

Science and the Structure of Social Experiment:
A Socio-political Study of Climate Data Infrastructure

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Abstract

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In recent years a growing literature on science addresses the problem - some may even call it a crisis - of science's public legitimacy (Douglas 2009, Oreskes and Conway 2010, de Melo-Martin and Intemann 2018). Analysts in this literature see in such media phenomena as climate change denial, vaccine hesitancy, and resistance to public health and environmental measures more generally a growing trend of public doubt over well-established, consensus generating scientific research. Each of these analyses has its own diagnosis of the problem and its own suggestions for the solution, which explicitly or otherwise usually includes a clarified understanding of the way science works and the way that it interacts with society and politics. The present dissertation seeks to add what seems to me a missing alternative among these accounts by focussing on just one of the common case studies in this literature - public response to climate change. I take as my point of departure the framework for discussing this set of issues offered in the values in science literature and exemplified by Heather Douglas' (2009) account in *Science, Policy and the Value-Free Ideal*. That framework presupposes a distinction between epistemic and non-epistemic values and asks what role the non-epistemic values do or ought to play within science's internal activity.

The aim of the dissertation is to challenge an overlooked sister dichotomy to that between epistemic and non-epistemic values, namely that between social and political values. Challenging the dichotomy with respect to science means challenging the view of science as an autonomous, free social association whose proper function is to operate independently from political institutions and political interests. Challenging a clean separation between science and politics reveals a complex borderlands (to borrow a metaphor from Phyllis Rooney 2017) between the social and the political. The dissertation offers some tools for navigating these borderlands beginning with an old philosophical ideal coming from Karl Popper, coming out of the Progressive era and the era of early experiments in Communism. That ideal, I argue, offered a social democratic view of the role for science in politics and a view of science as responsible to society through democratic institutions rather than a view of science as autonomous. Following another vein of thought from that era and from Popper, I conceive of *infrastructures* as politically organized social experiments which can be more or less scientific in character.

These tools help me to address what I consider a problematic tendency in the literature on climate denial to claim that climate science became politicized in the late 20th century with industry-sponsored attempts to undermine the conclusions of the IPCC (e.g. Oreskes and Conway 2010). Such narratives overlook the fact that the data and monitoring network upon which the science depends were developed under the auspices of a political organization - the UN - beginning in the 1950s. Narratives such as Edwards (2006) which portray this as an era in which the network became an *infrastructure* and ceased to be a free association are more attuned to the complex entanglement of science and politics. The aim of the final chapter is to offer an alternative historiographic analysis which pays closer attention to the role of political institutions through funding, appointments and other government activity which provided the conditions for conducting scientific research on climate in the mid 20th century. I focus on the research team led by David Keeling for measuring and calibrating CO₂. The tools developed in the dissertation allow me to make some normative points about the democratic legitimacy of the science-led effort to challenge the hegemony of fossil fuel energy infrastructure on the basis of its connection with climate change.

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Chapter 1: Introduction

In recent years a growing literature on science addresses the problem - some may even call it a crisis - of science's public legitimacy (Douglas 2009, Oreskes and Conway 2010, de Melo-Martin and Intemann 2018). Analysts in this literature see in such media phenomena as climate change denial, vaccine hesitancy, and resistance to public health and environmental measures more generally a growing trend of public doubt over well-established, consensus generating scientific research. Each of these analyses has its own diagnosis of the problem and its own suggestions for the solution, which explicitly or otherwise usually includes a clarified understanding of the way science works and the way that it interacts with society. The present dissertation seeks to add what seems to me a missing alternative among these accounts by focussing on just one of the common case studies in this literature - climate change denial.

Diagnoses of the public problem like Douglas's (2009) take as their point of departure an academic tradition I will call social theory of science (exemplified by Barnes and Bloor and the Sociology of Scientific Knowledge or SSK program) stemming from sociology, history and philosophy departments in the 70s, 80s and 90s (e.g. see Douglas 2009). These diagnoses want to suggest that some of the excesses of this academic tradition facilitated the trend of public doubt, for instance its implicit relativism and commitment to viewing science as socially constructed. Accordingly the analysts of scientific discontent listed above seek a tempered view of science's social dimension, one that doesn't collapse into relativism or generate tools for corporate hired-guns. These tempered accounts often acknowledge that science is social in a limited sense (e.g. as an intersubjective practice) that allows them to still claim the products of scientific inquiry and publication provide neutral or objective information within the public arena. I find these tempered social accounts of science unsatisfactory both in articulating actual science-society relations and in giving due weight to a literature of normative accounts of science's role in

society and politics that perhaps doesn't, at least as traditionally understood, provide ready ammunition for contrarians. In the chapters that follow I will be concerned with articulating what I will call science's *socio-political* dimension, which includes both a dimension of analysis in science-society relations and a set of aims and norms that, I maintain, are part of science's practice.

Epistemic and Non-epistemic Values

I take as my point of departure the framework for discussing this set of issues offered in the values in science literature and exemplified by Heather Douglas' (2009) account in *Science, Policy and the Value-Free Ideal*. That framework presupposes a distinction between epistemic and non-epistemic values and asks what role the non-epistemic values do or ought to play within science's internal activity. Douglas focuses on the process of scientific inference itself, arguing that appraisals of the social consequences of error (a non-epistemic value) should temper scientists' research conclusions. In Douglas' hands, this framework sheds important light on the role that science plays within policy advisory institutions and provides a compelling normative account of why the value-free ideal (or really the ideal which says only epistemic values should operate in science's internal operations) is a bad ideal for guiding scientific inquiry. But this dissertation takes an earlier point of departure with respect to the framework, harkening back to earlier discussions in which philosophers of science (e.g. Rooney 1989, Longino 1996, Laudan 2004) took issue with the distinction between epistemic and non-epistemic values itself. An implicit thesis of Douglas' diagnosis and solution would appear to be that such accounts align themselves with postmodern and relativist rejections of traditional accounts of rationality and that a workable understanding of science-society relations will need to presuppose some distinction between epistemic and non-epistemic values. I disagree with this appraisal, or at least think that presupposing a ready distinction risks ignoring important contributions made by those rejecting the distinction and thus risks saddling us with an ideal that is non-ideal in other important respects.

In particular, accounts rejecting the epistemic/non-epistemic value distinction often problematize the assumption found in Douglas that individual scientific researchers will be able to make accurate appraisals of the social consequences of their conclusions. The assumption is subject to a computational feasibility objection: the social consequences will be unpredictable or too complicated for individuals to accurately predict. But it is also subject to the objection that individual scientific researchers will, for psychological or sociological reasons, not be appropriately situated or prepared to make representative evaluations of the social consequences. Douglas offers a model in which individual scientists make such appraisals, but she discusses scientific roles within democratic institutions for which such individual appraisals are arguably subject to the additional constraint that they be *representative* of the democratic public. This constraint pushes us into the complex political and social project that John Dewey (1927) characterized as discerning the “public and its problems.” Within this debate, Dewey took up a position against a more conservative, technocratic extreme represented by Walter Lippmann (1922) who argued that in large, complex, technologically sophisticated, democratic societies, it is experts who will need to identify society’s most pressing problems and the public that they affect. Dewey countered with a more collaborative account that he illustrated with a metaphor: experts were like cobblers and the public like their customers. The customer knows best that the shoe pinches, but the cobbler is best prepared to remedy the issue. Subsequently, consciousness-raising theorists (Hanisch, Sarachild, MacKinnon) offered a position further left than Dewey, arguing that through political organization publics could become conscious of their own issues and their solutions independently from traditional expertise (or constituting their own expertise). Geographer Ruth Gilmore offers a compelling empirical case study (2007) in this activist tradition in which she traces the opposition to mass incarceration in California from the perspective of activist groups composed of mothers of incarcerated individuals. It is through a process of organizing to combat the deleterious effects of incarceration motivated by the shared personal experience of mothers deprived of sons and daughters that these groups first called attention to the problem of mass incarceration. They often did so, moreover, in opposition to expert criminologists, sociologists, economists and demographers. My own position (which is taken as a background assumption so that the empirical analysis can get off the ground in the final chapter) hews closer to the activist or organizer

position - that genuine public problems and their solutions require a public organizing itself and often don't require experts.

Douglas' assumption of the transparency of public consequences to scientific experts seems most viable within a framework like Lippmann's, whereas the further left we go, the less plausible it seems that individual scientists will be the ones appropriately situated to identify and evaluate public problems including ones precipitated by policy they themselves help to create.¹ This dissertation explores an overlooked position somewhere in the middle of the spectrum hovering around Dewey's position, but attributable to Karl Popper. As with Dewey's metaphor, Popper imagined that the only way to tell how a policy will impact the public is to try it on for size and solicit public feedback. Like trying a shoe on for size before running a marathon in it, he suggested that experimental policy interventions should proceed at a small scale first and expand in scope and scale, or contract, on the basis of public feedback about lived experience of the policy intervention. Popper's account, though much like Douglas' in its concern with the consequences of scientific error, departs in a couple key ways that may also bear some more general lessons for the values in science literature. First it offers institutional mechanisms for averting social risks rather than relying upon individual foresight or conscience. Second it envisions an active role for the public in the determination of the success or failure of scientific experiments which are to test scientific hypotheses. These differences gesture toward the insufficiency of a framework that wants to articulate science-society relations in terms of the relationship between social and epistemic values where these values are understood as desiderata of theory or hypothesis choice possessed by individual scientists. The clearest articulation of this assumption comes from Kuhn (1977) who imagined science as a space protected from social and political influence and who gave an influential articulation of the theoretical values as individual desiderata that met this need.

The institutional feature that is supposed to play a similar role to individual appraisals of consequence for Popper I call *electoral risk*. Just as elected officials within a democracy are subject to appraisal by the

¹ Douglas' treatment of this issue is more nuanced than I am presenting it here. For her elected officials will coach scientists on these values. Ultimately however, scientists are the ones making inferences internal to scientific practice, these inferences are ubiquitous and often don't take place within a policy context where scientists have access to elected representatives. For these inferences it is often scientists' untutored representations of social consequences that run the show. I also wish to develop a view that is critical of actual politicians' ability to represent these values.

public through elections, so too should scientific projects and experiments in the public arena be subject to public appraisal and election. Where the public deems a policy experiment a failure, we should expect to see the termination or transition away from the policy, which will include the termination of funding, institutional positions, and other forms of support for scientific personnel integral to the social experiment. I claim that the Popperian criterion of falsifiability should be understood as, in part, a requirement that scientific projects be subject to electoral risk.

Personal and Political Motivation

The focus on the issue of public reception and response to climate change science stems in part from a long personal engagement with the issue both in academic and activist contexts. As an undergraduate studying physics, I first encountered the issue of climate change as an issue identified by scientists such as Roger Revelle and James Hanson and amplified by politicians such as Al Gore. Following their lead, I saw climate change as one of the most pressing issues faced by humanity. But faced with the lack of public and political response to the issue, I became impatient and turned toward activism and public displays of support for the issue and protest against the obstacles in the way of its resolution. My participation in protests against local fossil fuel infrastructure projects opened my eyes to another dimension of public resistance against fossil fuel infrastructure that Kyle Whyte characterizes as resistance to the continuing legacy of settler colonialism. Nationally the convergence of climate change activists and Native American water protectors against the construction of new fossil fuel infrastructure received media recognition in the Standing Rock Sioux's stand-off with pipeline construction crews and police. More locally, members of the Seattle chapter of climate change activist organization, 350, stood in solidarity with members of the Puyallup tribe protesting the construction of a liquified natural gas terminal in the port of Tacoma. To Native land and water protectors, both projects are a continuation of a legacy of settler colonial legal and engineering projects that have radically transformed and disrupted their relations to land (Whyte 2017). The fact that the Keystone pipeline was to carry oil, while the LNG terminal would carry what many sensitive to the climate change issue consider a bridge or transition fuel illustrates a

deeper tension lying beneath the surface of the convergence between climate change activism and Native resistance. Often the projects billed as viable alternatives to oil and coal infrastructure - some new like expanded natural gas infrastructure, some old such as hydropower dams - are just as, if not more, disruptive to the ecological relationships prioritized by Native land stewards and scientists. Other times recognizing native sovereignty means accepting that some tribes sanction and participate in the extraction of polluting fuels (coal, uranium) from their own tribal lands. These tensions constitute a set of problems that white allies don't often take seriously enough (Whyte 2018b).

A growing literature on relations of allyship seeks to address these tensions and generate critique and sometimes new precepts for intellectual and activist engagement (Whyte 2018b, James and Mack 2020, Sullivan-Clarke 2020). One problem deals with the project of imagining a shared future. Climate change is often conceived of as an issue that will most seriously affect future generations. It thus challenges us to think seriously about the future that we hope to create. But who is the "we" envisioning this future and to whom this future belongs? Whyte (2018a) reminds us that present technologies and infrastructures are often the manifestation of the utopian visions of a prior generation of white, settler architects and planners. Yet, these same infrastructures help manifest an ecological and environmental situation that would have seemed radically *dystopian* to prior generations of indigenous peoples. White allies (myself included) thus need to think carefully about how they go about forging plans for shared action with native allies, they also need to think critically about existing economic and political planning institutions and the lack of tribal consultation within the processes that adjudicate land and resource use and ownership. James and Mack (2020) offer as a precept for activist engagement the decentering or unsettling of settler futurity. The formula recognizes that according to settler institutions, (e.g. such as financial futures markets) the future is literally already settled (someone's private property). Unsettling the future means questioning and usurping the legitimacy of these claims, while decentering settler futures recommends fostering an egalitarian space for collaborative social imagination and planning. This dissertation seeks to import this lesson into the discussion of scientific values, which are themselves future-oriented desiderata that determine the direction of development in science.²

² James and Mack call this principle an "ethic of incommensurability" suggesting that climate change activists cannot hope to reduce Native categories and issues to their own in order to unilaterally decide upon future action. As an issue identified by scientists, this

Another tension has to do with identifying the object of joint concern and resistance that both water protectors and climate activists converge to resist. For climate activists the answer is fossil fuel infrastructure. But for reasons mentioned above (so-called alternatives are often just as bad, some tribes welcome fossil fuel development) this falls short as a unifying object of concern for water protectors. As Whyte (2017) notes it is ecological and environmental relationships with the land, water and non-human others that water protectors seek to protect. Spice (2018) provides a convergent category “invasive infrastructures” that may do better. Invasive infrastructure recognizes the local disruptive ecological and social effects of various infrastructure projects (not just fossil fuel pipelines) and encourages us to think about the social and political dimensions of infrastructures, which situate them within a legacy of settler and economic colonialism.

Together with the position that ground-up or activist identification of an issue provides grounds for considering it a genuine public problem that are at least as strong as expert identification, this set of challenges from the activist context leads me to reexamine the historical genesis of the climate change issue as an issue with fossil fuel infrastructure in a critical light searching for problematic omissions and background assumptions and asking whether the aspirations of global political organizations to regulate the global fossil fuel economy on the basis of climate change science are appropriately critical in light of the critique. With a socio-political lens trained on the dynamics of fossil fuel infrastructure and climate data infrastructure development, I find a host of other issues with fossil fuel infrastructure deserving recognition within the critical democratic discussion and which perhaps have not received as much attention within the scientific literature - a fact which is reflected in a lack of infrastructure for studying them.³ I am guided by Popperian criteria for what would count as critical development, i.e. development that can (and does) learn from its mistakes, of economic infrastructure.

principle also suggests incommensurability between scientific values and Native values (or public values more generally) that philosophy of science has some tools for thinking about.

³ Or, perhaps more accurately, its privatization, which renders such information opaque to the public.

Chapter Summaries

Before I summarize each chapter, a few quick notes on reading: First, each chapter (with the exception of 4) can be read as a relatively self-contained project which offers a thesis up front. While I do draw connections between chapters and make attempts to situate each chapter within the broader framing sketched in this introduction, this constitutes a minimal aspect of chapters 2, 3 and 5. Thus readers could begin reading from any of these chapters. Second, the discussion begins with some fairly abstract discussions of theoretical values that don't find application in an actual case study until chapter 5. For the reader who feels impatient with the lack of application up front or who wishes to have a sense ahead of time for where the dissertation stands with regards to actual scientific practice, reading chapter 5 first may be a better approach. Finally, chapter 4 provides a bridge between earlier chapters, 2 and 3 and chapter 5. The claim is that the tools for analysis summarized in 2 and 3 are not quite sufficient on their own for understanding the analysis in 5. Thus 4 explicates some missing linkages between the first and second half of the dissertation. But for this reason it is likely to be less intelligible if read in isolation from the other chapters. These quick notes aside, I now turn to a summary of the four main chapters.

Chapter 2 articulates the idea that science has *socio-political* values by discussing Helen Longino's argument that the list of traditional theoretical values have a politically conservative valence. In Thomas Kuhn's view the theoretical values, which include such elements as simplicity, consistency, and empirical adequacy, provide public criteria of appraisal within the scientific community, albeit criteria that may be interpreted and applied differently by different practitioners even in regards to the same theory. This description of the theoretical values was meant to preserve a sense in which science could be rational while still being a social activity - one that depended upon the interplay of different practitioners with different perspectives. Yet Longino critiques the Kuhnian list for overlooking the political valence of these values and excluding such items as application to social needs. Longino's critique of Kuhn thus paints his vision of science as social, but in a limited sense excluding direct consideration of social needs more broadly and politics. The discussion of science's sociality often founders in my view on such ambiguity, employing vague notions of society and sociality to describe what happens outside of science and

employing abstract and internalist descriptions to describe scientific practice as social. To remedy the particular ambiguity in the Longino-Kuhn discussion and to begin to address this broader problem in social accounts of science, I look to Hannah Arendt's articulations of the category scheme distinguishing the personal, the social and the political for political and social contexts, i.e. in response to events brought about by (civil rights and feminist) activist movements in the 50s and 60s. I seek to repurpose this category scheme to locate science within society and to identify its socio-political dimension, where socio-political means something like the public management of economic institutions or political-economy.

In Chapter 3 I look to the work of Karl Popper to identify a socio-political valence to the theoretical virtue of falsifiability. The motivation for doing so is two-fold. The first motivation involves making a couple points in the history of philosophy of science. First, Popper is excluded from certain timelines of social accounts of science (see Mirowski 2004). Second, there is a tendency in accounts such as Mirowski (2004) and Douglas (2009) to view the value-free ideal of science as canonical or traditional and as overlapping substantially with logical positivism. Popper is a figure sometimes located within the positivist tradition (especially for his earlier work), but who develops a substantial account of the social and political dimensions of science (perhaps Neurath, who was directly involved in planning the Austrian war-time economy is one of the most involved characters).⁴ This includes a much more substantial role for scientific thought in structuring economic institutions and their development than many contemporary accounts. Through analysis of texts such as *The Poverty of Historicism* and *Conjectures and Refutations* I seek to recover this lost or ignored dimension of Popper.

The second motivation of this chapter is to develop some tools for combating climate skeptics from a perspective on science that does not deny it is deeply imbricated in social and political projects. Many skeptics rely, tacitly or explicitly, upon the idea that skepticism or a critical attitude are core features of the scientific ethos and since Popper is a central theorist of this critical aspect of science (one could perhaps also look to Merton in sociology), I find in this reanalysis of Popper some tools for rethinking the contributions of rogue or contrarian climate scientists who provide support to industry propaganda.

⁴ In Chapter 3 I will try to make clearer why I think characterizing Popper as a positivist is perhaps not accurate; in brief there are aspects of his view of science that move beyond a minimalist positivism such as Ayer's.

The analysis focuses on the fact that falsifiable theories are meant, according to Popper, to be *risky*. In an analogy with the risk faced by politicians in democratic societies, I characterize this risk as *electoral risk*, which indicates the possibility of losing power and leadership positions as a result of experimental failures.

Chapter 4 develops some tools and key concepts that will be needed for the analysis of a case study from climate science in chapter 5. One key shift of the analysis is toward the analysis of scientific data rather than theory. The discussion in chapters 2 and 3 centers around theoretical values as articulating the norms and dynamics of development for scientific practice. But theoretical values such as empirical adequacy or falsifiability depend essentially upon the identification of authoritative data that can corroborate or falsify theory. The social and political dimension of the production of data is thus an equally important object of study for understanding these theoretical values (although as Douglas 2013 notes they are accordingly perhaps better thought of as relational values between theory and evidence).

Secondly, analyses such as Edwards (2006) of the data network developed for measuring global climate variables in the 20th century argue that the network is best thought of as an *infrastructure*. Since climate change is generally regarded as an issue *about* fossil fuel infrastructure as well, infrastructure forms a key category of analysis for any socio-political account of climate science. In part this is because infrastructures are direct objects of state management that also provide economic necessities to national populations. I survey some of the literature from sociology, anthropology and history to articulate certain core features of infrastructure. Since infrastructures are often thought of as cases of *applied science*, I argue that the alleged failure of the smooth functioning of fossil fuel infrastructure identified by climate science introduces another possible (crumbling) pillar of scientific legitimacy amongst the public. For a public that is primed to consider the practical success of technology evidence of the truth of scientific theory and the efficacy of scientific method, the claim of climate science to have discovered a major ecological and practical failure of another technological system may simultaneously undermine its own credibility.

Chapter 5 analyzes a few key junctures in the development of the climate data infrastructure from the 1950s through the 1990s. I focus in particular on the measurement of background CO₂ by a team of researchers led by David Keeling. I contextualize this development within the broader history of the global development of fossil fuel infrastructure. The point of the analysis is to ask whether the climate monitoring network, developed under the UN and of which the measurement of background CO₂ is a core part, accomplishes the goal of identifying and mitigating social environmental risks surrounding fossil fuel infrastructure. Looking to the broader context of scientific research and the interests sustaining that research, I find troubling omissions and background assumptions which should make us question the democratic legitimacy of the scientific case for climate change policy.

The aim of this narrative with respect to the problem of public legitimacy for science is not to render an analysis of science that leaves its conclusions and operations unquestioned. In this I depart from the authors mentioned at the beginning of this introduction. I do so for two reasons. First, the historical analyses of climate change science in this tradition (see Oreskes and Conway 2011, Weart 2022) want to portray science as becoming politicized late in the 20th century (e.g. roughly beginning in the 80s or 90s). But this ignores the fact that climate science began to become deeply embedded within state structures beginning in the 1950s. I find claims that this connection with governments either leaves science autonomous or renders it better able to accomplish its socio-political aims unconvincing in light of an analysis of the historical record with particular emphasis on the social and ecological problems surrounding fossil fuel infrastructure identified by activist groups on the ground.

In summary the aim of the dissertation is to challenge an overlooked sister dichotomy to that between the epistemic and the social, namely that between the social and the political. Challenging the dichotomy with respect to science means challenging the view of science as an autonomous, free social association whose proper function is to operate independently from political institutions and political interests. Challenging a clean separation between science and politics reveals a complex borderlands (to borrow a metaphor from Phyllis Rooney 2017) between the social and the political. The dissertation offers some

tools for navigating these borderlands beginning with an old philosophical ideal coming from Karl Popper, coming out of the progressive era and the era of early experiments in Communism. That ideal, I argue, offered a social democratic view of the role for science in politics and a view of science as responsible to society through democratic institutions rather than a view of science as autonomous. Following another vein of thought from that era and from Popper, I conceive of infrastructures as politically organized social experiments which can be more or less scientific in character.

These tools help me to address what I consider a problematic tendency in the literature on climate denial to claim that climate science became politicized in the late 20th century with industry-sponsored attempts to undermine the conclusions of the IPCC. Such narratives overlook the fact that the data and monitoring network upon which the science depends were developed under the auspices of a political organization - the UN - beginning in the 1950s. Narratives such as Edwards (2006) which portray this as an era in which the network became an *infrastructure* and ceased to be a free association are more attuned to the complex entanglement of science and politics. The aim of the final chapter is to offer an alternative historiographic analysis which pays closer attention to the role of political institutions through funding, appointments and other government activity which provided the conditions for conducting scientific research on climate in the mid 20th century. I focus on the research team led by David Keeling for measuring and calibrating CO₂. The tools developed in the dissertation allow me to make some normative points about the democratic legitimacy of the science-led effort to challenge the hegemony of fossil fuel energy infrastructure on the basis of its connection with climate change.

The dissertation draws on a diverse literature transcending traditional philosophy of science. Inverting a traditional hierarchy that places physics at the top of a hierarchy of the sciences, I undertake an analysis of atmospheric physics that is led by insights from the fields of anthropology, history, sociology, geography and archaeology. While the failures of these disciplines to conform to the model of physics (e.g. its mathematization, predictive success, etc.) are often treated as grounds for considering them less “scientific,” the dissertation explores another criterion for scientific status on which they are arguably superior - namely, susceptibility to democratically determined criteria of appraisal. Past democratic

critiques, which challenged these disciplines' roles in systems of oppression, have arguably changed their practice and theory in a way that has yet to take place in "hard" sciences like atmospheric physics. I also draw on literature analyzing and theorizing activist engagement in the politics of climate change and fossil fuel infrastructure, since I conceive of activist analysis as the guiding voice for critique of science. In this focus, I am led by a traditional aim of philosophy of science - to look to the sciences and scientists for an analysis of science - but with a different social and democratic criterion for what counts as a proper science - a capacity to learn from past social mistakes.

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Chapter 2: The Socio-Political Values of Science

Introduction

In her 1995 article “Gender, Politics and the Theoretical Virtues” Helen Longino challenged the idea that theoretical virtues such as simplicity, consistency and explanatory power are purely cognitive values. The alternative view she pressed for was that, contextualized, each of these values could be shown to have a specific, often patriarchal or conservative political valence. This could be most clearly seen by juxtaposing an alternative set of *feminist* theoretical virtues. For example where the traditional list of theoretical values sees consistency with other theories (external consistency) as a virtue, a feminist analysis according to Longino sees external consistency as a problematic criterion when the other theories in question themselves may exhibit sexist biases. Thus for instance trying to square an anthropological theory about the use of grind-stones for processing grain with the theory that ancient societies primarily hunted for subsistence (where this is assumed to be a male activity and where this theory has been championed by men anthropologists) will likely lead to an androcentric explanation. Alternatively, the feminist theoretical virtue of *novelty* would allow for development of a theory of the use of grind-stones unconstrained by the conservative and androcentric man-the-hunter hypothesis.

In this chapter I try to recapitulate the motivations for this paper of Longino's, which is in some ways transitional between her earlier analysis of the social structure of science and her later pluralism. Why did the debate between feminist and traditional epistemologies of science focus on the theoretical virtues? Why did it take the form of a debate about their cognitive nature and political valences? Answering these questions will require both that we dig into the philosophy of science literature and specifically Thomas Kuhn's work in the *Essential Tension*, but also that we look to the social and political context of the

feminist and civil rights movements beginning in the 1950's. The hope is that this detour into political context will reveal more of what is at stake in analyses of science as having a *social* or *political* dimension. My aim will be to recapitulate ideological debates concerning the categories of the personal, the social and the political that gave rise to the feminist slogan "the personal is political" and to show how a version of the slogan informs Longino's distinctive analysis of science as social and political and her implicit critique of Thomas Kuhn's account of science. There are many different, often cross-cutting senses in which science is called social or political. The hope here is to establish a set of coordinates that is more closely attuned to understandings from public, political contexts, rather than internal discourse in the philosophy of science. In brief, I argue that Kuhn's picture of scientific inquiry portrays it as a free social association to be kept free of broader social and political interference. Longino and like-minded theorists challenge the nascent dichotomy between the social and the political that seeks to preserve this autonomy on the grounds of science's consequential role in politics.

The Political Valence of Theoretical Values

Let's begin with the philosophy of science history. Thomas Kuhn's *The Structure of Scientific Revolutions* (1962) is sometimes credited with introducing (or repopularizing in its contemporary context) the notion that science is a *social* activity carried out by groups of practitioners whose work is guided by scientific paradigms which include common beliefs and desiderata for good scientific work. Science is social in part because what determines the transitions between different theories during scientific revolutions (e.g. the transition from Newtonian to Einsteinian mechanics) is the social process by which practitioners align themselves - their beliefs, values and practical activity - with a new paradigm. Kuhn's insistence in *Structure* that new paradigms were *incommensurable* with old paradigms in part because paradigms include the desiderata used to judge candidate theories opened the door to analyses of revolutionary transitions as driven by purely social or political factors (e.g. national rivalries, interpersonal relationships). This, some traditional critics worried, made scientific change out to be an a-rational or even irrational process.

