Political Competition and Judicial Independence in Non-Democracies

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Abstract

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This dissertation examines the relationship between political competition and judicial independence in non-democratic polities. In it I advance two key arguments. The first is that the common "political insurance" explanation of judicial independence in democracies, predicated on the incentivizing effects of electoral competition, should also offer insights to explaining independence outside the democratic context. The second argument is that when present, electoral competition should be of greater salience in non-democracies due to the significantly higher risks associated with losing office in a non-democratic regime. I begin by discussing the concept of judicial independence and approaches in the existing literature seeking to explain variation in judicial independence in democracies, and how the literature fails to adequately address the question of regime type. I then address in Chapter 2 a fundamental aspect of the insurance model of independence that to date has been assumed but never empirically examined, showing that independent courts are highly associated with positive outcomes for leaders after leaving office. Tests of the two main arguments are presented over four chapters. Chapters 3 and 4 examine non-democracies exclusively, testing the first argument and finding that it holds up across different measures of the key concepts. Chapters 5 and 6 address the conditional relationship between competition and regime type, showing both globally and in an extended

study of the postcommunist region that the effects of competition are much greater in non-democracies.

This dissertation contributes to the study of judicial independence broadly, and the growing focus on judicial institutions outside of consolidated democracies. It further stands as one of the most thorough treatments of the dominant insurance model of judicial independence, providing the first ever test of one of its key assumptions and offering insights into the scope conditions and explanatory power of political competition. Most critically, I extend this explanation to the non-democratic context, arguing that the logic of the explanation implies it should be of even greater explanatory power in non-democracies. I test my argument exhaustively, using multiple measures of the key concepts and assessing the strength of the argument both globally and in the postcommunist states of Eastern Europe and the former Soviet Union.

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DEDICATION

to my parents, Dale and Sue

Chapter 1

INTRODUCTION

What can theories of judicial independence created to explain the phenomenon in democratic contexts tell us about the non-democratic world? Are the foci of these theories of equal or greater weight across regime type? Do fundamental assumptions of these theories that to date remain untested actually reflect to empirical realities? In this dissertation I offer answers to each of these three key questions, and in doing so offer answers to a number of ancillary questions as well. Throughout, I offer two primary arguments. First, that the dominant framework for explaining judicial independence found in studies of consolidated and emerging democracies is also important in understanding judicial independence in non-democracies. Second, I further contend that the underlying premise of the "insurance model" of judicial independence should be more critical in non-democracies due to the significantly higher risks associated with losing hold of political power. Until recently, there has been scant attention paid to the study of courts in non-democracies (Ginsburg and Moustafa 2008), and with one or two exceptions absolutely none paid to the question of judicial independence (Moustafa 2008; Popova 2010). In this dissertation I address these lacunae. In doing so I provide a theoretical explanation for why political competition is important for understanding judicial independence in in non-democracies, key tests of my argument with a variety of data and in multiple contexts, and a novel test of the underlying assumption that independent courts provide political insurance.

The structure of the dissertation is as follows. Section 1 addresses the existing literature and the theoretical propositions offered here. In Chapter 1, after discussing the concept of judicial independence generally, I introduce the "thin-strategic" expla-

nation for judicial independence that dominates the current literature on the subject (VonDoepp and Ellet 2011). This explanation focuses on the goals of strategic political actors in a democracy, contending that electoral uncertainty drives them to create minoritarian institutions able to protect them once leaving office. It is often referred to as the electoral logic or political competition account of judicial independence; in a seminal work on the subject, Ginsburg (2003) dubbed this the "insurance model." After providing an overview of the development and insights of this model, I argue that political competition when it exists in non-democracies should also drive those in power to allow, establish, and maintain higher degrees of judicial independence than they would otherwise. I further argue that these explanations, as they are predicated on the risks involved in losing office, should in fact be of more salience in the non-democratic context. In Chapter 2 I provide a test of one the most fundamental—and to date untested—assumptions of the insurance model: that independent judiciaries are associated with better outcomes for political actors after they no longer hold power. Using data on the post-tenure fate of leaders from 1960–2000, I show that leaders in countries with more independent judiciaries are significantly more likely to be safe from punishment after leaving office, regardless of a country's level of or history with democracy, the degree of economic development, or the institutional structures of governance. Thus, it it does in fact make sense to talk about independent courts as insurance policies, which until now has been assumed but never examined.

Section 2 tests the first theoretical proposition offered in the dissertation, that political competition is associated with more independent courts outside the democratic context. Chapter 3 examines the "onset" of judicial independence in non-democracies, what I refer to as judicial empowerment. Here I present counter-arguments of authoritarian judicial empowerment drawn from case studies in the existing literature, and show that they fail to possess any explanatory power when looking at 30 years worth of data on non-democracies globally. Employing logistic-regression derived survival models, I illustrate that political competition, on the other

hand, is consistently the best predictor the likelihood of a non-democracy empowering its judiciary. In Chapter 4 I examine variation in the level of judicial independence in non-democracies in four decades of country-year data. Here I find that across a wide variety of measures of both competition and autocracy, competition is consistently associated with higher levels of de facto judicial independence.

In Section 3 I test the second theoretical proposition, that political competition is more salient in non-democratic as opposed to democratic contexts. Chapter 5 presents a global analysis, showing that regardless of the measure of competition or regime type used, the effects of competition are much greater in non-democracies. This is not to say that we should expect more judicial independence in non-democracies, as competition is not the sole relevant factor. Rather, I find that when present, competition is associated with much higher levels of political competition in non-democracies. To better understand the limits of the global analyses offered in Chapter 5, Chapter 6 focuses on the role of competition and regime type in explaining de facto judicial independence in the postcommunist countries of Eastern Europe and the former Soviet Union. Doing so allows for both more fine-grained measures of the relevant variables as well as for the inclusion of variables that might confound the relationship between competition and independence in this specific regionally context. It furthermore presents an example of testing the broader arguments in a region possibly least amenable to the argument: to date, the only analysis of the relationship between competition and judicial independence in non-democracies focuses on a paired comparison from the postcommunist world, and it argues that competition inhibits independence. I show through an analysis of 15 years of data drawn from 27 postcommunist countries that competition is associated with higher levels of judicial independence, and that it is more salient in the non-democracies of the region.

I then provide a brief conclusion, summarizing the contributions to the literature offered in this dissertation. They are, primarily, four. First, I present the first cross-national empirical tests of the popular insurance model of judicial inde-

pendence. My findings offer partial support for the consensus in the literature, and further suggest other covariates that need to be included in future theorizing and modeling of the phenomenon. Second, I provide the first test of a fundamental assumption of the insurance model, finding that independent courts do provide a form of political insurance to office holders once they are no longer in power. Third, I show that contrary to conventional wisdom, the electoral logic of the insurance model is of great explanatory power outside the democratic context. Finally, I also show that the explanation not only extends to non-democracies, but that the insurance model is in fact more salient in non-democratic states.

1.1 De jure and de facto independence

At the most basic level, the distinction between the two types of judicial independence is the distinction between formal rules and actual behavior. That is, if we think of de jure as the constitutionally-derived rules of the game, de facto is merely how effective these institutions are at constraining the actions of other relevant players. The connection between de jure and de facto is clear: judges are de facto independent when transgressions against the de jure constitutional and legal provisions regarding their independence meet with failure, or are not even attempted in the first place. For example, if the rules regarding judicial selection and promotion are routinely ignored, or if judicial salaries are manipulated by the executive to produce compliance with executive preferences, then we can say that, regardless of the degree of de jure independence, de facto independence is impeded.

The conceptualization of judicial independence is, like many similarly imprecise yet critically important concepts such as democracy or the rule of law, fraught with difficulties and disagreements. Mirroring those debates, a number of these difficulties stem from more fundamental disagreements about what the concept means (Russell and O'Brien 2001; Burbank and Friedman 2002). This is especially true for de facto conceptions. Agreement on which formal rules matter is aided by consen-

sus documents like the United Nations' *Basic Principles on the Independence of the Judiciary*, whereas understanding just how to conceive of the failure to meet these ideals is more difficult. The fourth UN principle is a clear illustration of this, as it begins with, "There shall not be any inappropriate or unwarranted interference with the judicial process" (United Nations High Comissioner for Human Rights 1985). As a result, some scholars have even suggested that the contested nature of judicial independence prevents its analytic usefulness, contending that the confusion over its meaning is intrinsic to the concept (Kornhauser 2002). Beyond conflating empirical and theoretical issues surrounding our understanding of judicial independence, such claims also conflate an inability to reach a consensus definition with an inability to understand what others authors mean when using the concept. This extreme position offers few solutions, other than abandoning attempts to reach consensus or engage in parallel avenues of research employing differing definitions and measures. Such a position seems problematic, given that a consensus does exist on many of the basic principles of judicial independence, even among competing conceptions (Tate and Keith 2007; Ríos-Figueroa and Staton 2009). Even if we reject the claim that judicial independence lacks utility, however, there is significant value-added in keeping in mind the fact that disagreement is common if the result is close attention to how we conceptualize and measure independence.

A common distinction drawn between two components of de facto judicial independence is between autonomy and power (Burbank and Friedman 2002). Judicial systems are autonomous when judges are the "authors of their own opinions" (Kornhauser 2002, 48). That is, when the decisions handed down by courts are the result of judicial decision-making unadulterated by undue outside influence. This does not imply there is no outside influence, nor does it imply that the decision is "good" in any normative or reasoned sense. Outside influence on judges is an unavoidable attribute of the legal process in the form of oral and written arguments, as well as the decisions made by other courts. Such influence is not inappropriate. Similarly, judges may rule

in ways contrary to the demands of the legal system but in accordance with personal moral, philosophical, or political beliefs. While such judgements may be poorly made and illegitimate, they are not the result of the undue influence of other actors, and thus we can consider such judges autonomous, though perhaps of poor quality.

The outside actors that may inappropriately influence courts in such a manner as to violate de facto judicial independence are potentially limitless, but in practice are typically limited to those with power. Other state actors in the executive and legislative branches often have the ability to violate the independence of the judicial branch, and powerful non-state actors such as business interests, organized labor, and other social groups also have the potential to do so. In the context of transitioning societies, with organized labor and civil society weak, it is the first of these non-state actors that is most likely to be of relevance (Howard 2003; Crowley 2004). Inappropriate influence can take a variety of guises, from intimidation and threats to corruption and bribery. The former can escalate as far as murder; judges are routinely killed in Russia, for example (Deutsche Welle 2010; Miley 2010). In less dramatic variations, courts can become accomplices in the violations of their own autonomy, with individual agents of the judicial branch engaging in actions that harm the interests of their institutional principal.

Power is the second component of de facto judicial independence. Put simply, judiciaries have power when what they say matters. When the rulings of courts are routinely ignored by other state actors, or the required enforcement fails to materialize, courts cannot be said to be independent. As Hamilton made clear in Federalist No. 78 over two centuries ago, since judiciaries lack the money or executive power to enforce their decisions, they must rely upon other branches of government to support their decisions. This support may be in the form of modifying or rescinding governmental policies when courts rule against the government, or seeking to gain redress or exact punishment when rulings are against non-state actors. Regardless of the target of judicial rulings, when support by the executive and legislature does not

exist, regardless of the level of autonomy the courts may enjoy, they lack power, and are therefore not independent in the de facto sense.

It is the de facto conception of judicial independence that I will focus on in this dissertation. There are multiple reasons for this. First and foremost, behavioral independence could as easily be referred to as "actually-existing independence," and thus the kind that matters, as it affects the lives and experiences of those subject to the law. Related to this is the fact that since in many situations there is little correlation between de jure and de facto independence, there is a distinct need to understand the determinants of de facto independence, as they are clearly not the formal rules of the game (Tate and Keith 2009).[1] Third, there exists significantly more variation in de facto judicial independence than exists in de jure independence, regardless of regime type. Constitutional provisions outlining and ensuring the independence of courts change much more slowly than political alignments. Most clearly, if one were to analyze solely de jure independence, the more than 220 years of the United States history since the ratification of the US Constitution would exist as one observation.[2]

1.2 Explaining independence

Insurance models of judicial independence developed in a tradition of "delegative accounts" of judicial independence, all of which focus on the choice of actors in the political branches to delegate power to the courts. The foundational text for delegative explanations of judicial independence, including those focusing on the role that political competition pays in incentivizing those in power to insure themselves against

[1] In the postcommunist region, for example, there is a large divide between de jure and de facto independence. The Hungarian, Latvian, and Polish constitutions all enumerate fewer protections for the judiciary than any of the Central Asian, Slavic, or Caucasian former Soviet republics (with the exception of Turkmenistan), despite having consistently higher levels of de facto independence. The very fact that the Central Asian states—the most unreformed autocracies in the postcommunist region, with the arguable exception of Belarus—guarantee significant de jure independence suggests that it might not be of vital importance.

[2] This is not to say that understanding the process of constitutional adoption and change is not important. For recent work on this subject, see Elkins, Ginsburg and Melton (2009).

uncertain futures, is an article by Landes and Posner (1975). In the piece, the authors argue that the political branches establish and maintain independent judiciaries because such institutions are able to solve the time inconsistency problem plaguing legislatures. The problem is that while existing legislatures desire to maximize their campaign contributions and support from interest groups, there is nothing constraining the very same (let alone future) legislatures from reneging on campaign promises and policies enacted in exchange for support. An independent judiciary helps solve (or, probably more accurately, mitigate) this problem by increasing the difficulty of rescinding policies previously enacted. The obvious analogy is that of Odysseus and the Sirens: legislators know that in the immediate future their short-term interest will be to offer new deals to interest groups to maximize their contributions, but that doing so will minimize the value of their promises. Here, an independent judiciary plays the role of the crew, binding the legislature to its previous commitments and preventing it from inflicting harm on itself.

The analogy serves another purpose, which is to bring to the foreground the question of why any political actors would want to empower their agents in such a way that these agents are able to put constraints on the preferences of the political principals or to openly veto policies or programs the principal desires to see enacted (or ended). Following the popular conception of political actors as individuals and groups seeking both office and the ability to enact their policy preferences, the default assumption would be that they would have no desire to create institutions capable of checking their policies. They would do so, however, when independent courts are able to provide benefits, and only under these circumstances; courts have neither the power of the purse nor the power of the sword, and are generally incapable of forcing the creation of new de jure protections of their independence, or enforcing existing protections. This was obvious at a conceptual level to Hamilton, who stressed in Federalist 78 that the proposed court had neither force nor will, and is made obvious empirically by events as recent as those in Hungary, where the supermajority

government has moved to hamstring the power and oversight abilities of the country's previously-formidable Constitutional Court (Scheppele 2011; Venice Commission 2011).

McCubbins and Schwartz (1984) suggest that independent courts provide the benefit of cheap and effective oversight of the bureaucracy. In their conception, two forms of oversight are possible. The first is "police patrols:" a regular, formalized, and thus costly form of monitoring compliance. The second is "fire alarms." Here, some costs of monitoring the bureaucracy are passed on to private citizens, non-governmental organizations, and businesses. It is only when these non-state actors discover and report that the bureaucracy is failing to comply with the mandates of their political principals that government oversight begins, in the form of lawsuits brought by effected parties. Thus, for McCubbins and Schwartz, courts are an economical way to handle a number of principal-agent problems inherent in the relationship between a government and the bureaucrats in state agencies tasked with carrying out the government's policies.

Political insurance explanations of judicial independence are a direct outgrowth of these two prior delegative explanations. In a seminal piece comparing the independence of courts in the United States and Japan, Ramseyer (1994) details the inadequacies of the two accounts above. Post-war Japan and the US provide a comparison in which two countries with highly similar institutional arrangements for the judiciary also witness highly dissimilar experiences with regards to the independence of the judiciary. In both cases electoral incentives existed to tie the hands of the legislature with regards to commitments made to interest groups, suggesting that the account forwarded by Landes and Posner (1975) is incomplete. Similarly problematic for the McCubbins and Schwartz (1984) explanation, there existed significant need on the part of Japanese governments to effectively monitor the bureaucracy, and yet judicial independence was negligible, with the LDP government using alternative mechanisms to enforce bureaucratic compliance (Ramseyer and Rosenbluth 1993; Ramseyer and

Rasmusen 1997). Expanding on the electoral logic established by Landes and Posner (1975) and McCubbins and Schwartz (1984), Ramseyer (1994) argues that it is not just competitive elections that matter; again, if this was the case, post-war Japan during four decades of LDP dominance should have produced independent courts, for deals still need to be struck with constituents and bureaucrats still must be monitored. Rather, it is the expectation of possibly losing those elections that make the electoral logic obtain: only when those in power face some likelihood of losing office will they establish or maintain independent courts. They do this as a form of political insurance for when they find themselves in the minority.

The insurance model seeks to explain under what conditions electoral competition can lead those holding the reins of power to transfer some of this power to the judiciary, and argues that the key determinant is the degree of political competition within the electoral arena. When those in power suspect they may lose power to an opposition able to present itself as a credible alternative, they fear that unchecked power may be turned against them once leaving office. As a result they create (or sustain) institutions capable of checking the executive, insuring their protection after leaving power. That is, when an opposition party is strong enough to present a credible threat of replacement, those in power should be more likely to sustain or strengthen judicial independence to better protect their interests. This logic (which will be discussed further below) has been applied to studies of both de jure and de facto judicial independence.

The most prominent example of the electoral logic applied to variation in de jure judicial independence is Ginsburg's (2003) study of three emerging democracies in East Asia. In the book that introduced the term "insurance model," Ginsburg argues that the level of political competition during the time period of constitutional drafting and adoption directly affects how expansive or narrow the powers of a country's constitutional court. The strength of the South Korean Constitutional Court is attributed to the robust competition between three political groups of similar strength.

Taiwan is offered as a contrasting example: once the communists abandoned constitutional negotiations with the Kuomintang, the agreement to create an American style court with broad powers of review and easy access by claimants was abandoned in favor of a court with limited access, centralized appointment procedures, and comparably weak abstract review powers. Appointment and reappointment procedures, promotion, tenure, review powers, and access are all examples of formal, constitutional provisions that determine the degree of de jure judicial independence possessed by any given court or judicial system.

In Ramseyer's (1994) original formulation of the argument, the focus was on de facto judicial independence: the de jure constitutional rules concerning the judiciary in Japan and the US are exceedingly similar, a result of the US influence on the drafting of the Japanese constitution after World War II. The overwhelming majority of scholars who have applied and developed the insurance model framework have done so in a de facto sense, nearly always trying to understand temporal variation in the behavioral independence of a judiciary within a given state. For example, in case studies of Mexico, scholars argue that as the PRIs control waned in the 1990s, courts experienced a greater degree of de facto independence (Finkel 2005; Ríos-Figueroa 2007). Similarly formulated causal accounts have been forwarded in Western Europe and other areas of Latin America (Vanberg 2001; Chavez 2003).

As noted, the basic logic of the political insurance model of judicial independence is that when those in power have reason to believe they may lose power, their fear of majority encroachments on minority interests drives them to establish or maintain independent judiciaries as a form of insurance. But how precisely do courts provide insurance, and what do they insure against?

The first form of insurance provided for by independent courts is policy stability. This form of insurance is reminiscent of the logic articulated by Landes and Posner (1975). In the insurance framework, courts provide policy stability not by solving the time inconsistency problem, but instead by ensuring that the opposition,

after taking power, is unable to engage in wholesale revision or removal of its predecessor's policies (Ramseyer 1994). This form of insurance provided by independent courts can be thought of as similar to the insurance functions of independent central banks (Horn 1995; Boylan 2001) or impartial bureaucracies (Geddes 1994). It is not that those in power *cannot* infringe on the independence of courts to reverse their predecessor's policies, it is that their expectation of themselves being out of office in the not-too-distant future "keeps them honest." In other words, parties in power respect independence and forgo some policy objectives that could be gained by violating it, so that their opponents do the same once they take power (Ramseyer 1994). A recent example of this from the American context would be President George W. Bush's acquiescence to a number of Supreme Court rulings against his administration's policies toward detainees imprisoned for suspected terrorism (Lane 2006; Stout 2008), as it was certainly neither public opinion nor a lack of commitment to the policy that led the Bush administration to accept the string of decisions against its policies.[3]

While it may seem an extreme concern for those whose reference points are consolidated democracies, in many new democracies—not to mention competitive authoritarian regimes—the downside of losing office is not merely the reversal of favored policies but often the threat of expropriation, and independent courts can provide insurance against such expropriation (Ginsburg 2003).[4] This second form of insurance provided by independent courts can protect against infringements of both political and property rights. Targeting the property rights and economic assets of political minorities and those formerly in power is far from uncommon, as the commonplace

[3]A further example of this would be recent attacks on the US Supreme by Republicans after its 5–4 ruling upholding the Affordable Care Act ("Obamacare"). By and large, Republicans have suggested the court ruled improperly and that they will seek legislative solutions if they win the presidency in 2012, rather than attacking the court's ruling as illegitimate. A high profile exception to this is Kentucky Senator Rand Paul, who said in a statement, "Just because a couple of people on the Supreme Court declare something to be 'constitutional' does not make it so. The whole thing remains unconstitutional." (Hernandez 2012)

[4]The overwhelming focus in the law and courts literature on established democracy has made these possible forms of insurance less examined than the provision of policy continuity.

investigations and prosecutions of former government officials on charges of corruption make clear.

In the postcommunist region, examples abound. The most famous case of expropriating the property rights of a political minority in the opposition is undoubtedly that of Mikhail Khordorkovsky, whose involvement with the political opposition to Russian President Vladimir Putin cost him both his oil company and his freedom on what are dubious at best charges of tax evasion (Sakwa 2007). The Russian government has made headlines more recently in a similar manner, by beginning investigations into the tax history of socialite-turned-political activist Ksenia Sobchak (daughter of Anatoly Sobchak, the former mayor of St. Petersburg and principal author of the Russian Constitution), which independent observers consider doubtless to be politically motivated (Whitmore 2012). Also recent is the investigation of Nariman Tyuleyev, the Kyrgyz parliamentarian and former mayor of the capital city of Bishkek. While Tyuleyev has often been linked to less-than-legal practices in the past, the circumstances of the current prosecution suggest political motivations: Tyuleyev is linked to the former president and is a notable member of the only opposition party in parliament (Trilling 2012). His status as a member of parliament gives him legal immunity from prosecution, which only a majority of members of parliament are able to revoke. In this case, the prosecution office, controlled by the president, has stripped him of his immunity in an extra-legal action, an action possible in no small part because of Kyrgyzstan's severely dependent judiciary. These examples highlight the tight connections between the expropriation of economic and political rights that can occur to those formerly in power or vocally in opposition (or both), and can especially happen in circumstances where judicial bodies whose charge is it to provide oversight are not independent of those in power.

The expropriation of political rights need not occur through the means of direct infringement of these rights through targeted investigations and imprisonment, though certainly this is often the case, as the examples above and countless others

in Eastern Europe and the former Soviet Union make clear. The expropriation of political rights can instead consist of policies that disenfranchise those out of power. Electoral fraud is the most obvious example of this, and it is common in both emerging democracies and those non-democracies with competitive elections (Myagkov, Ordeshook and Shakin 2005; Tucker 2007). Expropriation of political rights can also involve the use of "administrative resources" during the periods between and immediately before elections. This differs from the simple distribution of the (legal) spoils that occurs after elections in many advanced democracies, and instead involves the illegal use of official positions to secure the results of the next election. This includes activities such as the use of the tax police and procuracy against opponents, the manipulation of mass media in the support of a particular party, and the pressuring of business interests to fund specific candidates (Colton and McFaul 2003).

A judiciary that is independent in a de facto, behavioral sense and not just in a formal de jure sense can serve as an effective check on these activities (Ramseyer and Rosenbluth 1993; Ginsburg 2003; Moustafa 2008). Independent judiciaries can serve as an important bulwark against abuses of power, providing independent forums in which those in the political minority who have been targeted by those in power are able to advance their interests. Without an opposition that is able to provide a credible threat to those in power, however, there is no incentive for those in power to acquiesce to creating or maintaining the independence of an institution capable of providing a check on the attainment of their preferred policies (legal or otherwise). This is the crux of the insurance argument: that independent courts are able to offer real protections, and for this reason their existence is advanced and tolerated by those in power only when they fear being in the political minority, and thus think they might have need for such protections. Judicial independence is thus, fundamentally, a "risk-reduction device," offering electoral losers protection, even if it is not ironclad (Ginsburg 2003, 32).

Once created, there are conditions that might exist that allow de facto inde-

pendent judiciaries to maintain their independence in times when the political arena is controlled by one party or constellation of like-minded parties that do not face serious threats of infringement; this is not to say that independence can become a self-sustaining equilibrium, but rather that other factors beyond competition can insure independence is maintained. Vanberg (2001, 2005) offers the most sustained account. He presents formal models and an empirical study of post-war Germany, arguing that even popular parties with a safe hold on the electoral arena should refrain from violating the prerogatives of the judicial branch due to a fear of public backlash. Such an explanation, of course, is predicated on the assumption that violating the integrity of the judicial branch would inspire a public backlash, which recent events in Hungary last year cast doubt on: after two years of dominating politics with a supermajority government and curtailing the powers of the courts, the Fidesz party still received almost 60% support in polls (MTI 2012; All Hungary News 2012).

Stephenson (2003) critiques the explanation offered by Vanberg, arguing that the electoral backlash Vanberg theorizes is exogenous to the model and unable to account for equillibrium outcomes. Rather, he argues via a formal model and a limited empirical test that it is simply the fear of untrammeled executive power after leaving office that better explains variation in judicial independence. The degree to which the insurance model holds when previously-robust competition becomes minimal (either temporarily or permanently) is as of yet underdeveloped (Helmke and Rosenbluth 2009), and a fruitful area for future research of vital importance to the literature. It is worth noting at this juncture, however, that instances of parties winning elections and reneging by violating previously independent judiciaries is the exception, rather than the rule; the Hungarian example is worth noting and has become of great qualitative value precisely because it exists as such an outlier.

A key limitation of existing research advancing the insurance model of judicial independence is the fact that nearly all of these studies have drawn on one (Chavez 2003; Finkel 2005; Vanberg 2005; Ríos-Figueroa 2007) or a small handful of cases

(Ramseyer 1994; Ginsburg 2003), and almost exclusively those in Latin America and East Asia.[5] An extremely limited number have examined the question comparatively. Stephenson (2003) tests his game-theoretic model using a cross-section of countries in 1995, and Ramos (2006) analyzes the relationship between competition and de jure independence with data covering 128 constitutions and the degree of competition during their adoption. These studies are all able to help us gain significant insights into the processes and nature of how competition relates to independence, but they make it nearly impossible to determine how important competition is when compared to other relevant factors, or how competition might interact with institutional characteristics. For example, the paired comparison offered by Ramseyer (1994) compares two advanced industrial democracies, making it impossible to determine the role economic development might play. Ginsburg's (2003) two key case studies suffer from the same problem, as both are East Asian "tigers." A second important limitation of the literature is the lack of attention paid to the question of regime type, to which we will now turn.

1.3 Addressing regime type

In the existing literature on the relationship between political competition and judicial independence, the question of regime type is rarely addressed, despite burgeoning interest in the role courts play outside the democratic context (Ginsburg and Moustafa 2008). The manner in which existing studies deal with how the insurance model might extend to non-democracies can be said to fall into one of three categories, which I will briefly sketch below, starting with the most common approach and ending with the rarest.

[5]This is in no way to suggest that these studies are not valuable or insightful, but rather to suggest that research exploring the model within a broader comparative context ought be an integral part of the overall literature on the insurance model.

1.3.1 Irrelevance of competition

The most common approach to the study of judicial independence in non-democratic contexts is to assume that competition plays no role in explaining the phenomenon. In the introduction to their edited volume on courts in authoritarian regimes, Ginsburg and Moustafa (2008, 28) go so far as to claim that "the electoral logic of judicialization in democracies clearly does not apply in authoritarian settings," though they offer no evidence or argument for why this is the case. Such an approach to politics in non-democracies presumes a monolithic regime where competition is either non-existent or not a serious issue. Given that a major impetus for the study of courts in autocracies is to move beyond such understandings of authoritarian states, bracketing off the potential for political competition mattering is surprising. More often, the idea that competition is irrelevant in non-democracies is expressed implicitly rather than explicitly like in the previous quote. Ramseyer (1994) does not address how regime type might matter, even in the context of Imperial Japan, and Ginsburg's (2003) focus on competition after the collapse of authoritarian regimes implicitly assumes that competition prior to this point isn't relevant.

