 2.64 | 9.21 | 15.69 |
| 2015-2019 | -1.77 | -6.94 | 4.95 | -4.85 |

[1] Excluding 2011 due to distortion from statistical redefinition

Source: NBS, Author's Calculations

*Table A2-2*

| North<br>2000-2019, Average Growth Rates | | | | |
|---|---|---|---|---|
| | Enterprises | Return on Assets | Value-Added | Net Profits |
| Beijing | | | | |
| 2000-2004 | 14.76 | 4.52 | 15.76 | 34.21 |
| 2005-2009 | 0.30 | 4.08 | 8.12 | 15.38 |
| 2010-2014 | -0.42[7] | 3.89 | 10.15 | 15.97 |
| 2015-2019 | -3.26 | -5.07 | 3.85 | 3.86 |
| Hebei | | | | |
| 2000-2004 | 6.54 | 16.59 | 13.13 | 31.43 |
| 2005-2009 | 7.20 | 2.76 | 16.07 | 23.06 |
| 2010-2014 | 8.02[7] | -1.54 | 9.99 | 14.24 |
| 2015-2019 | -2.13 | -5.35 | 2.42 | -3.14 |
| Inner Mongolia | | | | |
| 2000-2004 | 14.32 | 50.37 | 14.35 | 76.39 |
| 2005-2009 | 14.63 | 15.67 | 23.50 | 49.20 |
| 2010-2014 | 2.22[7] | -7.27 | 11.41 | 11.59 |
| 2015-2019 | -6.34 | 0.29 | 6.39 | 3.56 |
| Shanxi | | | | |
| 2000-2004 | 12.20 | 57.17 | 24.18 | 84.04 |
| 2005-2009 | -4.13 | -3.36 | 17.16 | 17.58 |
| 2010-2014 | 2.93[7] | -10.61 | 10.16 | 4.54 |
| 2015-2019 | 4.73 | 32.69 | 4.07 | 41.82 |
| Tianjin | | | | |
| 2000-2004 | 4.86 | 16.53 | 14.12 | 28.20 |
| 2005-2009 | 5.66 | -0.92 | 15.69 | 15.93 |
| 2010-2014 | 1.25[7] | 9.99 | 9.97 | 25.50 |
| 2015-2019 | -4.57 | -7.75 | 2.03 | -8.15 |
| Total Region | | | | |
| 2000-2004 | 8.88 | 20.36 | 15.81 | 36.61 |
| 2005-2009 | 4.39 | 3.38 | 15.74 | 21.77 |
| 2010-2014 | 4.05[7] | -0.21 | 10.16 | 15.18 |
| 2015-2019 | -2.52 | -3.80 | 3.29 | -0.26 |

[7] Excluding 2011 due to distortion from statistical redefinition

Source: NBS, Author's Calculations

*Table A2-3*

| Northeast<br>2000-2019, Average Growth Rates | | | | |
|---|---|---|---|---|
| | Enterprises | Return on Assets | Value-Added | Net Profits |
| Heilongjiang | | | | |
| 2000-2004 | 6.23 | 2.58 | 8.73 | 8.78 |
| 2005-2009 | 7.22 | -2.15 | 10.21 | 8.37 |
| 2010-2014 | 7.60[1] | -6.15 | 5.83 | 5.00 |
| 2015-2019 | -3.86 | -10.63 | -5.56 | -9.35 |
| Jilin | | | | |
| 2000-2004 | 6.66 | 14.80 | 6.86 | 23.36 |
| 2005-2009 | 13.84 | 16.36 | 18.75 | 33.17 |
| 2010-2014 | 1.77[1] | 7.62 | 12.55 | 23.45 |
| 2015-2019 | -7.40 | -4.81 | 1.78 | -5.20 |
| Liaoning | | | | |
| 2000-2004 | 17.31 | 18.91 | 5.62 | 30.93 |
| 2005-2009 | 17.51 | 12.99 | 17.79 | 33.41 |
| 2010-2014 | -1.23[1] | 3.00 | 6.02 | 13.50 |
| 2015-2019 | -11.80 | 2.64 | 0.89 | 3.88 |
| Total Region | | | | |
| 2000-2004 | 12.46 | 6.50 | 6.90 | 15.58 |
| 2005-2009 | 14.97 | 0.23 | 15.09 | 16.08 |
| 2010-2014 | 0.63[1] | 0.48 | 7.06 | 12.95 |
| 2015-2019 | -10.74 | -7.38 | -0.70 | -5.87 |