In a later essay, "Objectivity, Value Judgment, and Theory Choice," contained in the *The Essential Tension* (1977), Kuhn sought to mollify these critics by arguing that scientists in different paradigms could, in fact, come to rational agreement on broad criteria Kuhn termed *theoretical values*, listing simplicity, consistency, scope, accuracy and fruitfulness as examples. These criteria were amenable to different reasonable interpretations. Thus scientists with different research histories may have reasonable disagreements about which empirical implications are most central to assessing a theory's accuracy. He thought the criteria were not simultaneously satisfiable and that one could not develop an algorithm to weigh them against each other. Thus, in many ways, the list and its application to history of science preserved an emphasis upon the social complexities of historical scientific dialogue and preserved some of the flavor of his earlier analysis in *Structure* (recall: where paradigms include different criteria of analysis and these are incommensurable). Yet at the same time, Kuhn thought that, in the long run and through discussion, scientists would eventually converge on which theory was best in terms of these criteria, making the transition between paradigms at least partly rational (one could ultimately point for justification to judgments such as one that the new theory was more accurate).

Longino's work can be situated within two general lines of response to Kuhn. One line of response challenged Kuhn's solution on its own terms, doubting that the list could really lead to a rational consensus or that these values were properly *epistemic*. Among these, Phillis Rooney (1992) (canvassing the likes of Kuhn, Longino, Laudan and McMullin) noted the seeming lack of convergence on a specific list of theoretical values. Since the list was in part meant to serve the function of demarcating appropriate vs. inappropriate criteria of assessment, she thought the lack of consensus on this list revealed a lack of a clear distinction between epistemic and non-epistemic values. Later, Larry Laudan (2004) argued that particular values on the list, for instance scope, were neither necessary nor sufficient for a theory to be true. In light of this fact, he doubts whether the theoretical values will operate within scientific discourse to select true theories (though he is optimistic that they may still be *rational* in a broader sense).

The other line of response came from the feminist philosophers of science who extended the earlier Kuhnian program of identifying social or political influences on the course of scientific debate to cases where these influences included androcentric or sexist background influences (see Merchant 1984) or where they included alternative feminist underpinnings (Keller 1984). Among the former were texts such as Carolyn Merchant's *The Death of Nature* (1980) which portrayed leading figures of the enlightenment and scientific revolution as motivated by the identification of nature as feminine, which brought with it various stereotypical assumptions such as that nature was essentially passive while male scientists were active. Among the latter are works such as Evelyn Keller's portrayal of Barbara McClintock's work on DNA transmutation in corn (1984). Bleier (1984) argued that Keller's portrayal suggested a characteristically feminine attitude toward nature at work in McClintock's research. In identifying episodes of science where different political beliefs exercised an influence on the course of scientific theorizing, these authors extended the earlier Kuhnian program of situating science within a social context of debate.

Among feminist philosophers of science, the attitude toward the later Kuhnian attempt to rescue the rationality of science was split. In a review of feminist attitudes toward the concept of scientific rationality, Longino (1989) noted a division between those who saw the notion of scientific rationality as so thoroughly infused with patriarchal bias that it was irredeemable (e.g. Luce Irigaray 1987) and those who thought some notion of scientific rationality could still be salvaged (though how to square this notion with scientific sociality would need to be rescued from Kuhn's backslide). Longino includes herself among the latter camp and in her 1990 book *Science as Social Knowledge* she sought to articulate conditions that characterize a rational scientific community capable of producing objective knowledge, arguing that they include features of the *social* organization of science. One such feature, echoing Kuhn, is the presence of shared or public standards by which theories are assessed.⁵ Here she recognized some of the theoretical values that Kuhn listed, but she sought at this earlier juncture to make room on the list for "epistemic, as well as social values" (Longino 1990, p.77), thereby including new items such as "relevance to or satisfaction of particular social needs" (ibid).

⁵ For the unfamiliar reader, the other three conditions are: 1) recognized avenues for criticism including peer reviewed journals and conferences; 2) uptake and community response to criticism; and 3) equality of intellectual authority among peers.

Later articulations of the list of shared standards (1992, 1995, 1996) provided point-by-point *alternative* theoretical values to the Kuhnian list and sought to explain how the alternative list was characteristically feminist. Thus Longino juxtaposes *novelty* with consistency, *ontological heterogeneity* with simplicity and so on, as well as adding items such as (again) application to social needs and diffusion of power. The function of the list of alternative values was that “the ideological dimension of mainstream theory appraisal may be revealed by comparison with an alternative set” (Longino 1992, p. 339) and that - more specific to the feminist philosophy of science - the values on this list “prevent gender from being disappeared” (ibid) or “reveal gender” (Longino 1995, p.391). Thus to return to the example of the introduction, an anthropologist who centers the value of theoretical novelty to provide an account of the practical role for grindstones might suggest that the society under study relied heavily or even predominantly upon grain that was harvested and processed by women rather than having to make their conjectures consistent with the mainstream man-the-hunter hypothesis, which centers hunting as the primary means of subsistence. Deploying the alternative value leads to an alternative theory which through contrast with the mainstream standard can be seen to reveal an androcentric bias among the present field. Longino did not necessarily think there was something *inherently* feminist about the abstract values she lists but rather that their feminist character was revealed when contextualized, interpreted and deployed as an alternative to the mainstream list. Contextualized versions of the values revealed what Longino called their “political valence” also challenging the idea (e.g. McMullin 1982) that the values on the list were purely epistemic and thus more generally that only epistemic values were relevant to the appraisal of scientific theories.

The notion that scientific values have a *political* valence distills what I consider to be a two-part feminist critique of the original set. Both critiques can be understood as rejecting conceptual dualisms that are traditionally associated with a binary understanding of gender. On the one hand is the dualism between the social and the rational (or epistemic). On the other is the dualism between the personal (or social) and the political.

The role of the first dualism and its critique receives more explicit attention in the literature on the feminist philosophy of science and within Longino’s work. To see the category scheme used to describe a

difference between women and men respectively, one could look to Lawrence Kohlberg's stages of moral development and Carol Gilligan's (1977) critique. Kohlberg asserts that a detached rational understanding of morality (in terms of principles and rights) forms the highest stage of moral development in children (stage 6) while an understanding of morality in terms of relationship care and repair (a *social* understanding) forms a prior stage (stage 3 in his schema). This theory also found, not coincidentally, that boys were consistently "more developed" than girls because they made it to final rational stages more quickly or more often than girls who were often "stuck" at the social stage. Gilligan points out and challenges the inherent male supremacist assumption of Kohlberg's stage theory and with it the dualism between social and rational and its association with women and men. Though Gilligan's critique may be seen, on some level, as keeping the dualism intact while seeking to deny its correspondence to an evaluative dualism (rationalist understandings of morality are "more developed" than social ones), Longino in her 2002 book *The Fate of Knowledge* hewed closer to the third wave of feminist critique of the dualisms in seeking to dissolve the binary between rational and social by articulating a social account of scientific rationality. The social account of rationality she develops more thoroughly in *Fate* is nascent in her work on the theoretical values which she characterizes as both epistemic and, once contextualized, social (Longino 1996).

Thus this first critique insists that calling the theoretical values epistemic or rational does not preclude thinking of them as also social. The second critique recognizes that for Kuhn the theoretical values were always meant to be social in a certain sense (that Kuhn didn't backtrack all the way to an individual account of scientific inference), but that this sense of the social was narrow and conservative, and intended to block the claim that science is political or social in a broad sense. To better understand this critique it will be necessary to understand the concept of the social as intermediate between the personal and the political.

That the dichotomy between the personal and the political (and its rejection by feminist theorists) informs Longino's thesis that the theoretical values have a political valence can be seen in her 1995 paper, in her discussion of the one value on which Longino's list agrees with the Kuhnian original - empirical

adequacy.⁶ Longino seeks to distinguish a feminist notion of empirical adequacy from the mainstream version. The main difference is that the former pays closer attention to *contextualized* versions of variables or to the context in which measurements of quantities were made. Longino writes, “[t]he decontextualization of experimental variables is analogous to the way in which activity in the public domain of modern industrial societies is analytically detached from the material conditions of its possibility in the private domain” (Longino, p.395). A contextual version of empirical adequacy would by analogy reject the analytic detachment of experimental variables and context along with the detachment of personal and political. In the final section I seek to make the connection between the personal-political dichotomy and the variable-context dichotomy (here billed as just an analogy) clearer.

Because the trichotomy between the personal, the social and the political has received less attention within the literature on the theoretical values, and because Longino’s description of a contextual version of empirical adequacy does not include the intermediate category of the social, I spend the next section providing context for understanding its relevance. I will deal with a tripartite schema involving the personal, the political and the social as developed in the context of the social movements of the 50’s, 60’s and 70’s and where the concept of the social is not identical to the sense often juxtaposed with the rational as in for instance the Gilligan/Kohlberg debate (the social as relational) or in Longino’s *Fate* (the social as interactional) though they are related in ways I hope will become clearer. I expand the schema because it is essential to understanding other social critiques of science and to understanding a specific critique of Kuhn who may be said to recognize certain inherently social aspects of science while denying that it is a political or broadly social activity.

The Personal, the Social and the Political

⁶ The distinction is treated in many contexts as synonymous with the distinction between the private and the public. The association with the gender binary is captured by Jean Elshtain’s title *Public Man/Private Woman* (1981) (see Saxonhouse 2015 for a contextualization).

The slogan “the personal is political” was popularized by Carole Hanisch (1969)⁷ in a piece of the same name written about consciousness raising circles within the feminist movement (for further activist discussion see Sarachild 1973). In consciousness raising meetings participants (typically just women) would focus on questions posed to the group by participants (Hanisch’s examples: “Which do/did you prefer, a girl or a boy baby or no children, and why? What happens to your relationship if your man makes more money than you? Less than you?” (Hanisch, p.3)). Each participant takes a turn answering, going around in a circle, and then at the end participants attempt to summarize or generalize about what has been said. Sometimes these meetings led to political actions such as protesting at a Miss America pageant. In the article Hanisch sought to answer critics who labeled these sessions as *therapy* where this connoted a specifically personal practice regarding personal issues. From the perspective of this (patriarchal) critique, consciousness raising circles were personal both because the issues discussed often involved the family and the domestic sphere a paradigmatic personal or private context (MacKinnon 1982), but also because any disaffection or criticism of dynamics arising within these contexts were seen as the responsibility of individual women to deal with through *therapy*. Hanisch insists on the other hand that consciousness raising sessions are both therapy *and* politics, denying any easy distinction between what is personal and what political. Denying the distinction, Hanisch sought to make women’s issues a part of the political debate, attempting to overcome pressures toward isolation.

Hanisch and other feminists found inspiration and precedent for the practice of consciousness raising circles from Black activist groups in the civil right’s movement (Hanisch, p.4) (some practices and trainings run by SNCC may have been especially influential). In Martin Luther King Jr.’s 1967 speech to the American Psychological Association (APA) he sought to address a similar critique to the one addressed by Hanisch. King targeted the concept of “maladjustment” as a favorite explanation of psychologists and sociologists for Black unrest in the 1960’s US. King wrote that, “...there are some things... to which we should never be adjusted... We must never adjust ourselves to racial discrimination and racial segregation...to religious bigotry...to economic conditions that take necessities from the many to give luxuries to the few... to the madness of militarism and the self-defeating effects of physical

⁷ I am grateful to A.Y. Odedeyi for originally suggesting this piece for thinking about the role of political values in science.

violence” (King 1967). The concept of maladjustment implies that the responsibility for unrest lies primarily with individuals from the black community and by extension the black community itself. It implies that the problems black communities addressed in the 60’s were properly “personal” or “social” (in a sense that excludes the political and will become clearer shortly) rather than political. A similar subtext existed in the insistence on calling the uprisings in Black communities “rebellions” instead of “riots.” The latter term portrays these events as “devoid of any political motivation or content” (Hinton 2021, p.3).

To get a clearer grasp on the position that both Hanisch and King criticize it will be helpful to look at a more principled development of the category scheme that separates the personal, the social and the political. As someone who wrote both philosophical manuscripts and contributed as a public intellectual to the popular debate over these concepts, Hannah Arendt is well positioned to provide a guiding account of this schema. In 1959, Arendt published *Reflections on Little Rock* (RLR) in *Dissent*, a popular political journal, where she took the controversial position critiquing the Eisenhower Administration’s decision to send the federal guard to Arkansas to escort students to the newly desegregated Little Rock Central High School over the protestations of Arkansas’ governor and white citizens. Arendt criticized the decision on the grounds that the schools were a part of the social sphere and as such should be kept separate from political interference. To articulate her theoretical framework dividing the personal, social and political spheres I will draw from her prior philosophical works *The Human Condition* (HC), *The Origins of Totalitarianism* (OT 1951), and *On Revolution* (OR 1963) as well as another popular article *The Crisis in Education* (CE 1954). The analysis of Arendt’s framework that follows is guided by Kathryn Gines’s work which takes a critical look at Arendt’s analyses of Black political struggle in the Americas (see her 2014 book for a more extensive treatment). The aim here is not so much to give an extensive treatment of Arendt as to point toward the aspects of her view that find a resonance within the popular debate, subsequent philosophical analyses and political constitutions. In particular, for Arendt, the social sphere is the economic sphere and the sphere of freedom of association. And as we will see, in liberal societies, her view is that these two senses overlap.

For Arendt the private sphere is roughly that of the home, the social sphere that of the economy and the political sphere that of government institutions. In ancient societies (Arendt's main focus in *The Human Condition* is on Greek society), the home and economic spheres were indistinguishable - the household and family, in an extended sense including not just hereditary relations but also servants and slaves, provided the material necessities of life (another characterization of the private sphere in ancient society is that it is the domain of necessity or of maintaining human life (Arendt HC, p.12, p.35) in contrast to the political sphere which is the sphere of freedom from necessity, action and speech (Arendt HC, p.12, p.25)). In fact, Arendt counts only the two spheres, the private and the political, as having existed. The emergence of the social realm coincided roughly with the division of labor that predated the industrial revolution and the rise of modern societies where individuals needed to leave the home in order to secure a wage and through it the material necessities of living. This economic sphere in which people take care of their needs by performing a job constitutes the hybrid public-private sphere Arendt calls the *social* or society. In society "...private interests assume public significance..." (HC, p. 35). The rise of society corresponds to "...the rise of the "household" (oikia) or of economic activities to the public realm...[where they] become a "collective" concern" (HC, p.33). For Arendt this collective concern became political explicitly in the French Revolution which sought to address the social problem of poverty though it appeared earlier in the very notion of a "nation." It was articulated theoretically by the "political economists" first implicitly by Smith, then explicitly by Marx (p.33). Arendt notes that for the Greeks, the notion of "political economy" would have been a contradiction in terms combining notions of *polis* and *oikia* that were understood as mutually exclusive (HC, p.29).

Though the three spheres have melded together historically, Arendt in places seems to regret this fact (Gines notes a similar theme in Arendt's deadpan analyses of oppression and colonization in her CH 6), and still thinks they constitute separable domains that ought to be kept free of interference with each other. The domains are separable in part because each has a unique governing principle: for the political sphere the governing principle is equality (equal franchise, public offices are equally open to all), the guiding principle of the social sphere is *discrimination* and that of the private sphere is *exclusion* (RLR, pp.51-52). Thus, for instance while whatever citizens have in the political realm (e.g. the right to vote, to

speak publicly, etc.) each must have equally, in the social realm individuals ought to be able to freely choose who they share their time and resources with discriminating between individuals (Arendt uses the example of deciding who to vacation with RLR p.52) and leading to non-equal outcomes (everyone is not equally entitled to vacation with Arendt). Arendt notes that for the Romans *societas* originally had a “limited political meaning; it indicated an alliance between people for a specified purpose” (HC p.23). In this sense the paradigmatic social organizations may be interest groups whose membership is determined by the presence of shared aims and values.

Each of these spheres must be allowed to operate by their governing principles, in order to ensure stable political institutions (OR, p.92), a recognizable form of society with the possibility of group formation and free association (RLR p.51) and a domain of intimacy and belonging in the home (OR p.96, HC p.70). According to Arendt revolutionary political attempts to address problems in the social sphere such as poverty - she cites the French and Russian revolutions - inevitably lead to terror (terror becomes institutionalized in totalitarianism) and the failure of the revolution to establish stable political institutions (see OR, p.112). Successful revolutions (she cites the American revolution) have stuck to ensuring a bill of rights that is concerned primarily with ensuring freedom from political interference (OR, p.108) or so-called “negative” freedoms within non-political spheres. Within the US Bill of Rights, we find rights corresponding to the protection of Arendt’s private and social spheres and their governing principles. For instance the right to *exclude* law enforcement except under very specific conditions (e.g. having a warrant) from one’s home codifies one freedom of the private sphere - an *exclusive* right to the use of private property. Freedom of association helps codify a freedom of the social sphere - the right to *discriminate* in one’s social relations choosing to associate with some individuals but not others.⁸

Arendt’s critique of federal interference in Little Rock is that while the courts were in the right to abolish all legally- or politically-sanctioned discrimination (e.g. Jim Crow Laws, the Black Codes), the principle upon which they had done so was the sovereignty of the social sphere and the need within liberalism to protect

⁸ While some aspects of Arendt’s distinctions are unique to her analysis, many find resonance within liberal political theory. See for instance the modern immigration debate in which Wellman (2008) argues citizens may have a right to exclude foreigners from immigrating on the basis of freedom of association. See also Blake’s (2012) response.

it from state interference. Arendt counts attending the school of one's choice - like and related to the decision of what community to live in - as a freedom of the social sphere and the schools as a social institution. This claim is complicated by the fact that, as she recognizes, educational institutions perform the political role of educating future citizens (OR p.73). It is an institution that seeks to facilitate the transition between the private and the public world in children's development and attendance is required *by the state* (CE p.9). Still education is also primarily (and originally) an extension of parenting, a private sphere activity. Parents choose to bring children into the world, how to raise them and which social world they will encounter (CE, p.8). In many ways education is also concerned with social advancement, social status and the development of skills (OR p.73). And there is a way in which the idea of education is inimical to the political sphere where the idea that some adults need to be educated (presumably by other adults) would undermine the notion that in politics all adults encounter one another as equals (CE, p.3).

To summarize then: for Arendt, the schools are a social institution. Social institutions ought to be governed by the principle of free choice (in this case of parents). And these free choices ought to be protected from political interference. Applying this argument to the Eisenhower administration's intervention meant that the federal government was wrong as well to enforce *integration* within a social institution by political means.

With the three spheres in clearer view, we can turn to some philosophical attempts to articulate the objections nascent in Hanisch and King's critiques. One line of objection notes a necessary relation between the subjugation of some within the private (or social) sphere and their non-equality in the political sphere. Bernasconi (1996) criticizes Arendt for extolling ancient society and a category scheme that finds its primary application in a civic order that relied upon the unjust institutions of slavery and women's exclusion from the political sphere. Arendt fully recognizes the reliance upon women, children and slaves in Greek society for creating the condition of freedom required for propertied, male citizens to participate in politics arguing that to the Greeks suppression of some to achieve this freedom would have seemed fully justified (HC p.31). Carole Pateman (1988) makes a similar critique of liberal theory when she argues that women's domestic labor provides the conditions for men to enjoy liberal political freedoms in

modern societies. Charles Mills extends this line of thought in *The Racial Contract* (1997) by noting how the US social contract - the Constitution - explicitly made the exclusion and subjugation of African Americans a basis for the freedom of propertied whites from codifications of slavery (the fugitive slave act) to attributions of partial personhood (the three fifths clause). This line of thought helps to articulate part of the Hanisch/King critique - personal or social issues where they inhibit comprehensive application of the guiding principle of the political realm, equality, are an appropriate target of political critique. Call this the positive critique in that it attempts to show how women's subjugation within the private sphere or African Americans' subjugation within the social sphere (that is their inequality and unfreedom) are related to and form the preconditions for so called (but inauthentic) political "equality" and "freedom" of others.

The other part of the critique is negative in that it attempts to show how attempts to draw the line between the personal, social and political are often regressive in reinscribing patriarchal and white supremacist hierarchy within their contexts of power. In claiming that the issues women discussed in consciousness raising circles were properly private, the regressive critique at once makes the issue the responsibility of the individual woman alone, while simultaneously relegating the issue to a context in which patriarchal relations of domination may continue to operate unimpeded by external critique. MacKinnon 1983 makes the resonant point that political relations of domination - for instance those codifying rape law and its determination by the male legislators and judges - mirror and reinforce private relations of domination, arguing, in a step beyond Pateman that the liberal state *is* inherently patriarchal, and that the traditional family constitutes a site of political power relations (Saxonhouse 2015). In a legal context where the two spheres are mutually supportive, one can expect a defense of male control in the private sphere against its legal challenge. Gines (2014) notes how Arendt's critique overlooks how social institutions like the schools, but relatedly workplaces and neighborhoods, have been shaped by both *de jure* and *de facto* practices of white supremacy. Arendt fails to see how thoroughly life in the segregated South is shaped by these practices and implicitly provides cover and sanction for them by reinscribing segregation within the social sphere. Though Arendt argues for the abolition of all *de jure* segregation, she does not recognize the ways in which unified practices of social discrimination (from homeowners associations within

neighborhoods, practices ensuring employer discretion over hiring, admissions decisions at universities) could act in tandem to undermine the ability for black citizens to realize social and political equality.

Arendt's discussion also seems to ignore the fact that for African Americans, the New Deal constituted an era of massive *de jure* discrimination. The New Deal saw enormous political involvement in social spheres of education (e.g. the GI bill), housing (e.g. the federal loans program) and employment (e.g. through public works projects). But these programs were notoriously discriminatory (for instance in housing through practices of red lining which designated certain, typically, non-white neighborhoods as excluded from federal housing loan assistance - Rothstein 2017). Indeed, the reason why the political slogan "the social is political" never had the same resonance as "the personal is political" was that while the 1950's US kept a fairly tidy division between family and political life countless white Americans received some form of federal economic assistance during this era or agreed with these programs. For these groups it would have seemed obvious that the social is political (in the next chapter I discuss how the New Deal was inspired by public works programs following the explicitly social Russian Revolution in the Soviet Union). The New Deal consensus formed such an ingrained feature of the political landscape that it was a *Republican* president (Eisenhower) that Arendt criticized for excessive federal interference in the social domain. It was also Eisenhower who oversaw one of the largest federal public works programs of the last century in the construction of the interstate highway system - a fact exploited by leaders of the civil rights movement in their occupation of the streets as a public space (in the broadest, i.e. federal sense). The absence of any critique of these programs as they applied to white Americans in Arendt's analysis of the Little Rock intervention constitutes another highly problematic oversight.

Having laid out the tripartite scheme between the personal, the social and the political, we are in a better position to understand what challenging the trichotomy (the assumption of their conceptual separation) entails. Even though Arendt's categories have become thoroughly blended historically, and there is something of a tacit consensus on the left denying the social/political dichotomy, they help us to see more of what is entailed by denying the distinctions and perhaps places where the critique has not yet been followed through to its logical conclusion.

Challenging Freedom of Inquiry as Social Association

In the last two sections we have seen how the category of “the social” is multi-faceted and ambiguous. In listing the theoretical values as a social feature of science, Longino recognizes Kuhn’s account as social in a certain sense. Yet, in her identification of a social or socio-political dimension of the Kuhnian values, she offers a critique of Kuhn as failing to recognize the values as social in another sense. At times Arendt uses the category of the social to demarcate an activity as separate from the political or public sphere - as in her paper on federal involvement in Little Rock. In other contexts the social names those aspects of private life that have already become a subject of public or political concern. Going forward, I suggest we distinguish between a narrow sense of social which is at home in liberal societies and a broad notion of social as the concept figures in *socialist* political regimes. The former sense treats as social those activities properly governed by norms of social associations, which I expand upon in the next section but which involve free association and expectations of conformity conditional upon membership. The latter sense treats as social those activities which are of broader public and political concern - as when for instance governments regulate or operate economic institutions for the public good. Socialist theorists (where the “social” in “socialist” is understood as broad) do not recognize a tidy division between economy and politics and for this reason, they may use phrases (as Longino does) such as “socio-political” to describe certain issues or activities. (I find that calling the values “political” or “socio-political” rather than “social in a broad sense” is less confusing and for this reason employ these phrases in the next chapter). In this section, I explain how this distinction borrowed from Arendt in a more thorough-going political discussion can be applied to clarify Longino’s critique of Kuhn as tacitly, but inappropriately relying on the social-political dichotomy as applied to science to portray it as a free association.

As I suggested above, Longino’s 1995 critique of the conception of theoretical values inherited from Kuhn should be thought of as containing a double critique. The first lies closer to the surface in her paper and

was articulated more explicitly in her 2002 book - the denial of the dichotomy between cognitive (or epistemic or rational) and social values. The second can be articulated as an implicit denial of the trichotomy between the personal, the social and the political and may be summed up by the two slogans: the “personal is political” and the “social is political.” The latter of these two critiques can help to explain how Longino can acknowledge the Kuhnian picture as social in a certain (as we will see narrow) sense while failing to recognize it as social in another (broad) sense. Because the critique of the tripartite schema gets less discussion and because I think in many ways it remains implicit in her papers on the theoretical values, I will focus primarily on it here.

To begin, we can see the relevance of the slogan “the personal is political” to the philosophy of science and understand Longino’s analogy between the way that empirical evidence is decontextualized and the way in which private and public spheres get analytically separated by considering the positivist distinction between the context of discovery and the context of justification (Rooney (2017) calls this an ancestor distinction to the epistemic/non-epistemic distinction). The distinction was introduced by Reichenbach (1938), but found fairly broad acceptance among positivists (including in earlier writings Popper, who I discuss in the next chapter). The *context of discovery* refers to the circumstances in which an individual researcher first develops or happens upon a hypothesis. It may include such factors as the researcher’s personal history, observations and experience, their cognitive interests and the specific circumstances that facilitated their insight (e.g. taking a bath or staring into a fire). The context of discovery is meant to be set aside (or at least put into appropriate perspective) when appraising the general validity of the hypothesis. This process of general appraisal constitutes the *context of justification*, which involves its publication, discussion and experimental test within the scientific community and which must take into consideration not merely the personal experience of the hypothesis’ original advocate but in addition the evidence for or against it stemming from other researcher’s experience and efforts. It is in the context of justification that the hypothesis encounters criteria of *public* appraisal. In many cases it is seen as inappropriate to bring aspects of the context of the discovery into its public appraisal. In the extreme, Popper writes that doing so leads to psychologism or the reduction of the hypothesis to idiosyncratic individual background factors of the researcher which are taken to explain away the hypothesis as an

individual whim. Psychologism blocks the hypothesis from being considered as a valid possible generalization, i.e. as a candidate for public acceptance within the context of justification. The strict separation between discovery and justification helps explain the positivist aversion to the kind of contextualization of scientific hypotheses that later critiques of positivism (such as Longino's) attempt. And as can be seen, the distinction also often coincides with factors that are private and public respectively. As we will see in the next chapter and as alluded to in the previous section - the positivists, many of whom were also fleeing the persecution of totalitarian regimes in Europe - had a political motivation for keeping separate the private and the public spheres. Their persecution in some cases could even be characterized as making their personal lives and identities grounds for their political exclusion.⁹

While Longino's analogy only references the private/public or personal/political distinction, there are also some ways in which her critique can be seen to challenge the later Kuhn (of the *Essential Tension*) as making science out to be a social activity in the narrow sense rather than also a political activity (or social in the broad sense). In place of a historiography of great individuals staring into fires by the bathtub, Kuhn's *Structure* substituted a picture of science as a social enterprise organized according to paradigms that articulate a common worldview, set of technical skills, pedagogical commitments and theoretical standards for a research community. But this picture developed a view of science as social in a narrow rather than a broad sense. Accounts like Douglas (2009), Fuller (2000) and Longino (1995) articulate ways in which Kuhn maintained a vision of science as insulated from broader social and political issues. Kuhn's conception of science is narrowly social, or social in the minimal sense of a social association in at least two senses - it portrays science as conformist and as self-electing - two core features of a social association. I turn to describing these features below.