Similarly, in his studies of the Egyptian judiciary, Moustafa (2003, 2007, 2008) argues that a number of factors allowed for courts to be more independent after reforms initiated by Anwar Sadat in the late 1970s, including the need to effectively monitor the bureaucracy as envisioned by McCubbins and Schwartz (1984). Despite the increasingly competitive nature of politics in Egypt after 1977 as demonstrated in Figure 1.1, competition is not considered as a possible explanation, either for the increase in de jure independence or de facto court powers; because the regime isn't democratic, the electoral argument is ignored as potentially having explanatory power. Another example of this implicit assumption of the irrelevance of competition in non-democracies is the data selection procedure used by Ramos (2006). He analyzes 128 constitutional events from 112 countries, restricting the analysis to those

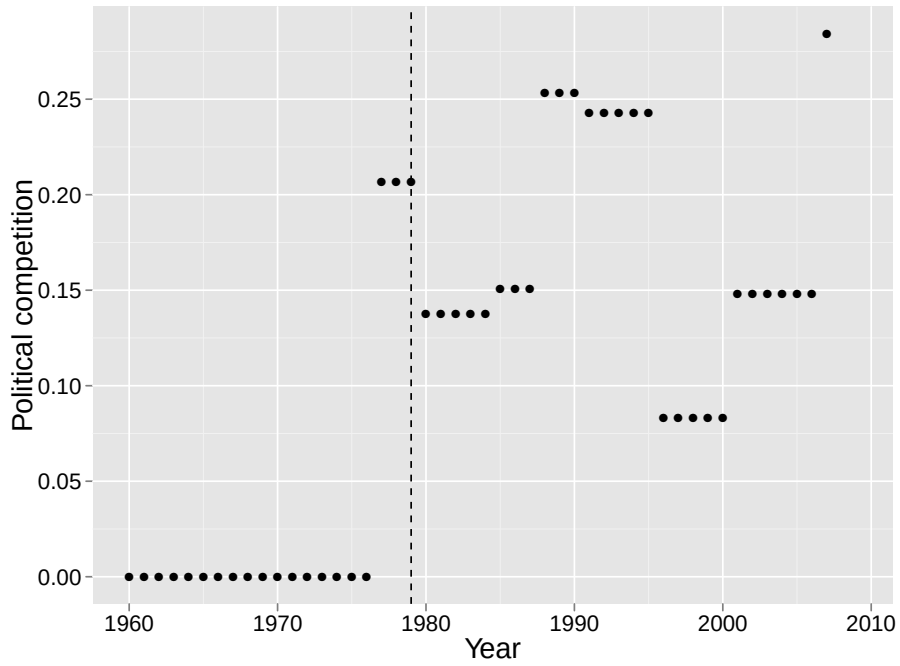

Figure 1.1: **Competition in Egypt.** A plot showing the level of political competition in Egypt during the 1960–2007 time period. The y-axis is a 0–1 measure of political competition, and the dashed vertical black line is the timing of Anwar Sadat's de jure empowerment of the Constitutional Court.

constitutions which were the result of a democratic transition.[6] Testing a variety of competing explanations of the (de jure) development of independent courts, he finds that competition best explains variation. Implicit in Ramos's (2006) analysis is either the assumption that neither political competition nor judicial independence vary in non-democracies, or that the former is irrelevant for the latter outside of the democratic context.

[6]Ramos further uses a regime's Polity IV score to determine whether or not a constitutional event resulted in a democratic transition, despite the fact that a portion of a country's Polity score is determined by the degree of judicial independence found in the country. The severe problem of endogeneity that results from using specific measures of democracy when attempting to understand judicial independence is discussed in Section 2

1.3.2 Transitions from authoritarianism

As noted previously, in case studies of Mexico, scholars argue that as the electoral dominance of the Partido Revolucionario Institucional (PRI) waned in the 1990s, the level of independence experienced by the judiciary increased (Finkel 2005; Ríos-Figueroa 2007; Magaloni 2008); a similar account is offered to explain variation in court independence in Argentina (Helmke 2005). The commonality offered in these accounts is the contention that when elites are fragmented and a transition to democracy is underway, the incentive structures are such that political actors who would otherwise prefer to keep power centralized devolve some of it to the judiciary, either to insure themselves against the future or enforce political order as their control diminishes.

In this portion of the literature, this causal account is forwarded not in the context of consolidated or new democracies but instead states emerging from authoritarianism: variation in the level of judicial independence in non-democracies which are not undergoing transitions to democracy is not considered.[7] That the role competition might play in determining the level of judicial independence in non-transitioning autocracies is almost universally ignored is puzzling when one considers the attention given to "competitive authoritarianism" over the past decade (Levitsky and Way 2002, 2010).[8] In their work Levsitsky and Way present an influential typology of non-democratic regimes, and focus on the role of international and domestic factors in one type, what they call competitive authoritarian regimes. These are non-democracies where elections that matter happen, but the process may be marred by systematic

[7] In some sense, there is a teleological dimension to much of this literature: non-democracies transition to democracies, and part of this process is the expansion of judicial independence. This is highly problematic, given what we know about democratic backsliding (Gasiorowski 1995; Gibler 2012), as well as the "stagnation" some have argued exists, with regimes stuck somewhere between authoritarianism and democracy (Collier and Levitsky 1997).

[8] Although not identical (see for example, Diamond 1996), some scholars conflate the idea of "electoral democracy" with competitive authoritarianism, using the terms synonymously (Popova 2010).

irregularities that tilt the playing field in favor of those in power. Regimes where, "in other words, competition [is] real, but unfair" (Levitsky and Way 2010, 3). For Levitsky and Way, the competition present in in some autocracies has important implications for regime outcomes, making these regimes qualitatively different than non-democratic regimes in which competition is absent. As such, they classify autocracies where competition is present as a different regime (sub)type.

Briefly discussing the utility of the competitive authoritarianism concept will clarify what I mean by regime type and competition, and why the intersection is important. While I certainly concur with the contention that competition has implications for a number of outcomes in both democracies and non-democracies, considering it a different *type* of regime seems problematic.[9] The effects of competition matter only when competition is robust enough, i.e. when opposition parties present credible threats to those in power, and this fluctuates with time within regimes. In other words, if the level of competition matters, then classifying regimes by whether they are competitive is beside the point, and potentially self-defeating.[10] If it is the "nature" of the regime that matters, then the observable implication is that competitive authoritarian regimes should produce similar results. Rather, I contend (and find) that it is the level of competition itself that is of direct import. In this conception, creating a new classification scheme adds little value: different regimes—democratic and authoritarian—have different levels of competition at different times, and understanding the effects of this competition is key. A similar conception is offered by Gryzmala-Busse (2007): in postcommunist democracies where robust competition

[9]The creation of too many conceptual categories may be as serious a worry as the concept-stretching that Collier and Levitsky (1997) warn of: the proliferation of typological categories can make the findings and often even the arguments of different researchers and schools of thought incommensurable.

[10]This is similar to problems with the Polity IV democracy measure, which scores regimes differently depending on the numbers of constraints on the executive at any given point in time. The result is the scores of long-standing, established democracies often decreasing when there is legislative-executive alignment across and within branches, or, in other words, when citizens' preferences are most obvious and articulated.

existed, state exploitation was minimized. Frye's (2010) work follows the same principle, and he finds that in postcommunist countries (both democracies and autocracies) competition is associated with significantly more successful institutional and economic reforms. There is no need to conceptualize two forms of postcommunist democracy, and over time as the degree of competition shifts countries can find themselves producing different outcomes. Such is the same for the role of competition with regard to judicial independence.

1.3.3 An inverse relationship

The only scholar to explicitly discuss the political insurance model in the context of stable non-democracies not in the process of transition is Popova (2010, 2012). She forwards a "strategic pressure' theory, in which competition causes leaders in non-democratic contexts to *decrease* the independence of the judiciary as a means of maintaining power. Like the political insurance explanation, Popova focuses on the risks and benefits presented by courts. In her account, the benefit provided by dependent courts of helping regulate the electoral arena and keep out the opposition outweighs the potential benefits played by independent courts if power is lost. Thus, her theory predicts that a s competition increases in non-democracies we should see less judicial independence.

Popova tests this account by examining court rulings on electoral registration disputes in single-member districts in Russia and Ukraine in 2002–2003, finding that pro-government candidates in Ukraine are more likely to receive favorable rulings than in Russia, attributing this to the more competitive nature of Ukrainian politics. Her argument, however, has difficulties generalizing beyond the two cases examined, and indeed beyond district-level electoral disputes; it is Ukraine, after all, which witnesses a Supreme Court ruling against those in power in support of further elections in late 2004, whereas Putin's manipulation of election results that same year was not commented on by the court. Some of these problems with the argument are in part a

function of unclear conceptualization of the relevant phenomena. The precise conception and measurement of competition, for example, is unclear: at points competition is conceptualized as regime-level openness, whereas at others it is the closeness of election results at the district level.[11]

The problem with the former is that it is unmeasured: Popova asserts that Russia is less competitive because of regime level characteristics, namely that "Russia was building a 'managed democracy' under an increasingly popular and dominant Putin" (Popova 2010, 9). Here, competition seems to be analogous to the level of democracy in a given state rather than the degree of electoral competition. Popova contends that politics in Russia is significantly less competitive, although fails to offer any evidence for this: in 2002–2003 both countries scored 6 on the Polity scale, and were rated as "partially free" by Freedom House. While it is certainly the case that during the 2000s Russian President Vladimir Putin increased the strength of the Russian presidency at the cost of other institutions, asserting without evidence that it is obvious that the country was less competitive than Ukraine in 2002 seems a case of hindsight bias.

At other times Popova conceives of electoral competition in a more conventional sense: the closeness of the support for those vying for office, and measures this by including a dummy variable for whether a given district was competitive. Here, her findings are inconclusive. While competitiveness in this sense significantly influences the likelihood of using the courts in a dispute in Ukraine it does not do so in Russia, and in both cases is insignificant in affecting the outcome of a court case (her measure of judicial independence). A fundamental problem with this operationalization of competition is that it is based on the closeness of the election *result*, such that the dependent variable is measured temporally before the independent variable. This

[11]Popova (2012) also examines judicial decisions in defamation lawsuits between 1998 and 2002, similarly finding that Ukrainian courts were less independent. Here, competition is merely assumed to be a regime-level variable, and remains asserted but unmodeled and unmeasured.

produces a serious endogeneity problem, as there is good reason to believe that the outcome of an electoral disputes affects the results of the election in that district; if it did not, there is no reason candidates would engage in such disputes, as their goal is to remove the opposition candidate from the ballot.

Excepting these empirical concerns and focusing on the theoretical level, Popova's (2010, 2012) framework would seem to predict that more competitive non-democracies should produce more judicial independence, although due to the unclear conceptualization of competition and the lack of clear or granular measures, the exact form of the relationship is indeterminate. Regardless, it remains the sole attempt to think theoretically or empirically about the effects of competition for judicial independence in non-democratic states, and is the clearest alternative to the framework I will now present, as it predicts precisely opposite empirical results.

1.4 Insurance in non-democracies

The overall contention of this dissertation is that political competition account of judicial independence popular in the study of democratic systems also provides significant value to understanding variation in the study of judicial independence in non-democracies. In this section I detail why this is the case, and then why in fact competition—when it exists—is more salient in the autocratic context. Such arguments are predicated, however, on the contention that variation exists in non-democracies, both when it comes to political competition and judicial independence. As many might consider both these phenomena to be the purview of democratic polities, I will first show that significant variation exists in the non-democracies of the world.

1.4.1 Variation in non-democracies

Despite the classical perception of non-democratic regimes being monolithic states in which those in power have near-complete control over political and social outcomes,

significant variation exists. The post-Cold War era has seen significant attention paid to the question of political competition, as the earlier discussion of Levitsky and Way; Levitsky and Way's (2002; 2010) work suggested. The attention has been widespread, however, with scholars studying, for example, under what conditions parties of power in non-democracies successful maintain dominance (Brownlee 2007), how they succeed electorally (Magaloni 2006), and the sources of societal and economic support of opposition candidates (Sjöberg 2011).

Despite the increasing attention paid to elections and electoral contestation in non-democracies, significant political competition is not the norm, nor should it be expected to be. Many non-democratic regimes operation under the control of a single party, and while elections may exist, they are merely symbolic affairs. The Soviet Union and most of its communist client states are the classic examples of this. Still, given that the baseline expectation for many is just this conception of non-democratic regimes, that significant competition is present in a significant minority of non-democratic states is of vital importance. Figure 1.2 shows a histogram of the levels of political competition in non-democratic states for the time period of 1960–2007.[12] The data exist on the 0–1 range, where 0 means that the executive is completely unconstrained by legislative opposition, and 1 means that the executive exists in a system where the entirety of the legislature is from opposition parties. The latter is hypothetical: in both democracies and non-democracies, the highest observed levels of competition range from 0.72–0.73.

Figure 1.2 shows only those observations above 0. If the 75% of the 4,321 non-democratic observations in this time period that report 0 on the political competition measure were included in the histogram, conveying the variation when competition does exist would be nearly impossible: the y-axis would extend to slightly over 3,000, and one bar on the far left of the x-axis (those observations where competition was

[12]The data used are the `polcon3` measure from Henisz's (2000) dataset on political constraints. See Chapter 4 for a thorough discussion of this measure.

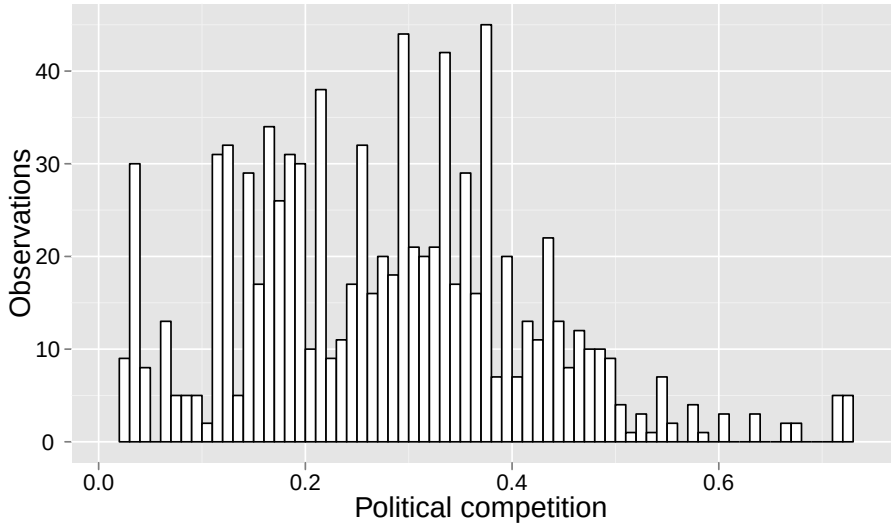

Figure 1.2: **Political competition in non-democracies.** A histogram showing the non-zero observations of political competition in non-democracies, 1960–2007. Three quarters of the 4,321 observations in this 47 year time span show zero political competition, and are excluded from the histogram for interpretability. The mean value for the observations shown in the histogram is 0.27.

0) going that high would make all other bars appear identical. Figure 1.2 shows clearly, however, that in 25% of non-democratic country years in the 1960–2007 period there is competition, and that significant variation in the levels of that competition exists (the mean value for these 1,073 observations where competition is present is 0.27). That one out of the every four non-democratic observations over nearly 50 years are ones in which political competition is present shows the vital importance of understanding competition in non-democratic contexts. This is even more the case when one considers the fact that in the 17 years of post-Cold War data, competition is present more than 40% of the time.

The idea that significant variation exists in judicial independence in outside the democratic world is likely less common than the idea that political competition varies in non-democracies. Until recently, the study of courts outside of long-established democracies was exceedingly rare, and until very recently, the study of courts in

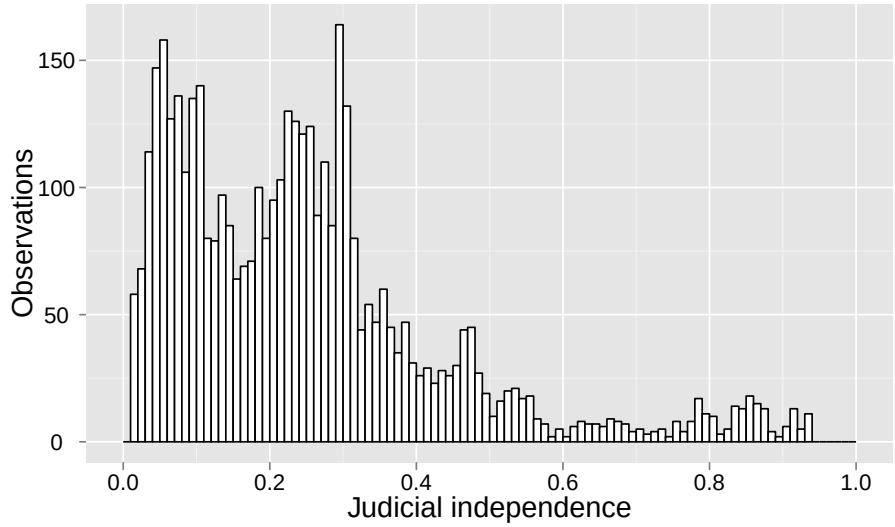

Figure 1.3: **Judicial independence in non-democracies.** A histogram showing variation in de facto judicial independence in non-democracies, 1960–2007. The mean value for the 4,321 observations is 0.25.

non-democratic contexts was all but non-existent (Ginsburg and Moustafa 2008). When one moves from a dichotomous framework for courts—are they independent or not?—to one in which significant gradations can exist, however, it becomes clearer that significant variation *must* exist outside of democratic regimes. Not all autocracies are able to so adroitly employ the "telephone justice" for which the Soviet Union was famous (Pepys 2003; Hendley 2009). This is all the more obvious when one considers that in some instances, non-democratic rulers inherit courts from the previous democratic regime, and clamp down on their independence with varying degrees of success (Helmke 2005).

Figure 1.3 plots a histogram showing variation in de facto judicial independence across 4,321 non-democratic country years in the 1960–2007 period.[13] The measure of independence employed in Figure 1.3 is bounded by 0 and 1, and as can be seen there is

[13]The measure here is Linzer and Staton's (2011) de facto judicial independence measure. See Chapter 5 for a thorough discussion of this measure.

variation across almost the entire range of values (the actual minimum and maximum values observed are 0.02 and 0.94, respectively), with a mean of 0.25. The majority of observations fall on the left of the x-axis, as is to be expected given the fact that the data being analyzed consist of non-democracies. Despite this, there is variation even here, with a significant cluster of observations falling to the extreme left of the x-axis at approximately 0.05, and another cluster to be found in the 0.2–0.3 portion of the x-axis. In other words, even among those non-democracies with low levels of de facto judicial independence, a large amount of variation still exists. This variation compounded by the roughly 20% of observations with a judicial independence score at or above 0.4. Here, the observations cluster in around 0.45, and then spread out evenly as they move further up the x-axis. Clearly, there is more variation in judicial independence in authoritarian states than is commonly thought.

As this analysis and the information presented in Figures 1.2 and 1.3 make clear, significant variation in levels of judicial independence and political competition exists in non-democracies. Why the latter should be important for explaining the former is to what I now turn.

1.4.2 Why competition matters in non-democracies

The electoral logic of the political insurance account of judicial independence is fundamentally about the risks of no longer being in power, and expectations about the probability of losing power. When opposition political groups present credible electoral threats, rational politicians in power will establish or maintain independent judiciaries because they are an effective and inexpensive minoritarian institution. This is, of course, a probabilistic argument, even if it is typically framed in these deterministic terms in the literature: in instances of high competition, political actors should be more likely to establish independent courts or maintain existing independence.[14]

[14]When political actors might seek alternative means of protecting their interests or create alternative minoritarian institutions in the face of high levels of competition is to date under-examined,

There is nothing in the above account that is exclusive to democracies. Political leaders in autocracies face a similar expected utility calculation, a product of the risks of losing office and the probability that such an event might occur. Obviously, under a more typical non-democratic situation, when the level of competition is minimal or non-existent and the risks of losing power electorally are similarly minimal, the expectation would be that courts are weak and lack independence. When competition does exist, however, and opposition parties are able to provide credible alternatives to those in power, the expectation of losing office is similarly high.

The question that naturally follows is whether or not autocratic rulers ever fear losing power as a result of electoral defeat. The answer is undoubtedly yes, as Levitsky and Way explain in their thorough examination of non-democracies with electoral competition: incumbents cannot rest easy in such circumstances, as "Government officials fear a possible opposition victory (and must work hard to thwart it), and opposition leaders believe they have at least some chance of victory. In competitive authoritarian regimes, incumbents are forced to sweat." (Levitsky and Way 2010, 12) The "Color Revolutions" in the postcommunist world are some of the most striking examples of autocrats losing power as a result of elections (Way 2008). In fact, the Soviet Union itself provides similar examples of how competition can bring down even the most powerful-seeming regimes, as it was in many ways the opening of the electoral space to those not hand-picked by the party that led to many of the constituent Republics being controlled by anti-Union forces (Lieven 1994; Pabriks and Purs 2001; Smith 2001). Also demonstrating that the perception by autocrats that electoral risk is real is the careful attention they pay to elections in neighboring autocracies. The collapse of communism in Eastern Europe and the Soviet Union and the Arab Spring both illustrate how opposition can diffuse across borders and imperil regimes. Electoral defeats like those in the Color Revolutions are no different,

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and an area ripe for research.

and caused autocrats in neighboring countries to perceive the significant possibility of electoral defeat in the face of robust political competition (Myagkov, Ordeshook and Shakin 2005). Even when riddled with fraud and abuses of power, elections in non-democracies can have serious consequences, and competition clearly affects the expected utility calculus of those in power.

If the possibility of losing office is real and often perceived by those in power, the next issue is the other half of the calculation. Are the risks of losing office also real? The first risk that independent courts can insure against is the reversal of policies advanced by those previously in power. The example of post-Orange Revolution Ukraine demonstrates clearly the degree to which previous policies can be reversed after losing elections: after the defeat of his hand-picked successor, former President Leonid Kuchma saw some of his most critical policies reversed. His successor Victor Yushchenko shifted Ukraine's foreign policy, focusing more on relations with the European Union and the United States, as well as aligning Ukraine against Russia during its conflict with Georgia over South Ossetia. Similarly, linguistic policies in the country were vastly transformed (Olszański 2012), with Ukrainian implemented as the state language, removing Russian from a place of equality, a conflict that is still volatile to this very day (Polityuk 2012).

The second form of insurance offered by independent courts is their ability to guard against the violation of property rights, one of the reasons many political economists consider them one of if not the most critical component in the law-growth nexus (Feld and Voigt 2003; La Porta et al. 2004; Haggard, MacIntyre and Tiede 2008). Even in emerging democracies, control of the state can mean control of significant economic resources, as Gryzmala-Busse (2007) illustrates in her study of Eastern Europe. Such control is an even more lucrative business in the case of non-democratic systems (Levin and Satarov 2000). When courts are weak and justice is politicized, who owns what is a function of who holds the reins of power, not a result of rules and procedures established by and codified in law. Many observers of the Russian

political scene attribute the significant political battles among the elite that arose during the transition from the first presidency of Vladimir Putin to Dmitri Medvedev to the uncertainty surrounding the property rights of assets accumulated during the early and mid-2000s (Whitmore 2007).

The third threat against which independent courts can provide is insurance is the expropriation of political rights. Even in consolidated democracies, incumbents have access to resources that those out of power do not, and can use legal means to affect the playing field in their favor. In the US context, this is demonstrated by the continued attempts in some battleground states controlled by Republicans to pass laws designed to protect against "voter fraud," requiring forms of identification that groups that consistently vote Democratic systematically possess at a lower level (ACLU 2012). The phenomenon is more prevalent in consolidating democracies, where policies such as lustration laws are often instituted as a systematic mechanism to expropriate the political rights of those previously in power. Courts that are independent can and often do rule against governments, striking down lustration laws as unconstitutional (Cienski 2007; Horne 2009; Dimovski 2012). In non-democracies, such expropriation is ubiquitous, including outright removal of opposition leaders from electoral lists, lawsuits challenging opposition party registration procedures, and the use of other "administrative resources" against challengers (Colton and McFaul 2003; Popova 2012).

1.4.3 Why it matters more in non-democracies

Obviously, the expropriation of the economic and political rights of minorities is a significant risk of being out of office in the non-democratic context, and the significance of these risks are clearly more pronounced than in democracies. Because the logic of the insurance model is predicated on a calculation of the risks and probability of losing office, if the risks of losing office are more significant in non-democracies, the salience of competition should be even greater. In other words, competition when it is

present, should be more important in driving those in power to establish or maintain judicial independence. This is the second key argument advanced in this dissertation, and it will be subjected to empirical scrutiny in Chapters 5 and 6.

There are three main reasons that the risks of losing office should be greater in non-democracies. The first of these is that the risks associated with the expropriation of property rights after losing power is far, far greater than in democratic states. In autocracies, those who acquire state power also secure substantial economic assets and power to a degree unsurpassed in democracies (Tullock 1986; Robison 1988; Coolidge and Rose-Ackerman 1997). The access to economic resources that power grants raises the stakes of losing power to opposition groups even higher than they would be otherwise, as it also likelihood that those taking power will attempt to gain control of those resources for themselves, making the need for depoliticized justice all the greater.

Compounding this is the fact that the access to administrative resources and electoral fraud means there is a good chance that if the opposition is successful in taking power, the chances of prior incumbents returning to power are lessened. In other words, if you lose the other person gets not only your economic resources, but also control over the levers of power to keep you from returning to power. And, as the Color Revolutions throughout the postcommunist space have demonstrated, opposition can at times be successful at defeating incumbents even in the face of overwhelming administrative resources (obviously, political and economic expropriation need not be an either/or situation, as countless former autocrats—and quite a few democrats—can attest to).

The third risk of losing office that is significantly greater in a non-democratic state is the physical security of one's person. Put simply: it's a lot riskier to be a former leader in a non-democracy. Figure 1.4 charts four types of post-tenure fate a state leader can experience in the year immediately after leaving office.[15] As can

[15]This data is drawn from the Archigos dataset (Goemans, Gleditsch and Chiozza 2009), which

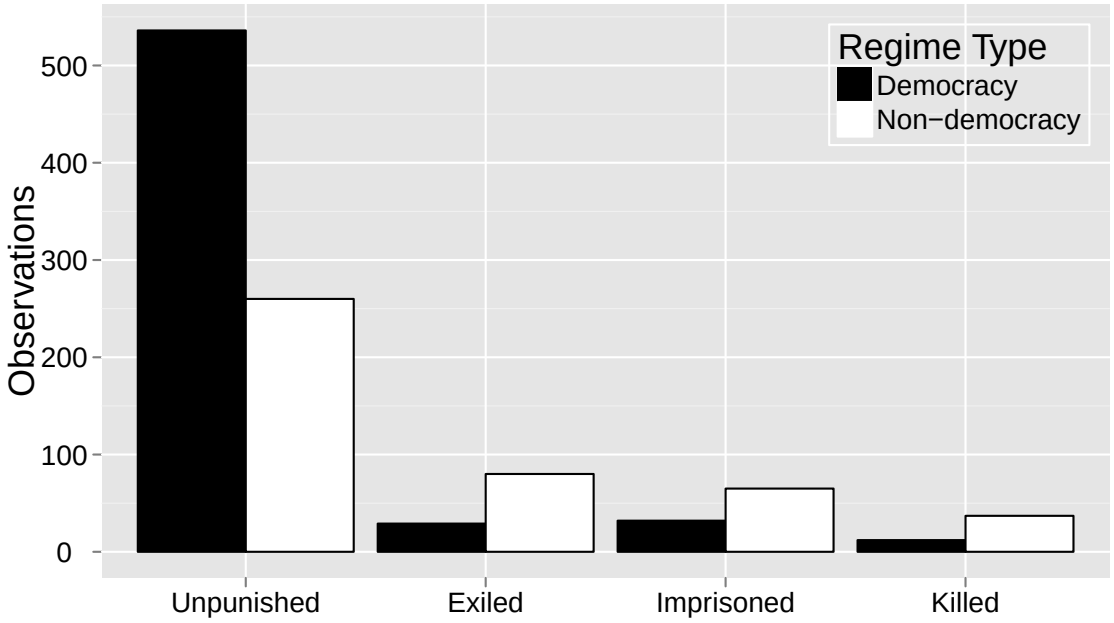


Figure 1.4: **Leader fate by regime type.** A chart showing four types of post-tenure fate experienced by leaders between 1960 and 2000, by regime type.

be seen, former leaders of a democratic state rarely experience negative outcomes of being in office, at least as measured by whether they go to prison, are exiled, or killed. These figures, in fact, *deflate* the security that democracy usually offers, as the plot is by the type of regime in the *previous* year. As such, a significant portion of the democratic leaders experiencing problematic fates are due to a collapse of democracy. If one considers only the type of regime the year after the leader was in office, i.e. the year being analyzed in the data shown in Figure 1.4, democratic leaders are much safer, as the likelihood of each negative outcome falls by a third. Being a non-democratic leader, however, is quite dangerous. Over 40% of former leaders in autocratic states were punished after leaving office. And the majority of the time the punishment isn't exile to a friendly state and a comfortable retirement: almost 60%

will be thoroughly discussed in the following chapter.

of these punished leaders were either imprisoned or killed, which is the fate of almost 25% of all—punished and unpunished—former autocrats.

Unsurprisingly, autocrats who lose power as a part of democratic transition are less likely to be punished than former autocrats in states that remain non-democratic. That being said, however, the rate is still high: nearly one out of five find themselves punished. This contrasts sharply with former democratic rulers in democracies, where only one in twenty suffer punishment after losing power. It is clear that the physical integrity of former autocrats is significantly more at risk when compared to their democratic counterparts. This, the most basic of securities and the most significant of risks to a former leader, suggests strongly that the logic of the insurance model should be of even more salience outside of the democratic context. Risk is of vital importance in assessing the potential benefits of independent courts, and risk is significantly greater in the non-democratic context.