[1] Excluding 2011 due to distortion from statistical redefinition

Source: NBS, Author's Calculations

*Table A2-4*

| Northwest<br>2000-2019, Average Growth Rates | | | | |
|---|---|---|---|---|
| | Enterprises | Return on Assets | Value-Added | Net Profits |
| Gansu | | | | |
| 2000-2004 | -7.74 | 56.85 | 13.92 | 70.24 |
| 2005-2009 | 0.79 | 17.97 | 17.70 | 37.89 |
| 2010-2014 | 11.75[1] | -6.93 | 14.82 | 9.07 |
| 2015-2019 | -2.59 | 40.03 | -0.10 | 39.94 |
| Ningxia | | | | |
| 2000-2004 | 15.24 | 22.56 | 18.66 | 40.80 |
| 2005-2009 | 8.19 | 19.14 | 21.21 | 48.69 |
| 2010-2014 | 11.65[1] | -5.88 | 13.07 | 13.90 |
| 2015-2019 | 0.55 | 8.32 | 7.88 | 17.34 |
| Qinghai | | | | |
| 2000-2004 | 1.48 | 185.92 | 17.99 | 216.72 |
| 2005-2009 | 2.82 | 12.87 | 23.32 | 35.45 |
| 2010-2014 | 11.98[1] | -7.63 | 13.38 | 7.94 |
| 2015-2019 | 0.63 | -29.15 | 10.38 | -25.96 |
| Shaanxi | | | | |
| 2000-2004 | 4.64 | 27.31 | 19.29 | 44.25 |
| 2005-2009 | 8.54 | 6.71 | 21.60 | 30.22 |
| 2010-2014 | 9.00[1] | 2.81 | 18.33 | 20.50 |
| 2015-2019 | 6.87 | -0.53 | 4.64 | 6.83 |
| Xinjiang | | | | |
| 2000-2004 | -0.05 | 19.74 | 15.60 | 32.38 |
| 2005-2009 | 7.29 | 2.03 | 17.39 | 23.07 |
| 2010-2014 | 14.95[1] | -7.17 | 16.34 | 12.59 |
| 2015-2019 | 5.17 | 2.33 | 4.36 | 8.48 |
| Total Region | | | | |
| 2000-2004 | -0.71 | 26.62 | 17.07 | 39.47 |
| 2005-2009 | 6.07 | 5.68 | 19.73 | 26.91 |
| 2010-2014 | 11.07[1] | -4.11 | 16.69 | 16.17 |
| 2015-2019 | 4.02 | -7.73 | 4.25 | 5.01 |

[1] Excluding 2011 due to distortion from statistical redefinition

Source: NBS, Author's Calculations

*Table A2-5*

## South Coast

2000-2019, Average Growth Rates

| | Enterprises | Return on Assets | Value-Added | Net Profits |
|---|---|---|---|---|
| Fujian | | | | |
| 2000-2004 | 18.92 | 19.94 | 14.25 | 38.73 |
| 2005-2009 | 8.84 | 6.45 | 16.67 | 25.30 |
| 2010-2014 | 5.88[1] | 1.52 | 15.55 | 18.09 |
| 2015-2019 | 1.89 | 5.69 | 8.02 | 13.34 |
| Guangdong | | | | |
| 2000-2004 | 15.96 | 11.78 | 17.01 | 27.31 |
| 2005-2009 | 8.94 | 7.02 | 16.73 | 24.28 |
| 2010-2014 | 2.46[1] | -0.16 | 10.34 | 12.49 |
| 2015-2019 | 6.32 | -3.55 | 5.83 | 5.62 |
| Guangxi | | | | |
| 2000-2004 | 5.35 | 28.60 | 11.87 | 42.08 |
| 2005-2009 | 8.96 | 3.64 | 18.90 | 24.84 |
| 2010-2014 | 5.94[1] | 14.91 | 12.89 | 35.23 |
| 2015-2019 | 2.60 | -6.17 | 5.49 | -1.07 |
| Hainan | | | | |
| 2000-2004 | -0.28 | 44.89 | 20.75 | 61.99 |
| 2005-2009 | -2.84 | 15.24 | 15.20 | 31.65 |
| 2010-2014 | 1.82[1] | -10.12 | 11.35 | 2.48 |
| 2015-2019 | 2.09 | 3.20 | 4.34 | 9.42 |
| Total Region | | | | |
| 2000-2004 | 15.25 | 20.62 | 16.00 | 29.83 |
| 2005-2009 | 8.77 | 7.69 | 16.85 | 24.50 |
| 2010-2014 | 3.53[1] | 0.45 | 11.69 | 14.75 |
| 2015-2019 | 4.86 | -0.45 | 6.31 | 6.82 |