For Arendt social organizations are frequently characterized by a convergence of aims or interests. This makes it so that social groups expect *conformity* (HC 39-41). Those who do not share the aims, values or

⁹ Transferred to the American context (where the primary issue could perhaps be characterized as the domination of the political sphere by the social or private spheres rather than domination of the private or social spheres by the political sphere as in totalitarianism) this doctrine of keeping the private separate from the public often carried a conservative valence that overlooks the ways in which, for instance, domination masquerades as "freedom" in the private sphere.

interests of a given social group may form their own group, but they have no basis on which to demand a reordering of the group's values. This is in sharp contrast to the political sphere which is characterized by a plurality of voices and viewpoints (HC p.7) and where it is appropriate or even definitive of the political domain to attempt to persuade others to change their minds (persuasive discourse is what Arendt considers "speech" which defines the political domain and which she contrasts with *commanding*, familiar to the private domain (HC p.26)). Kuhn's 1977 attempt to articulate values upon which all scientists must (eventually) agree, thus makes it out to be a social organization in the narrow sense.

Another feature of social organizations connected with their being free associations is their prerogative to be self-electing and self-governing - that is *current* members may decide amongst themselves (and in consultation only with themselves) who will become a new member of the organization and upon the direction for the organization without reference to those outside the organization. In this sense as well, Kuhn's articulation of the theoretical values as determined by its historical practitioners and passed down through the pedagogical commitments of a paradigm makes scientific "revolutions" out to be an insular affair and science out to be a narrowly social organization.

Commentaries like Douglas (2009) and Fuller (2000) characterize Kuhn as making the new desiderata explaining scientific transitions primarily desiderata that scientists and scientists alone are equipped to appraise and discuss, promoting a vision of science as autonomous from broader social and political influence. Fuller 2000 discusses its intended implications for science and history of science pedagogy at Harvard where Kuhn taught - intended to train administrators of science to appraise but not question judgements of scientists regarding which scientific projects deserved funding. In gesturing toward the many contexts in which scientists make socially and politically consequential judgments, Douglas (2009) challenges the view that science ought to be free of external social considerations. Longino's 1995 critique is similar in looking toward areas of research (e.g. in socio-biology) where the social and political ramifications of scientific theory are clearer to make the argument that the theoretical values have a political valence. Still, the notion that the political ramifications of scientific theory in one area of research render science elsewhere also political depends upon a lingering unity of science thesis that is somewhat

at odds with Longino's introduction of an alternative value-set and could seem somewhat out of step with her later commitment to pluralism. That unity is expressed by the idea that the theoretical values are shared criteria (across the sciences) of public appraisal. Taken together - the idea that in some areas the theoretical values have a clear political valence and the claim that these are values shared across the sciences - we have an argument for considering the values themselves as having a political valence.¹⁰

In short, the picture of scientific research offered by Kuhn was of a social association whose organizing aim was the discovery of the best secular metaphysical theory based on empirical evidence. His notion of a community of researchers insulated from broader social or political influence also resurrected the positivist protection of a private freedom to investigate whatever one likes and in the manner one prefers as a freedom of practitioners to associate with other members of a paradigm - i.e. as a social freedom of association in research.

Those who note how science plays publicly (socially, politically) consequential roles suggest a way to problematize the notion that science is or ought to be a free association (e.g. Longino 1990 focuses on the importance of academic science to public debate; Douglas 2009 focuses on the role that scientists play in policymaking; Oreskes and Conway 2010 discuss its role within the media). In doing so they provide grounds for rejecting the social-political dichotomy that attempts to preserve this freedom within scientific institutions and promoting the alternative view that the social is political.

¹⁰ There is a lingering question whether Longino takes the alternative set to be a better set - to modify or replace the original - or merely a set that could viably organize an alternative (feminist) research program. In her later writings it sometimes seems as if she leans toward the latter, which undermines the unity of science thesis upon which the claim about the political valence of theoretical values depends. I choose to resurrect the project of claiming that certain values have a political valence by in part rejecting the viability of too fine-grained a pluralism about scientific practice. This is for a number of reasons. For one thing there seems to be no *a priori* way of distinguishing which branches of science will be relevant to political or social questions. The analysis of climate change theory which is itself an application of more general theories such as thermodynamics and carbon chemistry in the final chapter illustrates the point. Prior to that chapter I discuss a general theme characterizing the way in which environmental science may become implicated in politics - in adjudicating private from public goods - but this theme is teased out of historical analysis rather than programmatic of environmental sciences. In general there aren't ready principles for discerning when and where science may become relevant to politics, so that science would do well to have general norms for navigating these interactions. For another thing, historical analysis of science reveals an interweaving of influences from diverse fields and arguments from external consistency that because a method or theory works in one field it ought to work in another. So the notion that methodologies are radically particular may not hold up to empirical scrutiny. Finally, arguments that a particular area of science is authoritative often rely tacitly or explicitly upon the authority of other areas of science in virtue of these shared methods or influences. Those who defend the authority of climate change science don't do so on the grounds of, say, the past empirical success of climate models alone, but on the grounds for instance that it is an application of theories that are confirmed in a diverse range of other phenomena and disciplines.

For one thing, this role may be inherently problematic considering the appropriate role for social associations within a democratic society generally. We often think such associations may form interest groups which lobby or campaign for their issues of concern to receive uptake within political institutions. But any closer connection (direct advising, policy authorship, providing personnel) is often treated as an inappropriate *capture* of democratic institutions by parochial interests. This is in sharp contrast to the role played by scientific personnel in the era from WWII through the present. Jasanoff (1990) argues that in their regulatory and advisory roles, scientists form the fourth and fifth branches of government respectively. How can this privileged political role be justified alongside the claim that science is or ought to be an exclusive, yet self-appointing or self-governing institution, aspects that properly belong to a social and not a political organization?

One of the earliest attempts to reconcile this tension, Vannevar Bush's *Endless Frontier* (1945) - a memo written for FDR and providing the institutional blue-print for the NSF - suggested a coincidental harmony. It just so happened that by allowing science to pursue *basic* or *pure* research, i.e. research of intrinsic scientific interest to the community (operationalized in a review process by which candidate projects for federal funding are appraised by scientific peers alone), the democratic public would also receive the greatest possible benefit from science. This happy harmony is reminiscent of Adam Smith's *invisible hand* principle which makes a similar argument for free associations within economic affairs on the grounds they will promote the most overall benefit. But to borrow a line from Arendt who pithily explains the Marxist response to Smith: "...the difference between Marx and his forerunners was only that he took the reality of conflict, as it presented itself in the society of his time, as seriously as the hypothetical fiction of harmony" (HC p.44). The other line of critique within philosophy of science searches the empirical record for Bush's coincidental harmony offering a historical analysis of the socio-political valence of allegedly autonomous science. This critique is exemplified by the style of analysis summarized from Longino above. This dissertation attempts to move beyond challenges such as Douglas' (2009), which challenge the value free ideal in the abstract, and seeks to look at the historical legacy and political valence of the institutional counterpart to the ideal - basic science. The final chapter investigates an episode from climate science with this aim.

To summarize, this chapter has sought to juxtapose a paradigmatic sense of the social drawn from outside of science - the social as the economic sphere and the sphere of free association - with some internal senses of the social as structuring the direction of inquiry and scientific communication and debate (manifest in the general category of theoretical values). I have argued that Kuhn's vision of science-society relations portrays science as a free association to be protected from political interference in a way that is inappropriate given its role within government institutions and politics more generally. I view theorists such as Longino who draw our attention to the way that science operates in political contexts as challenging the application of the social-political dichotomy to science nascent in its portrayal as a free association.

But before turning to an analysis that attempts to move beyond the social-political dichotomy, I offer an extension of Longino's argument regarding the theoretical values to a value that wasn't on Kuhn's original list (for good reason), but which in some form or another makes it onto later lists - falsifiability. I discuss this value because it encapsulates what some scientists take to be a valid basis for scientist-led denialism about global warming - the critical or skeptical spirit of science. Longino notes how scientific values can have either a conservative or liberatory valence. In the next chapter I discuss the theoretical value of falsifiability as developed by Karl Popper arguing that it was intended to have a politically liberatory valence. Popper articulated a vision of science as political insofar as it was not completely autonomous but rather open to what I shall call *electoral risk*. Electoral risk challenges the notions of both self-election or the ability for the community alone to decide membership and self-governance or the ability of the community to decide on a direction for research. It is this valence, which I argue was a core component of the value as Popper understood it, and which undermines implicit appeals to an ethos of skepticism (discussed in Oreskes and Conway 2010) from climate denialism's scientific supporters.

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Chapter 3: Falsifiability and Electoral Risk

Introduction

The present chapter has both a specific and a broad aim. The specific aim is to develop the idea mentioned in the introduction that climate change denialists from the scientific community can and perhaps ought to be understood as doing bad science not because of failures to adhere to methodological norms (e.g. peer review) which are meant to depoliticize science, but because the political valence of their work is antidemocratic and anti-emancipatory. This latter critique rests on the idea that the normative category of “science” carries with it democratic or emancipatory connotations. I develop this critique of the climate denialists mentioned in accounts like Oreskes and Conway (2010) by undercutting an appeal to the skeptical or critical spirit of science which gives the contrarian actions of the climate denialists a veneer of legitimacy. Whether it be publishing in an oil industry sponsored (non-peer reviewed) journal, or blogging under the banner of past scientific heroes (see the Australian “Galileo Society”) seen as contrarians of their time, or accepting exceptional political nominations to advisory committees upon which they adamantly insist upon an interpretation of the scientific evidence at odds with the rest of the committee, these actors implicitly appeal to the idea that the scientific ethos includes a skeptical, contrarian or critical spirit.¹¹ Perhaps the most well-known philosophical theorist of this idea is Karl Popper, whose vision of science demanded that scientific theories always be at least capable of being overturned and whose methodological picture of science required that practitioners devote themselves to attempted refutations of communally proposed conjectures. Popper’s work is a touchstone for philosophers of science who attempt to articulate what is involved in scientific criticism (Longino 2002) as

¹¹ Though I take this as implicit in the actions of denialists, it is openly defended by Millian theorist such as Intemann and Demelo Martin (2018) who argue that allowing skeptical or critical contributions is essential for scientific progress and therefore not the root of the problem with climate denialism. This chapter can also be seen as articulating a more appropriate notion of scientific critique.

well as popular understandings of the scientific ethos. This chapter takes a closer look at Popper's articulation of this aspect of the scientific inquiry, arguing that for Popper the skeptical spirit of science always had a liberal perhaps even social democratic socio-political cast that is at odds with its invocations by libertarian skeptics.

The broader aim is to weigh in on a controversy within the history of philosophy of science over the timeline of disciplinary thinking about the "social dimension" of science. Accounts like Mirowski's (2004) explicitly wish to omit Popper from the timeline on the grounds that he didn't have a robust social epistemology or talk much about existing scientific institutions. Yet, other authors like Longino (2002) see themselves as carrying on a set of questions and a tradition of social epistemology *from* Popper. My hope in this chapter is to convince the reader that the social dimension of Popper's thought is substantial, though it is also critical - not particularly interested in leaving the boundaries of the scientific community where they stood.

In what follows I will try to show how Popper's view is motivated by the question: what are the appropriate conditions for setting up and running scientific social experiments? In considering this question we will see that Popper considered both traditional epistemic criteria *and* moral and political criteria. The resultant role that Popper imagines for science in society represents an addition to Dewey's synthesis, discussed in Mirowski, of democratic and scientific institutions - a synthesis of democratic, scientific *and* liberal economic institutions. Throughout texts like *The Open Society and Its Enemies* (OS 1945), *The Poverty of Historicism* (PH 1956) and *Conjectures and Refutations* (CR 1963) Popper sought to dismantle a view of science as capable of acting in a central economic planning capacity and instead suggested, given the proper scientific orientation toward social risk, liberal democracies with their guarantee of free speech and economic institutions that are small-scale because privately owned provide the best setting for the progress of science. While some have seen in this synthesis an unabashed apologetics for a Cold War libertarian ideology spelled out by thinkers such as Friedrich Hayek or Milton Friedman (see Thorsen 2011), I find in Popper's work an account that gives priority to considerations of social accountability over libertarian institutional precepts of free speech and property rights. While maintaining they are not perfect

and will need a critical supplement, Popper thinks that liberal economic institutions are the best answer to the prior demands for social accountability within his view of science-society relations. This means that for Popper scientific critique is justified by a demand for social responsibility rather than as an individual right of speech or industrial experiment (i.e. a private property right). In short Popper leaves us with some critical tools that do not straightforwardly rationalize the status quo of capitalist societies, and a view worth criticizing insofar as this will bring us closer to an adequate understanding of science-society relations. Accordingly, I offer some critique in the present chapter and a reorientation in the next chapter in preparation for a socio-political critique of climate science in the final chapter.

The common route taken toward establishing these two aims builds on the discussion of the theoretical values from the last chapter. I focus on Popper's criterion of falsifiability or degree of falsifiability as another virtue of scientific theories often placed on the list of theoretical virtues. For example, Popper offers an interpretation of simplicity - a value on the paradigmatic Kuhnian list - in terms of falsifiability, suggesting he thought the latter should be replaced or reduced to the former as a criterion of theory appraisal. Other modern accounts seek to add elements of falsifiability to the canonical list - see for instance Schindler's addition of non-ad-hocness to his account (2018). I borrow the framing from Longino's analysis to argue that falsifiability is not purely epistemic (in the traditional sense to be explained shortly) but has a distinctive socio-political valence. To do so I offer a contextualization of Popper's work and his central examples. The prior chapter offered an analysis of what is meant by socio-political. To begin the present chapter I offer an account of what is meant by calling this valence "non-epistemic." This will be important for distinguishing the account offered here from a number of accounts that are more purely "epistemic" and thus omit the socio-political dimension of Popper's thought.

Epistemic vs. Non-epistemic Value

I have introduced the idea that a theoretical value may have a "non-epistemic" valence in the last chapter's discussion of Longino's 1995 piece on the theoretical values. This distinction between

epistemic and non-epistemic values is discussed in a broader literature that seeks to sort values onto one side or another or to identify the appropriate roles for such values within science (see Rooney 1992, Laudan 2004, Douglas 2009, Rooney and Lacey 2017). There are also the nearby distinctions between cognitive/non-cognitive (Longino 1996, Douglas 2013) and rational/arrational (Longino 2001). The distinction rests upon a particular, traditional view of epistemology inherited from the logical positivists and analytic epistemologists. (For the purposes of this chapter I will understand “epistemic” as having this traditional sense, though I am more sympathetic with the idea from social epistemology, e.g. Longino 2002, that the epistemic ought to include social and socio-political considerations). The logical positivists claimed, roughly, that scientific knowledge was justified by a combination of basic empirical observations and logical operations on the theories that recovered them - observation and logic. The analytic epistemologists tended to turn their sights toward scientific methods and results to discern general precepts of justification and instances of truth and in doing so they implicitly sanctioned (though in some instances like Ayer 1936 they were explicit) a view of epistemology as exhausted by an ideal empiricist understanding of science. The narrow view of epistemology that results from combining these would treat anything that doesn't follow from basic empirical observations and logical operations involving those observations as non-epistemic (in Ayer's account this includes all moral and political claims). This is, even by historical standards, quite a narrow view of epistemology.

Another approach that has a basis going back at least as far as Kant and which Habermas (1968) revived in the 20th century recognizes several types of rationality and knowledge. For Kant there was speculative reason, moral/practical reason, and aesthetic reason. For Habermas there were technical, moral and aesthetic cognitive interests embodied in the social institutions of science, law and art. Each of these types of reason generates its own kind of truth by a distinctive method. In Kant, while speculative reason operates using the general categories of observational experience, moral reasoning uses the device of the categorical imperative, which asks reasoners to imagine the community of agents who act on their maxim. The contemporary field of social epistemology of science, which starts from the assumption that the methods by which scientific communities arrive at truth are themselves social, study a convergence point for different normative practices of theorizing. A rule that says: “anytime a colleague reports a direct

observation that contradicts a prior, communally-accepted generalization we ought to abandon the generalization” has both a logical and a moral source. The logical source has to do with the fact that generalizations are true only so long as all of their instances are. The moral source has to do with trusting and respecting fellow practitioners as in some important sense equally capable of generating authoritative observation reports and of drawing inferences from them.¹² These two valences are in turn interactive and mutually reinforcing - practitioner’s understanding of logic may set the background of expectations for appropriate and respectful dialogue within the field. And sometimes a growing sense of bias, omission, disrespect or disregard causes us to reexamine formal rules that appear to provide cover for morally suspect discursive interactions. On the view developed in this chapter, both moral and traditionally epistemic criteria are necessary for generating scientific knowledge that can act as a basis for public action.¹³

Some have attempted to characterize the moral and political valence of social epistemology under the rubrics of epistemic justice and injustice (Fricker 2007). But this characterization is also, for my purposes, not entirely helpful. For the articulated kinds are often spelled out in terms of a background truth that is either unarticulated or unacknowledged, but which can already serve as a reference point. Or, relatedly, they are spelled out as challenging individual’s identities as *knowers* (i.e. as people who already know). The point is that the very norms challenged by epistemic injustice, (where these injustices are spelled out in a slightly deflationary way, not as denials of truth or nonrecognition of knowers, but closer to the way Dotson 2011 does as denials of experience, testimony, opinion, observation reports or inferences within a social enterprise of epistemic production) are *constitutive of* epistemic communities and the knowledge they arrive at. What plays the reference of truth in a more thoroughly discursive epistemology (e.g. like Longino’s 2002, quasi-Piercian epistemology) is precisely the end result of a conversation which does not happen when injustice is present, a conversation that does not violate the moral norms of discourse.

¹² One can see a moral concern with discursive action in Kant’s discussion of lying.

¹³ See also Habermas’ view that norms of scientific production presuppose some of the Kantian discursive norms that are thoroughly moral.

While both of these literatures have been formative in thinking through the set of issues I wish to explore in the current chapter, I opt for using Longino's language of the "political valence" of theoretical values, applying this idea specifically to falsifiability. The thesis is that Popper was aware that falsifiability had a political valence, and that this political valence was essential for understanding the role he thought appropriate for science in society or the social dimension of science.

The Other Faces of Falsifiability

In this section I discuss some interpretations of falsifiability that are more thoroughly epistemic (in the above traditional sense) so we have a contrast class for thinking about its socio-political aspect. For purposes of this discussion, I will deal with the value of falsifiability, construed by Popper as having the following key features articulated in his earliest account from *The Logic of Scientific Discovery* (LSD 1934). A theory or hypothesis, T, is (more) falsifiable if

(a) It entails (more) basic observation statements, O.

(b) The theory would be rejected upon the observed falsehood of some such statement.

(b1) In particular practitioners will avoid *ad hoc* maneuvers (LSD p. 81) or purpose-specific adjustments to the theory which allow it to avoid refutation.

(b2) Falsified theories (T) may be saved by introducing new auxiliary hypotheses (A), but the new conjunctive premise (T & A) which entails the potential falsifier, O, should be treated as a "new system" (LSD p. 82/83).

(c) The theory is *risky* (or riskier) (LSD p. 280, CR p. 36, p. 240).

Falsificationism, to remind the reader, is the methodology which prioritizes the proposing and testing of highly falsifiable theories or hypotheses - theories or hypotheses with many implications that can be

tested and can turn out false. It involves articulating precisely the observable implications of a theory and also a commitment to rejecting the theory should its test implications turn out false.

The first style of account to discuss are the purely formal or logical accounts. Combined, (a) and (b) can be paraphrased as saying that a theory is falsifiable if it is capable of functioning in a *modus tollens* argument of the following form:

1. If T then O
2. Not O
3. Therefore not T

Where T is the theory and O is some observation report. Falsifiability is also supposed to come in degrees (it is not merely the necessary condition of non-tautology) and there are various formalizations of this possibility from that degree being expressed as the number of observation reports that a theory entails (in Popper 1934) or the number of free parameters in a polynomial equation that describes a best-fit curve for data (Forster and Sober 1994). These purely formal notions make no specification regarding how this argument form or the analysis of the degree of falsifiability map onto social features of scientific practice. In fact, they are perfectly satisfiable by a single scientific agent. For this reason such accounts have been called “monological” (Habermas, Longino). Monological accounts of falsifiability ignore its social dimension, which is present in Popper’s earliest accounts as the requirement that falsification be an “intersubjective” phenomenon, but is spelled out more clearly and with examples in *Conjectures and Refutations*. There it becomes clearer that Popper imagines the process of falsification as involving one agent who proposes a theory and a separate agent who attempts to falsify it - as in the Einstein-Eddington case (discussed more below). These cases make it clear that the possibility of falsification required for falsifiability is not merely a logical but also in some sense a social one, viz. an intersubjective possibility. It is important to note that monological accounts generally do not *exclude* the possibility that multiple actors are involved in the provision of theory and test observations. But they do not and perhaps cannot offer an account of why this social division of labor might be important.

The problem is not unique to *formal* descriptions of falsifiability or its operation within the methodology of falsificationism. Latour (1983) offers what we might consider a mono-*technical* (as in *techne*) notion of falsifiability in his account of Pasteur's synthesis of the anthrax vaccine. In Latour's "Give Me a Laboratory and I will Raise the World," Pasteur moves through the stages of conjecture and refutation by himself in a laboratory where he grows the anthrax bacteria and possible vaccines in a Petre dish. By the time he is ready to deploy the vaccine as part of a state agricultural program, he and a team of administrators are able to proceed in semi-authoritarian style. They stifle public interpretation of the vaccine's effectiveness *in situ* through prior tests and through a system of information that effectively sends all critiques back through the lab for official validation. Latour's point about the importance of measurement devices and apparatuses that can sanction an observation report and to which different social actors have differential access is an important critique of the positivists, but the critical suggestion that there is something wrong with the criterion of falsifiability because it allows Pasteur to self-sanction the effectiveness of the vaccine through trial and error conducted by himself in the laboratory misses the mark. Falsifiability for Popper has a social aspect too; it describes a potentiality for critique among different social agents. This is connected to a further aspect of falsifiability that these accounts either ignore or leave unexplained - feature (c) listed above - falsifiability's connection with *risk*. We hear from Popper that conjectures ought to be "bold", "risky", "exposed to the hazards of refutation" (LSD p. 280, CR p. 36, p. 240) and so on. The problem with either the monological or monotekhnical accounts is that they imagine a theory can be falsifiable without necessarily being risky or where they reduce the risk to upsetting an individual practioner's private expectations.

Another variety of account at least offers a model of the social structure of inquiry that may help to articulate Popper's notion of risk, but I find its account of risk within inquiry inadequate for resolving the social problematics of inquiry which Popper presents in PH and CR. This is Kitcher's (1990) account on which different scientific actors pursue different often competing hypotheses at the same time as part of the most efficient division of communal labor in pursuit of truth. On Kitcher's account, individual researchers engage in a risk calculus where they weigh the benefits, usually articulated as some kind of prize for discovering the truth, and the chances that their following different research avenues (constituted

by different methodologies or focus on different hypotheses) will lead them to discover the truth. Practitioner's calculations are responsive to how many researchers are pursuing a given avenue already (a saturated avenue leads to a lower individual probability of discovery). The result of these individual calculations leads to a division of cognitive labor at the community level that is more efficient or takes less time to discover the truth.

An implication of Kitcher's account is that since different researchers or teams often pursue mutually incompatible hypotheses, one research team's success amounts to the falsification or failure of another team's hypothesis or approach. Thus on Kitcher's account individual practitioners face a risk associated with failure to achieve the prize awarded to the successful team and whatever resources may have been expended in the process (though Kitcher's discussion does not explicitly count these amongst the costs of inquiry). The rationale for this risk is that it drives individual practitioners to organize themselves in an efficient way at the community level for the pursuit of truth. It is meant to be a virtue of Kitcher's account that the *loss* associated with the risk is seemingly a non-epistemic good - a prize, prestige, social gratification or something of this sort - while the overall function of the risk is to promote the timely discovery of truth, an epistemic aim. Kitcher's account thus constitutes a challenge to accounts suggesting science has non-epistemic aims insofar as these aims may still be shown to promote truth indirectly by a route such as he sketches. At this juncture it would be difficult to explain why I do not think Kitcher's formulation can be leveraged to provide an adequate account of the function for risk in falsifiability. To begin to see the difference we will need to delve into the context and social problematics surrounding Popper's own discussions of falsifiability.

The Context and Problematics

To start, consider the core examples of scientific and pseudo-scientific theories Popper identifies in *Conjectures and Refutations*: Einstein's General Theory of Relativity; and Marxist Sociology, Freudian

psychoanalysis, and Adlerian psychology. As an example of genuine science, Popper discusses Einstein's general theory of relativity which made predictions of observable stellar positions during a solar eclipse confirmed by the Eddington expedition of 1919. Historians have illustrated how relativity theory took on an ideological significance through public scientific venues in increasingly nationalist and anti-semitic Germany prior to WWII (Van Dongen 2007, Josephson 1996), with accusations that relativity represented a distinctively Jewish brand of physics aired publicly at scientific conferences, as well as the notion of that relativity acted as ideological support for internationalism.¹⁴ In fact, Eddington's observations, which marked a collaborative effort between British and German scientists undertaken against a historical backdrop of intense nationalist rivalry, were to some degree driven by and confirmed the value of scientific internationalism, a value which dovetailed with the pacifism of both Eddington and Einstein (Stanley 2003).

As examples of pseudo-science, Popper discusses Adlerian psychology, Freudian psychoanalysis and Marxist sociology. All of three of these "pseudo-sciences" were highly politically and socially influential before Popper's publication of *LS* in 1934. Marxist sociology formed the analytical basis of the growing socialist and communist movements in continental Europe and eventually the Russian Revolution of 1917 (Josephson 1996). The *language* of Marxism was itself in part the outcome and basis of a series of internationalist assemblies (e.g. the First, Second and Third Internationals), which served as a kind of counterpart to Western international economic institutions during the post-war period. Freud's theory of the subconscious and its drives provided a basis for psychoanalysis among the bourgeoisie (Scull 2015) and for new methods of persuasion becoming prominent in propaganda campaigns abroad and advertising campaigns domestically (within the US see Bernays 1928). Adlerian psychology was a formative doctrine in education programs within Austrian society. People based important, even sometimes life-and-death, decisions on the authority of these theories and part of Popper's aim seems to be to challenge the legitimacy of this influence. A story from Popper's autobiography, *Unended Quest* (UQ 1974b) illustrates the point with respect to Marxism.

¹⁴ Gallison (2003) reminds us that *conventionalism*, a stalking horse of Popper's, was also an international organizational format - scientific conventions for the establishment of measurement standards took place regularly between different European nations and North America in the 19th century.

As a teenager in Austria shortly after WWI Popper participated in local socialist movements (social democratic and communist) through participation in various student associations. He writes that he was permanently disillusioned with communism by an event that occurred shortly before his seventeenth birthday (Popper, UQ p.33). Several young activists attempted to free fellow party members from a local police station during a demonstration and were shot in the ensuing struggle. Following Marx, Popper seems to think, the members of the party believed in the prediction of the collapse of capitalism and the rise of the proletariat to power, which would implement a socialist government. Moreover, they saw this outcome as inevitable, arising from the necessary dialectical laws of history. In light of this belief, Popper thought that members of the movement saw their only available historical role as precipitating the revolution. Individuals could, through their action, hasten or deliver the revolution, though they couldn't change the course of history (ibid, p. 35).

Later (in PH) Popper would identify Marxism's underlying deterministic view of history as the core of the problem. This *historicist* view lay at the root of the activist's decision to risk and ultimately lose their lives. He notes that the particular theory of history had already made predictions (regarding the character of the coming revolution) that had turned out false in the Russian Revolution of 1917 (Popper, UQ p. 43).¹⁵ The proper thing to do subsequent to those falsifications, and what would befit true science, would have been to mark the original theory disproved. Instead it was kept alive with an *ad hoc* modification with more or less the same commitment of its followers. By contrast, Einstein expressed a willingness to consider his own theory untenable if certain predictions that it made should turn out false. These predictions were then tested by an international scientist from a historically rival community.