Finally, it is possible that competition is more critical in autocracies because it is less expected, and as a result both signals greater competition and greater uncertainty than it would in democratic regimes. Even in emerging democracies, there is expectation of regular political competition and the occasional replacement of those in power, even if such expectations vary according to the degree of competition in the system.[16] In electorally-open (to varying degrees) non-democracies, viable opposition parties should signal to both the regime and other actors (including the electorate) that there is a substantial degree of weakness and the potential for change. Given the nature of the regime, these signals should be much more significant than they would be in a democratic context. Put differently, in a democracy, there always exists at some level the expectation of the possibility of electoral turnover removing those in power, some baseline level of fear of being forced to hand over power that does not exist or does not exist to the same degree in autocracies. This is most certainly the case

[16]This is related to democracy being the "institutionalization of uncertainty," as forwarded by Przeworski (1991).

in younger democracies, which witness significantly higher levels of electoral volatility than long-established democracies (Roberts and Wibbels 1999; Epperly 2011). This logic would suggest that the effects of viable political competition should be greater in autocracies because this opposition—given the ability of those in power to manipulate results—is more viable than its percentages might suggest, and because—given the higher stakes—the need for insurance is greater.

The logic of the insurance model hinges on the risks of losing power and the uncertainty around such an event happening. When the possibility of losing power is real, rational leaders will establish or maintain minoritarian institutions capable of protecting those in the political minority from being trampled by the majority. Judicial independence is an inexpensive and effective minoritiarian institution, and as a result when this possibility is higher we should expect higher levels of judicial independence. Political competition signals to those in power that the possibility of losing office is real. When an opposition party is able to control a significant portion of the electoral space, political leaders face the real threat of finding themselves out of office. This logic is in no way restricted to democratic contexts: competition—when it exists—in autocracies should produce the same fears and incentives for those in power. In fact, as much of the force of the insurance model flows from the risks faced by leaders after leaving office, the model should be of even more explanatory value in the non-democratic context: competition in non-democracies should be of more salience for judicial than competition in democracies, because the insurance offered by independent courts should be more valuable. Whether or not independent courts provide insurance is the topic of the next chapter. The subsequent chapters in Sections 2 and 3 test the two key arguments of this dissertation: that competition matters in non-democracies, and that it matters more.

1.5 Democracy

Fundamental to the tests of these arguments about political competition, judicial independence, and regime type is the question of what constitutes a non-democracy. Contrary to what many might expect, the direct relationship between democracy and de facto judicial independence is not obvious. Many, particularly those whose default reference point for democracy is the American experience, consider judicial independence as a defining characteristic of democracy, and many measures of democracy either implicitly or explicitly consider it as such; it is found in the executive constraints component in Polity scores, for example, as well as the civil liberties index that is part of the broader Freedom House measure.

1.5.1 Concept

On a conceptual level, there are serious reasons for concern when a measure of judicial independence is included—either implicitly or explicitly—in a measure of democracy. First, it nests a measurement of a difficult to measure, latent concept into a measure of another difficult to measure, latent concept. One can more or less easily determine whether or not a given country in a given year has a parliament in which opposition parties have seats, or whether a country has elections (although determining if they are free and fair is another matter). The inclusion of a measure of de jure judicial independence could be straightforward enough, and convey whether or not a country's constitution contains provisions ensuring that its courts are independent according to the law. This, however, only tells an observer whether or not a country has a constitution that enshrines independent courts, not whether courts are independent. Given that there is little or no association between de jure and de facto judicial independence (Ríos-Figueroa and Staton 2009; Tate and Keith 2009), and that countless non-democracies have constitutions enshrining judicial independence, including such in a measure of democracy is meaningless, similar to counting the Soviet Union as

having elections because its constitution enshrined them in law. Including de facto, behavioral independence would not present this problem, but then it returns us to the fact that it imports a difficult, latent measure into another latent measure.

The second problem is that a not insignificant number of countries widely considered democracies possess judiciaries that many would consider to be lacking independence vis-à-vis the "political branches" of government. Historically, both the Netherlands and the United Kingdom have made explicit commitments to legislative supremacy vis-à-vis the judiciary, to the point that some consider the courts to be seriously lacking in independence (Helmke and Rosenbluth 2009). While recent developments enshrining an independent judiciary with powers of judicial review have changed this in the United Kingdom, this example only highlights a related concern with considering judicial independence as either a necessary condition or important component of democracy: historically, independent judiciaries were rare beasts, even (or perhaps especially) in the long-consolidated democracies of Western Europe.

The positivist legal traditions that developed in the democratic states that emerged in Europe in the 19th and early 20th century viewed courts as obedient servants of the state, a tradition that went hand in hand with the republican ideals of the time and place. Here, judges were conceived of as agents of the elected principals in the legislature and cabinet, and their role was the follow the instructions set down by those principals to the letter (Scheppele 2002). It was not until after the collapse of European democracy in the 1930s and 1940s that this perspective was challenged, with the exception being inter-war Austria and Czechoslovakia. Elsewhere, Hans Kelsen's idea for a court whose role was partially independent of the dictates of its political principals, and with the power of abstract constitutional review, remained unrealized until the writing of new constitutions in the post-war era (Hirschl 2004). Obviously, if we consider an independent judiciary as a necessary or critical component of democracy, such empirical realities complicate the idea that democracy existed outside of the United States and a handful of other places before the 1950s.

If we give any credence to the republican tradition that the legislature, acting as an agent of the people, is or ought be sovereign, we are forced to conclude that a strong and independent judiciary should not be imported into how we conceptualize—let alone measure—democracy. At the most basic level, a Hamiltonian conception of an independent judiciary exists opposed to the democratic pillars of state institutions, as one of its explicit roles is to check the democratic impulses—the "occasional ill humors"—of the legislature and society (Hamilton 1788). To the extent that we conceptualize democracy as being something broader or more basic than its American variant, including judicial independence in our conceptions is both theoretically and historically problematic. Finally, a simple pragmatic reason exists for divorcing our conception of democracy and of judicial independence:[17] there is enough acrimonious disagreement as to what constitutes democracy (for evidence of this, look to the routine practice of scholars using their own measure of democracy in their work) that we do not need to nest within it another broad and vague concept that is itself subject to serious debate and disagreement.[18]

Even if one comes down on the opposite side of the fence regarding whether or not judicial independence is a necessary condition of democracy, it is beyond dispute that the inclusion of such in a measure of democracy makes determining the relationship between the two concepts problematic if not impossible. To the degree that we might care to understand whether judicial independence helps establish and consolidate democracy, or the reverse (Larkins 1996; Epstein, Knight and Shvetsova 2001; Howard and Carey 2004; Gibler and Randazzo 2011), it is necessary to be able to distinguish analytically between the two concepts. Similarly, if we think that independent courts play a role in other outcomes also affected by democracy such as

[17]This is separate from the pragmatic and compelling reasons offered by Ziblatt (2006) and Geddes (2007), regarding the need to break down big concepts to better understand their causes and effects.

[18]For an example of such debate and disagreement, see the edited volume by Burbank and Friedman (2002), which contains chapters such as "Judicial independence: How Can You Tell it When You See it? And, Who Cares?" and "Is Judicial Independence a Useful Concept?"

economic growth or the protection of human rights (Keith 2001; Feld and Voigt 2003; Powell and Staton 2009), we also need to be able to differentiate the concepts and produce correspondingly distinct measures.

One route is to break down the larger measures of democracy and use their constituent parts. For example, in their analysis of judicial independence and democratic backsliding, (Gibler and Randazzo 2011) remove the executive constraints portion of a state's Polity IV score, as this is in part determined by whether or not a country has an independent judiciary, and its inclusion in an analysis of the relationship between changes in independence and democracy would introduce a serious endogeneity problem. While this might to some degree mitigate the problem, it creates another: executive constraints other than the judiciary are often conceived of as a critical component of democracy. As democratic backsliding can very well and often does consist of the executive infringing on the prerogatives of the legislature—a phenomenon common in the strong presidential systems found in the postcommunist region (Easter 1997)—such a strategy makes identifying the exact goal of such an analysis impossible.

It furthermore introduces question of concept stretching and bias: if there exist even small systematic biases or mseasurement error in any given indicator of democracy (Bollen 1993; Munck and Verkuilen 2002; Treier and Jackman 2008), these patterns are likely to be replicated in the newly-separated measures of judicial independence and democracy. As a result, measures created by such bifurcation can be highly correlated, not as a result of association between the underlying latent concepts, but because of the measurement process itself. These two reasons suggest that serious problems can and often do result from splitting saturated measures of democracy into measures of multiple concepts and then employing them both in the same analysis.

1.5.2 Measurement

In their extensiveness discussion of democracy, Cheibub and Gandhi (2010, 68) argue that despite their high degree of correlation, measures of democracy are "significantly different in terms of both their theoretical grounding and operationalization and, for this reason, should not be treated as interchangeable... the choice of measure should be guided by its theoretical and empirical underpinnings." Drawing upon the earlier classification scheme presented by Przeworski et al. (2000), their conception and measure classifies a country as a democracy if it meets all four of the following criteria. First, the chief executive must be popularly elected, or chosen by a body that was itself popularly elected. Second, the legislature must be popularly elected. Third, more than one party must compete in elections. Fourth, alternation in power must have occurred, and the rules under which it occurred must be the same as those that brought the incumbent to power.

Although Freedom House's "Freedom in the World" index has been subjected to much criticism (see, for example Cheibub and Gandhi 2010) for an overly-broad conception of democracy, its component indices each more accurately represent an aspect of Freedom House's overall conception of democracy. The overall democracy score is a function of two separate measures, one determining the degree of political rights and the other the degree of civil liberties. In terms more common in the political science literature, the former encapsulates a more limited view of democracy, while the latter a more substantive or liberal view of democracy. For my purposes, the former is a good measure of democracy, as it classifies regimes according to whether people can freely participate in the political process: whether they are able to vote for alternatives in legitimate elections, compete for office, join political parties, and whether those elected affect political outcomes (Teorell et al. 2010).[19]

[19]The civil liberties index produced by Freedom House suffers from the problems discussed above, as it looks to whether a state has the rule of law, citizens have 'personal autonomy,' and a host of specific rights not directly related to the organization of political power.

At first glance, theses conceptions and measures (from here on out referred to as the Cheibub and Gandhi and Freedom House Political Rights measures) might not appear to adequately match my discussion of competitive authoritarianism, as it is at heart a minimalist conception of democracy. As noted above, however, more substantive conceptions of democracy typically require an independent judiciary, as well as other institutional outcomes better suited to a conception of the rule of law than to that of democracy (Levi and Epperly 2010). As Munck and Verkuilen (2002, 54) suggest, institutional outcomes such as de facto judicial independence, civilian control of the military, and the like are "aspects of the state as opposed to a regime type."

It is the fourth requirement of the Cheibub and Gandhi measure that prevents autocracies with political competition from being included as democracies, as it requires an alternation in power to occur that takes place according to the rules. For Freedom House, the critical threshold is the degree to which elections are free and fair. As long as Russian President Vladimir Putin, for example, continues ruling in a system with pervasive fraud, harassed opposition, and a tight grip on power, both concepts and measures would say that Russia is not a democracy. If, however, despite electoral irregularities Putin were to lose an election to the opposition and step down, both would surmise that the system was in fact a democracy at that point.[20] A contemporary example that explains this well is the Mexican general elections held on July 1, 2012: if the alternation in power ends up occurring according to the rules that brought incumbent President Felipe Calderon to power, Mexico will continue to be classified as a democracy. If they are not—regardless of which candidate eventually assumes the presidency—Mexico will no longer be classified as a democracy by

[20]An alternation that does not occur according to the previous rules, such as an alternation resulting from a popular uprising against fraudulent elections that leads those in power to step down does not meet this criteria as conceived, but as will be noted in Chapter 6, this coding is not always consistent, and the degree to which the Cheibub and Gandhi measure retroactively codes previous years a democracy is problematic in certain instances.

either conceptualization or measurement. Throughout the empirical analyses in this dissertation both measures of democracy will be deployed in tandem, or individually, as appropriate.

Chapter 2

TESTING THE ASSUMPTION OF INSURANCE

Although it has been noted that in the past decade and a half "strategic approaches have recently emerged as a leading framework to investigate" judicial independence (VonDoepp and Ellet 2011), one of the most fundamental assumptions of these insurance models has remained untested to date. A key assumption of this electoral logic of judicial independence forwarded in this dissertation is that independent courts *do* in fact provide benefits to those recently out of office; without the provision of these benefits independent courts are only costly, inhibiting political actors' prerogatives while still in office. Despite the importance of such provisions, in the existing literature the ability of courts to provide insurance for policies or prerogatives is merely assumed, or at best on occasion justified by reference to anecdote rather than systematic analysis. Lacking such, insurance models of judicial independence are at best incomplete and at worst rest upon shaky foundations. Thus, before turning to empirical tests of my main arguments that competition matters for judicial independence in non-democracies, and in fact matters more than in democracies, I examine the underlying assumption that courts do in fact provide a form of political insurance.

Using global data on the fate of leaders after leaving office, this chapter offers a first test of this assumption of the strategic approach to explaining judicial independence. I find that judicial independence is a strong and consistent predictor of the post-tenure fate of leaders, providing evidence for the validity of this key assumption and offering ancillary evidence for the accuracy of strategic models of judicial independence. The chapter proceeds as follows. First, I present a novel way of testing the assumption of the model: the post-tenure fate of leaders. I then present measures of

both post-tenure fate and judicial independence. After this I present the results of a number of logistic regression models and interpret these models via simulation and visualization. I conclude with a discussion of the implication of these findings.

2.1 The assumption of insurance

One of the key assumptions of the strategic model of judicial independence is that courts decrease the uncertainty of being out of power: independent judiciaries serve as insurance, decreasing the likelihood that a number of negative outcomes come to pass after a leader or party leaves office. Ginsburg's (2003, 32) conception is that independent courts serve most fundamentally as a "risk-reduction device," offering post-tenure protection. Ginsburg's monograph is arguably the most exhaustive treatment of the insurance model, and he goes to great lengths to demonstrate the role that competition played in creating independent courts in Taiwan, Mongolia, and Korea, and explain the variation in the degree to which constitutional courts in these countries were able to assert themselves as independent actors after the establishment of democracy. When it comes to a systematic analysis of whether or not these courts were effective at providing insurance in the cases studied, however, Ginsburg's otherwise impressive work is less convincing, offering a handful of cases where courts ruled against those in power as evidence of the efficacy of insurance.

Arguably the most critical test of whether a judiciary is able to offer insurance to outgoing leaders is whether or not said leader is able to save his (or her) skin. Although traditional conceptions of the insurance model focus primarily on policy continuity (Landes and Posner 1975; Ramseyer 1994; Ginsburg 2003), the logic articulated in the last chapter suggests that there is little reason to assume insurance need stop there, as one's physical security is also an important factor that those formerly in power must consider (as will be shown subsequently). In fact, making the traditional assumptions of self-interested actors, a leader's post-tenure fate is the *fundamental* test of the insurance assumption: that a court can maintain a given policy is nigh

meaningless if it is unable to prevent those newly-empowered from exiling, imprisoning, or killing those recently out of office. In other words, following the insurance logic articulated in the literature, if independent courts are unable to provide insurance for the physical security of leaders after leaving office, we must question the models of political insurance that currently dominant in the literature.

As noted, the critical test employed here is the fate of leaders after leaving office. An independent judiciary that is unable to be manipulated by those newly in power after a leadership transition should be significantly less likely to be employed in politicized prosecutions of previously leaders. On the other hand, where judges and the courts are dependent on political elites and subject to significant political pressure, the security of those who have left office should be lower. The history of the second half of the twentieth century is replete with the phenomenon of leaders, both in democracies and non-democracies, that have found themselves faced with legal sanctions in the period immediately after leaving office: according to the leading data on the question, of the approximately 1,000 leaders in the 1960–2000 period, slightly more than 20% have faced legal sanctions in the year immediately following their departure from power (Goemans, Gleditsch and Chiozza 2009). In many if not most cases, critics and observers have noted the political nature of such prosecutions, and often the strong-arming of judiciaries that goes along with them. Not only might independent courts lead those who take power to refrain from the politicized prosecution of former leaders because they doubt their inability to influence the judiciary, but they may also do so for fear of sparking public outcry. In the democratic context, Vanberg (2001, 2005) argues that the fear of public backlash and electoral punishment causes those who occupy the political branches to refrain from violating judicial independence. Even in a one-party autocracies, a context in which the salience of backlash should be less critical, overt pressure on and meddling with the judiciary can result in public outcry and protest (Moustafa 2003, 2007). Such outcomes should not be limited to prosecutions, but extend to the physical security of former leaders.

In a context where a strong and independent court system exists, those who assume power—by legitimate means or otherwise—might be less likely to risk flouting the law by either forcing former leaders into exile or murdering them.

At first glance, using the frequency of judicial rulings against the government as some suggest might appear as an acceptable alternative way of testing the assumption that independent judiciaries do in fact provide insurance. Unfortunately, using such a direct mechanism to test the implications of the insurance model is impossible because of the strategic nature of judicial decision-making. Two theoretical approaches to judicial decision-making illustrate why this is the case. First, judges may be ruling for or against any government policy or action because of their true preferences (for a larger discussion of this attitudinal model of judicial decision-making, see Segal and Spaeth 2002), and these preferences need not always be in opposition to upholding or strengthening the power of the executive. In other words, there is little reason for an actor to expect an ability to predict the preferences of appointed judges, or that such preferences are static over time (Martin and Quinn 2002). If the attitudinal model is correct, one cannot infer from a ruling against the government that a court serves an insurance function.

The second approach argues that even if judges do desire to act on their preferences, the attitudinal model is incomplete because their decisions are constrained by the strategic nature of interbranch relationships (Gely and Spiller 1990; Epstein and Knight 1998). Here, judicial decision-making is strategic, depending not only on the court's makeup but also the makeup of other branches of government (Ferejohn and Shipan 1990).[1] Given that both of these change over time, the discretion with

[1]It is worth noting here that although they share a focus on self-interested political actors and the word 'strategic,' the strategic model of judicial independence and the strategic model of judicial decision-making are not analogous. The latter focuses on the strategic decisions made by judges, i.e. judicial rulings taking into account the expected responses of other political actors. The former focuses on the strategic decisions made by those in power (typically the executive), i.e. the decision whether to establish or maintain judicial independence taking into account their expectations of remaining in power.

which courts can rule against the government also changes (McNollgast 1992, 1994; Chavez, Ferejohn and Weingast 2003). Outside the context of stable democracies, a government's strength and hold on power, rather than cohesion across branches, has also been shown to affect judicial decision-making (Helmke 2005). Similar to the attitudinal model, if the strategic model of judicial decision-making is correct, testing whether courts serve as insurance is not possible by looking at judicial decisions.[2]

An effective test of the assumptions of the insurance model needs to determine whether or not judicial independence is associated with an outcome that those who might in the future no longer be in power would care about, and whether such an outcome might be minimized by an independent judiciary, but one that is not directly implicated by a strategic or attitudinal model of judicial decision-making. In other words, an outcome in which the existence of judicial independence might constrain *other*, non-court actors, from infringing on the rights of those who have recently lost power. Post-tenure fate is just such a test.

2.2 Data

The two main variables needed to test the assumption of insurance in the manner specified above are the fate of leaders after leaving office (the dependent variable) and the degree to which courts exhibit de facto judicial independence (the independent variable of interest). In this section I discuss measurement issues for these two variables and the data employed.

2.2.1 Post-tenure fate

Until recently, comprehensive data on the fate of leaders was unavailable. With the publication of the Archigos data, however, this has changed (Goemans, Gleditsch

[2]Whether either the attitudinal or strategic model of judicial decision-making better explain judicial decisions is not directly relevant here. For discussions and tests, see Bergara, Richman and Spiller (2003) and Jacobi (2006).

and Chiozza 2009). These data compile (among other things) the post-tenure fate of leaders of 188 countries for the 1875–2004 period. The Archigos data identifies and codes the effective ruler of a given polity, that is, the person with de facto control over government. As such, these data do not focus on ceremonial heads of state such as sovereigns in constitutional monarchies and presidents with ceremonial powers such as those in Germany and Estonia (but do in presidential parliamentary systems with strong presidencies, such as France and Russia).[3]

One of the unique features of Archigos is that is does not code observations in a country-year format, but rather in a leader-year format, allowing for the analysis of the fate of *leaders*, rather than countries. For post-tenure fate coding, the authors's coding "records one of four types of post-exit fates: when a leader suffers 1) no punishment, 2) is exiled, 3) imprisoned, or 4) killed." (Goemans, Gleditsch and Chiozza 2009, 273). The coding of this variable is explicitly ordinal, as the authors only code the highest form of punishment any given leader receives (for example, imprisonment includes includes house arrest, and therefore if a leader is put under house arrest before being exiled, they are coded as having been imprisoned rather than exiled). Figure 2.1 illustrates the relative frequencies of the four categories of leader outcomes in the Archigos data (unlike Figure 1.4, it is not separated by regime type). As can be seen, while during this period most leaders did not face problematic experiences after leaving power, a full quarter of those who held the highest office were either exiled, imprisoned, or killed after leaving office.

Although as noted due to the coding scheme the Archigos data are explicitly ordered, they need not be used in this manner, and in the next section I will analyze these data in a variety of different ways to better understand the relationship between judicial independence and post-tenure fate. The first way in which the Archi-

[3]One of the most admirable aspects of the data collection efforts has been how hard the authors strive for transparency: approximately 750 pages of case descriptions and justifications for coding decisions are available on the data's website: `http://www.rochester.edu/college/faculty/hgoemans/data.htm`.

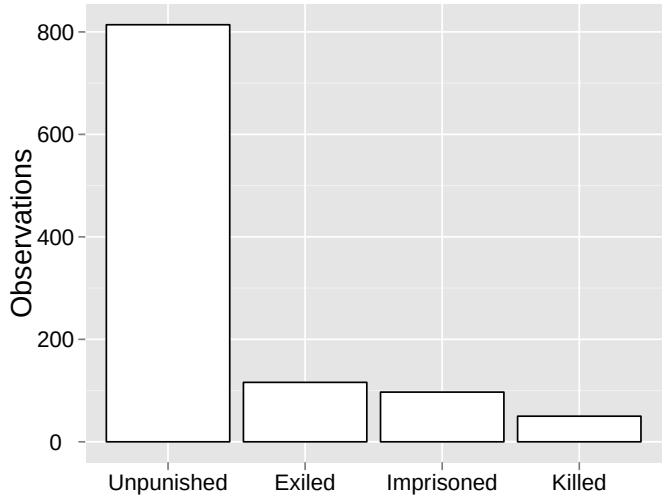


Figure 2.1: **Four categories of post-tenure fate.** A histogram showing the relative frequency distribution of the four categories of post-tenure fate of leaders from 1960 to 2000.

gos data will be analyzed is by dichotomizing the outcome, considering all instances of post-tenure punishment as one category. Although given the paucity of data within each of the three punishment categories as compared to the "baseline" category of no punishment, there are methodological reasons for doing so, more compelling is the theoretical reason. A leader worried about his or her post-tenure fate is likely most worried about *any* punishment, rather than the specific nature of the punishment imposed by those succeeding in power. Put simply: exile, imprisonment, and execution are unhappy outcomes to be avoided, and insurance if effective would likely be insurance against all forms of extreme punishment. If a leader is "only" exiled, it can probably be said that any insurance policy that he or she hoped an independent judiciary might provide was one that could not be collected.[4]

The second way to employ the post-tenure variable in analysis is as a cate-

[4]An additional reason for collapsing the punishment categories in some models is the fact that the coding of the data do not inform us whether or not a given leader suffered multiple types of "punished" fates.

gorical variable, either as an ordered variable (as is implied in the coding scheme) or as an unordered, nominal variable. The ordered categorical implementation follows the coding scheme developed by the creators of the Archigos data. One could also envision the different categories as being qualitatively different, rather than ordinal in nature. Because of this, I also employ the post-tenure fate variable as a nominal categorical variable, to both this theoretical reason and to test the parallel regressions assumption inherent in ordered probit/logit.

2.2.2 Judicial independence

A recent survey of measures of judicial independence by Ríos-Figueroa and Staton (2009) is a commendable example of careful attention to the relationship between conceptualization and measurement. The authors examine the relationships among 13 different measures of judicial independence, concluding with cautious optimism that the extant measures of de facto independence possess both convergent and construct validity, while pointing out that serious issues of missingness exist in nearly all measures. That the missing information is not missing at random means that bias in models employing these measures must be accounted for, which has generally not been done in the cross-national quantitative literature on judicial independence and economic development (e.g. Feld and Voigt 2003; La Porta et al. 2004). Generally, the missingness in these measures is correlated with the level of economic development (with one exception, which we will soon address). In fact, this is often so much the case that in one common measure, the average income of states with no missing data is four times that of states where there are missing values for country-years (Ríos-Figueroa and Staton 2009).[5]

There is also, unsurprisingly, a temporal dimension to this issue, with more

[5]This is the case for the Fraser Institute's measure of the legal structure and security of property rights, from their annual *Economic Freedom of the World* report. See: `http://www.freetheworld.com/`http://www.freetheworld.com/

missingness (both in poor and rich countries) the further back in time one goes. In fact, for many measures the problem unfortunately holds even in the 1990s and 2000s. Contract Intensive Money (CIM), for example, which measures the degree to which economic transactions occur without hard currency being exchanged, has been used as a proxy capturing general trust in courts, and is one of the few measures that is available for a long enough period to capture a reasonable time-series. Even it has its limits, however. In the 1990–2005 period, for example, nearly 20% of the CIM country-year observations in Eastern Europe and the former Soviet Union have missing values, and multiple countries are completely absent.

Prior to the work of Howard and Carey (2004), no measure of de facto judicial independence was available that met the criteria of being replicable, systematic, and available for more than a limited selection of states. The measure they created is based on the US State Department's annual *Country Reports on Human Rights Practices*,[6] which specifically discusses whether or not the judicial system is free from the undue influence of other branches of government, as well as if bribery and corruption are problems facing the institution. The State Department reports are an effective source of information, as they are the yearly product of in-country experts, and their discussions of judicial independence are independent of any broader concerns regarding the level or status of democracy in the country.[7] While a worry could be that systematic biases affect these reports, scholarship finds that with regards to judicial independence, no such bias exists (Poe, Carey and Vazquez 2001). Howard and Carey's measure is trichotomous, with each country-year being coded as having low, moderate, or high judicial independence.

Tate and Keith (2007) and Cingranelli and Richards (2010) have also offered measures of judicial independence that draw upon the same underlying data as the

[6]Available at `http://www.state.gov/g/drl/rls/hrrpt/`http://www.state.gov/g/drl/rls/hrrpt/ accessed July 13, 2010.

[7]Like many series of State Department reports, however, they except the US from analysis, potentially a problem if one is interested in global analyses.

Howard and Carey measure, and these measures have received a fair amount of attention and use in the literature. The three measures are predictably (given that they share underlying data) highly correlated, although each group employs slightly different coding schemes.[8] Two alternative measures are derived from expert and popular surveys: one of these is produced by a non-governmental organization, the Fraser Institute (Gwartney and Lawson 2007), while the other is produced by the firm Political Risk Services Group (2012). Finally, a handful of others based on a variety of sources are produced by groups of academics, relying on methods such as expert coding and the aggregation of economic data (Feld and Voigt 2003; Bertelsmann Transformation Index 2008). One of these, created by Henisz (2000, 2002), will be discussed in more detail in the following chapter.

Until recently, researchers were forced to choose one or more of these indicators of de facto independence based on their specific needs and the coverage the indicator was able to offer. As most of these measures agree in many cases and are correlated with one another, there is reason to suspect the literature is moving in the right direction (Ríos-Figueroa and Staton 2009). A key factor in determining which measure to use in a given study has often been the issue of coverage, both temporal and cross-sectional. Unfortunately, the indicators with the most internal and external validity (such as the Tate and Keith measure employed in the Chapter 6) are often crippled by modest temporal coverage (for example, only being available for the 1990s), while those with the best temporal and cross-sectional coverage are those with less transparent and more problematic linkages with the concept of de facto judicial independence (Ríos-Figueroa and Staton 2009; Linzer and Staton 2011).

In recent work, Linzer and Staton (2011) address this problem, offering a new measure, based on a hetereoskedastic graded response IRT (item response theory)

[8]Tate and Keith (2007), for example, criticize Howard and Carey (2004) for considering due process and other components in their measure that do not explicitly relate to the autonomy and power of the judiciary.