[1] Excluding 2011 due to distortion from statistical redefinition

Source: NBS, Author's Calculations

*Table A2-6*

**Southwest**

2000-2019, Average Growth Rates

| | Enterprises | Return on Assets | Value-Added | Net Profits |
|---|---|---|---|---|
| Chongqing | | | | |
| 2000-2004 | 6.83 | 54.10 | 15.58 | 67.25 |
| 2005-2009 | 20.77 | 6.24 | 16.64 | 26.58 |
| 2010-2014 | 9.46[1] | 8.21 | 16.91 | 29.35 |
| 2015-2019 | 1.73 | -6.08 | 4.07 | 0.63 |
| Guizhou | | | | |
| 2000-2004 | 5.40 | 33.53 | 14.61 | 50.91 |
| 2005-2009 | 2.26 | 10.80 | 18.22 | 27.56 |
| 2010-2014 | 15.80[1] | 9.52 | 17.04 | 29.41 |
| 2015-2019 | 4.42 | 2.28 | 9.45 | 9.43 |
| Sichuan | | | | |
| 2000-2004 | 14.62 | 15.91 | 14.32 | 27.88 |
| 2005-2009 | 12.85 | 19.09 | 23.17 | 44.06 |
| 2010-2014 | 3.20[1] | -0.37 | 14.00 | 16.43 |
| 2015-2019 | 1.93 | 1.55 | 4.28 | 6.70 |
| Tibet | | | | |
| 2000-2004 | -13.14 | NaN | 12.03 | NaN |
| 2005-2009 | -10.36 | 2.30 | 16.59 | 32.67 |
| 2010-2014 | 17.11[1] | -5.90 | 15.61 | 13.97 |
| 2015-2019 | 8.97 | -2.23 | 13.87 | 18.00 |
| Yunnan | | | | |
| 2000-2004 | 2.70 | 22.62 | 10.79 | 38.08 |
| 2005-2009 | 8.66 | -4.05 | 15.37 | 13.12 |
| 2010-2014 | 9.12[1] | -5.41 | 14.06 | 10.38 |
| 2015-2019 | 2.87 | 18.68 | 5.87 | 23.36 |
| Total Region | | | | |
| 2000-2004 | 8.62 | 26.39 | 13.80 | 36.70 |
| 2005-2009 | 11.97 | 9.55 | 19.43 | 29.43 |
| 2010-2014 | 6.88[1] | 0.58 | 14.98 | 19.06 |
| 2015-2019 | 2.33 | -0.14 | 5.21 | 6.33 |

[1] Excluding 2011 due to distortion from statistical redefinition

Source: NBS, Author's Calculations

*Table A2-7*

# Appendix 3

## Summary of ROP and ROR in the Existing Literature

The Rate of Profit (ROP) and Rate of Return (ROR) measurements derived from the existing literature and used (alongside my own measures) in the composition of the average rates of profit visible in Chapter 3 are summarized here:

*The Extended Penn ROP*

The first is the ROP measure already calculated in the Extended Penn World Tables (EPWT), derived from an earlier version of the PWT. This is a pre-made measure calculated by the EPWT creators, Marquetti and Foley (2011), and it only extends from 1995-2007.

*Maito 2014*

The second is the ROP measure calculated by Esteban Maito (2014), which is the most common measure of the Chinese profit rate referred to by Michael Roberts. In a 2014 paper, Maito uses his ROP measures for various countries around the world to argue that China has been central to global profitability in the past few decades. But Maito's ROP measure for China is simply an aggregate of the following ROR measures listed below (Bai et al 2006 and Qu et al 2013). That said, his data is included here for comparison and listed as his own calculation for two reasons: a) his is the most common citation, aside from Li, for the calculation of the Chinese ROP and b) he inflates the measures he takes from his two sources by about six percentage points, likely in the conversion from ROR to ROP.