It is also important to situate Popper's development of the criterion of falsifiability within the political climate of the time (1930's through the 1960's), specifically, with respect to an ideological struggle over the concepts of "science" and "experiment." As Arendt (1951) notes the three principal political ideologies of the time - Naziism centered in Germany, Communism, e.g. in Russia, and Liberalism, e.g. exemplified by Britain and the US - claimed scientific status for their economic and political programs in their propaganda and institutional doctrines. Naziism promoted eugenic and race science through institutions

¹⁵ Specifically, that communism would first take hold in a capitalist country.

of population demographics and administration grounding its conception of the German *Volk* and the Jewish people in convenient anthropological and eugenic theories (many of the eugenic studies coming out of the US) (Josephson 1996). Communism based state planning measures on a Marxist theory of history, and justified large-scale industrial interventions during the mid 1930's including the collectivisation of agriculture on the basis of Lysenko's distinctively "proletarian" revival of Lamarckian genetics (Josephson 1996, Elliott 2017) and infrastructure projects such as hydroelectric and irrigation projects in the Aral Sea (Peterson 2016). Liberalism continued to find social backing from Social Darwinist ideas of a self-interested struggle of all against all leading to ever greater social progress and fitness (Arendt, 1951 p. 330). As mentioned above, these ideological battles reached even into physics where new theoretical advances such as relativity theory or quantum mechanics often served as a proxy battle ground over ideological and epistemological debates concerning relativism and materialism (Josephson 1996, Hessen 1971, Gerovitch 2004).

Besides claiming to be grounded in certain already recognized scientific theories that they then promoted to a place of prestige within government and scientific organizations, different national ideologies also took a stance on the scope and role of *scientific experiments* applied within a social context. Early travelers from the US and Europe to the Soviet Union, brought back excited reports of the large-scale social and economic experimentation that was being conducted there. The idea that these were "experiments" seems to have come from this traveler perspective (Feuer 1962, Namboodiripad 1991).¹⁶ Pragmatist and progressive social reformers such as John Dewey and Jane Addams traveling from the United States saw analogs of their own local social experiments (e.g. Dewey's laboratory schools or Addams' Hull House) being carried out at the level of national policy in the Soviet Union. In the other direction, Soviet reformers openly admired Taylorist and Fordist¹⁷ forms of social engineering within the US (Peterson 2016).

¹⁶ Historicism, which has a determinist view of historical progression, denies its activities are experimental. The category of "experiment" is more at home among progressive or pragmatist views.

¹⁷ Fredrick Taylor inaugurated the practice of scientific management which sought to promote worker efficiency through the subdivision of tasks and the study of manual labor. Henry Ford implemented novel factory-line techniques for mass production as well as social programs such as higher wages that allowed members of his workforce to purchase the cars they made. It was accompanied with a system of careful monitoring and surveillance that sought to ensure recipients of social benefits conformed to a moral code of conduct at home.

Up until the late 20's, American intellectual opinion regarded large-scale Soviet social experimentation in a favorable light. That opinion began to shift as information about the Stalin era began to trickle out. Stalinism began in the late 20's. Through a series of 5 year plans, Stalin orchestrated a drastic overhaul and industrialization of the Russian economy, and its Sovietized federations which brought about massive social and ecological disruption (Peterson 2016) as well as being accompanied and driven by a campaign of political terror and disappearance and forced labor institutionalized in the Soviet secret police and the Gulag system (Solzhenitsyn 1974). A well-known example of the programs implemented during this period was the collectivization of agriculture which forcibly transformed a system of small share-holders into industrial combines. The transition occasioned peasant revolts and crop failures that left millions dead (Josephson 1996). Biologist Trofim Lysenko stepped into an official capacity for the Russian state, in part promising to solve the ecological fallout and resulting food-shortages through a technological fix. Instead his failed revival of Lamarckism, emphasizing the importance of environmental vs. inherited factors, only prolonged the shortages and accompanying suffering.

There was thus a need for those sympathetic to the earlier framing of Soviet efforts as legitimate social experiments to account for what had gone wrong in the Stalinist planning period. There is also good reason to see Popper as sympathetic with earlier logical positivist visions such as those of Otto Neurath (Cartwright et al. 1996) of a central role for the social sciences in government policy making.¹⁸ Read in this light Popper's idea developed in *The Poverty of Historicism* that experimentation in the social sciences should always involve "piece-meal engineering" seems like an attempt to articulate the boundary between legitimate and illegitimate social intervention and organization.

Finally, Popper's own search for an academic position abroad as Naziism spread through Europe may be relevant for thinking through the issues in LSD. Popper was among a wave of academics that also included Einstein, who sought foreign academic posts to escape Naziism at home. Most academic positions required publication of a manuscript and this provided an impetus for Popper to publish LSD when he did (*UQ*). As I will explain shortly, this also helps to motivate the particular interpretation of the

¹⁸ Neurath took a lead role in planning the Austrian war-time economy during WWI (at least insofar as Popper still seems to reserve for social science a regulatory role of monitoring unintended consequences of social action).

risk involved in falsifiability as related to the risks at play in receiving or being accepted to academic posts within the sciences.

In summary, these contextual factors suggest that Popper's discussion of science found clear relevance in his contemporary social and political context, where to call a theory scientific was often to grant it the *authority* to act as a basis for social action and policy. Indeed in later texts, Popper himself seems to embrace a social and political problematics for explaining the merits of his earlier epistemology. In the introduction of CR, for instance, Popper draws various connections between epistemology and politics in an attempt to show how his own epistemology avoids certain authoritarian implications and tendencies of other epistemic doctrines (interestingly pragmatism is listed among the authoritarian doctrines).¹⁹ In the next section I discuss Popper's view that liberal economic institutions come closest to resolving this set of problems because they offer an economic system which meets the moral demand on social experiments that they avoid causing unnecessary harm.

Looking ahead, the authority of science can stem from its logical empirical merits, but it may also have other normative sources. In general, we may ask what gives scientific experts or scientific theories *legitimate* authority to act as a basis for social action, where the concept of legitimacy may be based on considerations that are non-epistemic in the traditional sense (as I will suggest in the next section one may decide what to do for a group legitimately because one's interests are suitably representative of the group).

The need to give an account of the authority of science when, on Popper's view, scientists always operate under conditions of persistent uncertainty - because the problem of induction is irresolvable - gives rise to a difficult variation of the socio-epistemic problem: what are the appropriate conditions for legitimate expert authority under uncertainty? It was this problem that led Popper to consider the appropriate scale of social experimentation and which I think leads to an account of falsifiability as electoral risk.

¹⁹ See also Lefevre's characterization of Popper as developing an epistemology "that does not result in an automatic defense of the closed society" (p.96).

Falsifiability as Electoral Risk

Perhaps the place to begin talking about how Popper's epistemology of science avoids some of the authoritarian tendencies discussed in the last section is to note his repeated insistence that the aims of science include identifying and minimizing risks, broadly construed as risk to society, including in science's pursuit of solutions to particular social problems.²⁰ Applied to social experiments or engineering,²¹ Popper thought that science could only properly be used for social applications in small-scale or what he called "piece-meal" engineering. As Koertge (1972) points out, there seems to be an inconsistency between the account of science oriented toward social issues in *PH* which makes a conservative demand on the scale of scientific experiments - that they be piece-meal and small scale - and the account offered in *LSD* which makes good science out to be a *risky* endeavor. Because of tensions such as this, authors such as Koertge (1972) and Lefevre (1974) have tended to see Popper's later writings as an ideological overlay upon an earlier more neutral epistemology. Resolving this apparent tension will require that we think about how *institutions* can be constructed which have the aim of minimizing risk to society. In particular, I suggest the appropriate notion of "risk" involved in falsifiability is the risk to scientists and engineers of discontinuing their scientific projects in face of public criticism about their impacts. The resultant account I call "electoral risk" after the risk faced by political leaders in their capacity as elected representatives.²²

In providing an account of falsifiability that looks "non-epistemic" by traditional criteria I hope to show how one can double down on the assertion of the irresolvability of the problem of induction, while finding direction for inquiry through considerations of accountability, openness and social relevance. I believe the resultant account deserves renewed attention in contemporary discussions of science's social and political dimensions.

²⁰ Popper tends to reiterate the uniformity of the social problem of identifying and eliminating unintended consequences of individual action, which he treats as the programmatic aim of the social sciences. Yet his rejection of overarching holist, teleological accounts of human history may also explain his insistence that the problem be "particular" or "specific" suggesting a Deweyian picture of a pluralist democratic public that discerns issues for itself through a rationally organized media.

²¹ Popper often uses "experiment" and "engineering" interchangeably.

²² Though engineering may connote application of settled science, while experiment connotes a continuation of inquiry, drawing this distinction too sharply would cut against Popper's insistence that even individual proprietors discussed later are conducting experiments. What else would provide the kind of low-risk context appropriate for conducting social experiments?

Let us start with the idea that for Popper, it is a central aim of science to minimize risk to the public.

In *The Open Society and Its Enemies* (1945), Popper suggests as one of his own principles for a humanitarian ethics the principle of minimizing suffering. He writes, "I suggest...to replace the utilitarian formula 'Aim at the greatest amount of happiness for the greatest number' or briefly 'Maximize happiness', by the formula 'The least amount of avoidable suffering for all', or briefly, 'Minimize suffering'" (OS p.235 n.6). The figure of the piecemeal engineer, who exemplified for Popper the ideal scientific attitude toward social experimentation or the application of science to policy, carries out this aim by adopting "...the method of searching for, and fighting against the greatest and most urgent evils of society, rather than searching for, and fighting for, its greatest ultimate good" (OS p.158).

Popper also insists that these moral principles must be safeguarded in politics "...by institutional means of legislation rather than by the benevolence of persons in power" (OS p.235 n.6). Popper writes that for political theory, one core organizing question for institutional design is thus: "How can we organize our political institutions so that bad or incompetent rulers... cannot do too much damage?" (CR, p.25). He says that the analogous question for philosophy of science is: "How can we hope to detect and eliminate error?" (CR, p.25). Though Popper offers the comparison as an analogy, I think the notion of error here should be construed broadly to include social failures of science-led social projects (the terrain where Popper's piecemeal engineer constitutes the ideal) and that the central questions for science and politics are thus more closely related.

Popper also insists that it "...is the task of the theoretical sciences to discover the unintended consequences of our actions" (CR, p.342). One may wonder here whose actions Popper is speaking of - scientists? state officials? members of the public? - but the point is a cornerstone of his social epistemology. While social scientists, as part of methodological individualism, can assume the behavior of individuals insofar as it affects only themselves is rational and intentional, the default situation when it comes to social action - action that affects others - is that much of what happens is unintentional (PH p.141, CR p.125).²³

²³ Individuals who perform experiments in living that affect only themselves form a kind of limit case where the default assumption should be that there is no additional basis for external regulation. But this limit case does not imply that organizations such as corporations which are often conceptualized as a case of private property fit this mold.

Finally, as mentioned in the last section, there is the idea that science ought to be engaged only in piece-meal social engineering - a constraint developed in response to the belief that large-scale, revolutionary overhauls such as those orchestrated in the Soviet Union inevitably lead to much unnecessary suffering, thus violating the principle of minimizing harm (CR p.343). This also stems from the idea that large-scale experiments are too complex to function as a means for extracting information from the experimental situation (i.e. there is no control).

These passages suggest that science has as an overarching aim the minimization of public risk. How will science, which is also often tasked with identifying this risk, achieve this aim? I focus on two main suggestions for scientific practice and institutions: first science should adopt the accountability mechanism of democracies that holds representatives accountable through electoral risk; second, science, in the stage of justification should remain open to critical considerations of practical and social value emanating from members of the public themselves (scientific “elections” should be open to external participation and influence).

Beginning with the first institutional feature, in a democracy, Popper notes, “...rulers will be compelled by the threat of dismissal to do what public opinion wants them to do” (CR p.345). In the political context, this threat is a *risk* of doing politics - call it *electoral risk*. Similarly, in PH, Popper draws an analogy in the opposite direction: “Scientific method in politics means that the great art of convincing ourselves that we have not made any mistakes, of ignoring them, of hiding them, and of blaming others for them, is replaced by the greater art of accepting the responsibility for them, of trying to learn from them and of applying this knowledge so that we may avoid them in the future” (PH p.88). The threat of dismissal offers a repercussion for officials who fail to learn from past mistakes, who overstep the bounds of actions that can be remedied or undone or whose projects and political platform society simply deems unsuccessful. Social interventions based on scientific hypotheses must be similarly positioned to classify actions as based upon a mistaken hypothesis and to discontinue the intervention in question. Elections in politics help to rule out political leaders who maintain political power despite public criticism. In scientific practice they help to rule out hypotheses that survive every possible test including through *ad hoc* modification.

These claims about politics often suggest the rulers know *how* to do what the public wants, and that they know *what* the public wants. How will we adapt the recommendations to the situation where we are trying to discover these things? I believe the answer to this question lies in the openness of the stage of justification to considerations of social relevance and importance - traditionally non-epistemic criteria. This point requires making more central certain pragmatist elements of Popper's thought that he himself side-lined.²⁴

Salmon (1981) offers a skeptical analysis of the practical rationality of using claims that pass through the gauntlet of falsificationism as a basis for action. What reason, he asks, can we have for using them as a basis for action if, as Popper insisted, they are not better confirmed or more probable than their rivals? Salmon considers a quote from Popper's reply to his critics: "Deciding to criticize a proposal from the standpoint of modern medicine (rather than, say, in phrenological terms) is itself a kind of "practical" decision (anyway it may have practical consequences)" (Popper 1974b, p. 1026). What determines whether a hypothesis continues to be tested is, thus, not how likely it is to be true alone. There is also room considering how important it is in other dimensions. In particular, how important it is as a basis for action. Within the sciences, there is a more complex sense in which a hypothesis that is being tested is used as a basis for the action of designing and carrying out an experiment. It may also be used as a basis for further experiments where it operates as an auxiliary or background hypothesis. The practical import of the hypothesis may reduce to this - it is very consequential for determining the future direction of research. But it may include besides the importance of the hypothesis for social action or policy more broadly. An ongoing area of research into cures for cancer, obviously finds external support in the form of funding from grant agencies and through individual scientists' concern for the social relevance of their research. Popper's claim suggests that science, in its critical stage, ought to be open to practical considerations, which may influence the decision to criticize or not criticize a hypothesis.

The idea finds additional support in Popper's attempt in CR to develop a notion of *content* that is "relative to a hypothesis or to a problem" (p.231) or that accounts for implications that are "interesting and relevant" (p.230). Which test implications are most interesting, central or relevant will likely come both

²⁴ Popper describes himself as a *critical realist*.

from those who propose hypotheses who make clear what they consider the implications of a hypothesis that are most central to the problem at hand, and from those who criticize hypotheses who are free to choose which among these test implications to test or may draw further implications they think more relevant or central to the explanatory project at hand. Hypotheses, Popper emphasizes, are never certain or beyond possible test, suggesting there is simply a *practical* decision at some point to stop testing them - the most central, interesting, or relevant implications have been identified and tested.

Finally, in his critique of Utopian engineering (discussed further below) Popper indicates that the openness of scientific inquiry to *public* interests and values is a necessary condition for scientific critique, or critique that exposes errors. He writes that Utopian engineers will invariably need to stifle expressions of public dissatisfaction and “[t]hus it will be difficult to ascertain the facts, i.e. the repercussions of the plan on the individual citizen; and without these facts scientific criticism is impossible” (PH p.89).

Taken together these considerations suggest the condition that falsifiability implies some degree of *risk* can be understood as electoral risk. Scientists who propose and stand to gain credit for general hypotheses, thereby suggest those hypotheses as a basis for action both within the scientific community and more broadly. When they are falsified it is an indication that they have failed to predict certain truths, but also that these are in some sense the most important truths to get right. It is not that the general hypothesis could not be saved by modifying an auxiliary assumption, but rather that to do so would be to insulate the scientist inappropriately from risk. The risk that the scientist bears is, thus, to some degree related to the degree that they represent and represent faithfully the practical interests generally of those who stand to criticize their ideas, where these interests may include more general social interests in a hypothesis or problem.

That this is an appropriate standard to hold scientists to is suggested independently by thinking about different normative sources for authority more generally. One source of legitimate authority is epistemic in a traditional sense. We agree to let *competent* rulers rule, because they know best what will happen under various circumstances. But an alternative source of legitimacy is based upon representation of our interests or values - we agree to let someone make a decision for us, even under uncertainty, because we think they will model our own evaluative orientation toward the situation. This latter source of legitimacy

familiar from the political democracies where we elect representatives substantially on the basis of their ability to represent our interests (Brown 2009) provides an *alternative* source of legitimacy when the more traditional epistemic source of empirical truth does not apply. The suggestion of this chapter is that falsifiability, as a value of inquiry, may provide a source of legitimate authority insofar as it ensures accountability to public practical and social interests.

Though specifying and defending concrete institutional mechanisms for enforcing electoral risk is beyond the scope of this chapter, identifying such institutions would seem to be necessary for the analogy with elections (an institutionalized process) to work. Indeed, what distinguishes the present account from others that treat Popper as primarily describing norms of individual responsibility is this institutional cast. I believe that there are such mechanisms in research funding institutions (grant agencies, foundations, universities), through promotion processes within academic, government and industrial scientific institutions (extending from graduation in education up through hiring decisions at universities, and in government and industry), and through publication and platform decisions in conferences, journals, and popular publications.²⁵ Chapter 5 discusses some of these mechanisms in depth.

To close this section, I offer some additional motivation for the choice of the phrase “electoral risk” by responding to a few possible objections.

First, one may feel dissatisfied with the institutional analogy between political elections and the mechanisms by which scientists and scientific projects are selected. In one, members of the public vote directly for political candidates, while in the other it is much harder to discern the participatory role of the common non-scientist citizen in the selection of scientific personnel and projects - certainly nothing like a direct vote takes place. While on the surface, this seems true, I hope to make clearer in the historical discussion of chapter 5 that there are indirect links between actual political elections and the mechanisms by which scientific projects and personnel come to operate. Still, this is not the only or even the primary point of the choice to call the risk of falsifiability “electoral” risk. This phrase also captures the idea that

²⁵ Though to my knowledge Popper does not talk explicitly about these institutions, I have tried to motivate the claim that for him some kind of institutional mechanism of accountability is necessary. From the ideal of a piecemeal engineer which Popper talks about so much, we must reverse-engineer the institutional constraints for instance by asking how their projects will remain small scale.

scientists are “the elect” or the chosen, which conveys the idea that their power to influence politics is a privilege with attendant responsibilities rather than a right of free speech and inquiry. It also conveys the idea that they are responsible to an “electorate” and captures the appropriate gradient of responsibility and risk between the elect and this electorate.

Second, one may feel that the analogy with elections places undue importance on the selection of the person, or individual scientist rather than on the hypothesis they test. This is out of keeping with a view of science as involving objective methods in the sense of their being impersonal and focussed on the truth of propositions. While in actual practice, voters may often select candidates on the basis of individual criteria - their moral fortitude, past history, attractiveness, etc. - political candidates are also representatives of a policy platform and a political party. By analogy individual scientists act as representatives of a hypothesis or research program. Their individual research history and demonstrated capacities are also often not irrelevant to their selection to positions of power and privilege.

Finally, one could object that political elections are problematic from the standpoint of truly democratic ideals. To list just two examples, actual elections commonly exclude large portions of the population affected by policy in virtue of various barriers to voting such as citizenship status, voter ID requirements, access to voting locations, etc.; they are also susceptible to a tyranny of the majority over minorities. Since I have portrayed Popper as championing a democratic vision of science, actual elections may seem a poor candidate for realizing this ideal. Given these concerns, one might wish to replace one-person-one-vote elections with a system of weighted participation capable of overcoming instances of exclusion and minority domination. While I am sympathetic with this critique, I would ask *who* is to determine the system of inclusions and weights that will counterbalance these anti-democratic tendencies. In keeping with commitments expressed earlier in the dissertation, I maintain it must be affected individuals and groups themselves “on the ground” who raise and develop consciousness around the inadequacies of traditional elections. My account is non-committal on the exact form that elections should take, but would still consider such individuals members of an *electorate* to which the scientific *elect* ought to be held accountable.

The Structure of Social Experiment

With these two features for scientific accountability in mind - electoral risk and openness let us return to Koertge's concern that Popper's social account of science might be inconsistent with his earlier account of the natural sciences because the former portrays science as conservative where the latter portrays it as risky.

To clear up the apparent contradiction we need to specify further *who* bears the risk of falsifiability and *what* the associated loss or cost is that they risk. The answer given above is that advocates of hypotheses in the scientific community (these may be individuals or groups) bear the risk and not society more broadly. This in turn is a mechanism for ensuring that society *doesn't* bear unnecessary risk as a result of scientific practice and, more positively, how science may act to minimize risks other social actors pose (science in a regulatory role). Moreover, the risk is one of no longer having one's favored hypothesis act as a basis for social action - whether within science in the stage of inquiry (no one continues to build on it or test it) or outside of academia in policy. I have also noted how the risk of losing this authority is related to the broader social and practical import of the hypothesis. When we take these considerations together, we see that hypotheses whose testing would be riskier are more likely to be criticized and to lose practitioners funding, the positions necessary to carry on their research, or access to publication. This in turn is likely to ensure that practitioners who wish to keep or gain funding and positions of power pursue modest projects at least to start (they don't go claiming to have found a genetic basis for differences between races in cognitive testing on the basis of a correlation).

To develop a clearer view of Popper's account of social experimentation and engineering it will also be helpful to review his discussion of the contrasts to piece-meal liberal economic experimentalism (his preferred model, which I expand on below) - Utopian and historicist conceptions of social experiment. Popper claims that the Utopian endorses large scale experiments where the historicist denies the validity

of such an idea entirely, though they propose to carry out interventions at a similarly large scale. The historicist doubts the validity of the concept of a large-scale social experiment because history never repeats itself and they are holists - the instance is explained by the context, through a consideration of its role as a part in an organic whole. Historicists also tend to be determinists about history. Together Popper thinks these ideas conspire to close off the possibility of transformative critique and must lead to a forceful suppression of dissent that characterizes totalitarian regimes. His critique of the Utopian (which he recognizes as involving “technical as well as moral reasons” (PH p.88)) is rather that the experiment is too complex to glean information from and must inevitably involve “considerable inconvenience” (ibid p.89) to the participants. The historicist or Utopian’s desire to perform the experiments anyway must then involve an attempt to suppress public criticism (ibid p.92) or to attempt to mold the individual into conformity with preconceived notions of success through education and propaganda (ibid pp.70, 80, 90).²⁶ This mirrors the structure of a hypothesis that is unfalsifiable - it eliminates critique *a priori* through the methodological privilege of *ad hoc* modifications.²⁷ Piece-meal experimentation on the other hand is receptive to critique and this leads to an openness as to the scale of experimentation as well (PH pp.68-69).

The idea that the scale of piece-meal experimentation is not fixed at any particular level provides an indication that a liberal-economic model (small scale, at the extreme, individual proprietors) is not an ideological postulate, but follows from principled considerations of science’s social responsibility. The scale can increase in response to repeated successes or failed attempts to falsify at a smaller scale, and it can decrease in response to repeated falsifications. Here it is important to distinguish between a particular hypothesis and a certain level or scale of hypothesis. The hypothesis may be that a certain economic intervention - building a factory - will result in a greater quantity of consumer goods for the local population. The scale of the hypothesis may have to do with the number of people who will be affected by the policy (or the size of the industry or business in which the intervention takes place) and the degree to which they are affected (what are the impacts and how important are they to people’s well being?).

²⁶ See here Gerovitch 2007

²⁷ It is worth noting here that Popper’s discussion seems to entail that his notion of critique is broad enough so as to include the voices of regular citizens and not just social planners.

The hypothesis may be defeated by a single falsifying instance - building the factory leads to a lowering of the average consumer package. But repeated failures of hypotheses at this same scale should lead us to question whether the project itself, conceived as linked to a particular scale, is only sustainable through the continual suppression of failures as isolated instances. In scientific inquiry, it must be some process like this that leads eventually to abandoning disciplines like astrology or alchemy as pseudo-sciences. Rather than taking failed astrological predictions as an impetus for a new interpretation of the heavens, a new prophecy, eventually, scientists abandoned the project of explaining individual and social human activity on the basis of stellar and planetary positions altogether. At some point, investigators must have had the sense that the project was being kept alive only by the constant *management* of critique through *ad hoc* maneuvers.

Ideas about the scale of social experimentation are, in turn for Popper closely related to what one might call a rationalist liberal institutionalism - or the idea that institutional forms of authority must be justified and where they cannot be justified ought to be abolished (CR p. 6 is suggestive in this regard: "rationalism has...always claimed the right of reason and of empirical science to criticize and reject any tradition, and any authority as being based on sheer unreason or prejudice or accident"). In *PH* Popper deploys these ideas regarding experimentalism and rationalist liberal institutionalism to argue that only democratic and liberal economic regimes actually deserve to claim scientific status for their institutions. This is further articulated in an account of the "institutional theory of scientific progress" at the end of *The Poverty of Historicism* (pp.152-159).

These considerations help to explain the three part institutional convergence mentioned in the introduction between science, state and liberal economy in Popper's articulation of the norms of science.

Popper treats the everyday experimentalism of private business owners in a liberal economy as exemplifying the appropriate scale for piece-meal experimentation. He gives the example of a grocer opening a new store as an especially conservative example, but goes on to include a monopolist changing prices, or an insurance company making policy changes as within the bounds of piecemeal

experiment (PH p.86). But he also goes on to suggest that it is not just experiment, but also critical thought that will be needed before these instances become cases of “a more scientifically minded technology” (ibid p.87). Crucially, in the process of trying to solve some practical problem, critical thought involves being able to learn from our mistakes.²⁸

He also argues that liberal democracies with their guarantee of free speech may be the only societies in which aspects of scientific discourse - publication, conferences, university activities - will not be suppressed to an extent that would limit scientific progress (ibid pp.154-155). (Though if what I have said above is right, democratic norms may be constitutive of the appropriate structure for inquiry).

Earlier I mentioned how Mirowski rejects timelines of social philosophy of science that include Popper on the grounds that Popper’s account was not adequately sensitive to actual scientific institutions. This is odd considering he reserves such a prominent place for Dewey who imagined a similar though only two-part synthesis between scientific and democratic institutions. Dewey’s synthesis is portrayed as going against the grain of the actual historical institutional structures in part because it is an ideal. For instance Mirowski writes: “Dewey concertedly and repeatedly blurred the definitions of “democracy” and “science” prevalent in his lifetime, so that he could conflate the two and provide a counterweight to the forces dragging science away from its liberal and liberationist potential. As he insisted, “democracy is not an alternative to other principles of associated life. It is the idea of community life itself. And from the obverse side, he saw himself ‘raising the question of what science can do in making a different sort of world and society...’” (Mirowski p.293). And “...science would cease to undermine liberal democracy and... the corporate sway over science would be progressively diminished *if and only if* we came to regard science and democracy as inseparable parts of the same communal activity; that is, a) the practice of democracy would come to resemble science at its best, which was procedurally non-dogmatic and experimental; and b) more science would be reorganized and conducted in the communal democratic interest” (ibid. p.294). Perhaps the Popperian synthesis, because it seems like a vindication of the status

²⁸ Popper distinguishes between two senses of experiment both of which are necessary for the experiment to count as scientific: (a) “an action whose outcome is uncertain” and (b) “a means of acquiring knowledge, by comparing the results obtained with the results expected” (PH p. 85)

quo within liberal economies doesn't come off as an ideal or revisionist account in the way that Dewey's does. Perhaps such vindications of institutions as they are have an additional burden to offer descriptions of actual institutions. But I hope to have shown in this chapter that within an international context, Popper's account is in fact revisionist and idealist.

I also think that the ideas regarding social experimentation presented above do not straightforwardly justify existing liberal economic institutions, perhaps contrary to a surface level reading of Popper. A non-dogmatic reading of Popper suggests an independent principle for judging the appropriate scale of social experimentation which many forms of modern capitalism probably fail. The evasion of social responsibility in globalizing production; the undermining of local democracy and community ownership and the rise of surveillance capitalism; and the gargantuan scale of operations for multinational corporations all gesture toward a picture of social engineering gone authoritarian. If reports on the ways in which capital has been radically unaccountable in the 21st century are to be taken seriously, this would be grounds for further processes of scaling down on a Popperian account.²⁹ Popper's own insistence that private enterprises perform experiments at an appropriate scale, albeit often without critical thought or the ability to recognize error, provides a blueprint for such critique. In chapter 5, I investigate whether existing liberal societies which supplement private industry with regulatory agencies which monitor the possible deleterious effects of private industry fulfill the function of introducing critical thought to liberal experiment.

The context of justification and representative risk

On my interpretation of Popper, inquiry is always open to pragmatic, social and contextual factors. Hypotheses are subject to electoral risk that is attuned to these contextual factors and this risk is felt by practitioners in the form of discontinuing support for researchers who promote the hypotheses (at least in their capacity as advocates of the disproved hypothesis, though also probably in the eventual withdrawal of support for criticism that signals the hypothesis' acceptance). One may wonder how this squares with

²⁹ Or at the very least denying the authority of their success claims.