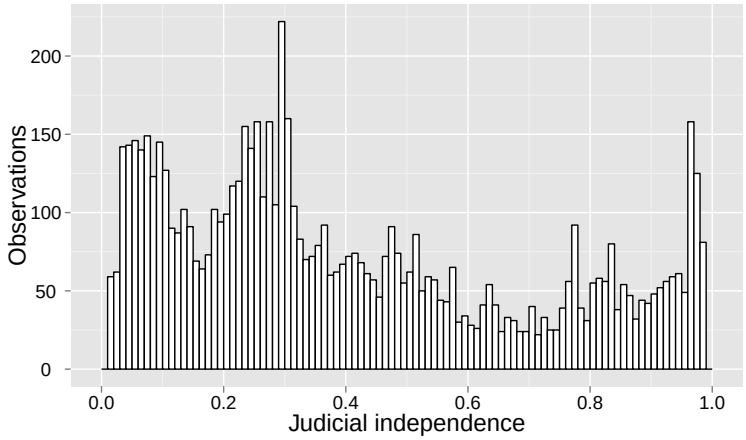


Figure 2.2: **Linzer and Staton's measure of judicial independence.** A histogram showing the relative frequency distribution of Linzer and Staton's (2010) IRT model measure of de facto judicial independence.

model they develop for time-series cross-sectional data. It uses eight of the common measures of judicial independence, taking into account missing data and directly modeling the time dependence inherent in the concept. The final value created for each country year is a value bounded by 0 (minimal independence) and 1 (maximal independence). Their measure takes into account a number of concerns that scholars analyzing judicial independence must face, and "makes use of the general agreement among the indicators, yet addresses concerns resulting from measurement error and missing data." (Linzer and Staton 2011, 13)[9] The measurement model draws upon the agreement found between the varying measures in years where they overlap, and uses this to better estimate the degree of de facto judicial independence in those years where coverage is only provided by measures with less content validity. Figure 2.2 plots a histogram of the relative frequency distribution of Linzer and Staton's measure of de facto judicial independence. It shows that significant variation exists across the value of the measure, with significant portions of observations possessing both low

[9]For a more thorough discussion, consult Linzer and Staton's (2011) paper, which is entirely devoted to presenting the details and results of their method.

and high levels of independence.

2.3 Analysis

As noted above, to begin examining the relationship between judicial independence and post-tenure fate, I collapse the categories of post-tenure punishment coded in the Archigos data set into one category. This creates a dichotomous variable, coded 0 when a leader is punished in the year after power and coded 1 when the leader is unpunished. Table 2.1 presents the results of eight hierarchical (multilevel) logistic regression models where the intercept is allowed to vary by country (random effects).[10] Each model includes a different combination of covariates that might potentially affect a leader's post-tenure fate. Because of issues of missing data for some of these, the number of observations (reported) differs between many of the models. As they are thus being fit on different data, the use of single-number fit statistics (such as the BIC, which is reported) is problematic. As the goal, however, is to ascertain whether judicial independence is associated with post-tenure fate, such comparisons will be set aside.

The first model in Table 2.1, labeled Null, only includes estimates for the overall intercept and the variable intercepts (because there are well over 100, each of these is not listed, as is common). Model 1 adds the Linzer and Staton measure of judicial independence, which is not only highly significant as a covariate but also produces a much better fitting model according to the difference in the BIC scores between the two models (as the data being analyzed—here the number of observations—is the same, a comparison between these two models is acceptable). Comparing across Models 2–7, all of which also include a bivariate measure of democracy, it can be seen that the importance of judicial independence in predicting post-tenure fate is

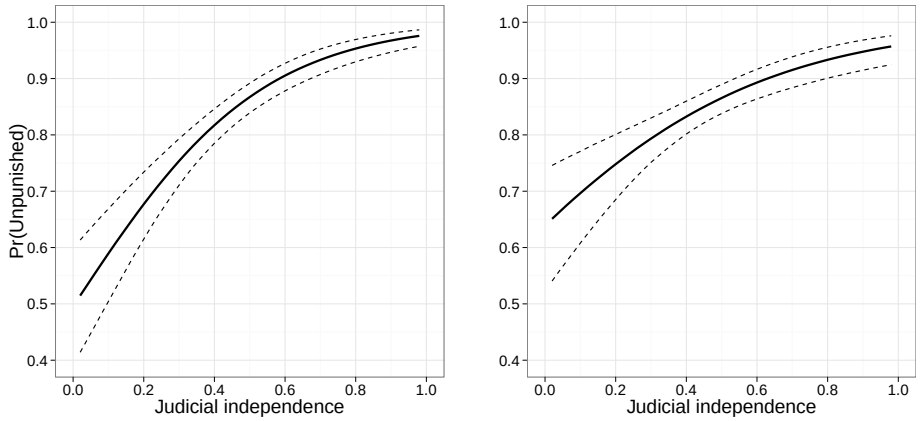
[10]These models were produced using the `glmer` function in the *R* package `lme4` (Bates, Maechler and Bolker 2011). For a discussion of fixed vs. random effects specifications, see the chapter appendix.

Table 2.1: **Hierarchical logistic regression results.** Eight hierarchical logistic regression models of the post-tenure fate of leaders from 1960–2000, with intercepts varying by country (country random effects). The dependent variable measures whether a leader was punished (exiled, imprisoned, or killed) in the year after leaving power. As can be seen by the fluctuations in the N, the inclusion of certain covariates significantly lowers the number of observations available for analysis, making the comparison of model fit via single-numer statistics such as BIC difficult. Asterisks denote the typical 0.05, 0.01, and 0.001 levels of statistical significance.

	Null	M1	M2	M3	M4	M5	M6	M7
Judicial independence		5.74***	3.79***	2.59***	2.22**	2.82***	2.36*	2.29***
		(0.46)	(0.57)	(0.61)	(0.68)	(0.83)	(0.92)	(0.65)
Demoracy			1.53***	1.47***	1.41***	1.19***	1.22***	1.61***
			(0.25)	(0.24)	(0.26)	(0.29)	(0.31)	(0.26)
log(GDP/capita)				0.37***	0.43***	0.39**	0.42*	0.34**
				(0.10)	(0.12)	(0.14)	(0.18)	(0.10)
Major war				−0.29				
				(0.24)				
Inflation					0.00		0.00	
					(0.00)		(0.00)	
Growth					0.03**		0.04**	
					(0.01)		(0.02)	
President elected by assembly						1.33**	1.18*	
						(0.41)	(0.52)	
Parliamentary						0.28	0.25	
						(0.34)	(0.35)	
Years democratic								0.01
								(0.01)
(Intercept)	1.38***	−1.06***	−0.92***	−3.18***	−3.56***	−3.55***	−3.67**	−3.04***
	(0.15)	(0.18)	(0.19)	(0.67)	(0.77)	(0.97)	(1.18)	(0.68)
Country random effects	2.00	0.25	0.38	0.24	0.17	0.18	0.27	0.17
	(1.41)	(0.50)	(0.62)	(0.49)	(0.41)	(0.42)	(0.52)	(0.41)
BIC	1103.23	924.93	891.89	891.25	733.33	589.90	527.93	832.95
N	1077	1077	1077	1077	905	751	689	1005

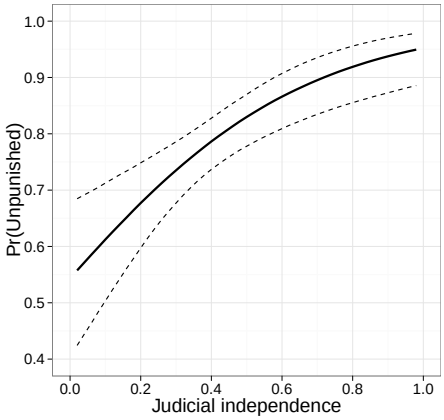
maintained regardless of the inclusion of other covariates and the concomitant drop in the number of observations.

Because interpreting coefficients in non-linear models is not straightforward, I plot the expected probability of a leader's *unpunished* post-tenure fate in Figure 2.3. Each plot is the result of 10,000 simulations in which the value of judicial independence is allowed to vary from its observed minimum (0.02) to its observed maximum (0.99) while all other covariates are held constant at their mean values. The plots show the



(a) Model 2

(b) Model 3



(c) Model 5

Figure 2.3: **Expected effects of judicial independence.** Three plots showing the expected probability of a leader *not* being punished as his or her post-tenure fate, for Models 2, 3, and 5 from Table 2.1. The solid black line is the point estimate drawn from 10,000 simulations where judicial independence is allowed to vary from its minimum to maximum observed values and all other covariates are held constant at their means. Dashes lines are 95% confidence intervals drawn from these simulations.

expected probability for Models 2, 3, and 5, to give a general sense of the substantive significant of judicial independence when the model and data vary.

The solid black lines in each are the point estimates of the probability of a "good" post-tenure outcome for that level of judicial independence, and the dashed lines show the 95% confidence intervals around these estimates, drawn from the 10,000 simulations. The plots in Figure 2.3 demonstrate that the expected effects of judicial independence are both statistically and substantively significant. At the lowest level of independence, holding all other covariates equal, the expected probability of an unpunished post-tenure fate in Models 2, 3, and 5 is approximately 0.65, 0.77, and 0.70, respectively. In none of these models is the expected probability of a good post-tenure fate when independence is at its maximum value less than 0.97. The reason that the predicted probability is so high even when judicial independence is non-existent is in part due to the distribution of the dependent variable: an unpunished post-tenure fate is observed in the data three times for every one time a punished post-tenure fate is observed, and as such the baseline probability of an unpunished post-tenure fate should be quite high. The results shown in Table 2.1 and Figure 2.3 provide strong support for the contention that judicial independence can provide effective insurance against unpleasant outcomes after leaving power, even accounting for the level of democracy or development, the instance of war, recent inflation and growth, the form of the electoral system, and the number of years a country has been a democracy.

Of course, as discussed above, the post-tenure variable from the Archigos data is originally coded as a categorical measure, with ordered values of unpunished, exiled, imprisoned, and killed. Discerning between the differences in an ordered logistic regression model is a more stringent test of the insurance hypothesis, due to the relative infrequency of leaders being imprisonmed or killed after leaving power rather than being unpunished. Table 2.2 shows regression output for six ordered logistic regression models of the post-tenure fate of leaders. Models 8a, 9a, and 10a are "classic" ordered logit in which all observations are pooled, while Models 8b, 9b, and 10b are partially-pooled, hierarchical (multilevel) ordered logistic regression models

in which the intercept is allowed to vary by country (country random effects).[11] The continuous covariates in each model were mean-centered to better estimate the cutpoints between each of the ordered categories. This has no effect on the estimates or errors of the covariates, and is commonly recommended for both hierarchical and non-hierarchical models where the estimation of the intercept(s) is crucial (Gelman and Hill 2007).

As can be seen, the regression output between each comparable classical ordered logit and the hierarchical version is almost identical. Interestingly, the classical ordered logistic regression models fit better, as well. The hierarchical version of each model only uses one more degree of freedom (to estimate the variance of the random effect), and the difference between the BIC between each comparable model (i.e. between Model 8a and 8b) is statistically significant, suggesting that the random effects do not help prediction.[12] The estimated cutpoints are statistically distinguishable from one another, in that the 95% confidence intervals around each do not overlap, which suggests we are able to ascertain which category a given observation falls into (if the cutpoints overlapped, a given estimate could likely be within the 95% confidence interval of more than one category).[13] Judicial independence is highly statistically significant across model specification and the estimated coefficient is robust to the inclusion of a number of different covariates. This provides further support for the insurance hypothesis, suggesting that independence is an important factor in predicting post-tenure fate when measured in an ordered manner.

Because the interpretation of ordered logistic regression is even more difficult

---

[11]The standard ordered logit models were produced with the `polr` command from the *R* package `MASS` (Venables and Ripley 2002), while the hierarchical ordered logistic models were produced with the `clmm` command from the package `ordinal` (Christensen 2012).

[12]This is in fact true, as the country random effects for every are at the level of the sixth to ninth decimal place (e.g. a 0.0000003 change to the intercept).

[13]This can be eyeballed by doubling the standard error of each cutpoint and comparing the interval to the neighboring cutpoint. For example, in the case of the 1|2 cutpoint, that would be comparing the 95% confidence interval of the first cutpoint $0.95 + (2 * 0.16)$, which extends 1.27, with that of the second, determined by $1.89 - (2 * 0.17)$, which is 1.55.

Table 2.2: **Ordered logistic regression results.** Six logistic regression models of the post-tenure fate of leaders from 1960–2000, with intercepts varying by country (country random effects). The dependent variable is ordinal from 1–4, the outcomes being no punishment, exile, imprisonment, or being killed in the year after leaving power. Models 8a, 9a, and 10a are all classical (pooled) ordered logistic regressions, while Models 8b, 9b, and 10b are hierarchical (multilevel) ordered logistic regression models where intercepts are allowed to vary by unit (country random effects). Continuous covariates were mean-centered to improve the estimation of cut-points, as is common. This has no effect on the estimates or errors for the covariates in the model (Gelman and Hill 2007). Asterisks denote the typical 0.05, 0.01, and 0.001 levels of statistical significance.

	M8a	M8b	9a	9b	10a	10b
Judicial independence	-2.17^{***}	-2.20^{***}	-1.87^{**}	-1.87^{**}	-2.24^{**}	-2.33^{**}
	(0.54)	(0.56)	(0.62)	(0.62)	(0.81)	(0.86)
Democracy	-1.34^{***}	-1.37^{***}	-1.27^{***}	-1.27^{***}	-1.01^{***}	-1.04^{***}
	(0.23)	(0.24)	(0.25)	(0.25)	(0.29)	(0.30)
log(GDP/capita)	-0.39^{***}	-0.39^{***}	-0.43^{***}	-0.43^{***}	-0.38^{*}	-0.39^{*}
	(0.09)	(0.09)	(0.11)	(0.11)	(0.15)	(0.16)
Inflation			-0.00^{*}	-0.00	-0.00^{*}	-0.00
			(0.00)	(0.00)	(0.00)	(0.00)
Growth			-0.03^{**}	-0.03^{**}	-0.03^{*}	-0.03^{*}
			(0.01)	(0.01)	(0.01)	(0.01)
President elected by assembly					-1.01^{*}	-0.99^{*}
					(0.49)	(0.50)
Parliamentary					-0.14	-0.13
					(0.31)	(0.33)
Cutpoint: 1—2	0.95^{***}	0.95^{***}	1.06^{***}	1.06^{***}	1.32^{***}	1.36^{***}
	(0.16)	(0.17)	(0.18)	(0.18)	(0.27)	(0.29)
Cutpoint: 2—3	1.89^{***}	1.91^{***}	1.89^{***}	1.89^{***}	2.13^{***}	2.18^{***}
	(0.17)	(0.18)	(0.19)	(0.19)	(0.28)	(0.31)
Cutpoint: 3—4	3.22^{***}	3.24^{***}	3.37^{***}	3.37^{***}	3.27^{***}	3.34^{***}
	(0.21)	(0.22)	(0.24)	(0.24)	(0.32)	(0.35)
Country random effects						0.12
						(0.33)
BIC	1454.15	1460.8	1182.99	1189.8	818.89	825.08
N	1077	1077	905	905	689	689

than standard logistic regression with a dichotomous variable, I plot the predicted proportional probability of each of the four categories of post-tenure fate in Figure 2.4. As before, other covariates are held at their mean values, and the trichotomous measure for electoral system is set to the value of an elected presidency. Because of the proportional nature of the predicted probabilities, at any given level of judicial independence the sum of the predicted probabilities of the four categories must be equal

to 1. The predicted line for an unpunished post-tenure fate (the solid black line) is incredibly similar to the plotted lines of the models in Figure 2.3 (although because of the different y-axis this is not necessarily apparent at first glance): at the lowest levels of judicial independence the probability of being unpunished is approximately 0.70, and at the highest level it is well over 0.90. It should be noted that the fact that the lines of the predicted probabilities for the three punished categories almost overlap is not necessarily an issue. This is because these plot the predicted probabilities of a change in the variable and at all points the four lines must sum to 1 (in fact in many ordered logit models these lines would actually cross as key covariates change in value). In other words, even if it were the case that each of the four categories had a 0.25 probability at some value of the covariate (and thus the four lines would intersect), it does not mean we cannot discern the likelihood that any given observation would fall into a given category. For individual plots showing the predicted marginal effects for each outcome category and their 95% confidence intervals, see the chapter appendix.

Although the post-tenure fate variable is by design coded as an ordinal measure, one might question whether in reality there is an ordered structure to the four categories of post-tenure fate, and that imprisonment is not always worse than exile. Despite the fact that the coding structure forces such an ordered relationship, Brant tests of the models suggest that the parallel regressions assumption of ordered logistic regression is violated for a number of covariates. Because of this, and to provide yet another test of the insurance hypothesis, I conduct multinomial (polytomous) logistic regression, which assumes that there is no necessary order to the dependent variable, instead treating the categories as nominal.

Table 2.3 shows the results of two multinomial logistic models with post-tenure fate as the dependent variable, and the unpunished category set to the reference/baseline category. The estimates of the covariates for each of the three levels (exiled, imprisoned, killed) report how that variable changes the predicted probability

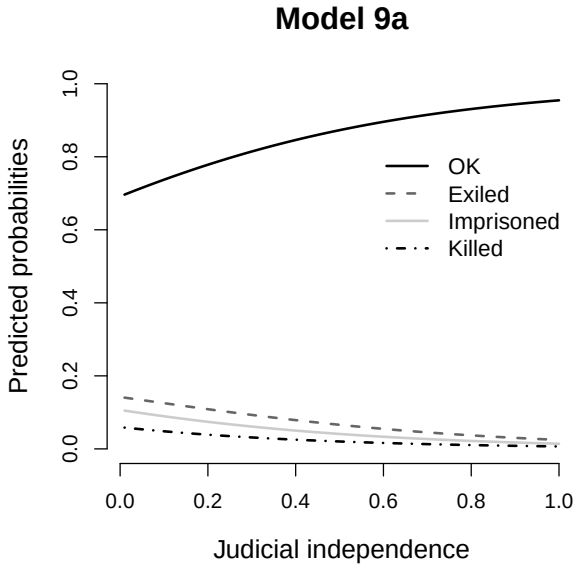


Figure 2.4: **Predicted probabilities of post-tenure fate.** A plot showing the predicted proportional probability of each of the four categories of post-tenure fate. The plot is the predictions of the ordered logistic regression in Model 10a, with all other covariates held at their mean values and the electoral system set to an elected presidency.

of being in that category as compared to the baseline category of being unpunished. As interpreting the results of multinomial logit models via regression output is effectively impossible, Figure 2.5 plots the probabilities for each category across regime type. The procedure is the same as that in Figure 2.4, and shows the same general relationship between judicial independence and the three categories as the plots in Figures 2.4 and 2.3 (as with the ordered logit results, plots of the marginal effects for each outcome category with confidence intervlas are presented in the chapter appendix).

Unsurprisingly, in both Model 11 and 12 the estimate for judicial independence is not statistically significant for the fourth category, killed. This is unsurprising due to the comparably small number of observations in this category (50 leader-years

Table 2.3: **Multinomial logistic regression results.** Two multinomial (polytomous) logistic regression models of post-tenure fate from 1960–2000, with a baseline category of unpunished. Continuous covariates were mean-centered to improve the estimation of cut-points, as is common. This has no effect on the estimates or errors for the covariates in the model (Gelman and Hill 2007). Asterisks denote the typical 0.05, 0.01, and 0.001 levels of statistical significance.

	Model 11		Model 12	
	Estimate	Std. error	Estimate	Std. error
Category 2: Exiled				
Intercept	-1.95^{***}	(0.24)	-2.09^{***}	(0.28)
Judicial independence	-3.78^{***}	(0.80)	-3.39^{***}	(0.95)
Democracy	-1.26^{**}	(0.32)	-1.31^{***}	(0.37)
log(GDP/capita)	-0.19	(0.11)	-0.20	(0.15)
Inflation			-0.00	(0.00)
Growth			-0.04^{**}	(0.14)
Category 3: Imprisoned				
Intercept: 3	-2.00^{***}	(0.24)	-1.98^{***}	(0.26)
Judicial independence	-1.90^{*}	(0.80)	-1.54	(0.95)
Democracy	-1.26^{**}	(0.34)	-1.25^{**}	(0.35)
log(GDP/capita)	-0.54^{***}	(0.13)	-0.63^{**}	(0.15)
Inflation			-0.00	(0.00)
Growth			-0.01	(0.01)
Category 4: Killed				
Intercept: 4	-2.37^{***}	(0.29)	-2.68^{***}	(0.35)
Judicial independence	-1.00	(1.01)	-1.09	(1.19)
Democracy	-1.59^{**}	(0.46)	-1.31^{*}	(0.51)
log(GDP/capita)	-0.52^{**}	(0.17)	-0.47^{*}	(0.21)
Inflation			-0.00	(0.00)
Growth			-0.06^{**}	(0.02)
AIC	1418		1141.9	
N	1077		905	

in the data used for Model 11 and 38 in Model 12) and the fact that in effect a separate regression is being fit comparing each category to the baseline category of unpunished. Statistical tests confirm this: a Wald test to determine whether it is appropriate to collapse pairs of outcome categories strongly confirms the differences

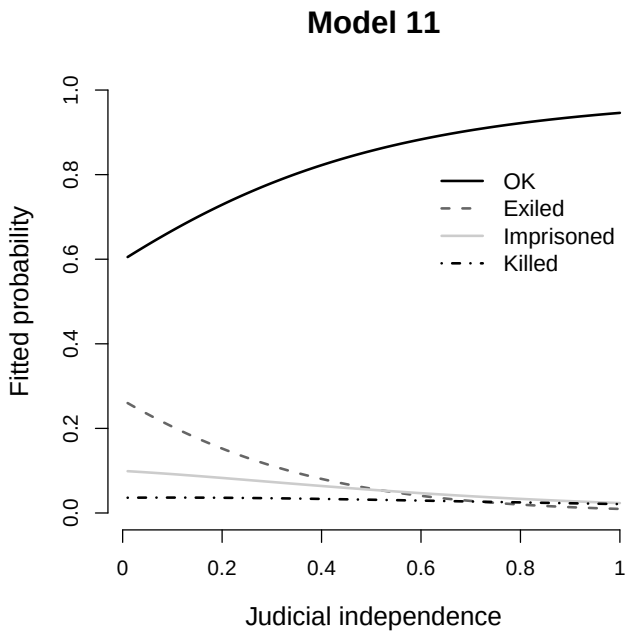


Figure 2.5: **Predicted probabilities of post-tenure fate.** A plot showing the predicted proportional probability of each of the four categories of post-tenure fate. The plot is the predictions of the multinomial logistic regression in Model 11, with all other covariates set to their mean values.

between all category pairs (in each case $p < 0.01$) except for the imprisoned/killed combination (here $p = 0.85$). In each case the three-category multinomial logit model fits vastly better than the four-category version according to the AIC scores (see the chapter appendix for more information).

What is noteworthy in the Models reported in Table 2.3 is the significant different between independence's predicted effect between the baseline category and the second category of exiled in both models and the difference between being unpunished and being imprisoned in Model 11. This is the most common category after unpunished, and the results in Table 2.3 suggest that even with a more complicated modeling structure and the rather low number of observations a statistically and substantively significant effect of judicial independence can still be discerned.

2.4 Conclusions

The key implication of the findings reported in this chapter is that the assumption that judicial independence can provide insurance to leaders once they are no longer in power has strong support when looking at data on the post-tenure fate of leaders. The chapter offers a test of an assumption critical to the strategic model of judicial independence that has unfortunately gone untested until now, despite the fact that the strategic framework for explaining judicial independence has become the leading framework (VonDoepp and Ellet 2011). In addition, it does so through the novel approach of using data on leaders that is used primarily in international relations scholarship (Goemans, Gleditsch and Chiozza 2009; Debs and Goemans 2010). I find that judicial independence is a strong predictor of the post-tenure fate of leaders. These results hold despite the inclusion of covariates and considerable shrinkage in sample size, and hold across a variety of model specifications, including dichotmous, ordered, and multinomial logistic regression.

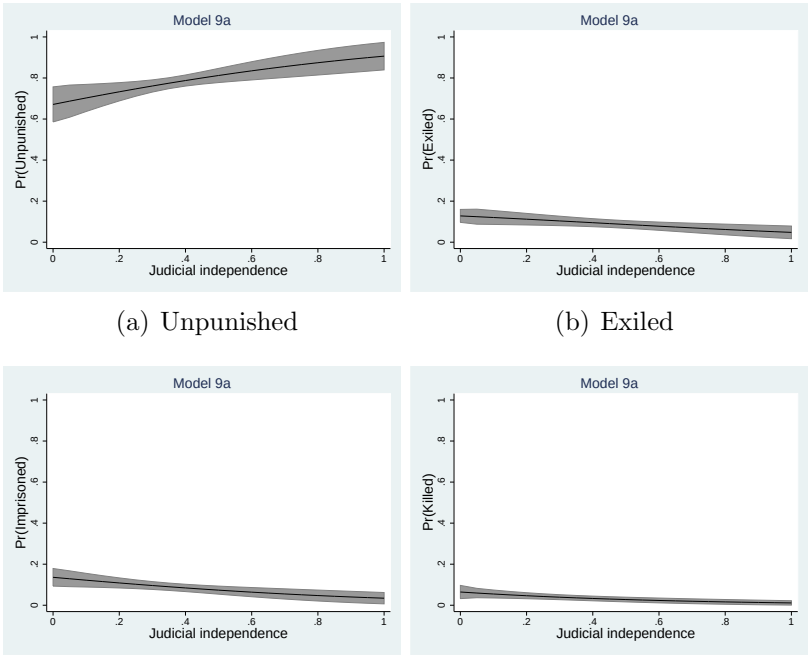
In those cases where judicial independence is non-existent or weak, the likelihood that a leader is punished after leaving office is significantly higher than in cases where independence is strong, suggesting that independent courts are able to serve as an effective insurance policy. Two possible mechanisms of insurance are consistent with the my findings. First, independent courts could serve as an ex post check on politicized punishment by those who recently assume power by ruling against any actions. Alternatively, strong and independent courts might serve as an ex ante check on such punishments, deterring those recently in power from attempting to punish their predecessors. Unfortunately, the tests conducted here cannot determine under what conditions these two possible mechanisms of insurance obtain (they are of course not mutually exclusive, and may be mutually reinforcing). This is, however, an avenue for future research, as both have important implications for understanding when and how courts can serve as checks on the political branches.

2.5 Appendix

2.5.1 Ordered logit models

Figure 2.6 shows the predicted marginal effects of judicial independence from Model 9a when other covariates are held constant at their mean values. The black lines are the predicted probability of being in a given outcome category and the shaded gray areas show the 95% confidence intervals around those estimates.[14] Each sub-figure shows each of the four ordinal outcomes possible in the variable post-tenure fate.

[14]These marginal effects figures were created using the `margins` command in `Stata 12`. Model results are identical to the models reported in the main body text that were produced in *R*.



(a) Unpunished (b) Exiled

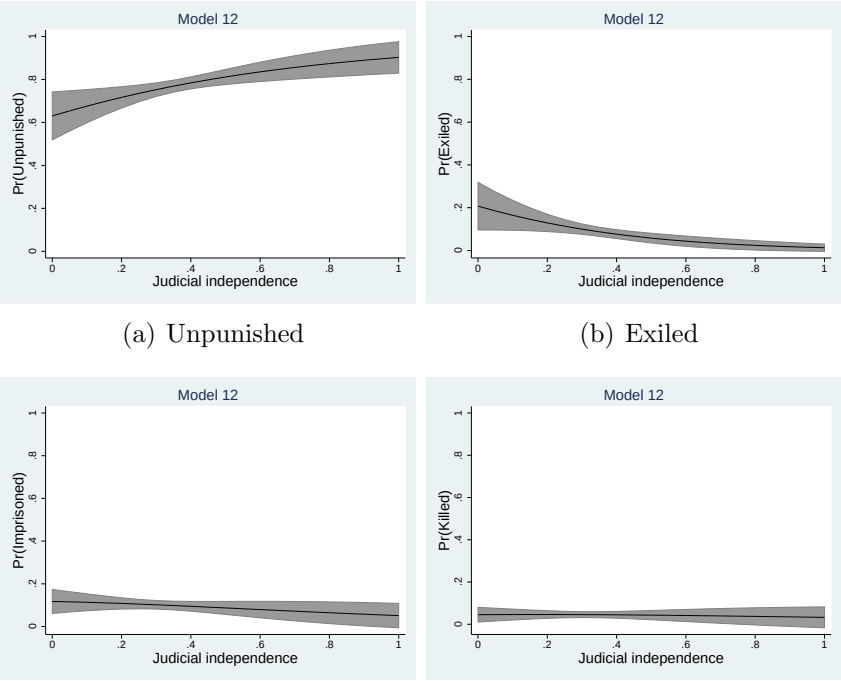
(c) Imprisoned (d) Killed

Figure 2.6: **Marginal effects of Model 9a.** These plots show the predicted marginal effects of judicial independence from Model 9a when other covariates are held constant at their mean values. The black lines are the predicted probability of being in a given outcome category and the shaded gray areas show the 95% confidence intervals around those estimates. Each sub-figure shows each of the four ordinal outcomes possible in the variable post-tenure fate.

2.5.2 Multinomial logit models

Figure 2.7 shows the predicted marginal effects of different levels of judicial independence from Model 12 when all other covariates are set to their means. The black lines are the predicted probability of being in a given outcome category and the shaded gray areas show the 95% confidence intervals around those estimates. Each sub-figure shows each of the four ordinal outcomes possible in the variable post-tenure fate.

As noted in the main text, Wald tests conducted on the results of multinomial models recommend collapsing the two outcome categories of imprisoned and killed.