*Bai et al 2006*

The main source Maito draws from for the years 1978-2005 is a paper by Bai Chong-en, Hsieh Chang-Tai and Qian Yingyi from 2006, which calculates the "rate of return on capital." The authors essentially calculate the ratio of output to capital and then multiply this by capital's share in total output (they then modify the resulting figure to capture changes in price over time, depreciation, etc.). Their ROR equation is: Capital Share * (Output/Capital). The missing value is the cost of labor, which does not appear in the denominator but is indirectly accounted for as the inverse portion of the capital share. In general, this rate of return measure will follow the same pattern as the ROP, but it will produce a lower figure—this is possibly why Maito inflates the numbers as he does, in an attempt to jump from ROR to ROP.

*Qu et al 2013*

The second source Maito draws from to fill in the remaining years from 2006-2011is an extension of the work of Bai et al by Qu Honbgin, Julia Wang and Sun Junwei, published in an HSBC research report. These authors use the exact same ROR calculation method as in the previous paper and extend it to 2011. Maito takes these figures and applies the same inflation as he did to the figures from Bai et al. Below, I will distinguish between the two measurements, but they can be seen forming a single series in the final graph.

*Wu et al 2020*

The most recent available measure of the Chinese ROP comes from a paper (currently only available in Chinese) by Wu Xiaohua, Shi Ying and Chen Zhicao published in 2020. The authors offer an attempt to calculate the ROP in the Marxist sense (at least as interpreted by authors like Roberts, Carchedi and Maito), rather than the ROR. Their measurement extends from 1994-2018 and alongside the ROP they include measures of the OCC and ROSV. However, the "appendix" that explains their methods in detail is missing from the online version of their paper, so the exact original source of each of their measures is currently somewhat unclear. That said, they cite the 2008 paper by Shan Haojie mentioned above in referring to capital stock. It's likely, then, that they use Shan's methodology or some variation on it to extend the capital stock measure, using this as a stand-in for Constant Capital (C) in the ROP formula. They then explain in a footnote (to Figure 1, showing their measurements) that their data does come from the NBS, so it's likely that they use official GDP data or some aggregate value-added measure (i.e. possibly one excluding certain sectors) for Surplus Value (S), and they likely use NBS employment and average wage bill data (which is calculated by sector) for Variable Capital (V). Their Wage Bill column, however, tends to lie about 10 percentage points below the Wage Share used in my own calculations below, based on both the PWT and the two papers cited directly above.

*Zhao and Liu 2017*

One additional measure of the Chinese Rate of Profit, albeit slightly less recent, can be found in another Chinese-language paper published in 2017, by Zhao Lei and Liu Hebei. Zhao and Lei use a different perpetual inventory series in order to obtain the capital stock, and they offer their own OCC and ROSV measures, alongside ROP. In addition to the primary ROP measure

(S/C+V), they calculate three variant ROP measures, based on the methods used by Gérard Duménil and Dominique Lévy, which add fixed capital and inventories one by one to the denominator, and include a variant that subtracts particular types of taxes (for a detailed description of these methods, see: Duménil and Lévy 2004, p.87). Below, I will only use their primary ROP measure, which is most comparable to the others. The latter three simply deflate the ROP value in different ways. All tend to follow the same pattern, regardless.

*PWT Rate of Return*

The PWT 9.1 and 10.0 both also include their own measurement of the rate of return, similar to that used by Qu et al (2013) and Bai et al (2006) above.[100] The shift between version 9.1 and version 10.0 saw several revisions which, though strongest in the PPP GDP measures, are also visible in the PWT ROR. Therefore, I will compare the ROR from both versions to each other, in a separate chart. These will not, however, be used in calculating the mean rate of profit between all measures, since it would be redundant with the ROP drawn from the PWT.

[100] The PWT 9.1 exists as a static version and was accessed continuously throughout 2020 and into 2021 via the API access built into the *pwt9* R package. The PWT 10 was released in January 2021 and updated once in February 2021. The changes made in February do not affect any of the variables used here. But the tables were downloaded and replaced in the R script after the update regardless—all figures were updated using the newest version.

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