Popper's commitment to a division between the context of justification and the context of discovery. The answer is almost assuredly: uneasily. I have chosen to weave together strains of Popper's thought that cut against other strains in which he seems to promote the decontextualization of hypotheses within inquiry. There is a way out of this bind that involves thinking of the stage of justification as a *process* of decontextualization rather than a space where contextual considerations are automatically off the table. On this view hypotheses must earn the right to become abstract, non-contextual, or general.

While Popper promotes a view of inquiry as open to external critique, one may wonder how well this translates into the aim of representing democratic interests in research that constitutes an alternative normative basis for scientific leadership. There are clearly some purely formal notions of openness (as the absence of explicit constraints) that will make it unlikely (where substantial positive conditions are necessary) that certain interests are represented. In order for inquiry to be considered "open," we might ask whether it is enough for scientists to listen to and attempt to model the social criticism they hear from "outside" of scientific institutions (for instance via media outlets), or whether as for instance standpoint theorists have argued, science will need to encourage the active participation of individuals who occupy affected social sites and identities in order to properly anticipate and incorporate their criticism. I have chosen the notion of electoral risk deliberately to indicate that while Popper thought science should be open in the former sense, I don't think there is much in his account to guarantee it should be open in the latter sense. Inquiry was for him still a kind of protected space much in the way that contexts of speech are protected within liberal institutions. But Popper's discussion of the role for hypotheses and theories in social experiment begins to blur the lines between speech and policy. In this context it no longer makes sense to make inquiry a low-stakes game. The stakes for inquiry must be made commensurate to the stakes of those affected by experimental interventions. Still there is a way in which electoral risk renders inquiry still comparably low-stakes. A scientist who runs a disastrous experiment which causes significant suffering risks merely the discontinuation of their research program. Stronger accounts of the appropriate risk for practitioners to face may be possible and more appropriate. Again, the possibility suggested by standpoint theory and the notion of electoral risk would make it so that practitioners (to the extent they are uninsulated by an academic or political position) stand to face many of the same risks that they are

responsible for studying (e.g. in virtue of the housing available to them, they will return to the same neighborhoods at night near polluting factories they are responsible for regulating). At the other extreme, practitioners could receive an equal share of the outcomes of experimental decisions - call it egalitarian risk. While at the conservative end of a spectrum of views which seek to represent democratic interests in inquiry, Popper's notion of inquiry and specifically of critique that is open to such pragmatic determinations of interest is still offered as a way of addressing the underlying call for social accountability.

Returning to the Other Faces of Falsifiability

With a clearer view of the political valence of falsifiability as electoral risk, we are in a better position to discuss how this aspect and account of the value differs from the other faces of falsifiability mentioned at the beginning. I will work in reverse order starting with Kitcher.

Where Kitcher imagines that scientific inquiry is structured according to a contest for discovery and the social prizes associated with it under conditions of uncertainty, his notion of risk hangs freely of any broader conception of social risk. The risk that inquirers face is cashed out as simply winning or failing to win a prize for being the first to discover. The support researchers receive in the form of grants, equipment or personnel in their quest for the truth are not part of the wager. They are simply the necessary conditions for performing the game until someone has won. While I do not deny that he captures a risk of inquiry, I do not think the risk he discusses will be sufficient to ensure accountability of the scientific community to the broader society. This was, at least for Popper, a key function of falsifiability.

The point can be leveraged into a critique of the notion of subjective probability more generally, which is often operationalized by asking what odds inquirers would be willing to take for a low-stakes bet (e.g. if you would bet a dollar on the occurrence of X for the possibility of winning \$2, your subjective probability for event X is 0.5). The probabilities employed by Kitcher to determine the relative likelihood of success

for different research avenues must be subjective probabilities operationalized by the researcher's willingness to devote time and resources to various research avenues in hopes of achieving a prize. However, if the stakes are too low, as they seem to be in Kitcher's discussion (where again not even the resources devoted to the research projects represented in the contest hang in the balance - inquirers simply have a positive incentive - the prize - to pursue truth) or are simply independent of the social consequences of scientific research, then subjective probabilities will be a poor guide to making scientific research accountable. As the foregoing discussion suggests, what is needed for accountability is making the risks of inquiry commensurate with the risks of social application.³⁰

Latour's account traces how what look like risky tests of Pasteur's anthrax vaccine are in fact careful extensions of techniques learned from a long process of trial and observation confined within the laboratory setting. To accomplish the extension Pasteur must also, in some ways, recreate aspects of the laboratory setting within the context of the vaccine's application at farms - e.g. through the use of sterile implements, controlling for other possible sources of disease, etc.. Where these extensions are lacking he has a ready excuse for ignoring alleged failures of the vaccine's use *in situ*. Latour's deflationary account of the "riskiness" of scientific conjectures makes it out to be a carefully managed production of a scientific elite who have the necessary means - a laboratory - of scientific production. Pasteur is able to win over popular opinion and public interest through a prophetic performance. Pasteur's ability to manage possible critique in a one-sided fashion ought to be troubling for a Popperian account. But I don't think an account of falsifiability which includes a social and political dimension is without tools for addressing the critique. An in depth analysis of the Pasteur case is beyond the scope of this chapter. But it is possible that Pasteur's intervention caused serious negative consequences *in situ* and that explaining these away as failures of extending laboratory conditions would constitute an inappropriate insulation from the risks of inquiry.

³⁰ Kitcher's later views on science and democracy (e.g. 2003, 2011) grapple more directly with the representation of democratic interests in inquiry, though they are proscriptive instead of retrospective - meaning the idea of accountability is built into an ideal for how science ought to proceed with little discussion of what happens with failures. They also introduce a third element - significance graphs - for mediating internal scientific and external public interests which give significantly more weight to scientific determinations of relevance and import. Finally they mediate public input with, again, the subjective probabilities of scientists regarding how likely any given research avenue is to succeed, which as mentioned may mask a further possible dimension of insularity or non-accountability.

Finally before discussing Sober and Forster, a brief interlude on Kuhn. Even Kuhn, who is often credited as laying out a framework for the discussion of theoretical values, wrote that the theoretical values may be "...an indispensable means of spreading the risk which the introduction or support of novelty always entails" (Kuhn 1977 p.332). Both Kuhn and Popper imagine that the process for allocating risk is intersubjective, i.e. that somehow the process of inquiry involves weighing the risks faced by different individuals through weighing the different theoretical values that each practitioner sees in the theoretical alternatives in dialogue. Where Popper and Kuhn disagree is on whether it is merely the risks to the inquirers that matter or also the risks to people in society more generally who are affected by scientific discourse. This disagreement is also central to their disagreement, as I see it, on whether science is a purely social (in the narrow sense) or a partially political activity - to draw on distinctions made in the last chapter.

The monological accounts lie on the other side of the spectrum as they omit entirely a discussion of the risks of inquiry and how they are to be shared among different individuals. Sober and Forster (1994) discuss how to balance the accuracy of a best-fit curve against a formal proxy for falsifiability - the number of adjustable parameters. They discuss Aikake's theorem which purports to deliver an algorithm for "specifying a precise rate of exchange between these two considerations" (p.11) (i.e. accuracy and falsifiability). Both the account of falsifiability and the implicit account of how theoretical values operate within inquiry are here given a purely formal, algorithmic interpretation. The implicit account goes both against Kuhn who claims the weighing of different values is an intersubjective and non-algorithmic affair and Popper who wants in some sense to extend the intersubjective process of weighing risk to a broader community. (Extending the metaphor of a rate of exchange, both Kuhn and Popper think this rate needs to be determined intersubjectively). While Sober and Forster do not claim that their account or its interpretation of falsifiability are exhaustive, I still think it is important to note the function of the theoretical values as they allocate risk, lest somebody think that in applying Aikake's theorem to their data analyses they have somehow accounted for this function. Applying this theorem and its applied rate of exchange is insufficient for ensuring accountability to other practitioners or society more broadly.

Surveying this list of alternative accounts of falsifiability, I hope to have established that not all accounts give due weight to the social and political dimension of the theoretical value of falsifiability. Indeed perhaps no account does. But rather than conducting an exhaustive review of the literature on Popper, this chapter has attempted to get a clearer view of the socio-political dimension of falsifiability offering the alternatives above as a potential contrast class of interpretations which omit or don't adequately capture this dimension.

Conclusion

To conclude, let us reconsider the case of corporate-funded, alternative publications to the scientific literature which conclude that climate change research is inconclusive in regards to whether fossil fuel emissions are a leading cause of global warming. One possible way of arguing these publications are non-scientific or deserve little public attention and credence is to say that insofar as they are sponsored by industrial capitalists who see any regulation of economic activity as an infringement upon individual property rights, and carried out by scientists with a record of doing research for hawkish Cold War interests, they have become politicized. Together with the premise that science that is influenced by political interests is bad science, this implies that these cases of climate denialism are unscientific. For reasons I hope to make clearer in the rest of the dissertation, I do not think this is a viable route of analysis. Specifically, I disagree that good science is apolitical. In this chapter I have offered an alternative analysis. The industry and military sponsored climate skeptics appeal to a sense of good science as having a critical, skeptical or even contrarian spirit. But when we look to the best philosophical account of that spirit, we find a scientific value with a distinctive socio-political cast. Far from justifying scientific critique as an individual right of speech or an individual property right, that account portrays critique as a means for ensuring social accountability, i.e. as a social responsibility. The socio-political dimension of falsifiability also in itself constitutes a challenge to the idea that good science is science

unaffected by social and political considerations and interests (though this of course touches on a broader shift in academic analyses away from social accounts of science upon which this analysis depends on the grounds that they cannot resolve certain crises of public legitimacy - e.g. the Science Wars (Douglas 2009)). Adding this dimension to our account of falsifiability also makes clear - in contrast to accounts like Mirowski's - that Popper offered an epistemology with a robust social dimension.

Interpreting the Popperian analysis of liberal economic institutions as the best concrete application of (social) science in part because they address problems of social accountability articulated as minimizing social risks, we have an account that treats a commitment to these institutions as contingent upon their ability (to be empirically determined) to accomplish this social goal. Far from a dogmatic commitment to a certain scale and style of economic organization, this implies that critique of actual liberal economic institutions is possible for a Popperian. We may wonder, empirically, whether liberal economies do a good job, or better job than others addressing this social criterion. The rest of the dissertation explores the central economic category of *infrastructures* with a critical eye turned toward this question. In particular I consider climate change science as an instance of science attempting to minimize the social risks posed by fossil fuel infrastructure, but as a part of a broader program of environmental monitoring which constitutes a kind of infrastructure - a data or monitoring infrastructure - unto itself.³¹ The turn toward the category of infrastructure makes clear the substantial political preconditions for a whole network of liberal economic enterprises. It also occasions what I consider a needed turn away from the focus on theoretical value so far expressed in the dissertation toward the underexplored ground upon which theories depend - data or the basic observation statements mentioned in the preceding discussion of falsifiability. Indeed it is a hallmark of some of the alternative accounts of falsifiability canvassed above that they bracket or blackbox the process by which researchers arrive at or measure data. But the production of authoritative data which are the potential *falsifiers* of theory is clearly as important as the provision of theories which may be falsified by the data. Nuanced social accounts of data production or measurement (Carnap, Chang, Collins, Gallison) reveal a whole other terrain for social analysis of science.

³¹ There is also a sense in which the component of the data infrastructure monitoring climate effects is a component of a potential *critical* fossil fuel infrastructure, that is, a fossil fuel infrastructure that incorporates critical thought.

In line with ideas expressed in this chapter, the turn toward data also reflects a conviction that focus upon theoretical value privileges a narrow group with editing access to high level generalizations. The focus back upon data is part of the necessary process of scaling down from a completely abstracted theoretical quantity. By looking toward context we are better able to tease out different, more accessible quantities from the original in part through their connection with social interests which help set the appropriate conditions for theoretical unity or difference. The theoretical quantity addressed in the chapter 5 is the *background concentration of atmospheric CO₂*, but before turning to an in-depth analysis of that quantity and the data infrastructure which produces it, I turn to a transitional discussion on the relevance of data, infrastructures and ideology critique of liberal political economy.

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Chapter 4: Data and Infrastructure

A Turn Toward Data, the Ground Upon which Theory Stands

In the next chapter I turn to an extended case study - developments in climate science in the mid to late 20th century - to illustrate some of the preceding points about theoretical value. But while the chapter includes (in the first main section) a discussion of the theoretical values, the discussion focuses primarily on the development of the measurement and data network for measurement of atmospheric variables upon which theoretical conclusions are based with particular focus on measurements of *background CO₂*. This shift toward data or basic observation statements which support theory can find motivation from a number of considerations and perspectives in the empiricist philosophy of science tradition. Yet while my analysis borrows substantially from this empiricist tool kit, it differs in its aims. In this section, I start with a discussion of the empiricist turn toward data. I then discuss where my analysis differs.

To motivate the turn toward data, we can start with an idea developed by Heather Douglas (2013) that not all of the traditional theoretical values are intrinsic properties of theories. Some like empirical adequacy are relational properties between theories and evidence. Judgments about a theory's ability or inability to explain the evidence depend upon prior judgments about which evidence stands in need of explanation, i.e. which data are authoritative. The introductory discussion of Popper in the last chapter noted a similar point. Judgments of theoretical falsifiability or falsification depend upon prior judgments about the truth of basic observation statements that are potential falsifiers. In scientific practice, admissible and authoritative observation statements are highly structured and regimented - not just any casual observation is admitted to the empirical record. This is so much so that as Kuhn (1962) notes it is

perhaps the norm to treat recalcitrant data points or even data sets as anomalous or as explainable by researcher error rather than as falsifications of theory.

Another way to motivate the turn is Helen Longino's idea (1990) that a contextual empiricism replaces a decontextualized with a contextualized version of the theoretical value of empirical adequacy. Part of what it means to contextualize empirical adequacy is to take into consideration the background assumptions that mediate relevance relations between empirical evidence and theory. The idea is that since claims about theoretical objects are made on the basis of directly observable phenomena, theoretical scientific claims (which are typically about entities that are not directly observable) will rely upon background assumptions (these were also called bridge assumptions by the positivists) of the form: observable state of affairs O is evidence of theoretical state of affairs T.³² As an example, a doctor's claim that a patient has measles is facilitated by various assumptions about what observable symptoms - a rash of red dots - are evidence of the presence of the measles virus. Someone operating with a different set of background assumptions - such as that a rash of red dots is a sign of possession by an evil spirit - will have a different diagnosis. Background assumptions may often carry politically or socially problematic presuppositions, but their status as background claims means they are also often taken for granted or less amenable to challenge. Contextual empiricism does not balk at identifying and reassessing the empirical merits of background assumptions, especially where social or political bias exists. Like diagnostic standards, measurement standards are a typical variety of background assumption. In the next chapter I discuss some of the political and social factors that motivate background assumptions about the siting, distribution and calibration standards for measurement stations in the meteorological network.

Third, the turn toward data for thinking about theoretical values can also be motivated by the idea that data are always substantially theoretically-laden, developed by Hanson (1958) and Kuhn (1962). To illustrate the point, imagine Tycho Brahe and Johannes Kepler staring at a planet moving in the night sky. Brahe (an Aristotelian or developer of a geo-centric model) sees a heavenly body orbiting the Earth on

³² Where Douglas notes that the value attaches to the relation between theory and observation, Longino identifies a class of relational claims - measurement standards - as important carriers of social and political bias.

the surface of a sphere at a fixed distance with various epicycles to account for the lack of linear motion. Kepler (a Copernican or helio-centric modeler) sees a planet which, along with the Earth, is orbiting the sun and whose relative motion is to be explained by thinking about their different, independent orbits (i.e. in three polar dimensions). Metaphysical assumptions about the aspect of the world to be measured are always necessary for getting empirical measurement off the ground. (In the foregoing example, Brahe must assume the planet travels on a sphere at a fixed distance from earth in order to measure its epicycles). Scientists must decide when, where and under what conditions they will measure a quantity of interest. Making such decisions amongst the infinite possibilities always requires making substantial initial assumptions about the thing to be measured. The next chapter discusses how David Keeling honed in on a method for measuring background CO₂ in part by making substantial theoretical assumptions along the way.

Finally, there is a substantial literature on measurement and standardization - Carnap (1969), Collins (1998), Cartwright (1999), Gallison (2003), Chang (2004), Tal (2016) - that gives these phenomena due weight as the central aspects of scientific practice they are while also often offering social and political contextualizations of the processes by which measurement standards become established. The analysis of the measurement network for climate variables would be at home amongst this literature. But coming on the heels of a discussion about theoretical values, the point of this shift is not merely to turn to another aspect of scientific practice, but to identify that aspect as a blindspot of accounts of values in scientific practice that are overly focussed on theory. Without measurement, theory has no ground to stand on.

While the following account makes use of some of these analytic tools from the empiricist tradition, it departs from the tendency of that tradition to recenter the problematic aim of logical closure, or the traditional idea that considerations of logic and empirical truth are sufficient to close off future debate. The first three accounts listed above take as one point of departure some failure of logical certainty - the identification of a gap between theory and observation that must be filled with one of a number of cross-cutting and differently-valent background assumptions; identification of an inductive gap between instances and generalizations (in Douglas 2009); or the discovery of a circularity to theoretical arguments.

A tendency of many such accounts is to suppose that something else “fills the gap” or provides the foundation for theoretical conclusions, even when that something is social or political; or they hold out hope that mathematical logic will be replaced by a qualitative logic (Douglas 2013). I maintain that recognizing political or social valences of theoretical values implies that they are best thought of as features of a different dimension of discourse than those highlighted by traditional accounts of validating empirical knowledge, i.e. one involving certain social and political conditions (e.g. non-coercion, inclusivity, equality). Where these social conditions, say of equality, are lacking we should anticipate problematic social valences to operative theoretical values.³³ Accounts like Longino’s which turn to social mechanisms for generating scientific knowledge provide a more appropriate focus on the social context of publication, debate, and justification, than do accounts which recenter the aim of logical closure. The account that follows is content to identify potential socio-political failures of the context of scientific debate and development which provides the foundation for theories of climate change.

These empiricist turns toward the data are thus often motivated by the desire to challenge the epistemic authority of science on its own terms. Indeed this way of attempting to motivate the present document’s shift toward a study of data would inherit this problematic subtext if all it offered were these reminders of the logical role that observation statements play in empirical argument. Where these logical challenges gain traction and cease in my view to be idle speculation is when logical alternatives can be linked with concrete social needs (not as Kitcher demands existing scientific alternatives). The value I see in Popper’s account is not in his analysis of scientific logic as having a *modus tollens* structure, but in his ability to link failures of this logical structure and *ad hoc* maneuvers to concrete cases of social and political injustice. Still, not too much emphasis should be placed upon failures of logic. Sometimes socio-epistemic injustice results from the standard, smooth operation of algorithmic reasoning as much as it does from its failures.³⁴

³³ See Pham for one such failure in the hierarchy of research teams.

³⁴ Indeed from the perspective offered by the framing of infrastructure discussed shortly, algorithmic reasoning is simply a technological or social-organizational process of computing and information infrastructure amenable to the same style of analysis as other infrastructure operations.

The turn toward data is also motivated by the sense that it is in the acquisition of data through experiment that science itself takes on a material, empirical dimension (in line with the last group of social empiricist thinkers mentioned above) and where we can begin to see its overlap and entanglement with objects of everyday concern that are often central subjects of social and political concern. The final chapter maintains a focus on *infrastructure* as naming a joint material and social structure which forms a subject of analysis that straddles both scientific and social domains of evaluation.³⁵ That analysis is guided by an idea from science studies that the data network developed in the second half of the 20th century for measurement, storage and dissemination of climate data can be thought of as an *infrastructure* (Edwards 2006). Infrastructures, as I will explain shortly, often facilitate a kind of blindness with regards to the material and social origins of the goods that they channel. Discussions that treat the main issue for scientific debate as interpretation of the data which are themselves seen as *given* (to borrow a phrase from Sellars) or beyond interpretation harbor a similar blindspot. (Or relatedly attempts to legitimate theory by reference to the transparency of data - e.g. it is open-source, available online, etc.). The next chapter will investigate some of the ways in which this framing is facilitated by the data network's infrastructuralization. A focus on infrastructure in climate science as both a facilitating precondition (data/information infrastructure) and an object of critique (fossil fuel infrastructure) also helps to provide a different model for the politicization of science. Under the Popperian view developed in the last chapter suggesting a unification of economic and scientific institutions, the direction of social inquiry just is, in part, the direction of economic development. Infrastructures as a network for providing the economic preconditions of public activity (including scientific inquiry) are also an object of substantial state concern and management. Thus, rather than a picture of unencumbered ideal inquirers who may (in a lapse of reason or in a carefully planned individual protocol) allow themselves to be swayed by their social or political beliefs, we find a picture of data and thus of theory that has become canalized and perhaps ossified as an object of state management in service of national interests. But these implications will not be clear until we have a better working understanding of the category of infrastructure and some of its relations with science. Accordingly, I turn to a discussion of infrastructure next.

³⁵ The idea of trading zones (e.g. Galison 2010) can also help us here, though rather than searching for currencies that could adjudicate between paradigms, we are searching for ontological categories that can allow for exchange between science and society.

Infrastructures and Ideology

In this section, I develop a view of infrastructures as technological, socio-political and ecological assemblages. As instances of technology, infrastructures represent cases of applied science. As technologies which provide the necessary preconditions for modern ways of life, they are often regarded as potent symbols of science's utility for society and concrete measures of scientific progress. But as assemblages whose functioning depends essentially upon social and political decisionmaking and operations activity, their operations and development are also objects of social and political choice and interests. This means that accounts of infrastructure which portray them as pure technological assemblages - an interlocking set of known and reliable means-ends relationships - overlook junctures of breakdown and suspension that form sites for social participation, maintenance and reconstruction.

To begin, infrastructures form potent modern symbols of science's success. In Shapin and Schaffer's account of the Royal Society in 17th century England, *Leviathan and the Air Pump* (1985), they discuss how Boyle and other experimentalists would perform experimental demonstrations for an audience of initiates seated in an amphitheater. Such demonstrations functioned to garner legitimacy for a theory such as Boyle's gas law by demonstrating how it explained the exhibited phenomena. Behind these performances of repeatable and predictable phenomena for an audience of peers, however, stood countless hours of tinkering with the apparatus, commissioning different glass-blowers to make the gas chamber, trying different coatings for the cork-stop to ensure the gas would not leak out, etc. and countless failures with each attempt.

In modern industrial societies, infrastructures and mass produced technologies play a similar legitimating role for science amongst a more general, public audience. A scene from the movie *Look Up* illustrates the point. In *Look Up* Leonardo DiCaprio is an astrophysicist whose colleague has discovered a comet that is hurtling toward earth. He has been thrust into the limelight as the defender of their scientific

conclusions, which (in a spoof on the phenomenon of climate change denial) the press, public and political leaders are all skeptical of. In one scene, DiCaprio has been sucked down a reddit rabbit-hole and responds to an online skeptic that his team's conclusions were arrived at using the scientific method, and that this is the same method that created the personal computer that the reddit skeptic is using to communicate their skepticism. DiCaprio thus suggests that the online skeptic has been caught in a performative contradiction - the very means by which they communicate presupposes the validity of the thing they question. His proposal illustrates the point that infrastructures are a powerful legitimating tool for science since they both furnish the necessary conditions for modern forms of living while also being instances of applied science.

While in many ways an apt parody of the media situation with climate change, *Look Up* misses one central aspect of the issue that is revealed by DiCaprio's reddit argument. The climate change hypothesis also constitutes a serious challenge to the smooth, reliable functioning of a core infrastructure - fossil fuel infrastructure - in a way that the hypothesis that an asteroid is about to strike earth does not. It suggests that there are enormous unintended and undesirable consequences of the operation of fossil fuel infrastructure many of which may undermine the symbols of its functionality and success: roads and buildings built with fossil fuel energy will be submerged under water and destroyed by storms; the convenience of goods mobilized by fossil fuels to reach stationary consumers will turn some of these same consumers into refugees, and so on. This means that climate scientists cannot perhaps quite so easily gesture toward the fact that climate denialists drive to work to catch them in a performative contradiction. The negative consequences of fossil fuel technology identified by climate science create a crack in the facade of scientific legitimacy for a general public primed to consider the practical benefits of technology evidence for the truth of scientific theory. And it creates a problem for any account of science that lists applications for public benefit as among the guiding values of scientific inquiry, especially those leaving substantial space for the public to determine for itself whether any given technology or intervention constitutes a public benefit. One of the central challenges for defenders of science within the public arena is to attempt to mend this crack or to find a new, more subtle rationale for the public to accept scientific conclusions in light of their imperfect connection with terms of technological success.

In the next chapter I attempt a more reflexive analysis of the relation between infrastructure and climate science - one that challenges the idea that infrastructures are pure technological assemblages whose smooth functioning constitute instances of applied science. In its place I employ an analysis of infrastructure as having substantial social, organizational, political and ecological dimensions. These dimensions are obscured by the understanding of infrastructure as a pure technological system - a view which serves to rationalize ignorance of its other dimensions and a view that is thus ideological according to a more inclusive analysis. I draw on literature in anthropology, sociology and history to establish this perspective. Though by no means exhaustive, I understand infrastructures as joint material and social assemblages having the following features:

Infrastructure codifies and regulates the flow of generalized goods. In Gupta's terms it "channels communication, travel and the transportation of goods" (Gupta 2018, p. 63). But conceiving of information or mobility as generalized goods or capacities, we may say that infrastructure is the assemblage responsible for the flow of generalized goods. These goods are generalized in that more specific resources, e.g. coal, natural gas, oil, are often interchangeable for the same intermediate use - driving some kind of combustion turbine - allowing them to go under the banner of "fossil fuel infrastructure", while fossil fuels are interchangeable with hydro-power or nuclear reactors for providing the same end use - electricity for appliances - allowing these to go under the still more general headings of electrical or energy infrastructure.

Infrastructure is a core object of state concern and a constitutive element of "national security" (Spice 2018). For Gupta it is "a biopolitical project addressing the health and welfare of the population" (Gupta 2018, *ibid.*). In connection with the idea that infrastructures provide generalized goods, they also satisfy needs or necessities of a population (thus there is a *need* for "energy" while a more specific need for, say, natural gas would not be as plausible). Infrastructure is always at least partially an object of public (vs. private) concern - minimally (when it is called "private") it involves land-grants (as with railroads), or zoning permissions (as with mineral rights). Far from mere permissions, these also include the

enforcement of particular uses and the threat or reality of force against those who challenge or oppose these uses. As we will see in the next chapter, we could perhaps add standardization of measures (e.g. provision of standard time) and coordination amongst users (e.g. traffic lights) to the list of minimal functions. But, less minimally, infrastructures can also be the object of direct state management and operation as in public utilities such as the Bonneville Power Authority, which administers and operates much of the electrical infrastructure for the Pacific Northwest (see Hughes 1993 for an international comparison of public utilities).

In connection with the idea that infrastructures provide generalized goods, they also satisfy needs or necessities of a population or goods that are considered preconditions for essential activity - e.g. mobility, communication, energy, food, water. Where there is a somewhat plausible "need" for "energy" a more specific need for, say, natural gas would not be as plausible. As preconditions, they are often invisible in their functioning - treated by their users as a pure means. They only become visible through rupture, failure or disruption (e.g. when the electricity goes out and a technician must be called in) (Star and Ruhleder 1996).

Connected with this last point, infrastructures are actually in need of constant maintenance, development and repair. Gupta proposes that we view infrastructures from this aspect of suspension - as in a perennial state of incompleteness, partial functioning and dependency. From this perspective infrastructures are already ruins whose functioning must be constantly maintained in both technical (repairing potholes) and ideological ways (having a grand-opening or re-opening; calling a project "complete"). This perspective of infrastructures as already in a state of suspension is a necessary counterbalance to the view codified in financial futures markets where the operation of new fossil fuel infrastructures for 50 or more years is already treated as a reasonable expectation of returns by investors to be protected by the state as an element of national security. As Gupta notes, the ideology of infrastructures includes expectations of their future operation - they are always conceived as having this future dimension.