(a) Unpunished (b) Exiled

(c) Imprisoned (d) Killed

Figure 2.7: **Marginal effects of Model 12.** These plots show the predicted marginal effects of judicial independence from Model 12 when other covariates are held constant at their mean values. The black lines are the predicted probability of being in a given outcome category and the shaded gray areas show the 95% confidence intervals around those estimates. Each sub-figure shows each of the four categorical outcomes possible in the variable post-tenure fate.

Table 2.4 reports the results of these models, which show the same substantive results as the models reported in Table 2.3. This is demonstrated in Figures 2.8 and 2.9. Figure 2.8 shows the predicted probability of post-tenure fate for Models 11a and 12a in which the categorical outcomes of imprisoned and killed are collapsed into one category, in the same manner as the figures in the main body text. Figure 2.9 shows the marginal effects of different levels of judicial independence from Model 12a as the other covariates are held constant at their mean values. Each sub-figure shows one of the three outcome categories, with the black lines showing the predicted probability

Table 2.4: **Multinomial logistic regression results.** Three multinomial (polytomous) logistic regression models of post-tenure fate from 1960–2000, with a baseline category of unpunished. These show results of the models reported in Table 2.3, with the third and fourth categories collapsed as recommended by a Wald test. Continuous covariates were mean-centered to improve the estimation of cut-points, as is common. This has no effect on the estimates or errors for the covariates in the model (Gelman and Hill 2007). Asterisks denote the typical 0.05, 0.01, and 0.001 levels of statistical significance.

	Model 11a		Model 12a		Model 13a	
	Estimate	Std. error	Estimate	Std. error	Estimate	Std. error
Category 2: Exiled						
Intercept	−0.69	(0.51)	−2.08***	(0.28)	−1.74***	(0.36)
Judicial independence	−3.78***	(0.80)	−3.38***	(0.95)	−3.09**	(1.10)
Democracy	−1.27**	(0.32)	−1.31***	(0.37)	−1.18***	(0.40)
log(GDP/capita)	−0.19	(0.11)	−0.20	(0.15)	−0.24	(0.17)
Inflation			−0.00	(0.00)		
Growth			−0.03*	(0.14)		
President elected by assembly					−1.55**	(0.55)
Parliamentary					−1.55*	(0.64)
Category 3: Imprisoned or Killed						
Intercept: 3	−0.10	(0.44)	−1.55***	(0.28)	−1.83***	(0.31)
Judicial independence	−1.58**	(0.66)	−1.39	(0.74)	−2.43**	(0.91)
Democracy	−1.37***	(0.28)	−1.28**	(0.30)	−1.04**	(0.34)
log(GDP/capita)	−0.54***	(0.11)	−0.59**	(0.15)	−0.49**	(0.16)
Inflation			−0.00	(0.00)		
Growth			−0.03*	(0.01)		
President elected by assembly					−1.11*	(0.48)
Parliamentary					0.24	(0.36)
AIC	1222.4		984.12		767.04	
N	1077		905		751	

and the gray areas showing 95% confidence intervals around that estimate. Both of these figures provide further support for the general relationship between judicial independence and post-tenure fate forwarded in the chapter. The relationship between independence and post-tenure fate is incredibly similar regardless of covariates included (which further attenuates the number of observations), the specific form of model used (binary logit, ordered logit, multinomial logit), or collapsing the dependent variable into three rather than four categories.

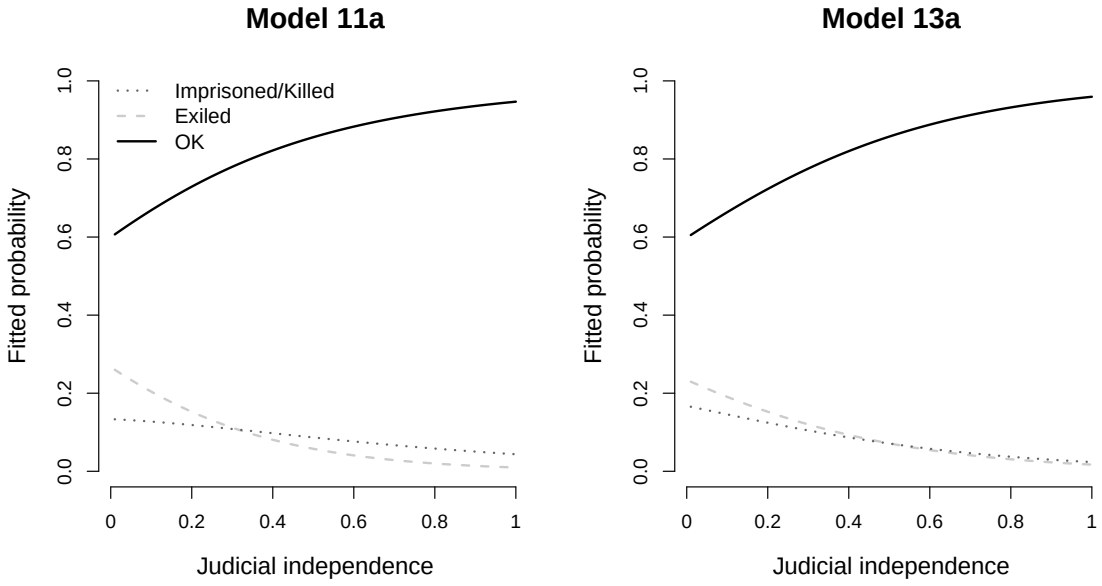
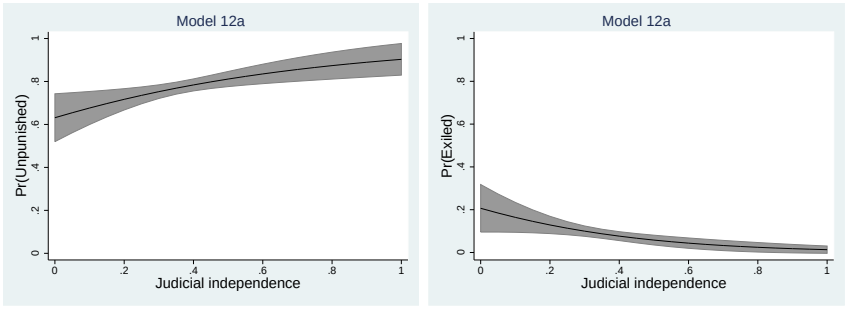


Figure 2.8: **Predicted probabilities of post-tenure fate.** Two plots showing the predicted proportional probability of each of the three categories of post-tenure fate. The plots are the predictions of the multinomial logistic regressions in Model 11a and 13a, with all other covariates set to their mean values.

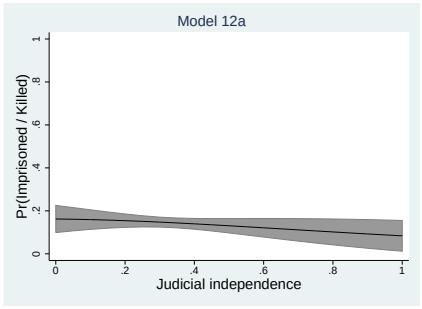
2.5.3 Form of leader entry

One potential other covariate is the manner by which a leader enters power.[15] It might be the case that leaders who take power by irregular means are more likely to be punished upon leaving office. Another possibility is that leaders who take power through irregular means may be more (or less) likely to be punished conditional on the level of judicial independence (which would suggest an interaction term). Thankfully, the Archigos data codes the manner in which a leader takes power in a trichotomous manner: regular mean, irregular means, or direct imposition by an outside power. Goemans, Gleditsch and Chiozza (2006, 3) discuss the coding scheme in the codebook to the data set:

[15] I thank an unknown audience member at the 2012 annual meeting of the Midwestern Political Science Association for suggesting this possibility.



(a) Unpunished (b) Exiled



(c) Imprisoned

Figure 2.9: **Marginal effects of Model 12a.** These plots show the predicted marginal effects of judicial independence from Model 12a when other covariates are held constant at their mean values. The black lines are the predicted probability of being in a given outcome category and the shaded gray areas show the 95% confidence intervals around those estimates. Each sub-figure shows each of the three collapsed categorical outcomes possible in the variable post-tenure fate.

"We code transfers as regular or irregular depending on the political institutions and selection mechanisms in place. We identify whether leaders are selected into and leave political office in a manner prescribed by either explicit rules or established conventions. In a democracy, a leader may come to power through direct election or establishing a sufficient coalition of representatives in the legislature. Although leaders may not be elected or selected in particularly competitive processes, many autocracies have similar implicit or explicit rules for transfers of executive power. Leader

> changes that occur through designation by an outgoing leader, hereditary succession in a monarchy, and appointment by the central committee of a ruling party would all be considered regular transfers of power from one leader to another in an autocratic regime."

To be considered foreign imposition, a leader must be directly imposed by a foreign power (or powers), rather than merely supported by a foreign power who attempted to indirectly influence elections. Table 2.5 presents the results of eight hierarchical logistic regression models of post-tenure fate. Each model is identical to the respective model presented in Table 2.1, except for the fact that each also contains a categorical covariate for the manner in which the leader whose fate is being analyzed entered power (normal means is the baseline reference category). Although interaction terms between judicial independence and manner of entry are not presented in the models below, in no cases are they significant, and in every respective model the substantive and statistical significance of all other covariates remain effectively unchanged. In addition, model fit (as measured by BIC) is worse when an interaction term is included.

As the results presented in Table 2.5 demonstrate, in no case does the form of leadership entry have any statistically significant effect on a leader's post-tenure fate. Only in one case—Model A6—is the BIC score better (lower) than in a comparable model from Table 2.1. Here the BIC score falls from 527.93 to 527.63. The p–value for such a difference, however, is 0.86, so it is impossible to suggest that the model fits better. Similarly, although the BIC in Model A4 is slightly higher than Model 4, the difference between the two is not statistically significant (the p–value is 0.41), so it is indeterminate whether the inclusion of the manner of entry covariate improves the model fit in these cases. However, given the fact that in both Models A4 and A6 the leadership entry covariate does not improve model fit and makes the model less parsimonious, it can still be said that its inclusion is unwarranted.

Table 2.5: **Including form of leadership entry.** Eight hierarchical logistic regression models of the post-tenure fate of leaders from 1960–2000, with intercepts varying by country (country random effects). The dependent variable measures whether a leader was punished (exiled, imprisoned, or killed) in the year after leaving power. These models are identical to those in Table 2.1 except they also include a categorical covariate for the manner in which a leader came to power (the reference category is normal means). As can be seen by the fluctuations in the N, the inclusion of certain covariates significantly lowers the number of observations available for analysis, making the comparison of model fit via single-numer statistics such as BIC difficult. Asterisks denote the typical 0.05, 0.01, and 0.001 levels of statistical significance.

	Null	A1	A2	A3	A4	A5	A6	A7
Judicial independence		5.96***	3.72***	2.60***	2.39**	2.80***	2.49**	2.27***
		(0.48)	(0.58)	(0.62)	(0.70)	(0.82)	(0.91)	(0.66)
Demoracy			1.51***	1.46***	1.40***	1.17***	1.17***	1.60***
			(0.25)	(0.24)	(0.26)	(0.29)	(0.30)	(0.25)
log(GDP/capita)				0.37***	0.43***	0.37**	0.41*	0.33**
				(0.10)	(0.12)	(0.14)	(0.18)	(0.10)
Major war				−0.30				
				(0.24)				
Inflation					0.00		0.00*	
					(0.00)		(0.00)	
Growth					0.04**		0.05**	
					(0.01)		(0.02)	
President elected by assembly						1.32**	1.23*	
						(0.41)	(0.52)	
Parliamentary						0.26	0.23	
						(0.34)	(0.35)	
Years democratic								0.01
								(0.01)
Entry by irregular means		−0.25	−0.16	0.09	0.08	−0.23	−0.10	−0.10
		(0.19)	(0.20	(0.20)	(0.22)	(0.25)	(0.28)	(0.20)
Entry by foreign imposition		1.33	1.38	1.42	16.36	2.04	17.78	1.12
		(0.87)	(0.88)	(0.90)	(853.14)	(1.16)	(1376.02)	(0.91)
(Intercept)	1.38***	−0.97***	−0.87***	−3.13***	−3.72***	−3.39***	−3.71**	−2.98***
	(0.15)	(0.20)	(0.21)	(0.70)	(0.80)	(1.01)	(1.20)	(0.70)
Country random effects	2.00	0.21	0.35	0.24	0.15	0.14	0.16	0.16
	(1.41)	(0.46)	(0.59)	(0.49)	(0.39)	(0.37)	(0.40)	(0.41)
BIC	1103.23	945.76	902.24	902.03	735.13	597.64	527.63	844.75
N	1077	1077	1077	1077	905	751	689	1005

2.5.4 Fixed vs. random effects

Questions and concerns often arise around the use of fixed vs. random effects. I use random effects in these model specifications for a number of reasons. First, because

of the inclusion of time-invariant covariates such as democracy war, and electoral system, fixed effects specifications are problematic (Beck 2001). Second, because the dependent variable of interest is leader-years rather than country years, I am by definition interested in the cross-sectional effects of judicial independence, which fixed effects specifications parse out in favor of analyzing within-unit variation. While this might be appropriate for time-series cross-sectional work when the number of temporal observations within each unit is large, it is less so when the number of units is significantly larger than the average number of observations per unit. Naturally, as each country does not lose a leader every year, the number of observations per country unit is significantly less in these data than in more traditional country-year panel data. The number of country units in Model 4, for example, is 139, while the average number of observations per unit is only 6.

Although these are compelling reasons to prefer random effects specifications, to check the appropriateness of the use of random effects I followed the suggestions offered by Clark and Linzer (2012) in a recent Society for Political Methodology working paper. In it they use monte carlo simulation to determine when to use fixed or random effects, showing that the traditional Hausman test often accepts or rejects random effects specifications incorrectly.[16] They suggest a heuristic based upon:

1. the level of correlation between the within-unit mean of the covariate of interest and the fixed effects estimation for that unit;

2. the sluggishness of the covariate being analyzed, and

3. the amount of data (units and observations per unit).

Given the low number of observations per (country) unit (6) in the data analyzed here, and given the relatively low correlation between the within-unit mean of judicial

[16]Although they focus on linear models, their findings with regards to the Hausman test and the tradeoff between potential bias and variance are the most thorough to date for the sort of TSCS data common in comparative politics.

independence and the unit estimated fixed effects (0.45), the use of random effects is preferrable.

Chapter 3

COMPETITION AND AUTHORITARIAN JUDICIAL EMPOWERMENT

As was explained in Chapter 1, according to nearly all existing accounts of judicial independence, there is little reason to expect autocrats would establish judiciaries with even some marginal degree of power and autonomy. Yet, in many autocracies this phenomenon does in fact occur. In this chapter I challenge existing explanations of authoritarian judicial empowerment developed in single-country case studies, and show instead that that political competition is key to understanding judicial empowerment in autocracies. Using logistic regression on a panel of 1,274 autocratic country-years between 1980 and 2000, I conclude that many of the explanations offered in the literature fail to hold up cross-nationally, and that the level of competition among political elites—a primary cause suggested in the literature on the creation of judicial independence in democracies—also has great explanatory power in non-democracies.

In the non-democratic context, judicial empowerment can be conceived of as a shift by the executive to a situation of restraint, in which the prerogatives of the judicial branch are no longer systematically violated. In other words, a move from a situation characterized by a lack of de facto judicial independence to one with moderate de facto judicial independence. In autocracies, this can be seen as a move away from an almost purely dependent judicial system towards a condition in which courts exercise some degree of independence. This is to suggest that the impetus to go from nothing to something with regards to judicial independence is worthy of examining in its own right, as it might be different than explaining variation in the (more granular) level of judicial independence (as was used to explain post-tenure

fate in the previous chapter).[1]

Examining the onset of judicial independence is also important because in recent years there has been a move to explicitly address the role of the judiciary in non-democracies, exploring the constraints faced by empowered courts, and the effects such quasi-independent courts have on political and institutional development. While looking at such questions, scholars have also made arguments as to the structural and institutional factors that increase the likelihood that an authoritarian state would establish quasi-independent courts (Moustafa 2007; Solomon 2007; Ginsburg and Moustafa 2008). Unfortunately, however, the attention paid to the role political competition might play has been nearly non-existent, a condition that this dissertation seeks to remedy.

This chapter proceeds as follows. First, I present existing explanations of authoritarian judicial empowerment and critique their ability to effectively explain the incidence or timing of empowerment. I then test the first hypothesis offered in Chapter 1, that political competition should also explain judicial independence in non-democracies, addressing measurement issues and present empirical results. I conclude with implications of these results.

3.1 Existing explanations

Three potential causal factors influencing judicial empowerment have been most clearly articulated in the literature on courts in authoritarian regimes. The first is grounded in analyses of principal-agent relationships, and focuses on the role empowered courts play in coordinating and controlling bureaucrats. Drawing on the insights of McCubbins and Schwartz (1984), this argument contends that quasi-independent courts serve as checks on bureaucratic malfeasance, providing credible monitoring

[1]The following chapter returns to the question of variation, using the Linzer and Staton (2011) measure discussed in the previous chapter to examine the degree of variation in judicial independence within non-democratic countries.

and sanctioning of bureaucratic agents for governmental principals. The reason such agency problems may be especially pernicious in autocratic states is the relative lack of effective non-state actors able to hold government accountable, such as a free (and adversarial) press or interest groups (Ginsburg 2008). Furthermore, the issue of corruption is even more salient in autocratic states, and independent courts can provide an effective mechanism for citizens to bring complaints against state agents who would otherwise go unmonitored and unsanctioned. The argument is not predicated on who controls a given bureaucracy, be they regime loyalists or otherwise. Instead, it is the nature of the principal-agent relationship between the executive (including in this case those appointed to oversee bureaucracies) and her agents, including low and mid-level bureaucrats. This argument has been employed in case studies of judicial independence in both Africa and Latin America (Widner 2001; Magaloni 2008). Following Rosberg (1995), Moustafa (2007) looks at Egypt, arguing that this function of courts is more necessary in autocracies with larger bureaucracies, as increased bureaucratic size comes with increased costs of control. The logic of the argument is grounded in strategic calculations by autocrats. As the institutional and structural circumstances change, so do leaders' preferences and incentives, and thus judicial empowerment is more likely.

The second causal relationship explored in qualitative studies is between economic performance and judicial empowerment. Moustafa (2007) contends that the desire to attract foreign investment and decrease capital flight was critical in the decision of the executive to increase judicial autonomy in Egypt. Generally, courts able to secure property rights and investment are in the interests of autocrats, as autocrats are often able to keep these courts from dealing with matters relating to political or civil rights (Root and May 2008). According to this conception, an autocrat's need to credibly commit to securing property rights and abandoning expropriation results in the decision to empower courts, better enabling them to protect property rights in

the face of infringement.[2]

A fundamental problem with these explanations is that they seek to explain uncommon events with common phenomena. While it may be the case that these factors were relevant in the specific cases studied, the theoretical justifications presented severely over-predict empowerment (this is similar to arguments explaining systematic and mobilized violence with reference to relative deprivation). The desire of state principals to better monitor and sanction bureaucratic agents is (much like inequality) ubiquitous. Even restricting such a need to autocracies with large bureaucracies, such a need is still far more common than incidences of empowerment. The Soviet Union and its satellite states offer the most extreme examples of the inability of the explanation to predict the phenomenon it seeks to explain. These states were massively bureaucratized, with party principals unable to effectively monitor their agents for half a century, and yet none had anything resembling judicial independence.

The same problem holds for the desire of leaders to increase FDI and improve economic performance. While there is obviously variation in economic performance and FDI inflows among different autocracies cross-sectionally and within autocracies longitudinally, there is still no reason to believe a state leader would prefer worse performance and less investment, regardless of the existing level. As this is the case, the causal logic offered in the literature would suggest that sooner or later all autocrats would experiment with independent courts as a means to spur economic growth

Even restricting the question to states with poor economic performance and an inability to credibly commit to respecting property rights, once again a common phenomenon explains a rare event. There are simply too many slowly-growing autocracies failing to empower courts for such to be an acceptable generalizable causal account.[3]

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[2]For the relationship between fear of expropriation and FDI, see Henisz (2002) and Brunetti, Kisunko and Weder (1998).

[3]This is not to say that in cases like Egypt the need for FDI did not spur empowerment. I instead stress that this is not a generalizable account. Moustafa argues that the shift to market

The third factor that has been hypothesized to increase the likelihood of judicial empowerment is the age of the regime in question. The problem here, however, is that the proposed causal relationship is less than entirely clear. On the one hand, Ginsburg and Moustafa (2008) look to the cases of Pakistan and the Philippines to argue that new autocrats often use courts as a mechanism to obtain legitimacy after coming into power, as does del Carmen (1973). Here, new leaders break with the past and engage in policy experimentation. Empowering courts is one way of doing so. On the other hand, both Landry (2008) and Moustafa (2008) argue that as autocratic regimes mature, they should be more likely to empower their courts, as popular support fades and the government experiments with new structures to shore up its (increasing lack of) legitimacy among its citizens. The argument that autocratic regimes lacking legitimacy need to engage in significant institutional change to shore up their popular legitimacy assumes, however, that lacking such legitimacy is somehow a threat to the regime. Such an assumption is tenuous at best, however, as countless authoritarian regimes have survived for long periods of time without popular support.

An explanation coming primarily from the economic literature deals with the historical origins of the legal system. The relationship between legal origins and judicial independence has received attention over the last decade, with a number of articles suggesting that countries with common law origins should and do have more independent courts than those from a civil law tradition (Glaeser and Shleifer 2002; La Porta et al. 2004). Scholars adhering to this "legal origins theory" have produced a number of articles suggesting legal origins affect various institutional and economic outcomes. They have received criticism for the breadth and assurance of their claims,

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liberalization under Anwar Sadat was critical to the need to credible commit to investors. This might suggest that the context of FDI levels is key. However, the time period of this sample is one of secular trends toward liberalization, and the "global trend toward economic liberalization [that] in recent decades... encouraged and facilitated the establishment or reform of more robust judicial institutions" means that a context in which investment is needed for growth is the overwhelming norm. See Ginsburg and Moustafa (2008, 9).

as well as an underdevelopment of the causal mechanisms undergirding the statistical findings (Helmke and Rosenbluth 2009).[4]

Following the argument offered in Chapter 1, I contend that political competition is a key determinant of judicial independence in non-democracies. This is because the logic of the insurance model is that the propensity for judicial independence is a function of risks of losing office and the probability of such happening. If there is a significant possibility of losing office—i.e. if there is a significant amount of political competition—and the risks of losing office are similar to those faced by democratic leaders, the electoral logic should be able to explain the empowerment of courts in non-democracies.[5] I now turn to discussing issues of measurement before presenting empirical tests of this proposition.

3.2 Tests of competing explanations

The causal factors proposed in the literature are formulated based on case studies of a handful of autocracies, and no cross-national empirical work examines whether such explanations hold generally. Similarly, these proposed explanations have not been subjected to tests comparing their explanatory power when faced with alternative explanations. This section demonstrates such tests.

3.2.1 Measurement

Judicial independence

The measure of judicial empowerment employed is drawn from Henisz's (2000, 2002) work on political constraints. Henisz's classification of states as possessing either some degree of judicial independence or none is the most extensive single indicator of judicial independence in terms of variation cross-sectionally and longitudinally. To

[4]See also the issue of the *BYU Law Review* dedicated to this debate, 2009(6).

[5]I address the second contention of the dissertation, that because the risks are higher competition is more salient in non-democracies, in Chapters 5 and 6.

compute the binary variable, Henisz takes the value for the Law & Order subcomponent of the International Country Risk Guide (ICRG) and the degree of executive constraints from the Polity IV database. These are combined because effective "law and order may be provided through a wellfunctioning legal system or through repressive policies" (Henisz 2000, 27). If a country scored equal to or greater than a 4 on the ICRG component, and was classified as having "slight to moderate limitations on executive authority" by Polity, it was classified as having some degree of judicial independence. This is to mean that the judicial system as a whole is able to function somewhat effectively and provide at least a partial check on executive power.

The advantages of the Henisz measure are discussed in detail by Gibler and Randazzo (2011) in their recent study of the role independent judiciaries play in preventing democratic backsliding, and the validity of the measure as compared to other measures is discussed by Ríos-Figueroa and Staton (2009) in their review of measures of judicial independence. To create an indicator noting the year in which a state empowers its courts requires some further work. Observations (country-years) in which a state is categorized as an autocracy and is not undergoing a transition are included: for these two measures, see Cheibub and Gandhi (2010). Transition years are excluded because institutions are regularly transformed as part of the transition process, and are not obviously instances of autocrats empowering courts. Those years Henisz scores as having judicial independence and are not preceded by a year in which there was already judicial independence are considered to have witnessed judicial empowerment. In other words, judicial empowerment is coded as a year in which a country is considered to have judicial independence when it did not have such the year previous, excluding years in which the entire political system is undergoing transition to democracy. Such years are excluded as it is impossible to determine from the data whether empowerment in these contexts is a function of regime change or authoritarian judicial empowerment. If anything, the exclusion of these cases increases the difficulty of these tests, as a large portion also possess significant degrees

of political competition.

Operationalizing judicial empowerment in this manner allows me to capture formal aspects of independence (inferred from a sub-score drawn from the Polity data set) as well as informal aspects (drawn from the law and order rankings compiled by the ICRG). Furthermore, the binary nature of the measure allows for a clear test of judicial empowerment: the *onset* of judicial independence. While other ordinal measures exist, the Henisz measure affords the best cross-national and temporal variation, and is the only that is explicitly binary. Its use is further bolstered by the high degree of correlation with other popular measures of judicial independence (Ríos-Figueroa and Staton 2009).[6] Robustness checks using one of the other ordinal measures of judicial independence, developed by Tate and Keith (2007), show results consistent with models using the Henisz measure, whether ordered logit is used or the middle category of de facto judicial independence is collapsed into a binary measure.

Political competition

To measure political competition, a measure of the degree of political constraints on the executive is used. While the measure employed was developed by Henisz (2000, 2002) to measure the viability of policy change at any given point at time, the manner in which such viability is measured neatly captures the level of political competition in a system while also not including any information about the role of the judiciary. This 0–1 index measures the number of executive and legislative veto points,[7] the extent of party alignment (of those parties in the legislature) with the executive, and the fragmentation of parties in the legislature (as more fragmentation of the opposition means less of a credible threat of replacement).

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[6]For a review of existing measures, see Ríos-Figueroa and Staton (2009). They show that the Henisz measure is correlated with the Political Risk Services measure at 0.73, the contract-intensive money measure at 0.48, and the Fraser Institute measure at 0.56.

[7]The potential veto point provided by an independent judiciary with judicial review is not included in this measure, for obvious reasons.

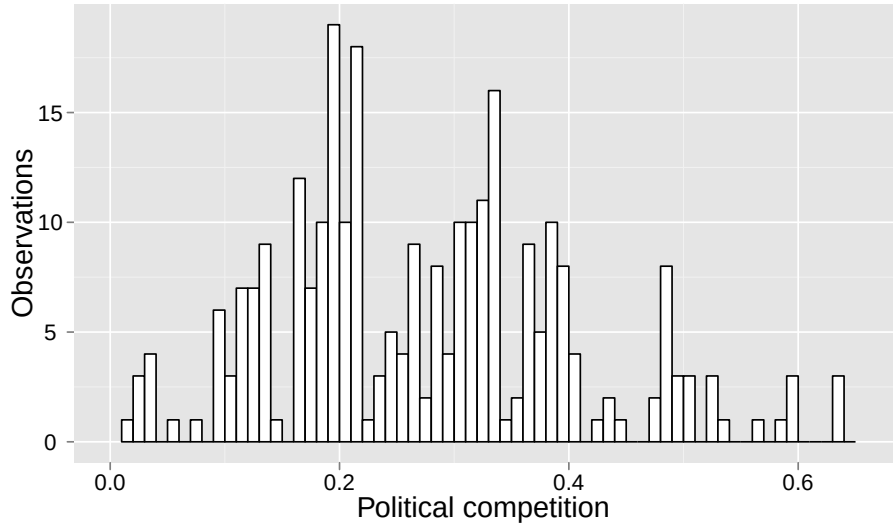


Figure 3.1: **Political competition in non-democracies.** A histogram showing the distribution of political competition across non-democracies between 1980 and 2000 when political competition is greater than zero.

The executive is highly constrained when large, homogenous opposition groups exist in the legislature, whereas a legislature completely aligned with the executive has a constraint measure of 0. This means that in those instances where a single party aligned with the executive has all seats in the legislature (i.e. a one-party regime), the level of constraints will be lowest. Obviously, it also is the context in which political competition is lowest. Similarly, if the opposition controls a given percentage of the legislature, its ability to provide an effective constraint on the executive is also effected by its degree of fractionalization. This effectively operationalizes the logic of the political competition model outlined above, as a number of small opposition groups controlling any given share of seats should provide less of a credible threat than a single opposition group controlling the same share.

Figure 3.1 plots the variation in the level of political competition as operationalized by the Henisz measure in the date used in the analysis in Table 3.1. The histogram shows 22% of 1,274 non-democratic country-year observations between 1980

and 2000. As 78% of the observations of competition have the value of 0, these are not included in the histogram for the ease of interpretability. As can be seen, when competition does exist in non-democracies, significant variation is observed.

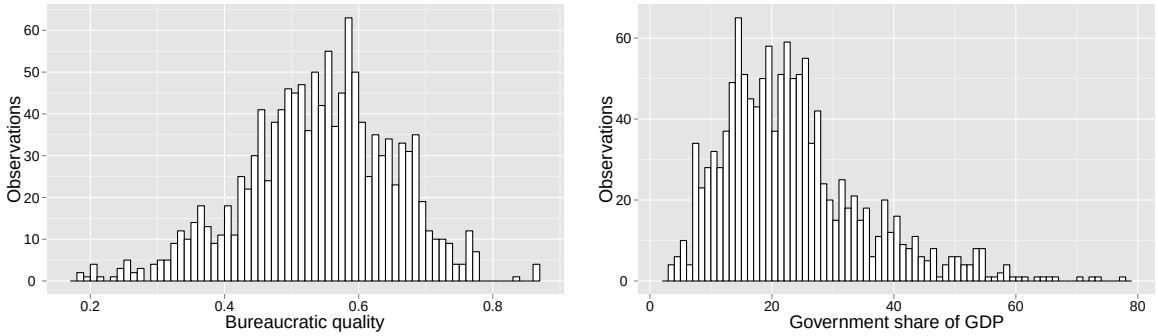
Monitoring bureaucrats

To test the hypothesized role of the desire to better monitor and control bureaucrats, I use measures of the size and quality of the bureaucracy. This can be measured in three ways: the absolute number of state officials, economic output of state sectors as a proportion of total economic output, or bureaucratic quality. While the first may appear to be accurate, and is the tool employed by Moustafa (2003, 2007), in his analyses of Egypt, it suffers from a large problem. The relationship between bureaucratic size and lack of control is not a linear process even in one country over time, let alone the same cross-nationally; the same number of state bureaucrats in Singapore and Egypt cannot be assumed to similarly affect the need for courts to serve as a means of monitoring. Additionally, issues with availability and accuracy of such data make it problematic.

As such, two measures are employed to better capture the two aspects of the conceptual relationship. Figure 3.2 shows two histograms, each showing the distribution of one of the two measures used to capture the hypothesis that increased need to monitor bureaucrats should lead to an increased likelihood of judicial empowerment.

The first of these is a 0–1 measure developed by Williams (2009). Williams demonstrates that the amount of information collected by state agencies effectively predicts the quality of the bureaucracy, and is highly correlated with other measures of bureaucratic quality and effectiveness. If the relationship hypothesized in the literature is generalizable cross-nationally, more effective bureaucracies should be inversely related to empowerment, since more transparent and higher quality bureaucracies should result in less demand for alternative monitoring and sanctioning mechanisms.

The second is the amount of government spending as a percentage of GDP,



(a) Bureaucratic quality

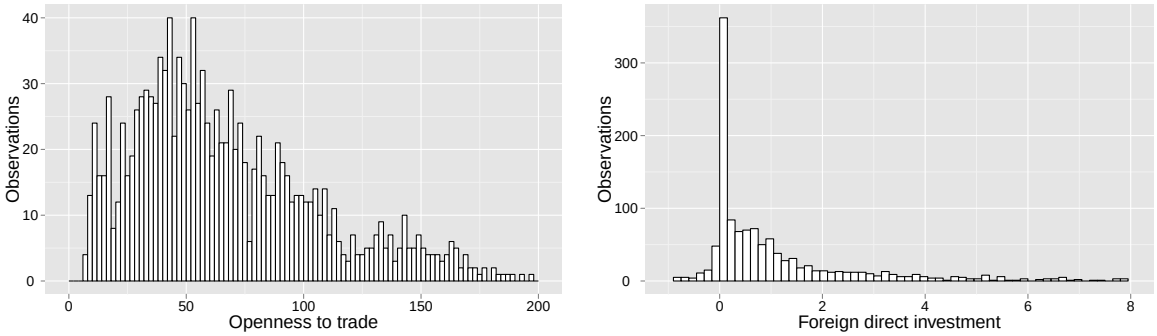
(b) Government share of GDP

Figure 3.2: **The size of the bureaucracy.** Two histograms showing measures used to capture the argument forwarded in the existing literature that more bureaucracy means a greater need for judicial empowerment. Plot (a) shows Williams (2009) measure of the quality of the bureaucracy, and plot (b) shows government spending as a percentage of GDP (from the Penn World Tables).

drawn from the Penn World Tables. Here, the opposite of the above is true: if the existing explanations are correct, when government spending is a higher share of GDP we should expect a greater likelihood of empowerment, as this means the government has all the more need to monitor state agents (as there should be more state-owned firms, more state-run schools, etc.).

The need for foreign investment

To test the theorized effect of the need for economic investments in a country, I employ two relevant measures. Figure 3.3 shows these. The first, shown in plot (a), is a country's openness to trade. Openness to trade is employed because it should capture how critical the need is to integrate with the global economy: when a country is more integrated, it should have less need to empower courts to attract foreign capital. The second measure, shown in plot (b) of Figure 3.3, is the level of foreign direct investment as a percentage of GDP, using data drawn from the World



(a) Openness to trade

(b) Foreign direct investment

Figure 3.3: **The need for foreign investment.** Two histograms showing measures used to capture the argument forwarded in the existing literature that a greater need for foreign capital means a greater need for judicial empowerment. Plot (a) shows a country's openness to trade (from the Penn World Tables), and plot (b) shows foreign direct investment as a percentage of GDP (World Bank and United Nations data).

Bank and the United Nations.[8] If existing explanations are correct, we should see an inverse relationship between FDI and empowerment, as these explanations contend that when levels of FDI are low, the need to attract foreign capital that will not be expropriated means autocrats should be more likely to empower courts.

Legal origins

To ascertain whether legal origins show a relationship to empowerment, data on a country's legal origins from La Porta et al. (2004) are used. In the data used in the overwhelming majority of models presented in the following section, 60% of all non-democratic observations between 1980 and 2000 are from countries with a French legal heritage, and 8% are from those with a heritage of a socialist legal system. The

[8]The total stock of FDI as a percentage of GDP was also used as a robustness check. This significantly shrinks the number of observations total, as well as observations where empowerment occurs. The results are generally similar, with political competition, bureaucratic quality, and the time trend showing similar coefficients and levels of significance.

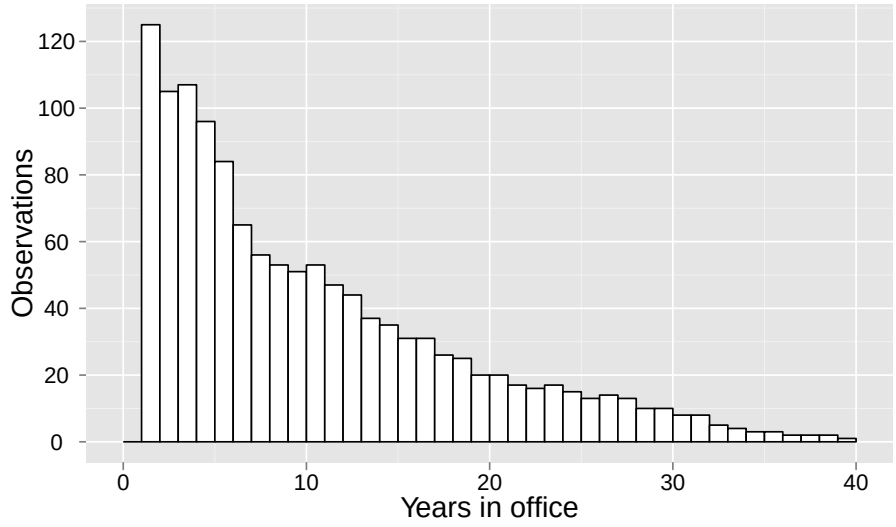


Figure 3.4: **Years in office.** A histogram showing the distribution of a leader's years in office across 1,274 non-democratic observations between 1980 and 2000. Data are drawn from the Database of Political Institutions (Beck et al. 2001).

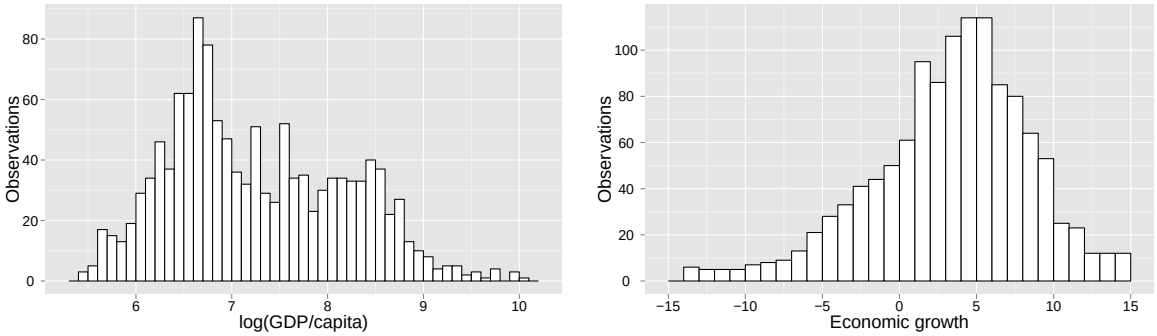
remaining 32% of observations (which serve as the baseline category in the following models) have a heritage of British common law.

Years in office

The measure used to determine whether duration of rule is related to empowerment is the length of executive tenure. Drawn from the Database of Political Institutions, this variable measures the time in office of the executive (Beck et al. 2001). Figure 3.4 plots the distribution of this measure over the 1,274 observations used in the data analysis presented in Table 3.1.

Economic development and performance

The level of economic development and performance more generally might also play some role in empowerment, and are included in models below. States at low levels of development may lack the resources, human capital, and infrastructure required for



(a) Economic development (b) Economic growth

Figure 3.5: **Economic development and performance.** Two histograms showing the distribution of measures of economic development and performance. Plot (a) shows a country's level of economic development as measured by the log of per capita GDP, and plot (b) shows economic performance as measured by yearly percentage change. Data for both are drawn from IMF International Financial Statistics.

effective courts, let alone courts operating independently of political power. Autocracies at high levels of development, on the other hand, are not only potentially more likely to have the resources to empower courts, but also to have economic actors independent of the state who may demand some degree of independence for the protection of their property. Economic performance more generally may affect the propensity for an autocracy to undertake institutional change. Scholars suggest that the need for FDI is a key reason for empowering courts not because FDI is itself a good, but rather because FDI is an important component of economic growth (Borensztein, De Gregorio and Lee 1998). It may thus be the case that when economic performance is robust pressure for changing institutions is lessened. Figure 3.5 shows histograms of a country's per capita GDP (plot a) and the level of economic growth that year (plot b). Measures for both are taken from the International Monetary Funds International Financial Statistics.

The judicialization of politics

As a secular trend in the general rise of judicial independence globally may exist, I include a covariate accounting for the year. In Europe, for example, many observe an increasing "judicialization of politics" over the past few decades (Stone Sweet 2000). Others note that in the post-Cold War era the developing world has pushed for the expansion of judicial independence and rule of law reforms elsewhere (Tate and Vallinder 1995). While generally presented as descriptive rather than causal accounts, that such a trend may exist warrants including a covariate accounting for it. As the number of observations per year is approximately the same, no histogram of its distribution is presented, as it would be roughly uniform.

3.2.2 Modeling strategy and results

Table 3.1 presents the output of five logistic regression models of judicial empowerment in non-democracies as defined by the Cheibub and Gandhi measure. The Cheibub and Gandhi measure is employed exclusively here due to the nature of the analysis. Because the dependent variable is empowerment—the onset of judicial independence—a clear and consistent way to exclude transition years is required; if not, a large number of "empowerments" would occur that were in fact just the establishment of judicial independence as part of a transition to democracy. The Cheibub and Gandhi data include a covariate for transition (which was used in the creation of the dependent variable: years with transition were excluded), allowing for a systematic and unbiased procedure for excluding transition years.

The models in Table 3.1 capture 1,274 country-years between 1980 and 2000, during which there were 19 cases of authortiarian judicial empowerment as captured by the measure described above.[9] The impetus for multiple models is to test the

[9]Continuous covariates in the following models are mean-centered to improve estimation of the intercept, per suggested best-practices (Gelman and Hill 2007).

Table 3.1: **Logistic regression results.** Regression output for five logit models of the empowerment of courts in non-democracies, 1980–2000. Model 1a tests all competing explanations. Model 2a tests those found in the literature on autocratic empowerment and the political insurance hypothesis. Model 3a tests the political insurance model and the legal origins explanation. Models 4a and 5a are restricted models with only those variables showing a consistent relationship included. The reference category for legal origins is British common law. Robust standard errors (heteroskedastic and autocorrelation consistent) are reported. Asterisks denote the traditional 0.05, 0.01, and 0.001 levels of statistical significance, and data are mean-centered to improve estimation of the intercept.

	Model 1a	Model 2a	Model 3a	Model 4a	Model 5a
Intercept	-3.98^{*}	-5.18^{***}	-2.40	-5.06^{***}	-4.90^{***}
	(1.64)	(0.38)	(1.55)	(0.34)	(0.32)
Political competition	5.44^{***}	5.48^{***}	5.51^{***}	5.15^{***}	5.87^{***}
	(1.55)	(1.56)	(1.10)	(1.25)	(1.08)
Bureaucratic quality	6.11^{*}	4.50		4.63^{*}	
	(2.69)	(2.47)		(2.32)	
Govt share of GDP	-0.03	-0.03			
	(0.03)	(0.03)			
Openness to trade	0.00	0.00			
	(0.01)	(0.01)			
Foreign direct investment	-0.11	-0.06			
	(0.08)	(0.08)			
Years in office	0.04	0.03			
	(0.03)	(0.03)			
French legal heritage	-0.73		-0.70		
	(0.53)		(0.53)		
Socialist legal heritage	-0.22		-1.40		
	(1.25)		(1.17)		
Economic growth	0.05				
	(0.08)				
log(GDP/capita)	-0.40				
	(0.42)				
Year	0.15^{***}	0.11^{**}	0.11^{***}	0.11^{***}	0.09^{**}
	(0.04)	(0.04)	(0.03)	(0.03)	(0.03)
BIC	236.73	212.25	195.51	186.75	184.14
N	1274	1274	1274	1274	1274

political competition explanation forwarded in this dissertation against alternative explanations found in qualitative case studies of the phenomenon and the broad claims

of the legal origins literature. Political competition is by far the most significant and substantively important covariate in each model, showing robust results regardless of the exclusion of other covariates. Model 1a includes all the variables described above, capturing the theorized causal accounts in the existing literature on authoritarian empowerment, legal origins theory, and role of political competition. Model 2a tests existing explanations and the political insurance model, while Model 3a tests the legal origins theory and political insurance account. Models 4a and 5a present restricted models including only variables significant in the fuller models; Model 4a includes political competition, bureaucratic quality, and the year covariate, while Model 5a includes only competition and year.

All models have similar coefficients for political competition, bureaucratic quality, and year. As covariates demonstrating no discernible relationship are removed and model fit increases, the only effects on competition and year are slight fluctuations in estimates and standard errors. While the coefficient for bureaucratic quality in Model 2a might suggest excluding it in restricted models, its inclusion in Model 4a shows a significant improvement (the null hypothesis that the models show similar fit can be rejected, as $p < 0.0001$) in fit as compared to Model 2a, as measured by the Bayesian Information Criterion (BIC). Using BIC as a measure of model fit results in an inability to discern a difference in fit between Models 4a and 5a because the null hypothesis of identical fit cannot be rejected.[10] Other methods of fit such as receiver operating characteristic (ROC) plots can be used (and will be presented in the subsequent section), however, and these suggest that Model 4a fits (slightly) better than Model 5a. As such, visualizations presented later will use Model 4a.

The overall results of the models provide resounding support for the contention that the political insurance model of judicial empowerment should also hold

[10]Generally, lower BIC scores are preferable to higher scores, assuming they are significantly different. To test the null hypothesis that there is no difference between two models' BIC scores, one takes half the value of the exponentiated difference between the models. In the case of Models 4a and 5a, the math is $e^{(184.14-186.75)}/2$

in non-democratic contexts (see the notes and appendix for comments on robustness to alternate model specification).[11] They also strongly demonstrate that the causal explanations offered by case studies in the literature on authoritarian judicial politics fail to show any consistent relationship with empowerment, suggesting that the critique of these explanations I offered above is persuasive. No model demonstrates support for the contention that the likelihood of empowerment is greater in cases where the regime needs independent courts to monitor and sanction the bureaucracy. In fact, the effect of Williams' (2009) measure of the release of information (shown to be an effective predictor of bureaucratic quality) demonstrates a positive relationship with the likelihood of onset: states with more effective bureaucracies are more likely to empower their courts, rather than the opposite relationship predicted by the literature. Measuring government spending as a percentage of GDP fails to reach even marginal levels of statistical significance.[12]

The contention in the literature that the need for foreign capital is a critical cause of judicial empowerment is also unsupported: neither a country's openness to trade nor its level of foreign direct investment show anything resembling a significant relationship with judicial empowerment. The mutually exclusive accounts of the effect of autocratic tenure in office on empowerment in the literature are unable to be adjudicated here, as there is no discernible relationship between years in office and empowerment. Neither of the two economic covariates are statistically or sub-

[11]Results are robust to a population averaged model with AR(1) correlations in the error term, suggesting that serial correlation is not a problem. Results are also robust to a dynamic logit transition model (first-order Markov Chain), the results of which demonstrate that competition matters for empowerment but that there is no relationship between competition and *dis*empowerment. Results are also robust to a hierarchical/mixed effects modeling procedure (partially-pooled, varying-intercepts), which accounts for possible interdependence among observations within the same country; while there are slight fluctuations in coefficient estimates, they were in all cases minor, and in no instance did the level of statistical significance of a covariate change.

[12]To fairly test this hypothesized explanation, I also aggregated the two measures, and employed them individually. A variety of aggregations of the two measures of the bureaucracy were all highly insignificant. Employing the government share of GDP without the Williams' measure produced no results, while the use of the Williams' measure without the government share of GDP showed only the most negligible change in the estimated coefficients and errors.

stantively significant, suggesting that neither the level nor yearly performance of the economy affect empowerment.

Similarly, legal origin has no relationship with judicial empowerment in the authoritarian context, with neither French nor Socialist legal heritage showing any statistically significant difference from the reference category of British common law. This suggests that perhaps the relationship reported in prior studies is driven primarily by democracies in those samples; if British common law is associated with higher levels of democracy and judicial independence (as some studies report), it could be that many of the cases were thus not part of this paper's sample of non-democracies.

The covariate for year consistently achieves the standard 0.05 level of significance, suggesting a secular trend toward empowerment in non-democracies. This provides preliminary support for the idea that the judicialization of politics has been occurring even outside of the democratic context. As the precise relationship between time and the increase in the importance of the judicial branch in politics has not been theorized by those suggesting such a relationship exists, I also modeled the relationship as quadratic and cubic (separately). In both cases the substantive and statistical significance of competition is unchanged. The additional time variable is highly statistically significant, but shows little substantive importance.

3.3 Model fit and robustness

Although the results presented in Table 3.1 are on face quite persuasive, it could be that employing alternative modeling procedures or alternative covariates produce results that call this into question. As such, in this section I present a variety of results that demonstrate how robust these results actually are; those interested primarily in the substantive implications of the models reported in Table 3.1 might want to skip to the subsequent section of the chapter.

3.3.1 Robust standard errors

It is common in political science to use standard errors that are robust, in other words standard errors that are consistently estimated when certain assumptions—such as homoskedasticity—underlying modeling procedures are violated. Although robust standard errors can at times be useful, King and Roberts (2012) argue that they often signal problems with the models presented in political science: when a model is properly specified, there should be minimal divergence between 'classical' and 'robust' standard errors. They further note that because understanding the substantive implications of models via simulating quantities of interest is predicated on a correct specification of a model's stochastic component (King, Tomz and Wittenberg 2000), many of the substantive claims made in the literature are potentially problematic.

Table 3.2 reports results of five models identical to those presented in Table 3.1, with the sole difference being that results reported in Table 3.2 employ classical rather than robust standard errors. As can be seen, the differences are negligible, suggesting that the models are not subject to misspecification. Comparing Model 1a and 1b, the differences in the errors of the statistically significant covariates are all approximately 10%: classical errors for the political competition coefficient (which is 5.44) are 1.42, and robust errors are 1.55. For bureaucratic quality (with a coefficient of 6.11) the classical and robust errors are 3.04 and 2.69, respectively. For year (with a coefficient of 0.15) they are 0.048 and 0.043 (Tables 3.1 and 3.2 only report to the second decimal place).

3.3.2 Rare events logistic regression

Often in quantitative political science, binary dependent variables are modeled using logistic regression, regardless of the relative frequency of ones and zeros in the dependent variable. The relative frequency, however, can affect estimates, and when the outcome of interest is rare the probability of occurrence may be underestimated, of-

Table 3.2: **Classical standard errors.** Regression output for five logit models of the empowerment of courts in non-democracies, 1980–2000. Each model is identical to the corresponding model presented in Table 3.1, except that classical rather than robust standard errors are reported. Asterisks denote the traditional 0.05, 0.01, and 0.001 levels of statistical significance, and data are mean-centered to improve estimation of the intercept.

	Model 1b	Model 2b	Model 3b	Model 4b	Model 5b
Intercept	-3.98^{*}	-5.18^{***}	-2.40	-5.06^{***}	-4.90^{***}
	(1.75)	(0.41)	(1.54)	(0.39)	(0.36)
Political competition	5.44^{***}	5.48^{***}	5.51^{***}	5.15^{***}	5.87^{***}
	(1.42)	(1.36)	(1.21)	(1.25)	(1.19)
Bureaucratic quality	6.11^{*}	4.50		4.63^{*}	
	(3.04)	(2.46)		(2.26)	
Govt share of GDP	-0.03	-0.03			
	(0.03)	(0.03)			
Openness to trade	0.00	0.00			
	(0.01)	(0.01)			
Foreign direct investment	-0.11	-0.06			
	(0.12)	(0.11)			
Years in office	0.04	0.03			
	(0.03)	(0.03)			
French legal heritage	-0.73		-0.70		
	(0.55)		(0.51)		
Socialist legal heritage	-0.22		-1.40		
	(1.23)		(1.11)		
Economic growth	0.05				
	(0.04)				
log(GDP/capita)	-0.40				
	(0.38)				
Year	0.15^{**}	0.11^{**}	0.11^{**}	0.11^{**}	0.09^{*}
	(0.05)	(0.04)	(0.04)	(0.04)	(0.04)
BIC	236.73	212.25	195.51	186.75	184.14
N	1274	1274	1274	1274	1274

ten introducing serious problems of misestimation (King and Zeng 2001). In the data used in this chapter, there are only 19 instances of judicial empowerment, occurring in 18 different countries. Because of this, the results presented in Tables 3.1 and 3.2 might be misestimated.

Table 3.3 reports the results of five models identical to those reported in Ta-

ble 3.1, except that here rare-events logistic regression is used. Rare-events logit is a logit specification specifically designed for when the number of events (ones) is a small fraction of non-events (zeroes) (King and Zeng 2001).[13]

Table 3.3 demonstrates that the use of rare events logit is likely unnecessary, as it produces results that are almost identical to those using standard logistic regression. In each rare-events model the coefficient estimate of political competition is almost imperceptibly smaller: in the instance of the largest difference in the estimate, occurring between Model 1c and Models 1a and 1b, the 'standard' coefficient is 5.09 and the rare-events coefficient is 5.44; the difference between the coefficient in Model 4c as compared to Models 4a and 4b only 0.01. Similarly, the coefficient for year is almost unchanged. The sole difference between the rare-events specification and the previous specifications is regarding bureaucratic quality. Here, the standard errors around the estimated coefficient in Model 4c fall just short of the traditional 0.05 level of statistical significance (the p-value for the coefficient is 0.056).

Given the rare nature of the event being analyzed (only 1.5% of the observations are those where empowerment occurs), that the results of the rare-events logit specification are almost identical to those reported using regular logit suggest the model does a good job of capturing the underlying data-generating process.

3.3.3 Model fit

In Tables 3.1, 3.2, and 3.3, model fit is reported as a model's BIC score, which is a measure of explanatory power that allows for comparisons across non-nested models. Single-number statistics of model fit such as BIC and AIC (Akaike Information Criterion) are sometimes unable, however, to tell us how accurate competing models are at with regards to acceptable levels of in-sample predictive accuracy, as a key goal of the models is to penalize complexity (parameters) and thus prevent over-fitting. That is,

[13]Rare-events logistic regression was conducted in the `Zelig` package in *R* (Imai, King and Lau 2007).

Table 3.3: **Rare events logit.** Rare-events logistic regression output for five models of the empowerment of courts in non-democracies, 1980–2000. Each model is identical to the corresponding model presented in Table 3.1, except that rather than a classical logistic regression specification, a rare-events logistic specification is reported. Robust standard errors (heteroskedastic autocorrelation consistent) are reported. Asterisks denote the traditional 0.05, 0.01, and 0.001 levels of statistical significance, and data are mean-centered to improve estimation of the intercept.

	Model 1c	Model 2c	Model 3c	Model 4c	Model 5c
Intercept	-4.23^{*}	-4.90^{***}	-2.73	-4.92^{***}	-4.80^{***}
	(1.64)	(0.38)	(1.53)	(0.39)	(0.32)
Political competition	5.09^{***}	5.23^{***}	5.44^{***}	5.14^{***}	5.84^{***}
	(1.55)	(1.56)	(1.10)	(1.25)	(1.08)
Bureaucratic quality	5.56^{*}	4.16		4.42	
	(2.69)	(2.47)		(2.32)	
Govt share of GDP	-0.03	-0.02			
	(0.03)	(0.03)			
Openness to trade	0.01	0.01			
	(0.01)	(0.01)			
Foreign direct investment	-0.14	-0.07			
	(0.08)	(0.08)			
Years in office	0.03	0.03			
	(0.03)	(0.03)			
French legal heritage	-0.63		-0.68		
	(0.53)		(0.31)		
Socialist legal heritage	0.28		-0.95		
	(1.25)		(1.17)		
Economic growth	0.05				
	(0.08)				
log(GDP/capita)	-0.37				
	(0.42)				
Year	0.14^{**}	0.11^{**}	0.10^{***}	0.11^{**}	0.08^{**}
	(0.04)	(0.04)	(0.03)	(0.03)	(0.03)
BIC	236.73	212.25	195.51	186.75	184.14
N	1274	1274	1274	1274	1274

it may be the case that models with highly significant covariates and seemingly good goodness-of-fit measures still produce large numbers of false positives relative to the number of true positives (Ward, Siverson and Cao 2007). A problem with comparing

the ratio of false to true positives is that the selection of a cut-point or threshold for classification is difficult: while classifying an observation with a predicted probability of 0.5 as a positive might seem the most obvious choice, many social science examples involve low predicted probabilities even for those observations with the highest predicted probabilities (as Ward et al. demonstrate with regards to militarized interstate disputes). And even were it the case that one single cut-point such as 0.5 was intuitive or possessed face validity, such would still treat observations with predicted probabilities of 0.51 and 0.99 identically.

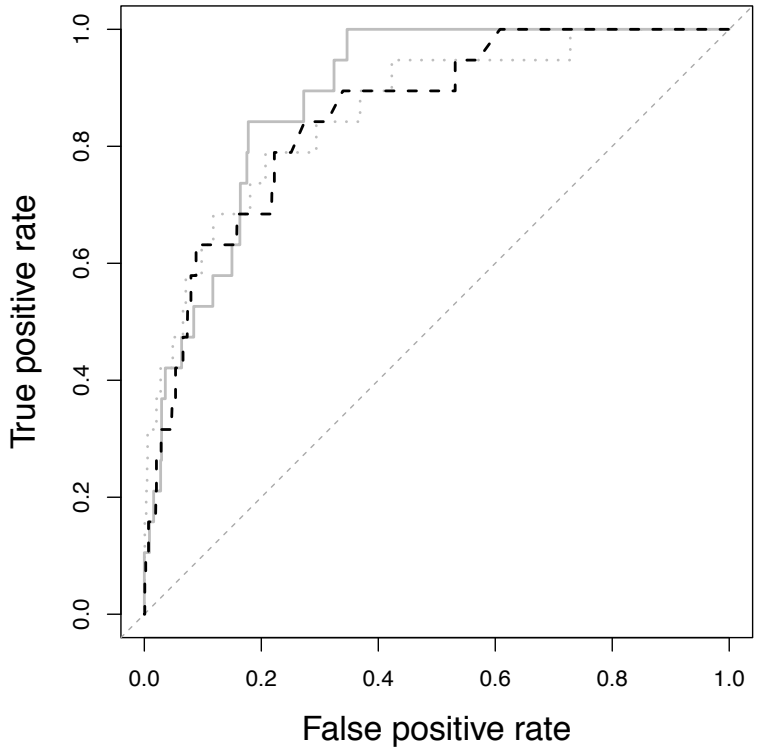
One way around this problem is the use of Receiver Operating Curves (ROC), which plot the ratio of true and false positives across all possible cut-points. Figure 3.6 shows the Receiver Operating Curves (ROC) of Models 1a, 4a, and 5a from Table 3.1. The dotted gray line is the ROC of Model 1a, the solid gray line is the ROC of Model 4a, and the dashed black line is the ROC for Model 5a. The dashed gray line at the diagonal demonstrates the curve that random guessing would produce (one false positive for each true positive). The greater the area under the curve in a ROC plot (i.e. the further the curve is from the diagonal), the greater the model fit, as the proportion of true to false positives is greater.