Infrastructures have an organizational and financial social dimension (Hughes 1993, Shiva 1991). Alongside the notion that infrastructures have a substantial political aspect as a subject of state concern, there is substantial organizational variety and texture to different infrastructures. This can be seen in the different social and financial structures of public vs. private utilities (Hughes 1993). For instance the former may include elected representatives on a board and price controls, where the latter are profit-driven and lack these features. It can also be seen comparing local vs. globalist agricultural systems as in Shiva's analysis of the social structure of Indian agriculture before and after the technological interventions of the Green Revolution. *Who* operates the machinery; *who* has access to land, machinery, seeds and the possibility of subsistence agriculture; population density in rural vs. urban areas and so on vary dramatically under local vs. globalist models.

I maintain that together, the social and political dimensions of infrastructure help to locate it as social in the broad sense identified in chapter 2 or as socio-political. As preconditions for economic activity managed by states, infrastructure represents the public or political management of housekeeping or the provision of the necessary preconditions for public being. As modern techno-social assemblages, infrastructures disrupt and reorder the schema of private and political from ancient societies. The necessary preconditions for politics are no longer invisibilized merely through their confinement to the private, patriarchal household, but may, due to modern shipping infrastructure and the division of labor it makes possible, operate out of sight, even half-way around the world.³⁶ It is also likely that these technological assemblages have made political participation more accessible by replacing some human labor with automation.³⁷ The development of the social sphere with its division of labor for operating technological assemblages, is partially constituted by the development of infrastructures.

Finally, infrastructures have an ecological dimension (Shiva 1991, Tsing 2015). Infrastructures mediate many of our most substantive and impactful relations with land (I use the term "land" in the Leopoldian (1949) sense which is closer to that of an ecosystem than as the concept appears in, say, real estate). As

³⁶ Though just as often they are right under our noses but driven through on an elevated freeway or mediated by a consumer interface.

³⁷ There are other countervailing tendencies such as the creation of additional "needs" expected in modern societies, e.g. for free internet access.

mentioned above, climate science purports to identify a major ecological impact of fossil fuel infrastructure. But this (initially) surprising impact on land and ecosystems is just one amongst many much more mundane impacts, such as those involved at extraction sites, for instance where coal is strip-mined from mountains, or tar sands dredged out of forests.³⁸ These in turn lead to knock-on effects such as poisoning local watersheds, air and habitats through both spills and standard operation. As an abstract commodity, “energy” obscures these relations to land as much as it obscures the social arrangements of workers and those who occupy and depend upon the land implicated in globalist schemes of capitalist extraction.³⁹

These remarks complete the brief survey of some general features of infrastructure that inform the analysis of the next chapter. In summary, infrastructures channel generalized goods; they are objects of state concern and management that form a constituent feature of national interest; they provide necessary preconditions for public activity, including economic activity; as technological systems they are in a constant state of suspension - incompleteness, dependency and repair. This state of suspension makes clearer the social dimension involving operations personnel who ensure daily functioning as well as central decisionmakers. Finally, infrastructures mediate our most impactful relations with land, making them a potent ecological force. I maintain that these aspects of infrastructure - especially its connection with economic activity and the role of states in maintaining it - make it a paradigmatic socio-political category, referring to material assemblages of technology, people and ecosystems.

In closing I wish to consider another central relationship between climate science and infrastructure. Not only is climate change regarded as an issue *about* energy infrastructure, but there is a growing literature developing a suggestion originally made by Paul Edwards (2006) to think of the data network upon which climate science depends as itself having become infrastructuralized in the second half of the 20th century (see Miller 2001, Olsakova 2017, Leibler 2019,...). In the shift from the data network developed under the International Meteorological Organization (IMO) which was primarily organized by the voluntary and

³⁸ Tsing (2015) also reminds us of the creative and generative potential of industrial margins and wastelands - an antidote to a kind of capitalist-theistic pessimism.

³⁹ In terms of geographical reach, food systems may represent industrial societies' most widespread infrastructural land impact.

temporary association amongst scientists during the first half the 20th century (Miller 2001) to its organization and development under the UN as the World Meteorological Organization's (WMO) World Weather Watch (WWW),⁴⁰ the network underwent a shift from *voluntarist internationalism* to *infrastructural globalism* according to Edwards. Reflecting the dual material and social aspects of infrastructures, this shift occurred because (a) the network came to share a set of more permanent and purpose-specific material infrastructures - telecommunications lines, meteorological stations, measurement devices and data archives - and (b) the operations became increasingly embedded within state and international institutions and increasingly codified as a matter of scientific convention (the social dimension). These shifts regularized, and stabilized the production and dissemination of climate data through material and social-organizational channels. But they also reflected an increase or at least a shift in state involvement and interest in the data network. State involvement during the voluntarist internationalist period treated international scientific activity as a possible venue for international diplomacy among relatively autonomous citizens. Thus for instance, the US State Department's involvement in mediating the tours by intellectuals of the Soviet Union during the 20s and 30s mentioned in the last chapter or perhaps (though this can also be seen as an early part of the infrastructure period) its role in mediating the International Geophysical Year in the late 50s, discussed in the next chapter. In the latter period national governments took on a more direct role in scientific operations providing personnel, funding, equipment, and institutional auspices under government agencies. In the next chapter I discuss the ways in which this shift in state involvement complicates and ultimately undermines narratives that portray climate science as having *become* politicized in the media campaigns directed against scientists toward the end of the 20th century, for instance when the IPCC began its series of authoritative reports (Oreskes and Conway 2011, Weart 2003, 2022).⁴¹ As with the ideology of infrastructure more generally, such narratives share in a tendency to invisibilize the ambiguous and socially constructed aspects of the data network to present a history of the theory of climate change that takes the data as given. Following this literature, I propose to give a contextualized account of the development of the CO2 data network by keeping in view the history of both fossil fuel (and related energy) infrastructures and climate data infrastructure.

⁴⁰ In the next chapter I discuss the GAW which leverages much of the architecture of the weather network into an global atmospheric data network.

⁴¹ Though to be fair, these accounts often trace the politicization of climate science back to the Reagan 80s. Weart also discusses the earlier development of the measurement standards and theoretical foundations, but his historiographic tendency is to treat these developments as always progressive - building up the foundation bit by bit upon which modern science stands.

Some Critical Socio-political Junctures

The next chapter contains three main sections each of which focuses on a particular socio-political juncture of climate data and fossil fuel infrastructures. These junctures form key points of socio-political interest, intervention, resistance and opportunity. Since these junctures form possible subjects of analysis for any infrastructure, I close this chapter by giving a brief introduction of the three junctures in general - land jurisdictions; employment and training criteria for operators; and election mechanisms for central decision-makers and central policy topics. (For other infrastructural analyses that touch on some or all of these aspects see Hughes (1993), Reisner (1993), Yergin (2011) or Shafiee (2018)). With each juncture is paired a central ideological category which forms a key focus of the ideology critique in each subsection of the next chapter - wasteland and anarchy, technical transfer, and free competition. I expand on each of these categories below.⁴²

Start with land jurisdictions. Infrastructure in its materiality occupies space, or on the ground, land. In order to be built, infrastructures must gain the rights or permissions to occupy and use this space in the ways they do. This applies to extraction and production sites, as well as distribution channels and terminals. There is often, additionally, a need for the license to operate in symbolic “spaces” such as the *marketplace*, in *airspace* or in *virtual, online* space. These rights and permissions are often granted by governments which oversee, regulate and organize the use of such finite spaces in a more comprehensive way. They are also regularly contested by activists on the ground (for instance Jane Jacobs’ resistance to an interstate project going through Greenwich village (Swift 2011) or in the next chapter the Standing Rock Sioux’s resistance to the Keystone pipeline (Spice 2018, Whyte 2017)). In the next chapter I will be particularly interested in discussing land jurisdictions (less so symbolic spaces)

⁴² Calling them ideological implies a critical stance toward them and the implicit position that where these categories are wielded they often amount to rationalizations rather than justifications. The shift from normative philosophy of science (or science and values) to ideology critique marks a further departure from the preceding material. Such a shift will be necessary where actual democratic fora for the creation, identification and understanding of political values (where such fora would need to meet certain criteria - e.g. non-coercion, inclusivity) are lacking. In such circumstances, proposed justifications are allowed to stand in virtue of the silencing or exclusion of critique from the venues in which justifications are publicized. In such circumstances such justifications are better viewed as ideology, rhetoric or propaganda.

which articulate the constraints and conditions for occupation, use and ownership of land (since *land* jurisdictions form a paradigm case, and since the concept of “land” can have broad, ecological connotations (see above) I use this concept as a general description). The notion of a *jurisdiction* also suggests some entity (typically government at some scale - municipal, state, federal) which authors and enforces the rules and regulations for use. Where the US federal government invokes eminent domain to open the way for interstates and railroads by preempting citizen ownership in service of a pressing national need, zoning laws adjudicate at a more local level (county or municipal) whether land may be used for residential, commercial or industrial development. In the next chapter I discuss the land jurisdictions of national park and biosphere reserve, which operate to furnish the ground for authoritative scientific studies of nature. Indeed we sometimes hear that these spaces are being preserved *for* science or as a territory of science, indicating that science is entangled with the institutions that determine appropriate land use within these spaces.

The central ideological categories to bear in mind for the discussion of scientific land jurisdictions are those of *anarchy* and *wasteland*. Just as the declaration of eminent domain makes the evaluative judgment that the community need for infrastructural development outweighs the individual property owner’s sovereignty over their particular piece of land, these categories, deployed at the frontier of colonial settlement, rationalize acquisition and exclusive use through the implication that the form of land-use or tenure of prior denizens lacked organization or value. Ownership is then supposed to belong to those who create value through productive and organized use.

Contextual critiques of liberal political philosophers such as John Locke who invoke the concept of a “state of nature” in which land is waste and there is no political organization help illustrate the ideological role of wasteland and anarchy in colonial settlement. In liberal political theory, the discussion of how and under what conditions the state can be justified or legitimate often begins or uses as a backdrop the concept of a state of human existence without government, called the “state of nature.” By exploring what the state of nature already possesses by way of rights or liberties and what the state of nature lacks, we are supposed to gain a better understanding of what the government should preserve and what it will

have to remedy in order to justify its existence. In Locke's version of the state of nature, individuals have property rights (minimally of themselves, but including also of land through productive labor), but individuals are unable to enforce them without waging total war and thereby becoming embroiled in violence and instability, hence the need for a central authority to which parties of a conflict can appeal. The prior benefits of the state of nature also set the bar of minimal legitimacy - when a government fails to protect certain rights possessed in the state of nature, or its members judge it in some important respects no better than the state of nature, then they may legitimately seek to change or overthrow it. In the traditional story told by political philosophers, individuals choose to exit the state of nature, which grants them liberty but little protection, and enter into a governed society voluntarily through the device of a social contract.

In the *Racial Contract*, Charles Mills discusses the way that thinkers such as Locke use the concepts of a "state of nature" and the "social contract" as justifications for settler-colonial conquest, domination and racial oppression (under which conditions, Mills refers to the social contract as the *Racial Contract*).

"In the *Racial Contract*...[t]he role played by the "state of nature"...becomes radically different. In the white settler state, its role is not primarily to demarcate the [temporarily] prepolitical state of "all" men (who are really white men), but rather the permanently prepolitical state or, perhaps better, *nonpolitical* state (insofar as "pre-" suggests eventual internal movement toward) of nonwhite men. The establishment of society thus implies the denial that a society already existed; the creation of society *requires* the intervention of white men... White men who are [definitionally] already part of society encounter non-whites who are not, who are "savage" residents of a state of nature characterized in terms of wilderness, jungle, wasteland." (Mills, p.13)

There is evidence from a contextual reading of Locke (see Arneil 1992) that as an administrator to the Carolina colonies, he was concerned with providing a philosophical justification for the colonial government vis-a-vis the rights and claims of American Indians using concepts of anarchy and wasteland. America and Native Americans form Locke's main concrete case study of an existing state of nature with

phrases like “...in the beginning all the world was America” (para. 49) occurring throughout his *Second Treatise on Government*. Locke’s portrayal of the state of nature describes an early colonial America inhabited by anarchic, but generally peaceful and egalitarian tribes, who have a limited conception of private property treating most things as held in common, alongside settlers who are in an active process of acquiring private property through their labor (i.e. homesteading). One of the central activities of the federal government during this period was to protect settler claims against Indian theft or attack (Grandin 2019, Buerge 2021). According to Locke an individual comes to rightfully acquire property when they mix their productive labor with land that has no prior claim on it. The lack of a prior claim means no one else has yet mixed their labor with it for productive use, making the land “waste.” These contextual purposes of Locke’s are most explicit in passages such as the following:

“...I ask whether in the wild woods and uncultivated *waste of America*, left to nature, without any improvement, tillage, or husbandry, a thousand acres yield the needy and wretched inhabitants as many conveniences of life as ten acres of equally fertile land do in Devonshire, where they are cultivated” (para 37, italics mine).

Here Locke defines productive labor to include tillage, husbandry and cultivating the land - all aspects of European land settlement and agriculture. Dunbar-Ortiz (2014) shows how Locke’s conception of value-adding and property-right conferring labor are Eurocentric in overlooking indigenous practices of cultivating corn and seasonal burns to preserve grazing land for buffalo. Arneil (1992) notes that in many cases it was Native Americans who taught settlers how to successfully live off the land. Thus Locke’s justification of the core institution of the settler state - private property - also follows Mills’ pattern. It is juxtaposed against the uncivilized, the wasteful use (or lack of use) of land. Indigenous practices of land tenure and stewardship are made by definition uncivilized and in need of ordering and cultivation common to members of the settler state.

In the next chapter I suggest that the jurisdictions of national park and biosphere reserve, as they were deployed within development of climate data infrastructure to inaugurate an experimental era which

required the monitoring of background CO2 levels in unused and unoccupied spaces (i.e. national parks/biosphere reserves or more generally wilderness), resurrect and recenter colonialist ideological categories of anarchy and wasteland.

Turn next to training and employment criteria. In the context of the domestic economy these are most familiar as educational or experiential prerequisites for paid positions. These criteria have the concrete effect of determining *who* operates, manages or otherwise carries out the tasks required for standard functioning of infrastructure in exchange for access to its products. In other words, these criteria help determine the social composition of infrastructure operations. Within operations there are hierarchies of positions with which come more or less power over operations and personnel and greater or less pay. Historically these criteria have been the subject of resistance movements that view them as encoding or laundering various political identity criteria (race, gender, nationality, ethnicity, disability status, etc.). In the next chapter we will be discussing the international transfer of operations from colonial (i.e. foreign) personnel to local or national personnel. The central target of ideological critique there is the concept of *technical transfer* which refers to the process by which new national personnel replace colonial personnel. In this context timelines for technical transfer often encode considerations of race, ethnicity, nationality and class that are the subject of labor and anti-colonial protests (see Shafiee 2018).

The rate and composition of technical transfer purports to weigh certain values against one another - for instance, as I discuss in the next chapter, values of national sovereignty vs. compensation to original developers. But it can also be conceived of as a limit or feasibility constraint upon shifts in ownership and operation. The idea suggests that technical knowledge, often proprietary, needs to be exchanged, purchased or learned in order for a transfer of operations or ownership to be viable. Some of this feasibility constraint is ideological; some is actual. There is always a tendency for those in power to think that only they have the capacity (technical, cognitive, moral) to perform their work (at the extreme lie various racist fantasies about the failed nationalization of industries). But even where the constraint is actual we should be on the look-out for cases of genuine incapacity vs. cases of encoding. Often infrastructures are characterized by a shifting array of junctures (which goes under the banner of

“technological progress”), which render old technology and old manual labor obsolete. These shifts do not necessarily introduce a new principle of operation that is elusive or hard to grasp, but mask principled understandings through encoding, which renders old lexicons and associations nonviable and embeds current coordinates in a social division of labor among compartmentalized workers. In the next chapter I suggest how the actual rate of technical transfer was substantially determined by political and social influences from below and above. From below we have demands for better working conditions and opportunities and more local sovereignty over resources. From above, we have colonial and corporate officials determining a rate of transfer that will adequately cover the colonial enterprise’s “costs” or compensate them “fairly.” The actual pace and composition of technical transfer within fossil fuel and climate data infrastructure was determined as much by pure political struggles for these interests as by any technical desiderata.

The final socio-political juncture of infrastructure I analyze deals with how central, network-wide decisions are made and the mechanisms by which decision-makers are held accountable. Within liberal economies, such decisions would be made by individual owners or by a board of directors that is accountable to share-holders. Within socialized enterprises, central decision-making and accountability mechanisms would involve a broader, e.g. national, public, with public participation coming through state elections. Other models may separate stakeholder assessments from decision-making power altogether - e.g. technocratic or authoritarian models. Regardless of the model, I use the idea of *election mechanisms* to indicate that some legitimation narrative, if not direct decision-making power is necessary for justifying the power and authority given to central decision-makers vis-a-vis the public that these decisions affect. In the next chapter I discuss two aspects of the climate data network’s infrastructuralization that made an important impact on election mechanisms within the data network. First, federal funding for climate data through NSF took on additional constraints in response to environmental democratic demands. Second, state agencies such as NOAA and EPA sought to take over various scientific measurement programs and make them in-house monitoring activities. These two changes tied relatively autonomous scientific institutions to the electoral risk of democratic institutions. Though indirect, the ability of elected officials to

appoint agency chairs or to draft legislation that affects federal funding criteria provide new mechanisms of institutional accountability and risk.

In liberal economies, the process legitimating central decisions over “private” enterprise (both in *who* occupies central positions of power and *what* decision-making power comes with those positions) is supposed to be adjudicated by *free competition*. The idea, which has roots going back to Social Darwinism (and before this the trickier legacy of Darwin himself and whatever he may have borrowed from social thinkers like Thomas Malthus), suggests that the best way to allocate the social opportunity of running or owning substantial public utilities is to allow all interested contenders to compete economically against each other. The competition should be formally open (nobody is prohibited from entering) and determined by competitors’ ability to survive in the marketplace (i.e. by selling enough to cover costs of operation) where they compete for customers and investors. The outcome is justified as leadership by the best as judged by their ability to win or survive an appropriately structured competition. In the next chapter this framing from liberal political economy - along with its pitfalls - structures scientists’ understandings of the period prior to infrastructuralization (called “international voluntarism” by Miller) and structures Keeling’s understanding of the “contest” between him and NOAA for setting central network standards. Here I list just two well-worn problems with the idea: first the formal openness of the competition, which gives the “free” in free competition a purely negative interpretation often masks the substantial positive conditions necessary for participation in the contest; second, and in part due to these substantial positive conditions, focus on the bare fact of competition suggests a range of options that may mask how narrow the option set really is both in terms of *who* can be elected to lead and *what* makes it onto the ballot of the electoral contest. In the next chapter I will attempt to show how the economic and political interests, which sustain the contest for central network standard-setting, also substantially constrain the scientific contest rendering it a battle amongst politically conservative options.

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Chapter 5: Critical infrastructures: Fossil Fuels and Environmental Monitoring

Introduction

In “Fighting Invasive Infrastructures,” Anne Spice follows the resistance of the Unist’ot’en to the Canadian government’s building of the Northern Gateway pipeline through their traditional territory in Canadian British Columbia. The government is pushing through the pipeline despite the insistence by the Unist’ot’en that they are doing so on unceded, sovereign land. While the government insists on grounds that the pipeline is “critical” infrastructure. Critical infrastructure in this sense is explained as follows:

“Critical infrastructure refers to the processes, systems, facilities, technologies, networks, assets and services essential to the health, safety, security or economic well-being of Canadians and the effective functioning of government... Disruptions of critical infrastructure could result in catastrophic loss of life, adverse economic effects and significant harm to public confidence” (quoted in Spice 2018).

As with pipeline protests at the Standing Rock Sioux reservation in the US, the issue as Kyle Whyte explains (2017) is not so much resistance to climate change as ongoing resistance to settler colonialism which for the Sioux involves a number of historical dispossessions and engineering projects that have altered their relationship to the land. Both struggles conceptualize the main environmental import of their resistance as the protection of water which Freda Huson describes as the Unist’ot’en critical infrastructure (Spice 2018) since it facilitates travel, provides drinking water, and allows salmon, a main food source, to spawn. This focus on resisting settler colonialism and only secondarily or tertiarily climate change is sometimes hard for white environmentalist allies to grasp leading to tensions at protest encampments

such as Standing Rock between allies and Native leaders who see certain ally contributions as departing from the movement's central message (see Sullivan-Clarke 2020).

In chapter 3, I discussed Popper's vision of how critical science gets applied to social experiments. The fact that this structure can be codified in norms of scientific practice suggests the possibility of a critical infrastructure in another sense - infrastructure that is set up to detect and avert social risks.⁴³ The current chapter follows Edwards (2006, 2010) in investigating the interconnected assemblage of measurement machines, data archives, transmission lines and trained personnel involved in monitoring climate change as a global infrastructure and asks whether this infrastructure is critical in the sense that Popper imagines or in the sense that Spice critiques. Though the two are seemingly disparate conceptions and the comparison may appear as a kind of pun or equivocation of terms, I believe they reflect different sides of the same spectrum (of openness) of risk management. On one end lies the conception of the US or Canadian state which insists on the mitigation of risks for a privileged class only (i.e. full members of the settler colonial state) often with this mitigation coming at the expense of imposing even more severe risk on an excluded other than is avoided by the mitigation measures. On the other end lies a conception that is genuinely democratic or open and seeks to mitigate all serious social risks. A monitoring infrastructure such as that involved in climate change would be genuinely critical if it functioned in the latter sense to facilitate the uptake and resolution of environmental problems which are genuine articulations of democratic concern. It would function as critical in the former conservative sense if it sought to preserve the quality of living of a "privileged" group as against the weightier claims of those amongst an oppressed group.

This chapter provides an in-depth analysis of a particular aspect of climate change science - the measurement of background CO₂ from the late 50s through the 90s - and the infrastructure that made this project possible. The narrative focuses on the research of a central protagonist of the network's development, David Keeling. I bring to bear various of the tools developed in the previous three chapters to level what I consider a sympathetic but severe critique of the network's development - one designed to

⁴³ An alternative way to think about the data network is as a supplement - providing the necessary critical dimension for Popper - to fossil fuel infrastructure. I will return to this possible framing in the conclusion.

find a more appropriate balance amongst the different voices and interests involved in environmental and anti-colonial movements against fossil fuel infrastructures.

The idea developed in chapters 2 and 3 that science has socio-political aims or values that are broadly emancipatory and democratic may be taken to suggest that science will be best able to accomplish its aims in partnership with a democratic or even a liberal democratic state. According to this idea the infrastructuralization of scientific data through its imbrication with self-proclaimed liberal democratic states may look like a good thing. Both chapter 4 and the present chapter depart from this suggestion in taking a critical stance toward both the claims of the participating states to be genuinely democratic as well as with some of the ideals of liberalism. Regarding the former I look to various activist movements for critiques of self-proclaimed democratic states, showing how these activist concerns are both relevant to and yet fail to receive uptake within the scientific environmental monitoring regime developed under the UN. Regarding the latter I give an ideological interpretation of liberal concepts such as freedom of speech and competition arguing the cases they are supposed to defend are not so normatively valuable.

In what follows each section discusses one of the socio-political junctures mentioned in the last chapter - land jurisdictions, training and employment criteria and election mechanisms. The first section discusses how scientific jurisdictions such as national park and biosphere reserve codified problematic background assumptions in the theoretical discussion surrounding the measurement of background CO₂ in the 50s and 60s. In particular I show how theoretical values, including surface-level versions of falsifiability can come laden with colonialist political valence. In the next section, I discuss the process of technical transfer as it relates to both fossil fuel and climate monitoring infrastructure, showing how key infrastructural positions and junctures are designed and staffed. The point here will be to show how social and political forces substantially shape the character of technical transfer. Finally I close with a discussion of funding for the project with a focus on a key transitional moment from a civilian team of researchers performing calibrations of the measurement network to NOAA, a US state agency, performing the calibrations. Surrounding this transition is a contest between the two parties for the central calibration role. I look at the interests that sustained this contest arguing that they preclude more

democratic possibilities. Throughout, I try to contextualize the development of this technical infrastructure within the context of broader political, social and environmental concerns surrounding fossil fuel infrastructure.

Natural Background: Scientific Jurisdictions

This first section investigates the connections between measurement conditions for background CO₂ and land jurisdictions which substantially articulate and materially create some of these necessary conditions. Comparisons with the theories that the CO₂ record was devised to corroborate or falsify reveal a common background of normative assumptions I describe under the banner of *scientific jurisdictions* in which scientists are imbricated with legal and political bodies defining and regulating use of land and handling of artifacts which make problematic evaluative assumptions about prior stewardship, use and historical record. To the extent that both theoretical projects of prediction and experimental projects of corroboration share this set of assumptions, I argue, they will lack a necessary dimension of risk for genuine democratic legitimacy.

Hawaii didn't become a US state until 1956, 11 years after the end of the second world war and as anti-colonial struggles for independence began in countries like India, Algeria and Vietnam. As the post-colonial world took advantage of Europe's destruction and focus on reconstruction, the United States continued the assimilation of new territories and protection of economic access zones in a process of settler colonial annexation and imperialism that spanned back in various waves to the first American colonies. It took over the mantle for countries like France in Vietnam, while it exercised economic colonialism from the oil fields of Venezuela to the sugar plantations of Cuba, as well as in the Middle East and Southeast Asia.

In 1957 the federal government began the construction of Mauna Loa weather station high on top of Hawaii's tallest volcano of the same name. The construction occurred in preparation for the International

Geophysical Year, an international scientific gathering to share data and research design across countries in the interest of charting a common globe (“triangulating the whole earth” quoted in Collis and Dodds 2008 p. 558). The IGY benefitted from coordination efforts by the World Meteorological Organization (WMO) which saw its infrastructural capacities grow during the event (Miller notes it “... created global telecommunications network linking meteorological research stations and forecast centers.” (p.169)) The civilian or voluntarist IMO had earlier in the decade - 1951 - become the WMO now under UN auspices (Edwards 2006). Characteristic of Cold War geopolitical tensions and under pressure from the US, WMO adopted the UN definition of sovereign statehood - a nation “fully responsible for the conduct of its international relations” Edwards 2006, p. 256) - as a requirement for participation. This blocked colonial territories, divided Germany and the People’s Republic of China from official membership (though it sought subsequently to include China as an “observer” (Edwards, 2006)). Far from initiating a purely collaborative effort, participants such as the US also bombed North-Vietnamese weather stations which were supported by Polish and ultimately Soviet scientific societies (Olsakova 2017, Edwards 2006, Daily Report: Daily Report, Foreign Radio Broadcasts, 1965).

IGY precipitated discussions about territorial rights to then unsettled spaces such as Antarctica and outer space. A dozen countries built research stations on Antarctica in preparation for the event. The USSR launched the first earth orbiting satellite - Sputnik - in 1957 and six other satellites launched during the following year (Collis and Dodds, 2008). The US declared Antarctic *terra nullius* (or “no-man’s land”) rejecting all prior territorial claims. The 1959 Antarctic Treaty would preserve the Antarctic as a “continent for science”. A treaty in 1967 called for the same jurisdiction to apply in outer space.

David Keeling was at Mauna Loa station in 1957 beginning his measurements of CO₂ using a specialized measurement instrument, an infrared gas analyzer that tested for the quantity of CO₂ in a chamber by measuring the absorption of infrared radiation by the gas sample, and a methodology for picking out background levels of CO₂ amidst noisy data honed over several years of research on the US West Coast. He went under auspices of Scripps Oceanic Institute (which would soon be connected with UCSD) whose director at the time was Roger Revelle. Revelle had in the previous year (1956) written a paper in which

he and Hans Suess overturned the idea that the ocean would absorb any atmospheric CO₂ that human industrial activity could produce. Instead, Revelle and Suess calculated the ocean would take up only a fraction of the extra carbon put into the atmosphere by industrial emissions (Weart, Revelle & Suess). The likely result would be rising atmospheric concentrations of CO₂, a trend that some, (like Callendar 1938), claimed had been going on for the last half century. But the CO₂ record to date was ambiguous (see Fonselius et. al. 1956 and Beck 2007) and there had been countervailing theoretical considerations that could explain alternative readings of the data, one of which - the absorption of any anthropogenic CO₂ by the oceans - Revelle and Suess now thought they had set aside. Revelle wrote with seeming confidence that humanity had now begun a “large-scale geophysical experiment” (p. 19 Revelle and Suess 1957 - for a fuller version of the quote, see section 3) and ended the paper with a call to make accurate atmospheric CO₂ measurements to see the rise as part of the upcoming IGY. Revelle and Harry Wexler (the head of the US Weather Bureau) would bring on Keeling to carry out the measurements.