As can be seen, all three models demonstrate high levels of fit, with large areas under the curve. Model 4a slightly out-performs Model 1a, with 88% of the area under the curve (compared to 85% for Model 1), and Model 5a—with the best BIC score—has the least area under the curve and worst fit according to the ROC. Figure 3.6 demonstrates visually that these models do quite well across different levels of cut-points, significantly outperforming chance and overall offering a high level of predictive accuracy.

3.4 Exploring substantive significance

Looking only at the regression output of logit models, the effect of a change in a covariate is difficult to interpret in any substantively interesting or meaningful way.

Figure 3.6: **ROC Plot of Models 1a, 4a, and 5a.** A plot showing the Receiver Operating Curves for Models 1a, 4a, and 5a in Table 3.1. This plots the relationship between true and false positives predicted by each model across each potential threshold of classification of an observation into a positive or negative prediction. The dotted gray line shows the curve for Model 1a, the solid gray line shows the curve for Model 4a, and the dashed black line shows the curve for Model 5a . The area under the curve for Model 1a, 4a, and 5a are 0.86, 0.88, and 0.85, respectively.

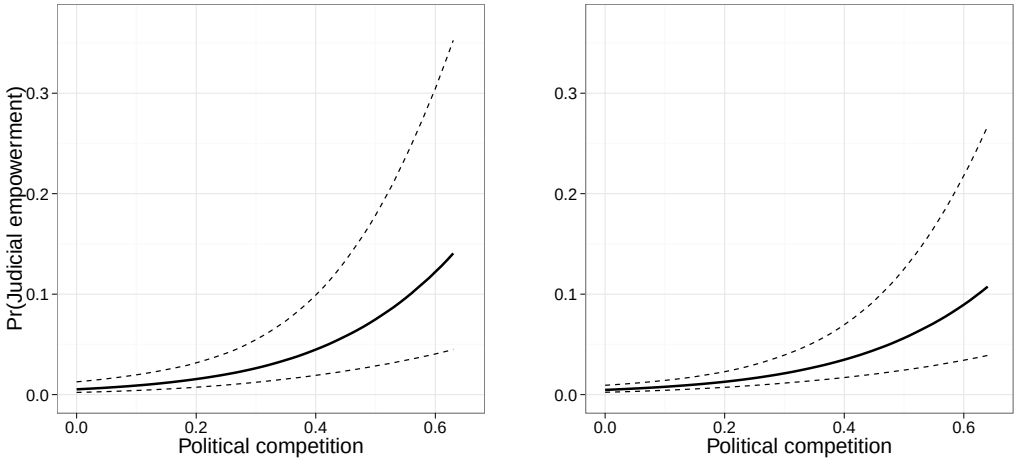


To address this problem, I present a number of figures of the predicted probability of empowerment across different values of the covariates for multiple models. This procedure can give further insight into the expected effects of covariates on empowerment, and prevent under-or-overestimating the effects of a variable. This is of critical importance in interpreting logit models as the effect of a change in a given covariate is not linear.

3.4.1 Political competition

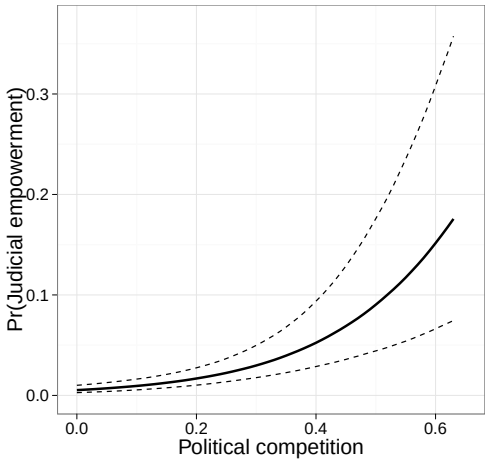
Figure 3.7 plots the effects of different levels of political competition on the expected probability of judicial empowerment for Models 1a, 4a, and 5a when other covariates are held constant at their mean values. In each sub-figure the solid black line is the point estimate of the probability of judicial empowerment when political competition is at the corresponding value according to the x-axis, demonstrating the rise in the probability of empowerment as competition increases. Figure 3.7 shows not only the uncertainty around the model estimates, but the fundamental uncertainty of the modeling process as well. Rather than use the standard delta method, which uses the uncertainty around the estimated coefficients contained in the standard errors, simulating confidence intervals allow us to use the uncertainty contained in the modeling process itself. To do this, I draw 10,000 draws from a multivariate normal distribution with means set to the coefficient estimates of the relevant model and the deviation around these means set to the variance-covariance matrix of the model. Each of these 10,000 draws thus produces simulated model parameters (estimates and standard errors) that contain the uncertainty around the modeling process. Using matrix multiplication of these simulated model parameters and specified quantities of interest (varying competition here and holding all else constant) builds up a distribution of observations at each level of competition, and allows us to determine confidence intervals empirically.

Figure 3.7 illustrates more clearly the effect on the probability of empowerment than regression output in Table 3.1. Looking at Figure 3.7, the relationship between competition and the probability of judicial empowerment is clear: at higher levels of competition the predicted probability of empowerment is substantially higher than at low levels. Non-democracies with political competition are much more likely to empower their judiciaries. This is not to suggest that such states are more likely to democratize; the work of Brownlee (2007) demonstrates that such states can remain



(a) Model 1a

(b) Model 4a



(c) Model 5a

Figure 3.7: **Varying political competition.** Three plots showing the effects of different levels of political competition on the expected probability of judicial empowerment for Models 1a, 4a, and 5a (holding all other covariates in each model constant at their mean values). The solid black lines are point estimates, and the dashed gray lines are 95% confidence intervals, drawn from 10,000 simulations.

solidly autocratic. A probability of empowerment of even 0.15 or 0.2 is extremely substantively interesting and important, as this is not the expected probability that

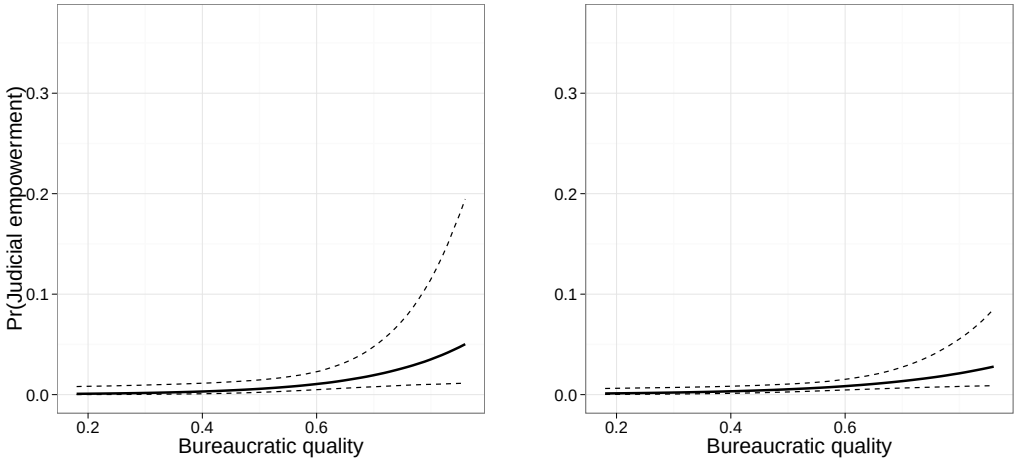
a regime will empower its courts over the course of its tenure, but in any given year. If elections in autocratic regimes produce different factions in different legislative or executive institutions, or variation in preferences within the legislature, these results are likely to last a number of years, with the resulting probability of empowerment in every year.

3.4.2 Bureaucratic quality

The same procedure was used to create Figure 3.8, which plots the effects of different levels of bureaucratic quality on the expected probability of empowerment when political competition and year are held constant at their mean values. Again, the area between the dotted lines contains the 95% confidence intervals around the estimate (black line), demonstrating the rise in the probability of judicial empowerment as bureaucratic quality increases. Interesting to note is the fact that the effect of bureaucratic quality shows little change until it reaches a value of 0.6 (on a 0–1 scale). At this point the effects begin to rise sharply. At the highest observed level of bureaucratic quality in the country-years under analysis, 0.83, the estimated change in the probability of empowerment is slightly below 5%.

Figure 3.8 demonstrates that in country-years with the highest quartile of bureaucratic quality (approximately normally distributed, this quartile begins at 0.61), the effects of incremental increases in quality are minimal. Given that the causal account in the qualitative literature on authoritarian judicial empowerment predicts the opposite, contending that the inability to monitor and sanction bureaucratic agents should increase the probability of empowerment, these findings are striking. They suggest that such a causal story is not only incorrect, but actually that the reverse is true. In those instances where bureaucracies are most effective and transparent—and thus best able to produce effective information—the likelihood of empowerment is greatest.

Figure 3.8 also shows that merely paying attention to values of coefficients and



(a) Model 1a

(b) Model 4a

Figure 3.8: **Varying bureaucratic quality.** Three plots showing the effects of different levels of bureaucratic quality on the expected probability of judicial empowerment for Models 1a, 4a, and 5a (holding all other covariates in each model constant at their mean values). The solid black lines are point estimates, and the dashed gray lines are 95% confidence intervals, drawn from 10,000 simulations.

their statistical significance is not enough to fully understand the predicted probabilities across different values of covariates: at the highest levels of bureaucratic quality shown in Figure 3.8 (a), the confidence intervals are still barely above a 0% probability of empowerment, and the point estimate is still below 0.05. This is despite the fact that bureaucratic quality is statistically significant in Model 1a, and that its coefficient, which is on the same scale as political competition, is larger than that of political competition.

3.4.3 Year

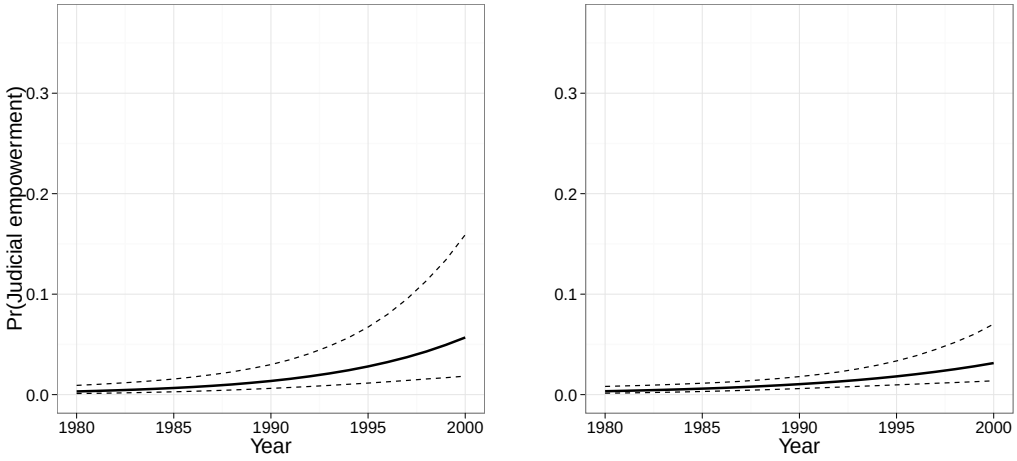
Figure 3.9 is constructed the same as the previous two figures, and plots the effects of time on the expected probability of authoritarian judicial empowerment. It makes clear what the table of regression coefficients does not, namely that a significant

coefficient need not have any substantive importance: the expected probability of judicial empowerment as year is allowed to vary in Models 4a and 5a—the best-fitting models—is only 3.1% and 2.5%, respectively. The upper bounds of the 95% confidence intervals for these two models only reach 7% and 6%, suggesting that the contributions made by the year covariate to the overall predictions of the models are exceedingly minimal.

Similarly, in combination with Figures 3.7 and 3.8 (all figures have the same y-axes for the sake of comparability across covariates), it further illustrates the difficulty in comparing unstandardized coefficients, as similar changes in probability at mean values do not translate to similar changes in expected probabilities across the value of a given covariate. While there is a statistically significant relationship between year and empowerment, the effect is minimal. While this provides some small support for the assertion that there has been a global expansion of judicial independence, and that it extends beyond democracies, it must be stressed just how small (and arguably substantively meaningless) this support is.

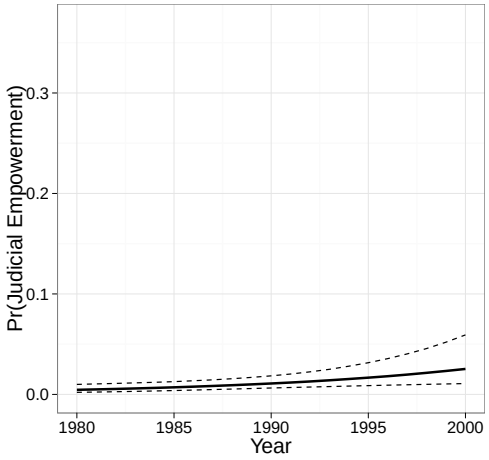
3.5 Conclusions

The implications of this chapter are threefold. First, my critique of the causes asserted in the existing literature on authoritarian judicial politics is supported by empirical evidence. I argue that neither the need to constrain the bureaucracy nor increase foreign direct investment are generalizable accounts for judicial empowerment in non-democracies because both are ubiquitous phenomena and the outcome of interest is rare. This intuitive critique is supported by the statistical results presented. The predicted relationship between the need for foreign capital and the likelihood of empowerment finds no support, as neither the covariate measuring the levels of FDI as a percentage of GDP nor a country's openness to trade reach anything approaching standard levels of statistical significance. Given that the years included in the analysis show a secular trend towards market liberalization, as well as domestic political con-



(a) Model 1a

(b) Model 4a



(c) Model 5a

Figure 3.9: **Varying year.** Three plots showing the effects of different levels of the covariate for year on the expected probability of judicial empowerment for Models 1a, 4a, and 5a (holding all other covariates in each model constant at their mean values). The solid black lines are point estimates, and the dashed gray lines are 95% confidence intervals, drawn from 10,000 simulations.

texts in which investment is increasingly critical, this result is noteworthy (Ginsburg and Moustafa 2008).

The government share of GDP, one of the two measures capturing the need to monitor and sanction large and unwieldy bureaucracies, also fails to approach statistical significance. In some models, the other measure testing this proposed relationship produces a statistically significant result in the direction opposite of what the literature suggests should be the case: in non-democracies, bureaucracies of better quality are associated with higher, not lower, probabilities of judicial empowerment. If it were the case that the costs of controlling these bureaucracies matters in enticing autocrats to empower courts, then the results should be the exact opposite of those found here, suggesting that such an account is flawed.

Similarly, both causal claims regarding the length of autocratic tenure—which has been argued by different scholars to both increase and decrease the likelihood of empowerment—fail to find any support. While these conclusions should not be interpreted as necessarily calling into question the role of FDI, bureaucratic control, or autocratic tenure in the specific case studies in which they were theorized to be important, it does recommend scrutinizing such relationships. More importantly, it calls into question the broader assertions made in the extant literature on the generalizable importance of these three factors in accounting for instances of judicial empowerment. While it might perhaps be that under specific conditions the need for investment drives empowerment, the overall account is not supported in cross-national analysis. Potential conditional relationships between the need for investment and other phenomenon that help explain the contradictory findings in the existing literature and this article suggest further theorizing is warranted.

The second conclusion is that a consistent temporal relationship exists, with the covariate for year demonstrating a significant relationship across models. This provides support for the argument that the power of courts has increased around the globe over the second half of the twentieth century, and that this secular trend holds even in no–democracies. As noted above, however, this conclusion must be clearly qualified by the fact this relationship, while statistically significant, is of minimal

substantive importance, as Figure 3.9 illustrates.

The third and most critical conclusion is that the relationship between competition and authoritarian empowerment hypothesized in Chapter 1 finds support in the analysis presented here: the political insurance explanation appears to hold in non-democracies as well, to the extant that competition is strongly associated with higher levels of judicial empowerment in the data analyzed. These empirical results suggest that the contention by scholars that political competition shouldn't matter in authoritarian regimes—typically offered without thorough analysis as to why—is highly problematic. Rather, the exact opposite appears to be the case. Political competition is by far both the most statistically significant and substantively important covariate in this study, and the relationship is robust across model types and specifications. Figure 3.7 demonstrates clearly that, holding all else constant, increased levels of political competition are associated with substantially higher expected probabilities of judicial empowerment than any other covariate. If it is insurance against the hazards of losing power that drives political actors to empower courts in democracies, this chapter suggests it can surely do the same in non-democracies as well.

Focusing on competition also explains why empowerment is a rare event, something explanations offered in the existing literature are unable to do. Spurring economic growth by increasing FDI is something that most regimes would find desirable, authoritarian or otherwise. That such desires are commonplace in non-democracies while empowerment is uncommon means that discerning any relationship is nigh impossible, and may be why FDI lacks any consistent relationship with empowerment. Significant political competition in non-democracies, on the other hand, is—like empowerment—an uncommon phenomenon. While autocracies are widespread, they do not often face vigorous opposition. And while elections are the norm in most non-democracies, elections producing real electoral threats are not. The results presented here show, however, that when alternatives to those in power are able to control a decent share of the legislature, the credible threat of losing power is associated with

greater probabilities of judicial empowerment.

3.6 Appendix

A previous section of the chapter presents model fit and robustness checks on the models presented in Table 3.1. In this section I present a number of robustness checks using alternative data, to demonstrate that the insights gleaned from the above analysis are not merely a result of specific data choices made.

3.6.1 Multiple imputation

One potential concern regarding the robustness of the models presented is that missingness in the data used to generate them is not random, but systematic, which produces biased model results (King et al. 2001). Though still uncommon in political science, multiple imputation has been shown to be a helpful mechanism in overcoming the problem of non-random missingness in data, and produces better results than listwise deletion (where each observation that has a missing value on one or more covariates is not included in the modeling process) when data are not missing at random.

Unfortunately, it is often difficult to determine whether missing data are missing at random. To overcome this limitation, I use multiple imputation, creating m data sets (in this case 15) and using the known information contained in the original data to predict the values of the missing information. The difference between any one imputed cell across the m data sets is a result of how well the model predicts the given cell; the use of multiple data sets is to include the uncertainties around the predicted quantities into the modeling process. Here, I employ the approach advocated by Honaker and King (2010), which explicitly deals with the issues surrounding multiple imputation in time-series cross-sectional data. All model results presented are the average results over the 15 imputed data sets.

Table 3.4: **Multiple imputation.** Logistic regression output for five models of the empowerment of courts in non-democracies, 1980–2000. Each model is identical to the corresponding model presented in Table 3.1, except that rather than using the 1,274 data resulting from listwise deletion of observations with missing data, results are the averaged results of 15 data sets created via multiple imputation, each with 2,280 observations. Asterisks denote the traditional 0.05, 0.01, and 0.001 levels of statistical significance. *Note:* data are not mean-centered as in prior models, which produces unbiased results but means that the value for the intercept is much larger than in prior models. Similarly, the BIC scores are not reported due to the fact that the log-likelihood is not easily extracted from `Amelia` objects.

	Model 1d	Model 2d	Model 3d	Model 4d	Model 5d
Intercept	−195.15**	−182.56**	−192.24**	−183.73**	−180.66**
	(60.87)	(60.68)	(59.13)	(59.61)	(59.58)
Political competition	3.15**	3.13**	3.39***	3.06**	3.50***
	(1.02)	(1.00)	(0.92)	(1.01)	(0.91)
Bureaucratic quality	1.43	1.58		1.70	
	(1.77)	(1.62)		(1.61)	
Govt share of GDP	−0.01	−0.01			
	(0.02)	(0.02)			
Openness to trade	0.00	0.00			
	(0.00)	(0.00)			
Foreign direct investment	−0.01	−0.00			
	(0.04)	(0.04)			
Years in office	0.01	0.01			
	(0.02)	(0.02)			
French legal heritage	−0.28		−0.30		
	(0.44)		(0.44)		
Socialist legal heritage	0.78		−0.95		
	(0.82)		(0.73)		
Economic growth	0.02				
	(0.02)				
log(GDP/capita)	−0.06				
	(0.22)				
Year	0.10**	0.09**	0.10***	0.09**	0.09**
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)

Unfortunately, creating imputed data effectively is an imprecise task. Determining if large differences between the distributions of observed and imputed values for a given covariate are the function of a data-generating process that is different for

the unobserved values (in which case listwise deletion would introduce systematic bias) is difficult. The dependent variable here (authoritarian empowerment), for example, is much more prevalent in the imputed data than in the "real" data. Furthermore, the observed and imputed distributions for the years in office and bureaucratic quality covariates are dissimilar. Determining whether this is a positive attribute (the data generating process in the case of missing values is different than for observed values) or a negative one (the imputation process requires improvement before one can be comfortable with its results) is an inexact science. Given these reasons, the results in Table 3.4 should be treated with some amount of caution. They provide, however, support for the contention that the relationship between political competition and authoritarian judicial empowerment is not a function of results biased by issues relating to missing data.

3.6.2 Alternative measures of empowerment

As noted above, the results of the model are robust to the use of alternative measures of judicial independence. Tate and Keith (2007) developed a trichotomous measure of judicial independence, drawing upon the US State Department's yearly Country Reports on Human Rights Practices. They analyze to what degree a judiciary's independence was infringed by other state actors during the year in question. As the measure is trichotomous, I collapse the three-level ordinal scale into a binary measure combining the two higher levels, "somewhat independent judiciary" and "fully independent judiciary." This is done because, given the nature of autocratic regimes, it should be much easier to allow for "some" degree of independence than "full" independence, which many consolidated democracies fail to achieve every year. The results are generally consistent with results using the Henisz-derived measure of judicial empowerment.

The results presented in Table 3.5 are encouraging: in Models 2e–5e, political competition reaches the 0.001 level of statistical significance, and in Model 1e it

Table 3.5: **Alternative measure of independence.** Logistic regression output for five models of the empowerment of courts in non-democracies, 1980–2000. Each model is identical to the corresponding model presented in Table 3.1, except that rather than using the Henisz-derived measure of judicial empowerment, a measure derived from the work of Tate and Keith is employed. Asterisks denote the traditional 0.05, 0.01, and 0.001 levels of statistical significance.

	Model 1e	Model 2e	Model 3e	Model 4e	Model 5e
Intercept	3.00^{***}	-0.41^{***}	2.86^{***}	-0.33^{***}	-0.32^{***}
	(0.73)	(0.10)	(0.64)	(0.09)	(0.09)
Political competition	1.96^{**}	2.31^{***}	2.61^{***}	2.39^{***}	2.84^{***}
	(0.65)	(0.60)	(0.56)	(0.57)	(0.54)
Bureaucratic quality	−0.02	2.12^{*}		1.87^{*}	
	(1.05)	(0.89)		(0.81)	
Govt share of GDP	0.01	−0.01			
	(0.01)	(0.01)			
Openness to trade	0.00	0.01^{***}			
	(0.00)	(0.00)			
Foreign direct investment	-0.12^{**}	-0.14^{***}			
	(0.04)	(0.04)			
Years in office	−0.00	0.01			
	(0.01)	(0.01)			
French legal heritage	-0.51^{*}		-0.46^{*}		
	(0.23)		(0.20)		
Socialist legal heritage	-2.33^{***}		-2.23^{***}		
	(0.47)		(0.41)		
Economic growth	−0.00				
	(0.01)				
log(GDP/capita)	0.51^{***}				
	(0.15)				
Year	-0.15^{***}	-0.16^{***}	-0.14^{***}	-0.17^{***}	-0.16^{***}
	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)
BIC	713.77	722.61	706.89	735.68	734.70
N	558	558	558	558	558

reaches the 0.01 level, suggesting that competition is important even when an alternative measure is used. The results of the bureaucratic quality and year covariates, however, are vastly different: while they generally retain their statistical significance, their signs are reversed. Foreign direct investment is statistically significant in Models

1e and 2e, but the sign is consistent with what is predicted by arguments forwarded in the existing literature: more FDI means a lower likelihood of empowerment. Similarly, the two legal origin covariates reach levels of statistical significance, with both predicted a lower probability of empowerment as compared to the reference category of British common law.

It should additionally be noted that determining any relationship is more difficult to the fewer number of observations and temporal coverage; the Tate and Keith measure is only available from 1990 onwards, leaving me with less than half the observations as reported in Table 3.1. That the relationship between competition and empowerment shows a degree of consistency with the measure used above provides a large amount of support for the theoretical argument on the relationship between competition and empowerment, and further leads us to think there is good reason to believe the findings are not a function of the choice of measures.

Chapter 4

VARIATION IN NON-DEMOCRACIES

Using a dichotomous measure of judicial independence, the previous chapter examined the question of judicial empowerment to determine to what degree political competition is able to predict the onset of judicial independence, and in doing so tested the explanation for authoritarian judicial empowerment offered here against a number of competing explanations drawn from the existing literature. Building on that analysis, this chapter seeks to determine whether or not political competition is associated with variation in levels of de facto judicial independence in non-democracies, as well as to triangulate the inferences made in the previous chapter with different measures of democracy, competition, and judicial independence.

As theorized in Chapter 1, the level of political competition observed in non-democratic countries should be associated with higher levels of de facto judicial independence, due to the underlying logic of the insurance model: the risks of life after losing power are key to incentivizing the establishment and maintenance of minoritarian institutions capable of protecting political minorities. If this account extends, as I argue, beyond the context of mature or emerging democracies, an observable implication would be a clear association between levels of political competition and de facto judicial independence.

I find that political competition is consistently associated with significantly higher levels of de facto judicial independence, regardless of the measure of democracy or political competition employed. In other words, regardless of the specific measurement of either phenomenon, the argument asserted in Chapter 1 finds significant support. Similar to the findings from the previous chapter, the quality of a

country's bureaucracy is also strongly associated with the level of judicial independence, and neither the years a leader has been in office nor the level of foreign direct investment are statistically significant. Contrary to the previous chapter, I find that the level of economic development has a highly significant positive relationship with de facto judicial independence.

This chapter proceeds as follows. First, I present the data to be used in the models examining variation in non-democracies. I then present a number of different models employing different measures of democracy (or, more accurately, lack thereof) and political competition. I then present substantive implications of the models, concluding with how the findings of this chapter relate to those of the previous as well as the overall arguments of this dissertation.

4.1 Data and measurement

While a number of the measures of democracy, competition, judicial independence, and various other covariates employed in this chapter are similar to those employed in previous chapters, not all are identical, and a discussion of data and measurement questions is warranted. In addition, as is common in data of time-series cross-sectional nature, significant variation exists in coverage among different measures of the same underlying concept, both cross-sectionally and longitudinally. In other words, the universe of observations employed in this chapter is not identical to the previous chapter, and as such I will briefly present the covariates used in the analysis and how they are distributed across the data.

4.1.1 Democracy

As discussed in Chapter 1, two measures of democracy are employed in different analyses in this dissertation as appropriate. The previous chapter exclusively employed the measure popularized by Cheibub, Gandhi and Vreeland (2009) and Cheibub and Gandhi (2010), based on the measure used in Przeworski et al. (2000). This was done

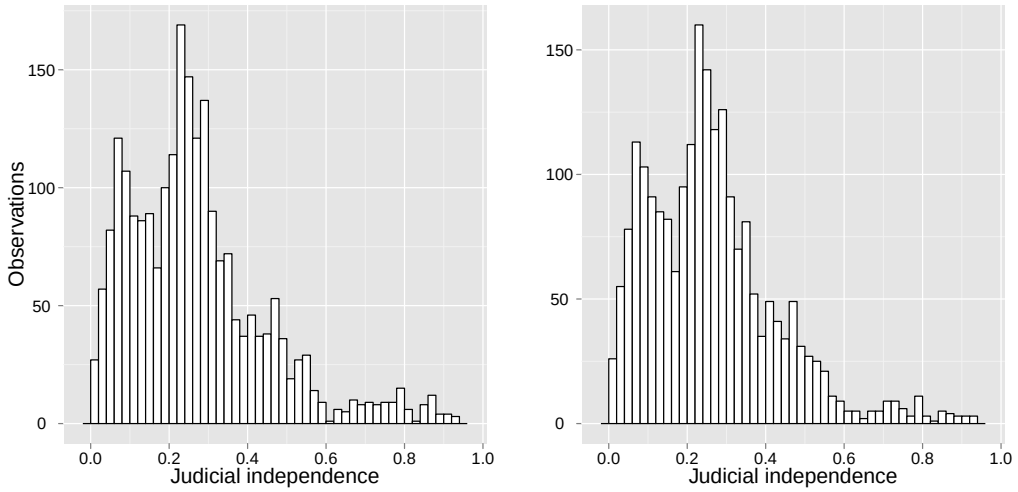
because the measure was created in part to ascertain changes in regime type and as such explicitly includes transitions, which if included in an analysis of authoritarian empowerment would have caused significant problems in determining the onset of judicial independence.