Keeling thought he had the means to make more accurate measurements of background CO₂ and that the problem with the historical record was the non-uniformity of measurement and sampling methods. What made the concentrations that Keeling measured *background* measurements? (this terminology persists - see Tans, Thoning 2008, though it finds variants in UNEP 1981). The data that Keeling inherited and that prior theorists (such as Callender) had consulted showed CO₂ levels varying erratically (see Fonselius et. al. 1956 or Beck 2007).⁴⁴ Why think there was a steady level? Keeling’s reasoning involved a statistical move - the *mean* had to be measured; a substantive physical assumption - that measurements had to be made in a natural place, i.e. one free of industrial and urban pollution; and a sampling assumption about the appropriate time of day to measure. Equipped with these assumptions and a protocol for collecting and measuring the gas, Keeling claimed to falsify the measurements made by a Swedish team who had developed some of the more erratic data. This same team (see Fonselius et. al. 1956) had theoretically hoped to track individual air parcels using measurements of CO₂ (for more on the political implications of an air parcel program see the final section of this chapter). The second

⁴⁴That some of the variation could come from missing a diurnal pattern (Keeling 1998), i.e. that measurements needed to be taken at a particular time each day.

two assumptions came from Keeling's experiences traveling to remote wilderness areas such as Big Sur, CA or the Olympic National Park in Washington, where Keeling discovered a steady level of roughly 310 ppm in the afternoon (concentrations at other times varied more) (Keeling 1998). Keeling made the expeditions to these areas partly on a theoretical hunch - that variability measured in the city came from human-made or artifactual emissions that would not be present in wilderness (and also seemingly out of a personal interest in spending time outdoors (Keeling 1998)). Mauna Loa station was located within yet another wilderness high above any sources of "man-made" CO₂ emissions that Henry Wexler in consultation with Keeling suggested for a course of CO₂ measurements during IGY. Mauna Loa received a national park designation (in 1961) and UNEP biosphere preserve designation (in 1980) adding legal protections that codify the park's exclusive uses, which exclude industrial activity, limit road access and tourist usage and preclude long term human habitation and harvesting including by native or original residents and take other protective measures to ensure the park remains in a natural, undeveloped or wilderness state. Often the language of a "territory for science" is used to describe national park and biosphere reserve land jurisdictions.

The episode illustrates several of the theoretical virtues at play. I will consider just three - accuracy, external consistency and falsifiability. For each of these traditional values I identify a settler colonial political valence that is revealed by contextualizing the values.

Keeling's measurements were billed as the most accurate measurements to date of background CO₂. While the bearer of the virtue of accuracy (see Keeling p.38) is, in this case, the data itself and not any particular theory about the data, accuracy of a theory or hypothesis such as the Callendar, Plass, Revelle, Suess hypothesis that CO₂ was rising, requires determining a data-set to be accurate *to* (see Douglas 2013). In the case of the CO₂ data record, there were competing records and interpretations of the available data which treated some as more reliable than others and thus as a more appropriate basis for determinations of theoretical accuracy. There was thus disagreement about the most accurate data set (for some of the difficulties besetting accuracy determinations at the level of data themselves see Collins 1998, Chang 2004 and Edwards 2010). Keeling's claim to accuracy and with it the claim of the rising

CO2 hypothesis to theoretical accuracy depended crucially upon having measurement locations in pristine natural areas untouched by local, artifactual sources of CO2. The case of Mauna Loa is typical of the kind of space - national park or wilderness - political jurisdictions that outlaw human industrial activity and other forms of habitation.⁴⁵

Fabienne Bayet (1994) argues that the designation of national parks belongs alongside those of wasteland, and *terra nullius* as political categorizations that erase indigenous presence both in the historical record and in ongoing uses. (For an explanation of the category of *wasteland* see the previous chapter). To mention just one way in which original land-tenure practices could seriously affect climatic CO2, man-made forest fires were commonly used by indigenous people throughout the mainland US to maintain meadows for cultivation of crops and habitat for preferred game (see Dunbar-Ortiz 2014). Many early US environmental efforts sought to quell this practice (see Powell 1879) and forest fires are today billed as an unequivocal evil consequence of global warming. Forest fires are large sources of CO2, but because of the smoke they give off, they also increase local albedo, so their net effect upon greenhouse warming is ambiguous. Background assumptions made by Keeling make national parks - really a settler colonial legal jurisdiction - necessary for the study and regulation of industrial CO2 emissions.^{46 47}

Revelle and Suess in their 1957 paper offer an example of external consistency in their use of carbon dating techniques from the disciplines of archaeology and anthropology. Carbon dating methods track the content of C14 as a proxy for age. C14 is an unstable isotope of carbon created by a reaction between sunlight and CO2 in the atmosphere, which decays with time in captured carbon. Concentrations of stable C12, especially those otherwise known to have been recently absorbed from the atmosphere are attributable to fossil fuel emissions, where the carbon has been trapped indefinitely and all of the isotope decayed out. Revelle and Suess studied how different carbon samples mixed with ocean water - in particular the rate at which ocean water would absorb atmospheric carbon. The discovery that this rate

⁴⁵ In later network building conventions there are also vegetation offset limitations specified for background stations on the model of the Mauna Loa station which is surrounded by rock.

⁴⁶ it is worth reminding ourselves how immense a system the carbon cycle is - all life emits CO2, so any land-management practice which significantly changes the composition of local ecosystems - deforestation, extinction of species, etc. - stands to have an effect on the carbon cycle.

⁴⁷ See how these criteria for acceptable measurement locations come up in later infrastructure building conventions and reports in UNEP 1980 and NOAA 1974.

was much slower than anticipated implied an expected build-up in the atmosphere. Their results thus depended upon the validity of carbon dating techniques which Suess had adapted from his prior research dating human and artifactual remains at University of Chicago (Weart 2022). In using the two domains - carbon dating for biological and chemical processes and anthropology and archaeology - to help stabilize and improve the accuracy of the carbon dating measurements in one another, Revelle and Suess's findings exemplified *external consistency*. Their findings depended upon the reliability of a measurement technique that was originally relied upon, calibrated and deployed in another theoretical domain.

Carbon dating of ancestral remains during this era was often pursued without consent or collaboration of the tribes whose ancestors' remains they were. Carbon dating is often a first step (even under NAGPRA which improved repatriation practices in the 1990s) to determine whether the remains are of Native Americans and should be returned to the relevant tribe. Some of the problematic aspects of the practice came to light during the Kennewick controversy, for an overview of the controversy within archaeology see Zimmerman 1998 and Wiley 2015. One of the key problematic assumptions of these practices is that they continued a historical lineage of erasing the presence of existing tribal communities with their own history and self-conception. Just how to integrate tribal oral tradition with the science of archaeology is a nuanced issue (see Wiley 2015) but from a perspective that seeks to give tribal oral tradition substantial *epistemic* weight, the use of carbon dating to initially classify remains is a controversial practice which often gives science more jurisdiction than is seen as appropriate - automatically favoring an external, settler colonial historical timeline to settle questions about ownership of ancestral remains.

Note also, in connection with the ideological category of anarchy, explained in the previous chapter, Revelle's idea that humanity had now (in 1957) begun a large-scale geophysical experiment. What sanctioned this view? The processes that created the emissions were the same as had been around for decades if not the better part of a century, had there been a sudden spike in their usage? Or was it that only now, with the measurement capacities developed during the preceding decade that the international scientific community had at its disposal the capacity to monitor the effects of anthropogenic CO₂ emissions? The answer is perhaps some combination of these two factors. But whatever the exact

meaning of Revelle's words, the possible implication that this was the first of its kind (the beginning of what geologists would call the *anthropocene*) lingers - was the implication that original stewards lacked either the power or the knowledge to perform such a geophysical experiment? Both of these implications - that prior stewards lack a history of their own and that they lack the power or knowledge to conduct ecosystem-wide experiments - are variants on the theme of *anarchy* and *wasteland* or the idea that what came before the modern settler state was an a-political condition of statelessness leaving the land unimproved, unused or even unaffected.

Finally, the theoretical duo of Revelle and Suess and the empirical team led by Keeling play off one another in what looks like a potential instance of falsifiability. Revelle and Suess seem to make a prediction - with the largest countervailing theoretical consideration disproven - they predict that an accurate measurement of CO₂ would reveal a steadily rising baseline concentration in the coming years. This prediction could have turned out wrong. In fact, it is what Keeling measured and calculations of the rate of increase by Revelle and Suess are apparently not far off the mark (Weart 2022, Keeling 1998). But is this a genuine case of falsifiability? How likely was a negative result by Keeling's IGY team? How risky was the prediction? While there is surely some risk in the scenario, the discovery of a consistent political valence among the background assumptions of both Keeling and Revelle seems to imply that certain assumptions are not at risk in the critical test. They are part of the *background*. This common set of background evaluative and empirical assumptions is characterized by the repurposing of earlier negative colonial categories of anarchy and wasteland and, positively, by scientific definitions of land use and natural history. Moreover, Revelle and Keeling shared an institutional home at Scripps and many of the same funding sources. For Scripps, the original endowment by the Scripps family and funds from the newly created UCSD; for Revelle's research there was substantial military patronship from ONR (see Rainger 2000); as we see below NSF and DOE provided substantial funds later on. As the director of Scripps, Revelle also had significant power to provide Keeling with funds directly (Weart). Keeling and Revelle are also from the same country, unlike Popper's paradigm case of Einstein and Eddington. For these reasons it is possible to see Keeling's test as not totally *independent* in the sense of being supported by independent interests. Accounts of falsifiability that include the socio-political dimension of

risk discussed in chapter 3, i.e. that require more than the logical possibility of falsification, may disagree with this apparent application. I will circle back to an assessment of funding and institutional sources of support in the final section.

Technical Transfer: Membership Criteria

In this second section, I investigate the process of technical transfer within international development of fossil fuel and climate data infrastructure. Technical transfer is used both as an evaluative category purporting to weigh on the one hand, values such as sovereignty over national resources and worker and environmental conditions, and on the other hand, considerations of cost and compensation for original foreign developers as well as international economic coordination and diplomacy. Technical transfer also names a descriptive empirical category which can be studied through the historical record of membership within the network of operators and technicians. The following discussion investigates how the dynamics of technical transfer within the climate data network were substantially shaped by social and political forces and values from below and above, drawing parallels with dynamics in fossil fuels.

Iran granted Britain its first oil concession in 1905 - a lease on the use of Iranian land for oil extraction to last 50 years. The deal was that Britain would pay part of the profit in compensation for the lease.⁴⁸ Subsequent Iranian political leadership in the 30s and 50s challenged the legitimacy of the original lease on the grounds that various of the concession terms (e.g. profit sharing arrangements) weren't being adequately followed and that the old deal didn't adequately respect Iran's sovereignty over its resources (for instance by employing local workers). To sweeten the deal in subsequent renegotiations, Anglo-Iranian Oil Company, AIOC (which would eventually become British Petroleum a subsidiary of the British government), promised that the subsequent lease would involve technical training of Iranian staff so that eventually Iran would be self-sufficient in running its own oil industry. Katayoun Shafiee (2018) uncovers company documents detailing a quantitative formula the company used to project the rate of

⁴⁸ The lease was first granted to a British national William D'Arcy, whose holdings then became AIOC and later BP. The company became more and more embedded with the British state as the concession became more contested internationally (Shafiee 2018).

technical transfer conceived in terms of the number of Iranian employees vs. the number of British employees over time. The formula built upon prior racialized stratification among skilled and unskilled workers which were reflected in a segregated workplace, living quarters and wages - a system which Iranian workers had organized against in the past, leading to a number of strikes and new political demands for better working conditions and more skilled jobs for domestic employees. The pace of technical transfer and observance of other stipulations was, to the Iranian public and Iranian prime minister Mohammed Mossadegh, still not sufficient, and Iran “nationalized” (really sought not to renew the British lease) British holdings in the early 50s. At this point the US helped to orchestrate a coup, overthrowing Mossadegh and installing the Shah who was willing to continue BP’s “lease.” (Yergin 2011, Shafiee 2018)

Meanwhile similar patterns of resistance and backlash to racially segregated labor in fossil fuel and fossil fuel intensive industry played out in the US. In Birmingham’s steel industry black workers encountered a ceiling to upward mobility keeping them in unskilled and more dangerous work, though often they would need to train the “skilled” workers whose jobs they were allegedly unqualified to hold (see Norell 1986). Conditions in the coal mines of Kentucky were perhaps more egalitarian with black and white miners working side-by-side, but still highly segregated with each returning home at night to completely separate company-designed neighborhoods with their own shops and schools (Brown 2018). The civil rights movement which began in the 50s to challenge these forms of segregation and made a powerful impact upon the legal landscape of segregation by mid to late 60s, also brought on reactions from industry that negated some of the promised progress. Companies shuttered factories and moved overseas or to states that were more lax on enforcement of new antidiscrimination (and also environmental) laws. Mining became increasingly mechanized, which in turn offered a pretext for dispensing of selective jobs (often those workers who were kept “unskilled” members of the workforce) (Brown 2018).

The pattern revealed in these histories was the beginning of a process of economic decolonization and anti-apartheid movements in oil and fossil fuels perhaps beginning with Cardenas’ nationalization of the Mexican oil industry in 1938 (or Russian Revolution 1917) and sweeping throughout countries in Latin

America and the Middle East as the century progressed. The wave saw countries and workers asking for better cuts of the profits and a pathway to self-sufficiency and upward mobility through technical transfer (see Yergin 2011). The oil crises of the 70s initiated by OPEC perhaps marked a turning point. OPEC was an intergovernmental organization of oil exporting countries that included most of the previous US and European economic colonies who had won varying degrees of control over their oil resources since the beginning of the century's scramble for international sources of oil and which sought to coordinate their national economic policies on the export of oil, including agreeing upon a fixed price for the group at which oil would be sold on the global market. In response to US interference in the politics of the Middle East, OPEC raised oil prices in 1973 (for the US's role in Israel-Palestine conflict) and 1979 (for its role, harboring the Shah, in Iranian revolution) which led to long gas lines, high prices and gas rations in the US. A beleaguered president Carter gave a speech asking Americans to reconsider their consumptive lifestyles in the late 70s just before he lost his reelection bid to Ronald Reagan.

Meanwhile in the 50s through 70s, WMO was also figuring out its own program of technical transfer as part of a process of standardizing a global international data infrastructure (Edwards 2006). The program sought to initiate newly sovereign (often through decolonization) participant states into the network through a program of training experts, agreeing on observational codes and calibrating instruments. Many states didn't already belong to the network because they were not technically equipped or were under previous colonial occupation (i.e. treated as non-sovereign by Europe and its allies), and as noted above some like China were excluded in virtue of even more tenuous political criteria for membership. Several analysts have noted the connections between natural scientific programs such as the WMOs and US or UN economic systems of record-keeping and loans.⁴⁹ To address the assimilation of new members WMO devised a network of regional training centers (RTCs) and standard curriculum to be taught at these training centers (see Van Mieghem 1963). The lengthy list of suggested curriculum reads like the standard atmospheric physics undergraduate and graduate course requirements from a US or European

⁴⁹ Leibler has shown how these same sovereignty criteria required the establishment of a network of economic data collection that linked economies to the UN-affiliated financial institutions - World Bank and IMO. In fact, as Miller notes, programs of technical transfer (first under US then UN) were primarily about economic coordination, or integrating economic systems and infrastructures. See also Miller p. 188, 189. Miller notes that technical transfer programs began with a program by Truman in the US for reconstruction of Germany and Japan along technical and political lines p. 177 (although this program also has precedents from the 20s and 30s see p.183 also material from Popper chapter). Miller notes (p.204) how the US explicitly sought to navigate the difference between participation in scientific conventions and political recognition.

university in list form - vector calculus, differential equations, various branches of physics (acoustics, optics, thermodynamics) as well as more specialized topics in meteorology and climatology...(Van Mieghem 1963). Trained personnel were broken into four classes corresponding to differences in duties and status (from directors of national meteorological or climate services to measurement technicians) and correlated with the extent of the education they received. In a separate document auditing the educational capacities of Africa, Van Mieghem remarks favorably of the Algerian university that it has a program for sending graduate students to French universities and that Cairo has graduate training capacities on site. (The mentioned universities are also, by and large, ex-colonial institutions, or modeled on imperialist and missionary efforts in colonial countries).

The efforts began with the 1951 decolonization of Libya (a prior Italian colony) which required replacing the predominantly Italian, staff with Libyan personnel (Edwards 2006, Miller 2001). Libya's independence came after Allied occupation during WWII, following a treaty between the Allies and Italy which required Italy to relinquish its colonial holdings (including Libya) and it took the form of installing a monarchy. So the independence movement was a far cry from more grassroots independence movements that took place in the following years (e.g. India, Algeria, Vietnam). In general there is a parallel between efforts by British petroleum executives to control the rate and composition of technical transfer in the Iranian oil industry (which we might describe, in a parallel with the discussion in CH 3, as a "monological" formula purporting to weigh values of sovereignty and compensation to developers)

in the 1930s and the WMO's highly codified program of meteorological transfer which found its first application as part of a transfer of political power carefully orchestrated by European and US imperial powers. Such carefully designed protocols represent a power imbalance in favor of colonial powers which allows for precise, top-down recipes for transfer in a way that is not possible with more resistant and independent political powers (e.g. no similar formula is allowed to adjudicate the rate of transfer after the end of the British concession in the 50s in Iran, when Britain had less of an international case to stand on). In connection with the story regarding CO₂, there are no participant nations with stations in the Global Atmospheric Watch network for greenhouse gasses from the Middle East (unless one includes

Egypt).⁵⁰ Thus participation in the WMO's quasi-regulatory monitoring network is more tightly constrained with broader participation in WMO meteorological activities taking on the cast of cultural diplomacy.

The program of training new meteorologists, was accompanied by a plan to integrate and standardize the global measurement network including standard machinery. Technical transfer involved substantial in-kind transfers of standard equipment to new participants (Edwards 2006). Key to the story about CO₂, measurement devices would be calibrated to the standard equipment at Mauna Loa, then under operation by Keeling's team from Scripps, through dissemination of reference gasses also prepared by Keeling's team and specific precision requirements set by WMO - earlier requirements were within 0.1 ppm though subsequently these would be relaxed to within 0.1%. Keeling's role producing reference gasses was officially codified by WMO in 1975 and would continue until NOAA took over calibration activities in 1995. In the interim, Keeling lost UNEP funding during the 80s (Keeling 1998) and UNEP experienced cuts to funding that rendered its programs in some cases unable to carry out their mandates (Miller 2001). It was partly due to pressure from NOAA for Keeling to turn over the key infrastructural activity of providing calibration samples that led to the initial formalization of this role, which he also performed prior to 1975. The transfer of these duties to NOAA, a US government agency under the Department of Commerce will be discussed in the next section.

Abstract determinations of calibration standards and criteria for technical transfer have the concrete social result of deciding *who* will be able to participate in the network, which, for reasons that will become clear shortly, determines *who* occupies a platform of observation and authoritative reporting within the monitoring network.⁵¹ And as historical events have unfolded, this, in turn, amounts to a decision regarding *who* will participate in determinations of the facts which form the basis for international environmental negotiations on fossil fuel development.

⁵⁰ Note also the only OPEC nation that is represented in the GAW CO₂ network is Algeria. See Japan-hosted World Data Centre for Greenhouse Gases: <https://gaw.kishou.go.jp/>

⁵¹ A comment made by officials at NSF who sought to discontinue funding for the Keeling program makes the connection between calibration and membership clear, though it will need to wait until the next section for contextualization. When Keeling sought funding from various sponsors in spite of messages that NOAA was to take over routine monitoring of CO₂, NSF threatened to find "calibrated" (Keeling p. 58) reviewers of the project, should Keeling be unwise enough to apply again.

In Miller's narrative of the WMO, the organization began to phase down its technical transfer activities during the 80s when national contributions (during the Reagan era in the US) for the program began to wane. This was accompanied by a concerted effort on the part of the US and Japan to develop satellite capabilities for measuring CO₂ which culminated in the mid 90s with the launch of the first satellite designed for the specific purpose of measuring CO₂. An internal, scientific motivation for developing satellite capabilities would have been the increased coverage, flexibility and mobility of sampling routines offered by mobile measurement devices. The move toward satellites was also a natural extension of the US satellite program which had long been a joint program of the Weather Bureau and other interested agencies, e.g. the Department of Defense (at least since IGY and the launch of TIROS in 1960). Satellites, long studied and understood as *dual-use* or *multi-use* technologies (White 2006. Grevsmuhl 2014) useful for both weather data and surveillance, offer a potent contrast case for thinking about the political and social dimensions of their counterpart - land stations. Land stations required national personnel who would often be empowered by their home governments to make measurements and observations transcending the criteria of the international network.⁵² To set up a network of land stations required substantial international diplomacy and coordination and training of personnel on the part of WMO. In these ways and many others, land stations, their personnel and equipment were also multi-use and multiply-valent platforms and tools of observation within the monitoring network. Observers are themselves multiply valent depending upon the institutions they belong to and the roles they occupy - scientists will tend to report aberrations of interest to new scientific hypotheses or discoveries while state officials might be more likely to report unusual observations of concern for national security (this issue will appear again in the next section in Keeling's efforts to show that his research was *basic* or relevant to unsettled theoretical questions, which he responded to by finding ever-more subtle implications of the data, for instance, as tracking El Nino events).⁵³

The turn toward satellite measurements calibrated to a few land stations (the first satellite used for the purpose of measuring CO₂ was launched in 1987) inaugurated a phase of development in the network

⁵² In this connection, the range of variables of interest monitored by GAW may reflect the interests of those who occupied the platforms upon which the environmental issue associated with that variable was discovered and initially studied.

⁵³ Noticing aberrations of interest and having the resources and means of publication to problematize or develop interest amongst a broader community articulates a concrete positive dimension of the "freedom" of inquiry.

that further restricted participation to a few countries with the technological capabilities (though just how central satellite measurements are depends on who you ask - for NOAA's Global Monitoring Laboratory (GML) they are the primary source of data (see Miller); for the UN's GAW, they are still supplementary to the network of land stations). The turn towards satellites also offers a striking illustration of the militarization of technological advancement - an instance of the space-race which in many ways accelerated during the Reagan years - in which vying participants competed for technological supremacy with the consequence that participation in environmental monitoring and the production of policy-relevant information became ever more exclusive. The turn toward satellites thus illustrates a role for technological advancement found elsewhere in infrastructural apartheid struggles - competition within a shifting landscape of technological junctures and requisite technical skills acts to supplant resistant workforces who are rendered unskilled and unequipped by technological change. From the perspective of this workforce, the "progress" or "advancement" of technological change primarily describes the benefits that accrue to the owners of new labor-saving machinery. For those who lose their line of work because of technological change, it is much harder to see how the change constitutes an advance or progress. Internal narratives which see satellites as a pure improvement because of the possible flexibility of sampling routines, must be juxtaposed with the loss of epistemic and political access and participation represented by a distributed array of land-stations operated by national personnel.

For the purposes of this section it is important to note the parallel attempts to set up a program of technical transfer as a project of building and maintaining legitimacy and access within a decolonizing Third World for development of both fossil fuel and meteorological infrastructure. At the same time as efforts in technical transfer took place, technological advancement pushed the bar for participation in the global data network further out of reach - the end result of which was an environmental monitoring network led by the usual group of imperialist powers but on technical and technological grounds as the bearers of the appropriate knowledge-producing institutions and technologies relevant for such a policy task.

Central Calibration: The End of a Research Program and the Beginning of Monitoring

This final section tracks the interrelations between three contests or competitions: first between the Keeling/Scripps scientific team and NOAA for the central CO₂ calibration role in the WMO data network; second a technological, environmental and political contest between nuclear and fossil fuel energy infrastructures that drove the scientific research project; and finally between different versions of the measurement network as envisioned by the UN vs. the US and led by Keeling vs. NOAA.

In 1970, David Keeling began to receive troubling messages from the main sponsor of the CO₂ program, NSF. The NSF sought to phase out funds for Keeling's program of long-range background CO₂ measurements on the grounds that it no longer constituted a case of *basic* science, but was rather *routine monitoring*. Behind the scenes was an institutional shuffle by Congress and the Nixon administration that sought to create new environmental regulatory and monitoring capacities. The shuffle would culminate in passage of new legislation (e.g. Clean Air and Water Acts) to be enforced by newly created agencies like EPA, and key to the CO₂ story, NOAA. Among NOAA's key prerogatives were administering an environmental monitoring network and taking over NSF duties for overseeing federal research funds on oceanography and atmospheric science at Sea Grant Colleges (which included UCSD Scripps). The new doctrine implemented at NSF with regard to these disciplines sought to distinguish between basic science (a traditional category going back to Vannevar Bush 1945, whose counterpart was "applied" research) and "routine monitoring" (a new category of applied research involving repetitive measurements of environmental variables).

The transition from a designation of basic to non-basic implied that, from NSF's perspective, no new substantial scientific hypothesis was at stake in the CO₂ measurements by the 70s. The original hypothesis of interest, that CO₂ was on the rise, would, by extension, seem to have been settled. Now the program simply kept track of the rise as an input to scientific research into its effects. To illustrate the idea, at one point, NSF required that Keeling produce two new discoveries per year on the basis of his continuing CO₂ measurements, an arrangement that Keeling thought absurd, but which emphasizes the

relevant aspect of “basic” science as understood by NSF - that it should be relevant to assessing hypotheses that are presently uncertain or unknown.

To understand the import of the NOAA's entrance into the CO2 monitoring effort requires a bit of institutional history. The creation of environmental regulatory agencies, EPA and NOAA, is often given a purely domestic origin story. According to this story scientific observers such as Rachel Carson sounded the alarm in the early 60s on harmful components of commercial herbicides and pesticides colloquially called “dioxins” - an abbreviation of their scientific name. Carson compiled information from national studies conducted by various government monitoring agencies. Together with events like Earth Day in 1970, organized partially in response to oil spills off the coast of California, domestic histories identify the need for new government agencies better equipped to regulate industry. In such histories the US government appears progressive and responsive to democratic demands, with the new agencies - EPA and NOAA - embodying these advanced democratic values. But parallel, international histories during this same era cast these developments in a different light - as revisionist strategies of containment against a global backlash from the US's role in maintaining colonial and imperial regimes.⁵⁴

At the same time that Carson was performing scientific detective work to discern the indirect consequences of using herbicides and pesticides for commercial purposes adjacent to residential and other land-uses, many of these same compounds (manufactured by the same corporations) were being wielded as weapons of war against Vietnamese citizens, whose homes, villages, agricultural fields and forests were doused by aerial spray of the substances. While foreign observers identified the use of defoliants such as Agent Orange as violations of the 1925 Geneva Protocol banning the use of chemical

⁵⁴ On the background behind NOAAs citing in Commerce vs. Interior: Wally Hickel, beleaguered by protest against his opening of Prudhoe Bay to oil exploration as governor of Alaska, now with another domestic oil spill to contend with in Santa Barbara, was then Nixon's Secretary of Interior. He sought to ban domestic application of harmful pesticides (*that* was an environmental issue, he was willing to champion). But Nixon seeing Hickel's view as sharing a cause with anti-Vietnam protesters (though Hickel likely rationalized it as part of his role protecting fisheries) decided both to house some of Interior's fisheries duties under NOAA which would then be under Commerce and to site pesticide regulations under an independent agency, EPA. The significance of the feud is easily lost: as a part of Interior, NOAA would have been responsible for conducting business in the national interest, broadly construed with allowance for future generations (for instance in saving a wild fish catch like salmon from overfishing - this is consequently one reason why the Obama administration sought to move NOAA back to interior); as a part of Commerce, NOAAs activity would be oriented toward the furtherance of US domestic business interests. the significance for climate change monitoring is that NOAA took over weather bureau activities, which is the institutional site for the US participation in the IPCC - a policy *advising* organization - and also the institutional site of the central calibration role, and the maintenance of a quasi-independent monitoring network - Global Monitoring Laboratory - GML - that is also (as of 1995) the standard-setter for the WMO network.

weapons in warfare, the US senate (as a result of lobbying in part from chemical industries) voted back in the 20s not to ratify the treaty and the US sought further to deny that the substances it manufactured and used were chemical weapons (since they weren't *direct* agents of attack against enemy combatants, but rather *indirect* attacks on their food, water and shelter) (Neilands 1970). Protest of the US's tactics came in part from US scientific organizations AAAS and NAS which sought to make independent studies of the impacts of the various substances used by the US on the ground in Vietnam in the late 60s (denying that domestic studies were sufficient for the purpose). Critics urged Nixon to finally ratify the Geneva Protocol, which didn't happen until 1975 under Ford, when the US officially withdrew from Vietnam. Even then, it ratified the Protocol selectively. The US sought to identify a further category of warfare - environmental warfare - which would be banned by international treaty in 1977 after the US and USSR were first able to negotiate a joint agreement. This agreement would prohibit many of the tactics the US pioneered in Vietnam (including cloud-seeding or intentional weather modification, and selective compounds designed to attack enemy combatants indirectly for instance by ruining their food and water supplies).

Internationally, growing concern over environmental pollution was reflected in the UN's first convention on the environment in Stockholm in 1972 where it established its program on the environment UNEP. Together with WMO the program sought to establish a global environmental monitoring network to study pollution (the Background Air Pollution Monitoring Program - BAPMoN - the precursor to UNEP's Global Atmospheric Watch network established in 1989 or GAW). This network would build upon the existing WMO network to include a number of different kinds of land stations. Some, like Mauna Loa, were tailored to measure *background* or *global* pollution (for these, citing and technological criteria were the most stringent); while others were located nearer urban centers and were designed to measure more *local* variables. De Koning, and Kohler (1978) note that the variety of stations and station-locations was designed to study a range of scales of environmental pollution ranging from local to regional to global. Some cases of pollution like acid rain are primarily regional and have thus historically been negotiated bilaterally between countries sharing a border, while others like anthropogenic CO₂ are considered a global issue and thus require global negotiations.

In this light, it is interesting to rethink the implications of the earlier Swedish efforts to measure the concentrations of CO₂ in specific, transitory parcels of air mentioned above, which would render CO₂ negotiations more like acid rain - a regional issue to be negotiated between a few neighboring countries at a time.⁵⁵ (For example a map in Fonselius et. al. shows a front of high CO₂ air emanating from St. Petersburg, Russia and traveling across the Baltic Sea toward Sweden). Keeling's measurement technique purported to falsify the Swedish team's efforts to "tag air masses" (Keeling p.41) finding constant values for Swedish samples for which Swedish measurement techniques produced different values. Given the metaphysical assumption that there was a stable background concentration of CO₂, transitory, regional values which tracked concentrations emanating from a source became a kind of applied work that was of lesser interest to both scientists and governments (transitory values would be identified by their *difference* with background values). Indeed it is not just funding cuts that explain the underdevelopment of the UNEP regional network, but the competition among both scientists such as Keeling and national agencies for the centralized activity of measuring and monitoring background pollution levels. At various stages NSF and NOAA attempted to divert the Keeling project toward helping develop the network of regional stations (both initially in the 70s (see p. 51-52) and later in the 90s (p.62)). Keeling notes that a colleague studying aerosols came under similar pressure from EPA to transfer their study to urban instead of natural aerosols (p.51 fn.4). Both Keeling and his colleague demurred seeing in the measurement of regional concentrations a less reputable, less scientifically interesting line of research. Keeling sought funds from other agencies to continue his standard-setting background measurements, while his colleague found funding overseas at a Swedish university to continue the study of natural aerosols. Measurement of regional values would be more fundamental, and the measurement network more distributed under the assumption that CO₂ concentrations are essentially transient rather than there being a stable, equilibrium background level. But under the assumption of stable background values, which also renders a highly centralized measurement network, measurement of regional values was applied and the attempt to shuffle the Keeling program appeared as an attempt to supplant a more fundamental scientific program.

⁵⁵ Gardiner 2011 discusses the normative implications for responsibility claims of the *well-mixed* background concentrations of CO₂.

The decision for both scientific and government personnel to focus exclusively on monitoring background or well-mixed CO₂ thus centered the importance of this line of inquiry and network arrangements for its development at the expense of the network envisioned by UNEP that would expand upon existing monitoring capacities to measure a broader array of environmental variables and at a diversity of scales. This centralization corresponds to a theoretical assumption that background, well-mixed CO₂ is the primary causal driver of climate change. And the fact that this concentration is well-mixed makes finer-grained attributions of responsibility impossible, effectively driving the decision to consider CO₂ emissions a global issue in need of global regulation.

Later in the 70s as NSF phased out his funding, Keeling was able to find support through another newly created entity, the Department of Energy, set up to address the nation's "energy crisis" in the 70s. DOE sought to meet the nation's "energy needs" amid an increasingly complex landscape of environmental, geopolitical (recall OPEC's price raises during the 70s) and technological considerations. In DOE's framing of the public problem, technology is front and center: which technology - nuclear or fossil fuels - would provide the growing demand for energy? (Note how the assumption that energy needs must or will be met is backgrounded). This framing was not new, but was also deployed by Revelle and Suess to motivate the research question of their 1957 paper:

"During the next few decades the rate of combustion of fossil fuels will continue to increase, *if the fuel and power requirements of our world-wide industrial civilization continue to rise exponentially, and if these needs are met only to a limited degree by development of atomic power.* Estimates by the UN (1955) indicate that during the first decade of the 21st century fossil fuel combustion could produce an amount of carbon dioxide equal to 20% of that now in the atmosphere (Table 1). This is probably two orders of magnitude greater than the usual rate of carbon dioxide production from volcanoes, which on the average must be equal to the rate at which silicates are weathered to carbonate (Table 2). Thus human beings are now carrying out a large scale geophysical experiment of a kind that could not have happened in the past nor be reproduced in the future... This experiment, *if adequately documented,* may yield a far-reaching insight into the process determining weather and climate. It therefore becomes of prime importance to

attempt to determine the way in which carbon dioxide is partitioned between the atmosphere, the oceans, the biosphere and the lithosphere.” (Revelle and Suess, 1957, pp. 19,20; italics mine).

For Revelle, who spent years monitoring the effects of nuclear testing in the oceans before he thought to address questions of the ocean’s role in the carbon cycle, the choice between nuclear and fossil fuel energy was a natural one in that both were technologies with a potentially broad (not contained to a specific nation) environmental impact on the atmosphere and oceans. For DOE, the framing also captured a difficult geo-political dilemma: fossil fuel development at least for the US and Europe was increasingly fraught with demands for national control by foreign suppliers exemplified by OPEC’s role in the oil crises of the 1970s, while development of nuclear energy brought with it some of the key capacities needed for development of nuclear weapons, a substantial security risk. Alternatives such as hydro-power may not have made the list because their environmental impacts were often constrained to a single country, developing additional capacity within the US was seen as having diminishing returns and their use did not trigger quite as many - though Mexico with the Colorado River or Canada with the Columbia would probably beg to differ - international concerns.

Thus, behind the continued support of the Keeling team’s parallel program, stood a technological interest in whether nuclear or fossil fuel energy was the most appropriate means of satisfying national energy needs.

Despite DOE’s support for Keeling, NOAA also regularly sought to block other key channels of support from DOE and WMO/UNEP on the grounds that Keeling’s work was redundant with its own. At one point during the 70s (according to accounts like Keeling and Weart’s) it seems as if the programs are set against each other as budgetary rivals by the “lead” agency on CO₂ monitoring, DOE. Among the key duties that NOAA sought to wrest from the Keeling team was the calibration of CO₂ measuring devices and the provision of reference gasses among the WMO network (a role that in some ways, as an intergovernmental form of organization, culminated the push for institutionalization that originally motivated the IMO (an instance of scientific internationalism) to become the UNs, WMO) Keeling’s work

was codified as standard-bearing in 1975 by WMO and provided with funding for these activities by UNEP. Though the dissonance between Keeling and NOAA would lead to a loss of UNEP funds in the early 80s.⁵⁶ NOAA would eventually succeed in taking over these duties in 1995 after adopting several of Keeling's methodological recommendations.

Though NSF sought to designate Keeling's program as redundant and non-basic, Keeling fought the designation tooth and nail by attempting to show at every turn that NOAAs own measurement and calibration methods were inferior on scientific grounds. For instance, Keeling objected to NOAAs attempt to use spectrographic rather than manometric methods on the grounds that the former were less reliable as a calibration standard. He also fought against what he perceived as relaxed standards of precision for admission into the global network presented by NOAA (0.1% vs. 0.1ppm as mentioned above). Thus while NOAA sought to portray the scientific program of CO2 measurements as a dead issue, Keeling sought to portray his and NOAAs activities as live scientific rivals, differing upon standard scientific grounds of appraisal - e.g. precision and accuracy.⁵⁷ When NOAA finally adopted a manometric standard in 1995, Keeling relented (in his paper he offers no further complaint, seemingly having exhausted the scientific grounds for continuing to insist on supplying the reference standards himself). Standard setting activities were shortly thereafter transferred to NOAA.

While NOAA may have been established in part by a democratic ground swell around environmental issues in the 60s and early 70s, it was also an attempt by the US to control access and participation within a global monitoring network that it could use to monitor other countries and could be used by other countries to monitor its own activities, some of which, such as those discussed in Iran and Vietnam, had already brought on an international outcry. The 70s as mentioned above also saw attempts by the UN under WMO and UNEP to increase its own monitoring network of which Keeling was a key part.

⁵⁶ At one point NOAA set up its own conference with WMO to talk about CO2 calibration declining to invite Keeling (Keeling).

⁵⁷ Keeling also demonstrates an awareness that the competition may be to a degree manufactured at one point remarking: "I was now in the uncomfortable position that my program's survival, paradoxically, depended on a continued disagreement between our data and NOAAs" (p.61).

The technological issue between nuclear and fossil fuel energy infrastructures would come to a head geopolitically in 1986 with the Chernobyl meltdown in Ukraine. Together with Gorbachev's *perestroika* (or "opening up") policies for the USSR, the incident led to a wave of environmental movements in Soviet states like Ukraine. During the late 80s and early 90s national elections were allowed for the first time to consider environmental issues with most of the political platforms taking a stance on energy development and the environment (Ukraine also rallied against the construction of a giant new dam that would provide hydropower).⁵⁸ With respect to nuclear energy, then, it looked as if one of the Cold War superpowers was willing to admit the failure of an experiment in central control and to delegate operation of the energy infrastructure and assessment of its impacts to a more local, i.e. national scale.

With regard to the other side of the Cold War conflict and fossil fuel infrastructure, the trend was the opposite. In two respects there was massive institutional scaling up: The 90s saw the largest mergers of US and European fossil fuel corporations since the first Roosevelt broke up Standard Oil nearly a century prior (Yergin 2011). And the European Union was formed in 1993 partially on the grounds that there was a need to regulate the indirect environmental effects of international commerce (exemplified by the outbreak of Mad Cow disease in the British beef industry) (Beck 1992).

As regards the legitimacy of these upscalings, the evidence is hardly favorable. To take just one case from the early 90s, in Nigeria (also a member of OPEC) the Ogoni people protested environmental conditions - poisoning of water and land by spills - surrounding oil extraction by Exxon (US) and Shell (Netherlands). The outcome of the protests provides a chilling counterpart to the newly independent Eastern European states. In a political trial several of the leaders of the non-violent protest movement including its spokesman Ken Saro-Wiwa were sentenced to death and executed by a government which sought to protect its relations with the foreign oil companies (Westra 1998). Thus while some communist experiments in the centralization of nuclear energy were scaled down - delegated to more local authorities - experiments in liberal fossil fuel infrastructures seemed to scale up with corporate mergers and new economic unions while remaining openly hostile to considerations of local environmental issues.

⁵⁸ This context partially explains the Bush (Sr.) presidency's decision to portray itself as the "environmental" administration, and to avidly support the creation of the IPCC and UNFCCC. See Dellapenna 2010.

What lessons can we harvest from the entanglement of these three contests - between Keeling and NOAA for the central calibration role; between different energy technologies; and between different versions of the monitoring network represented by UNEPs GAW and NOAAs GML?

There is a sense in which the competition between Keeling and NOAA is tied to the competition between fossil fuels and nuclear in that this is a key framing employed by DOE and Revelle to give science-led climate change research relevance. But a closer look at these two contests would reveal that not much seems to be at stake in the former that could affect the latter - the differences in measurement results between Keeling and NOAA could hardly amount to radically different understandings of the environmental impact of fossil fuels (though who is to say how the measurement record would have looked without parallel programs). There are other perhaps more valuable aspects of the contest, such as some degree of independence between the two parties that would ensure a degree of accountability in their research or monitoring activities. The quality of the data is ensured by orchestrating a contest, or competition between the two research teams. The result of the contest, in part, determines who performs the central standard-setting activity, which in turn determines who will belong to the network. But the contest between US scientific institutions and US scientific government agencies for this highly centralized network standard-setting activity also looks like a win-win situation for the US and other technologically advanced countries (e.g. Japan) and institutions (tech companies, universities, government agencies) and a loss for the more democratic iterations of the monitoring network which could have represented a broader public. An investigation into the interests that sustain this contest reveals the backgrounding of substantive and democratically controversial claims.

This section explains how development of the environmental monitoring infrastructure could have taken a turn toward considering a broader set of issues some of which may have implicated fossil fuel infrastructure at a more local level (where more precise attributions of responsibility would also be possible). Instead of turning to space-based measurement, the US might have supported an increasingly rich array of land stations equipped to monitor other sources of environmental pollution at other scales.

Some such program (or at least a program taking a step in this direction) already existed as an aspiration of UNEP. Instead NOAA's GML focussed on the development of a satellite based network calibrated to a few land stations (including Mauna Loa) owned and operated by US state agencies. With regards to energy technologies - hydro, nuclear, fossil fuels - there are also paths untaken. Instead of delegating power to more local authorities with respect to all of these options, the pattern of the 90s seems to have been to pronounce nuclear the loser of a contest that fossil fuels allegedly won.

Conclusion

In the period between 1980 and 2008, the US seemed more or less to double down on its strategy of ensuring cheap oil through military and political machinations in the Middle East. Reagan provided arms and other support to both sides of the Iran-Iraq war (which weakened OPEC since these are two of its member countries with the largest reserves); Bush sent troops to Kuwait to thwart Sadaam Hussein's effort to annex the country for Iraq; Clinton fired the first salvos of a "war against terror" in Eastern Africa; and Bush Jr. invaded Iraq and Afghanistan. By 2008, US net imports of oil were at an all time high - accounting for roughly 60% of total domestic consumption (EIA). Then, partially as a reply to the volatility this created in the US economy (price rises were, again, a factor in compounding the recession), partially as a first step toward the aims of climate change mitigation, domestic production began a steady rise (until today (2021) when domestic production more or less matches domestic consumption) and the balance of imports shifted to Canadian oil. Domestic oil and natural gas (which increasingly since Obama's presidency supplies the electrical grid) have come predominantly from shale and hydraulic fracturing (or "fracking" colloquially), while Canadian oil comes predominantly from tar sands. Both are technology and energy intensive and environmentally impactful forms of production. Oil sites where all that is needed is to drill a hole into a pocket of oil underground that then comes out under its own pressure are increasingly rare in the US. Instead oil is squeezed out of exploded rock and the resultant natural gas is captured, or oil is filtered out of sandy sludge. Locally, fracking causes many environmental issues from poisoning the water table, to polluting the air with benzene through gas well leakage and

creating local seismic activity that damages property (see Briggie 2015). The Keystone XL pipeline that the Dakota Sioux protested in 2016, and the Northern Gateway pipeline that the Unist'ot'en continue to protest both seek to carry excess of tar sands oil from the tar sands in Alberta to US and foreign markets (Canada as of last year accounts for 55% of US oil imports; Saudi Arabia is next at 8%; EIA). The grounds for the protest are the fear of inevitable spills that will pollute rivers, land, and water-sheds with the contents of the pipelines and runoff from extraction sites as well as resistance to an ongoing legacy of settler-colonial occupation and ecological warfare.

So is the monitoring network for CO₂ a critical infrastructure in the Popperian sense, or is it critical in Spice's sense? While the contextual history offered in this chapter does not provide easy answers, I have attempted to highlight a few tentative trends. The prize for an economic experiment that is genuinely falsifiable in the sense that it is open not just to changes in leadership but also revisions in scale, specifically revisions that amount to a scaling *down*, would have to go to Soviet leadership in the late 80s and early 90s with respect to nuclear energy. On the other hand, fossil fuel infrastructure exemplifies the opposite trend - scaling up in spite of convincing evidence that it has already failed numerous times at one level of centralization. The environmental monitoring network, of which CO₂ measurements are in some ways the keystone, would seem to have balked at expansions and alterations that would improve the network's capacity to capture issues of local concern while turning the measurement of CO₂ into an ever more rarefied ever more highly concentrated activity within technologically supremacist institutions and countries that reproduce problematic settler-colonial relations between settlers and prior and continuing residents through the land they occupy.

If the risk associated with states like the US monitoring their own climate impacts are meant to be measured in action taken to actually regulate industrial carbon emissions, the results are hardly confidence inspiring either. In the 52 years since the US began to invest heavily (not just in terms of money, but in terms of effort spent trying to quell civilian monitoring and publication of environmental harms) in CO₂ monitoring infrastructure and in the 27 years since it became the world standard setter for this infrastructure it has done close to nothing to mitigate its total carbon footprint (beginning with the

infamous Congressional rejection of Kyoto just five years after taking over the world calibration effort). If any progress has been made it has been since 2008, in the US's bringing the impacts of its own consumption closer to home by scaling up domestic production. But this may merely be having the impact of exposing the fissures, fault lines and chasms in American domestic democracy, where an ever more elite circle claims special citizenship status to the exclusion of the historically unrepresented and a new, growing economically precarious population. Some may object that the scientific infrastructure is meant just for monitoring or information and they may insist that in this regard the global network has done an adequate job (via the IPCC reports) for the last 30 years. Perhaps the issue lies with the state needing to take measures to ensure that its policymaking institutions are appropriately aligned with its reporting systems. The understandings of critical infrastructure discussed in this chapter have to do with the infrastructure's effect on social risk management. Publishing information about risks is one necessary step in mitigating them. But perhaps the balance of interests that supports this information gathering process is precariously poised to provide only the information that could improve US domestic business and consumer self-interest. Global background CO2 measurements provide an environmental issue tailor-made for ignoring existing institutions, such as OPEC, that also seek regulation of the global fossil fuel economy with an emphasis on fair economic relations between importing and exporting countries, and tailor-made to bypass considerations of local concern exemplified in labor struggles in Iran, guerilla resistance in Vietnam, environmental protest against fossil fuel extraction in Nigeria, and in indigenous resistance to tar sands extraction in the US and Canada. In this, the science continues to serve liberal economic institutions that want to view globalization as the end of history rather than viewing history as a repository of evidence regarding the success or failure of past experiments in liberalism.

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Chapter 6: Conclusion

The previous chapter was guided by an idea from the science studies literature that the climate data network represents an infrastructure unto itself - an information infrastructure - and that it took on this character as a result of its institutionalization within state structures beginning in the 1950s. I asked whether that infrastructure - really an aspect of a broader environmental monitoring infrastructure - could be considered *critical* in Popper's sense of identifying and addressing the most serious social risks associated with fossil fuel infrastructure. Of course, the environmental monitoring infrastructure keeps track of much more than just pollution from fossil fuel infrastructure (e.g. ozone depletion, acid rain, etc.). The focus on information about fossil fuel pollution specifically is guided by the more general Popperian project of imagining what appropriately *critical* (or scientific) economic development would look like. Popper thought that liberal economics had the scale right for social experiment (e.g.. individual enterprise in specific industries like oil refineries), but had reservations about the ability of private enterprises to think critically about their experiments. The monitoring and regulatory apparatus set up during the second half of the 20th century can also be appraised for its ability to provide this critical supplement to liberal enterprise.

The institutionalization of climate data led to new forms of electoral risk that were perhaps first operative within the CO2 story told in the last chapter in changes to the Keeling team's funding, then in NOAA's take-over of the central calibration function. These mechanisms again became operative in campaigns against the regulatory state by conservative elected leaders such as Ronald Reagan and most recently Donald Trump. These leaders used their powers to appoint agency chairs who would redirect funds and personnel away from climate data and other environmental monitoring projects. These actions are sometimes decried as politicization of science by critical analysts. From their perspective, the use of

electoral mechanisms to constrain or shape scientific monitoring projects seems an abuse of power and a distortion of science (Oreskes and Conway 2010, Weart 2022). The analysis in this dissertation points in a different direction. Electoral risk may be a necessary feature of any institution that calls itself critical - i.e. capable of learning from its mistakes through openness to the public they affect. The main problem is rather that these electoral mechanisms apply only to the critical supplement of fossil fuel infrastructure and not to fossil fuel infrastructure itself. This gives the public leverage to sustain, reorder or terminate critical information gathering activity, but not to affect the industrial activity that the information is about.

The pretense of the regulatory state is that this information can also be used to affect the industries that it monitors. But the previous chapter highlighted some considerations that call into question the authenticity of this pretense - from NOAA's housing in Commerce, which subordinates its aims to the promotion of domestic business, to the pattern of development which prioritized technologically sophisticated centralized measurement of a single quantity over a diversity of quantities with more accessible participation criteria. And the historical record paints a clear picture of its inefficacy - net emissions continue to rise, fossil fuel executives continue to shuffle through political office, the scientific reporting on the issue has been delayed and bogged down by allegations of uncertainty fueled by corporate-funded denialism for decades, and the supreme court recently ruled the EPA doesn't have the power to regulate carbon emissions. Hence the suggestion in the last chapter that these institutions always had the dual aim of *containment* - they sought to mollify more radical democratic critique of economic imperialism and corporate malfeasance by instituting "reasonable" constraints. Perhaps it is time to return to these more radical critiques.

I agree with Popper and with liberal analysts in thinking the centralization of these regulatory bodies and the indirectness of the electoral mechanisms are part of the problem. Centralization at the federal level promotes a focus upon the highly centralized activity of running a monitoring network for background CO₂ at the expense of more local and heterogenous issues associated with particular extraction sites and operations. The indirectness of electoral mechanisms means that the public considers climate data activities as part of a package of policies that make up the platform of one of two major political parties -

e.g. they are asked to lump together their views on environmental monitoring and healthcare. These features make the electoral mechanisms within the climate monitoring network blunt tools.

But these concessions do not override the diagnosis that there is no supplement for electoral risk for sustaining critical thought. In order for the development of fossil fuel infrastructure to be genuinely critical, it too must be subject to electoral risk. One possible solution to this set of problems would be to internalize electoral mechanisms within privately owned enterprise by converting them into something with features of worker and consumer cooperatives (which have worker and consumer representatives on the board) while delegating more substantial environmental authority to more local (state, county, municipal) governments which can represent a more local public affected by specific enterprises (rather than whole industries).⁵⁹ Such institutions would give substantial electoral power to groups that are basically unrepresented within current economic institutions in the US - workers and the non-consumer public. But as we can see with the Canadian Northern Gateway pipeline, which is now owned by the Canadian state, it is possible to have some degree of electoral accountability to a narrowly defined public while still foisting impacts upon unrepresented “outsiders.” Any institutional reordering would also need to grapple with the current paradigm of free trade which treats the whole world as a possible frontier for economic activity and thwarts the possibility of organizing more local publics.

For economic policy that is based on science instead of a modern variant of manifest destiny (Grandin 2019), the notion of a frontier as an unoccupied and wasted space that is ripe for development and use is a fictional space that never existed and should therefore be an easy one to jettison. The fact that science itself has had to learn this lesson historically through its participation in state projects of colonialism, genocide and ecocide provides a testament to its fallibility and a direction for *historical* inquiry for those who remain unconvinced. The analysis given in this dissertation is guided by inquirers from geography, anthropology, sociology and history who have grappled with their disciplines’ histories of complicity in

⁵⁹ See Briggie 2015 for an example from fracking of the legal and political issues facing more local forms of environmental governance.

such projects and who offer insights and new framings for thinking about the role of expertise in socio-political projects.⁶⁰

In this dissertation I have tried to gesture toward a potentially expansive role for science - in the structuring of economic development or in a socio-political capacity. I have tried to show how this is an *old* project - one that was developed by positivists such as Popper in an international context where different structures of economic development were considered *scientific*. I have also tried to show how analysts of this role balanced both moral and traditionally epistemic criteria to imagine an appropriate experimental space for social planning. Socio-political activity means the public management of the household, or the provision of the necessities of living. In a secular age and in liberal government it is only fitting that science should help articulate a conception of public rationality that can guide public planning. The alternative is the idea that the social sphere ought to remain a sphere free from political interference and public input.⁶¹ While I canvassed some arguments in chapter 2 for denying that allegedly social sphere activities are the proper subject of public or political critique, I opted in chapters 4 and 5 to consider a category of economic activity - that concerning critical infrastructure - within which the positive role of the state is explicit and substantial. Quite often states are the owners and operators of these infrastructures, which they manage in the interest of a regional or national public. For this category of activity we can ask directly whether it appears to be managed in an appropriately critical and scientific fashion (i.e. we do not need to argue first that it is socio-political).

The transition noted by Edwards (2006) in climate data from a voluntary collaborative effort to a state-managed infrastructure also suggests a transition within scientific inquiry itself from a free social association to a socio-political endeavor. This invites questions about the appropriateness of that transition and the nature of science - specifically whether it is an activity more appropriately carried out in

⁶⁰ Some may wonder how this project fits under the banner of philosophy of science which traditionally allows itself to be led by observation of scientific practice. My answer to this would be that it follows the lead of those disciplines imbricated in past socio-political mistakes, even where those mistakes may have led to them being considered non-scientific. The thought is that experts from these disciplines with historical consciousness offer a better guide to thinking about how scientists should comport themselves and design their institutions when wading into politics and social planning. Geophysics, atmospheric chemistry and other relatively new disciplines have perhaps not yet learned - because they are not part of the curriculum - these historical lessons. For exemplary work in this tradition see Graeber 2004, Gilmore 2007, Tsing 2015.

⁶¹ The other, false alternative is allowing ourselves to be guided by a new holistic and therefore dogmatic teleology, whether religious, as in Christianity, or quasi-secular, as in historicism or neo-liberalism.

or outside the public sphere. The analysis from chapter 5 suggests that in its capacity as a free-association, scientific monitoring of CO₂ smuggled in controversial background assumptions about the “natural,” pre-experimental condition of the atmosphere and the appropriate conditions for its measurement - in expeditionary and frontierist projects in allegedly un-occupied and unused landscapes. These background assumptions set the scene for a monitoring network whose membership was tightly constrained and whose import for energy policy was confined to selecting from technological alternatives for providing an unquestioned (because a national necessity) amount of energy.

While many environmental activists I know research and continue to make progress in debunking frontierist and colonialist presuppositions of their own activity, institutions seem to lag far behind. Native governance of reservations and ancestral land is still managed in a top-down, paternalistic way by the federal government. Institutionalized participation in the most important environmental questions for tribes is usually symbolic as when tribal representatives form an overwhelming minority in the holistic management of fisheries in the PNW (a process administered by NOAA as well) - i.e. despite having been granted half the catch by the Boldt decision, they are a small minority in management of the regional stock that they are to receive half of. One of the most significant impacts on salmon runs has been the dams constructed on major rivers to provide power to cities in the Northwest. But the existence of dams is represented as non-negotiable and the contrivances of hatcheries and fish-ladders represent the available attempts to offset the impact of giant concrete walls, while present runs remain a fraction of historical ones. Moreover, the PNW is the only region in the US with *any* tribal representation on its fisheries council.

Socio-political junctures where Native water protectors and allies resist development of new fossil fuel infrastructure demonstrate the efficacy of critical action in delay, suspension and cancellation of infrastructural development (the Obama administration’s cancellation of Keystone XL). In doing so they illustrate the efficacy of public values that are unrepresented or underrepresented in official practices of scientific monitoring (environmental impact assessments and the like - see Whyte 2017). Scientific institutions would do well to heed empirical studies of such junctures and to rethink their role in current

“critical” paradigms that seem incapable of making change in the direction of actual democratic demands. This dissertation has been motivated by the hypothesis that current academic discourses on climate change spend too much time combatting far right, ultraconservative speech that represents a tiny fraction of the democratic public. In doing so, they almost inevitably end up centering and backgrounding assumptions that are right of center, on the conservative end of the political spectrum. The critique of climate science from the left is a necessary antidote to this trend. Unlike the paradigm of critique as a contest for a winner between two positions that are impossible to reconcile, this critique is offered in the hopes of forging new coalitions for envisioning and co-creating a shared future.

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