In this chapter, I employ both the Cheibub and Gandhi measure as well as the Freedom House Political Rights measure. As the Freedom House measure is typically measured from 1–7 (with 1 being the highest level of political rights), a cut-off point is required to determine whether or not a country is or is not a democracy. The analyses presented in this chapter and others use the standard demarcation point: those countries scoring 3 or less on the scale are classified as democracies, while those scoring more than 3 are classified as non-democracies.

4.1.2 Judicial independence

Similar to Chapter 2, the measure of judicial independence employed here is Linzer and Staton's (2011) Bayesian item response theory (IRT) measure. The measure is bounded by 0–1, and uses information contained in eight commonly-used measures of de facto judicial independence, weighting each by its agreement with others, as well as explicitly taking into account the time-series cross-sectional nature of the data and temporal dependence.

Figure 4.1 presents the distribution of the Linzer and Staton independence measure across both sets of data used in the analysis: that in which countries are considered non-democratic according to the Cheibub and Gandhi measure and that in which they are considered as such according to the Freedom House Political Rights measure. As can be seen, the distributions of the measure across the two sets of data are highly similar, with only minor differences, something we shall observe with other covariates employed as well.



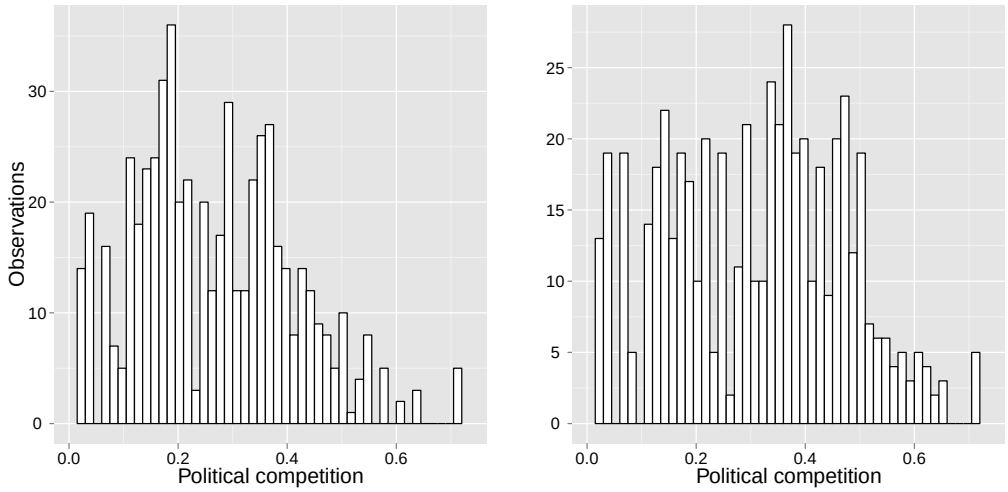
(a) Cheibub and Gandhi measure (b) Freedom House measure

Figure 4.1: **De facto judicial independence.** Two histograms showing the distribution of the de facto judicial independence measure in the data used for the models in Tables 4.1 and 4.2, the former containing 2,286 observations and the latter 2,202.

4.1.3 Political competition

Similar to the previous chapter, the measure of political competition employed here is drawn from the work of (Henisz 2000, 2002). This measure codes competition as a proportion, bounded by 0 and 1. 0 represents a situation in which the legislature has no opposition legislators, in other words when the legislature is completely in the hands of the party of the executive. If 1 were to be observed in the measure (it is not), this would signal a situation in which an executive faces a legislature entirely in the hands of opposition parties.

Figure 4.2 plots two histograms, showing the distribution of the political competition measure in each of the two data sets. Because of the nature of histograms and density plots more generally, observations where competition has a score of 0 are excluded from each plot. This is done because the majority of observations in each data set score 0 for competition, which would make interpreting the variation



(a) Cheibub and Gandhi measure (b) Freedom House measure

Figure 4.2: **Political competition.** Two histograms showing the distribution of the political competition covariate in the data used for the models in Tables 4.1 and 4.2, the former containing 2,286 observations and the latter 2,202. *Note:* In each plot slightly less than 74% of all observations report 0 political competition; these are omitted from the histograms for interpretability.

that does exist in the level of competition when competition exists almost impossible. In both Figure 2 (a) and (b), slightly less than 74% of observations score 0, which would mean a histogram bin rising to over 1,600 observations. If this was included, all the other bins, containing together over 500 observations, would all look identical. What this underscores is the fact that in most non-democracies, political competition is non-existent. As Figure 4.2 illustrates, however, within those observations where political competition does exist in non-democracies, there is significant variation.

4.1.4 Other covariates

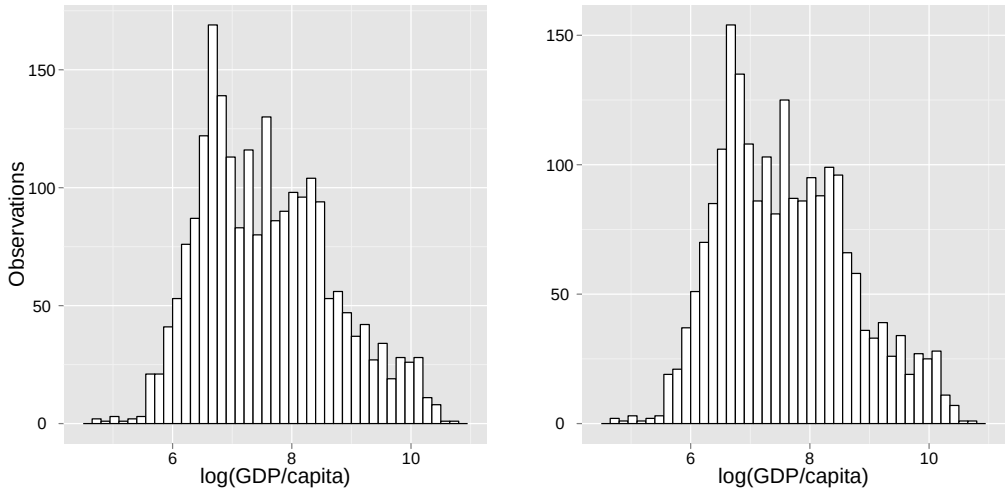
Although the three key phenomena examined in this dissertation are democracy, competition, and judicial independence, a number of other covariates should be relevant to an examination of the relationship between the three. I present here a number of

these, and the reasons for their inclusion in the analysis.

Economic development

The literature on the relationship between the rule of law and economic development is large and varied, with competing findings for most every potential covariate and specification (Haggard, MacIntyre and Tiede 2008). The consensus idea, however, is that the rule of law—of which judicial independence is a key component—results in better economic performance, as it secures property rights and spurs investment by helping the state credibly commit to refrain from engaging in expropriation (North and Weingast 1989; Weingast 1997). The cross-national literature attempting to determine the specific relationship between judicial independence and economic growth is also inconclusive; La Porta et al. (2004) contend that independence has positive effects on property rights and regulatory outcomes, while their co-authors Glaeser and Shleifer (2002) find no association between independence and long-run growth. This is potentially a function of measurement issues and the problems surrounding missing variable bias described above, but it may also be the result of the authors using de jure measures; Feld and Voigt (2003) find that de facto independence is associated with positive economic outcomes (but also face the same problem with regards to potential bias in the results due to missingness).

The direct causal relationship, however, is far from clear. These studies and the proposed causal mechanisms all focus on the determinants of long-run growth, such as the stability of property rights, and the short time span of the empirical analyses conducted to date makes inferring relevance problematic: Feld and Voigt (2003), for example, conduct their analysis on a cross-section of only 59 countries. Interestingly, none of these scholars has seriously considered the idea that the relationship may in fact be reversed: as economies develop and become more complex, the demands for effective and impartial justice increase, which leads to increased judicial independence. This would suggest that the relationship is reciprocal: economic development may not



(a) Cheibub and Gandhi measure (b) Freedom House measure

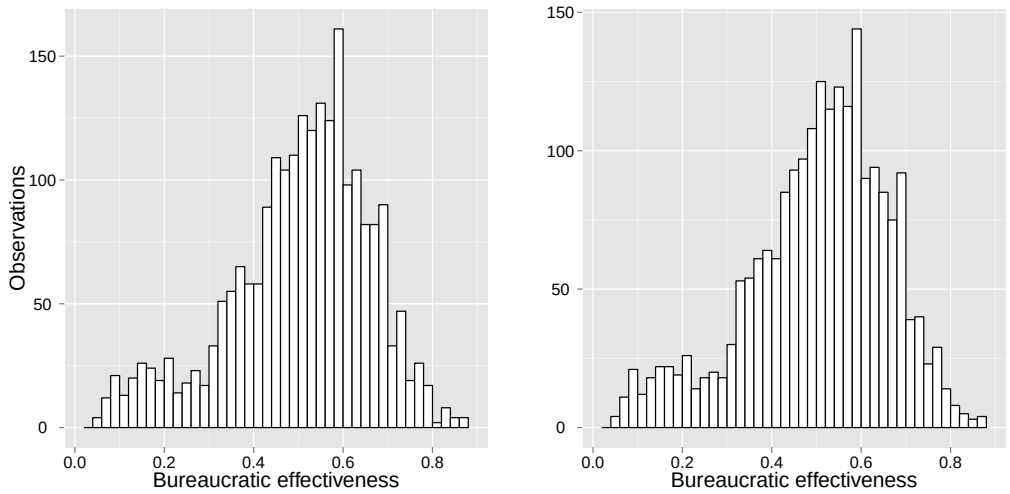
Figure 4.3: **Economic development.** Two histograms showing the distribution of the log of per capita GDP covariate in the data used for the models in Tables 4.1 and 4.2, the former containing 2,286 observations and the latter 2,202.

only be a product, but also a driver, of independent judicial systems. Untangling the nature of this causal relationship is difficult, and as the relationship between economic development and independence is not the focus of this dissertation, I refrain from attempting to directly untangle it.[1] Figure 4.3 plots the distribution of the log of per capita GDP in the two data sets analyzed here.

Bureaucratic quality

Although the findings from the previous chapter are the opposite of what existing explanations focusing on the bureaucracy hypothesize, they were typically highly significant statistically and not without substantive importance. As such, they are included in the quantitative analyses in this chapter as well. Figure 4.4 plots histograms

[1]I do present an argument why the analysis in Chapter 6 should to some degree address the direction of causality.



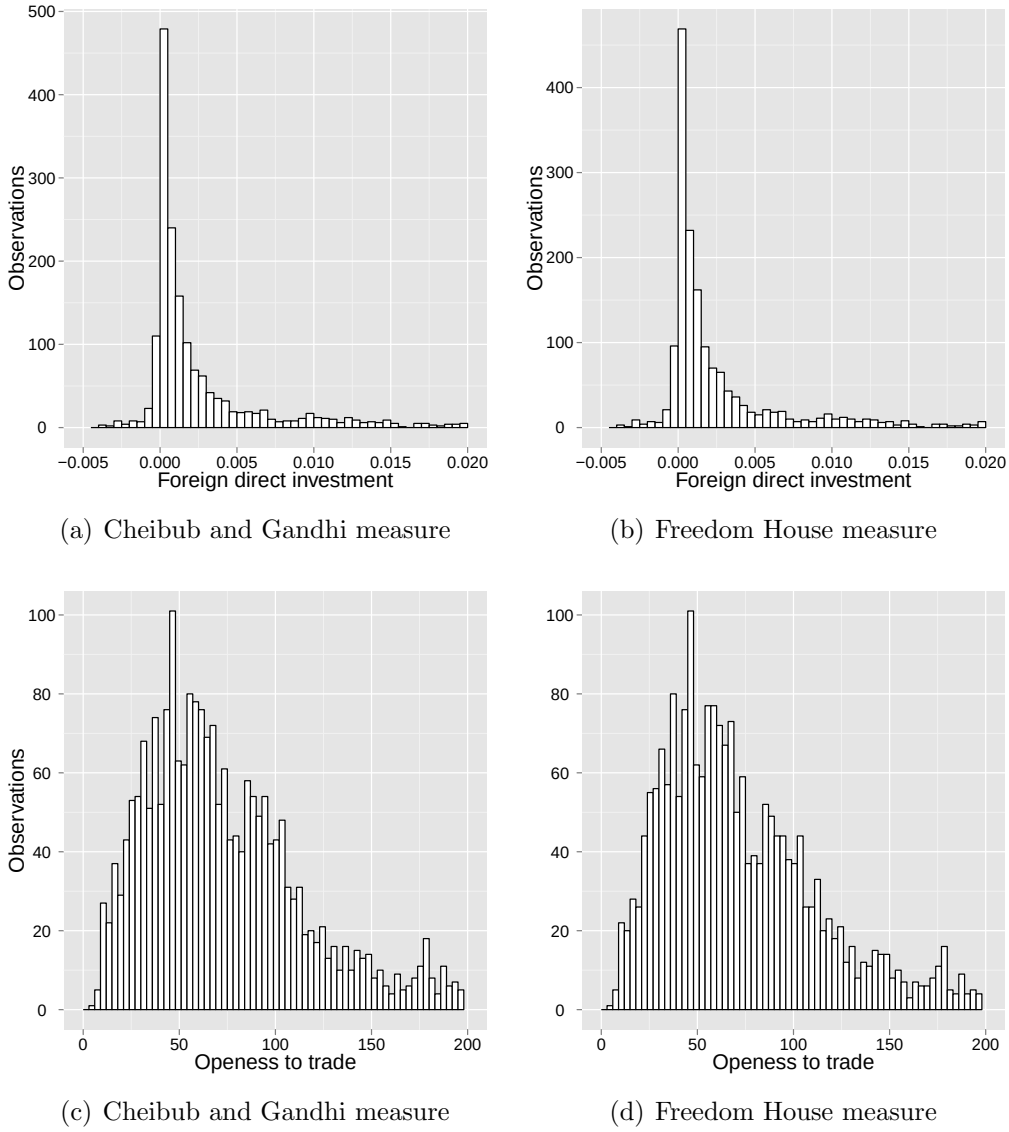
(a) Cheibub and Gandhi measure (b) Freedom House measure

Figure 4.4: **Bureaucratic quality.** Two histograms showing the distribution of the bureaucratic effectiveness covariate in the data used for the models in Tables 4.1 and 4.2, the former containing 2,286 observations and the latter 2,202.

showing the distribution of the Williams (2009) measure of bureaucratic effectiveness in both the Cheibub and Gandhi and Freedom House data. As the government share of GDP included in the previous chapter showed no relationship, it is excluded here (when included it is not statistically significant, and model fit as measured by BIC is worse).

The need for foreign capital

Both covariates measuring the need for a non-democracy to attract foreign capital discussed in the previous chapter are included in the analyses here as well. Histograms of the first, the level of foreign direct investment as a percentage of GDP, are shown in plots (a) and (b) in Figure 4.5, and the second, a country's openness to trade, are shown in plots (c) and (d). It should be noted that none of the four plots show the entirety of the data distributions. Approximately 5% of the observations in each



(a) Cheibub and Gandhi measure

(b) Freedom House measure

(c) Cheibub and Gandhi measure

(d) Freedom House measure

Figure 4.5: **The need for foreign capital.** Four histograms showing the degree to which a given non-democracy might need to attract foreign capital. Plots (a) and (b) show the distribution of foreign direct investment as a percentage of GDP in the two data sets (in each histogram, approximately 5% of the observations fall outside the limits of the x-axis and are omitted for interpretability). Plots (c) and (d) show the distribution of openness to trade for each data set (in each histogram 69 observations (roughly 3%) are above 200 and are omitted from the histograms).

of the FDI plots are outside the limits of the axis: while not significantly far away, the clustering of the data around 0 means that it becomes difficult to glean any information from the histograms if they are included.[2] Roughly 3% of the observations of openness to trade, histograms of which are shown in plots (c) and (d), are beyond the upper limit of the x-axis. Of these, 2/3 are the observations from Singapore, which, as a result of its status as a vital global port has total trade (imports plus exports) significantly larger than its GDP.

Years in office

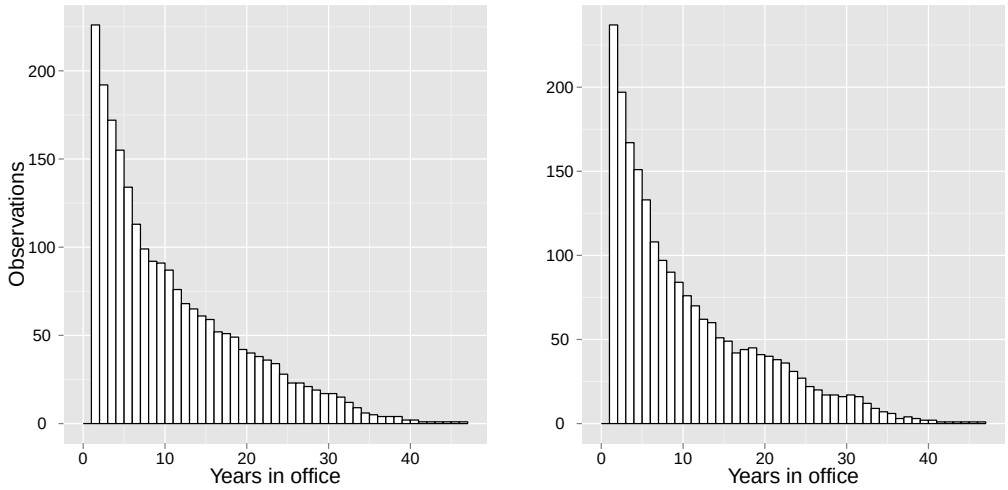
A measure noting the number of years a leader has been in office is also included in the analyses performed in this chapter, similar to those in the previous. Figure 4.6 plots histograms for the distribution of this covariate in both the Cheibub and Gandhi and Freedom House Political Rights data.[3] Looking at plots (a) and (b), it is interesting to note that the years in tenure decrease in a way that makes the histograms look like textbook examples of exponential distributions, suggesting that the hazard function of being an autocratic leader is constant. In other words, the plots suggest that an autocratic leader might have an equal likelihood of losing office in each year, that there is no safety in longevity.

Economic growth

Finally, it might be that case that recent economic performance has an effect on the level of judicial independence, either for the reasons speculated in the existing literature (when economies are performing well, it is possible that autocrats have

[2]Those beyond the upper limits shown in the x-axis are generally instances of a year or two in which a country's economy, and FDI is a not insignificant portion of GDP. Those beyond the lower limit of the x-axis are typically country-years in which large amounts of money are flowing out of the country due to not dissimilar circumstances.

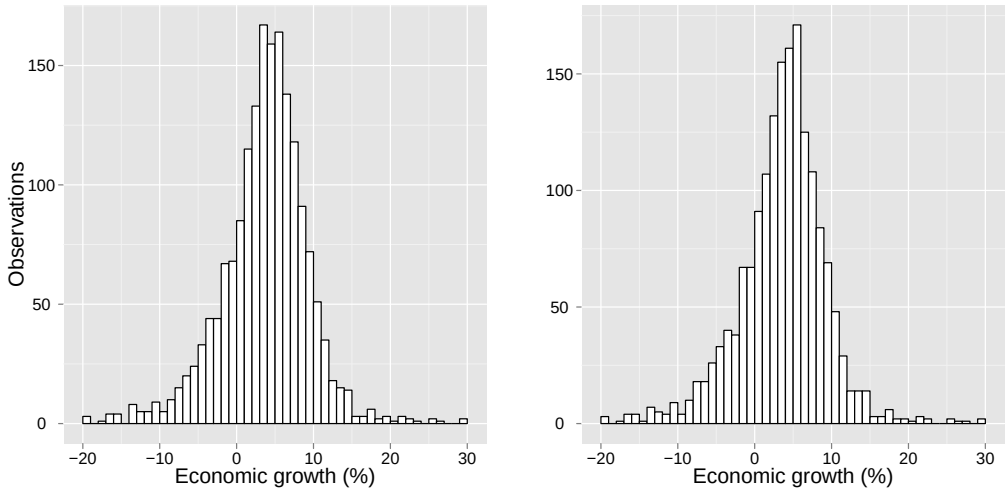
[3]Using the log of this measure, as might be suggested looking at the distribution, produces no change in the results.



(a) Cheibub and Gandhi measure (b) Freedom House measure

Figure 4.6: **Years in office.** Two histograms showing the distribution of the years in office covariate in the data used for the models in Tables 4.1 and 4.2, the former containing 2,286 observations and the latter 2,202.

fewer incentives for increasing judicial independence), or because growing economies have a greater need for courts to mitigate the transaction costs inherent in economic activity. Figure 4.7 plots two histograms of economic growth, one for each data set. Noteworthy is the fact that in both plots, where the x-axis ranges from a 20% decrease in GDP to a 30% increase, fail to contain the entirety of the data. In each histogram 12 observations fall below the lower limits of the x-axis, and 6 above the upper limits. In almost all instances, observations either below or above the limits of the x-axis in Figure 4.7 are those in which a country has just entered into or come out of a significant internal conflict—the countries beyond the axis include Angola, Bosnia Herzegovina, Liberia, and Rwanda—that had seismic impacts on economic production.



(a) Cheibub and Gandhi measure (b) Freedom House measure

Figure 4.7: **Economic growth.** Two histograms showing the distribution of the economic growth covariate in the data used for the models in Tables 4.1 and 4.2, the former containing 1,693 observations and the latter 1,645. *Note:* In each histogram, 12 observations fall below the -20, and 6 observations above 30. These are omitted for interpretability.

Conflict

There is good reason to believe that violent conflict might be negatively associated with the building of effective institutions of governance, including non-democracies. Scholars studying postcommunism specifically have found this to be the case, with conflict associated with poorer economic transition, among other things (Frye 2010). As war and domestic violence typically expand the power of the executive vis-à-vis the other branches of government even in democracies, conflict has ill effects on judicial independence (Rehnquist 1998). In non-democracies, where executives are typically less prone to respect such niceties, it might very well be the case that conflict also has a negative effect on the level of judicial independence.

To measure conflict, I employ the UCDP/PRIO data, from the Uppsala Con-

flict Data Program (UCDP) at the Department of Peace and Conflict Research at Uppsala University and the Centre for the Study of Civil War at the International Peace Research Institute, Oslo (PRIO).[4] These data distinguish among different types of conflict, such as interstate, internal, and internal conflicts that have become internationalized. Models presented in this chapter aggregate these different types of data, although disaggregating them results in no changes in substantive or statistical significance. These data also differentiate among major and minor, with the latter being conflicts with between 25 and 1,000 battle-related deaths a year. A major conflict is one in which 1,000 or more battle-related deaths occur. Again, these are aggregated, though no change results if they are not. 25% of the country-year observations in the Cheibub and Gandhi data undergo one type of conflict, a rate that rises to 27% in the Freedom House Political Rights data.

Common law

To keep the models as parsimonious as possible, rather than employ the four categories of legal heritage found in both the data sets used here (in addition to the three from the previous chapter, a handful of cases with a German legal heritage enter into the universe of cases), I collapse the the measure into a dichotomous measure noting whether or not a country has a British common law heritage. In the Cheibub and Gandhi data, 29% of observations have such a heritage, while 33% of the observations in the Freedom House data are common law. Results presented are robust to using the disaggregated measure: the other 3 categories have almost identical coefficients when doing so, all of which are the same as when aggregating them.

[4]For information, see `http://www.prio.no/CSCW/Datasets/Armed-Conflict/UCDP-PRIO/`

4.2 Analysis

This section presents the results of a variety of models using the data explained in the previous section. The majority of models presented are hierarchical linear models, also commonly referred to random effects or multilevel models. They are partially-pooled models, with random intercepts for country and year. Fixed effects (country and year) models, arguably more conventional in political science, are also presented.[5] One key difference between the two modeling procedures is the amount of information used: hiearchical models model reach andom intercept term (such as country) as normally-distributed, estimating specific random intercepts (each specific country) as being part of this random distribution. The result is that only one degree of freedom is used to estimate these random intercepts (in this case, as two random intercepts are modeled, two degrees of freedom are used). Fixed effects procedures make no assumption about the distribution of the fixed effects, and use one degree of freedom for each specific effect modeled. In the case of the models presented in Table 4.1, this means 130 degrees of freedom are used to estimate the fixed effects in Models 4a and 5a, whereas only two degrees of freedom are used to estimate the random intercepts for country and year in Models 1a–3a.[6]

4.2.1 Model results

The data used in the models presented in Table 4.1 are those country-years that the Cheibub and Gandhi measure classify as being non-democratic, and the results bolster those of the analyses presented in Chapter 3. Model 1a presents all of the covariates discussed in the previous section. Model 2a uses the same data as Model

[5]All hierarchical (mixed effects) models were created using the `lmer` command found in the `lme4` package in *R* (version 2.14.1), and the fixed effects models were created using the `lm` command in base *R*.

[6]There are other key differences between the two modeling procedures, but they are less relevant here. For example, fixed effects models can make estimating time-invariant covariates (such as legal heritage, or regime type, if they do not change) difficult.

Table 4.1: **Using the Cheibub and Gandhi classification.** Five models examining the relationship between competition and de facto judicial independence in non-democracies. The universe of cases is those country-years coded as dictatorships in the Cheibub and Gandhi (2010) data. Models 1a–3a are linear mixed effects (hierarchical) models with intercepts allowed to vary by country and year. Models 4a and 5a are standard linear regression models with country and year fixed effects. Models 2a and 5a present restricted versions of Models 1a and 4a, with fewer covariates and better fit (as measured by BIC). Model 3a presents this restricted model on a larger number of observations (as the variables not included have greater missingness). Asterisks denote the standard 0.05, 0.01, and 0.001 levels of statistical significance, and continuous covariates are mean-centered per standard practice.

	Model 1a	Model 2a	Model 3a	Model 4a	Model 5a
Intercept	0.27***	0.26***	0.26***	0.15***	0.16***
	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)
Political competition	0.15***	0.14***	0.18***	0.13***	0.17***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Bureaucratic quality	0.14***	0.13***	0.06***	0.17***	0.07***
	(0.03)	(0.03)	(0.01)	(0.03)	(0.01)
Openness to trade	−0.00***	−0.00***	−0.00***	−0.00***	−0.00***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Foreign direct investment	0.06			0.01	
	(0.05)			(0.05)	
Years in office	−0.00**			−0.00*	
	(0.00)			(0.00)	
Common law heritage	0.12***	0.12***	0.16***	0.19***	0.23***
	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)
Economic growth	−0.00			−0.00	
	(0.00)			(0.00)	
log(GDP/capita)	0.04***	0.03***	0.03***	0.01	0.02***
	(0.01)	(0.01)	(0.00)	(0.01)	(0.00)
Conflict	−0.00			−0.01	
	(0.00)			(0.00)	
Random effect: country	0.02	0.02	0.02		
	(0.13)	(0.13)	(0.13)		
Random effect: year	0.00	0.00	0.00		
	(0.02)	(0.02)	(0.01)		
Random effect: residual	0.00	0.00	0.00		
	(0.06)	(0.06)	(0.06)		
BIC	−4124.59	−4144.11	−8841.28	−3831.41	−8348.56
N	1630	1630	3399	1630	3399

1a, but presents the restricted model which best fits the data as defined by the BIC score. Here, FDI, years in office, economic growth, and conflict are all stripped from the analysis, and the result is a more parsimonious model with significantly better fit.[7] Model 3a uses the same covariates as Model 2a, but does so on a fuller set of data, as those observations with the pared covariates are no longer included (BIC scores are not comparable when models use different data, so the difference in the BIC scores of Models 2a and 3a is meaningless). Model 4a presents the same covariates as Model 1a, but does so with a fixed effects specification. Note here that the model fit is significantly worse; this is in no small part a function of the fact that Model 4a consumes almost ten times the information as Model 1a due to the nature of fixed effects models. Model 5a follows this same procedure with similar results, modeling the same covariates and data as Model 3a, but using fixed rather than random effects.

Despite the significantly better fit of the random effects (hierarchical) models, what is noteworthy is that the implications of all the models are nearly the same, as the estimated coefficients of the covariates and the error around these coefficients are nearly identical across model specification. Comparing the estimated coefficient for political competition between Models 3a and 5a illustrates this well: the former is 0.18, the latter 0.17. This suggests there is good reason to believe the models are adequately capturing the underlying data-generating process. Looking at the estimated fixed effects and random effects leads to a similar conclusion: the country fixed effects are almost perfectly normally distributed, with a mean very close to zero; these are the assumptions of the random effects specifications.

Table 4.2 presents similar models, this time using the data generated by using the Freedom House Political Rights classification of non-democracy. The results here provide further evidence that the models being used to explain judicial independence in non-democracies are effectively capturing the underlying data-generating process.

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[7]The test of the null hypothesis that the models have the same fit can safely be rejected, as the p-value is less than 0.0001.

Table 4.2: **Using the Freedom House classification.** Five models examining the relationship between competition and de facto judicial independence in non-democracies. The universe of cases is those country-years coded as non-democracies by their Freedom House Political Rights score. Models 1b–3b are linear mixed effects (hierarchical) models with intercepts allowed to vary by country. Models 4b and 5b are standard linear regression models with country-fixed effects. Models 2b and 5b present restricted versions of Models 1b and 4b, with fewer covariates and better fit (as measured by BIC). Model 3b presents this restricted model on a larger number of observations (as the variables not included have greater missingness). Asterisks denote the standard 0.05, 0.01, and 0.001 levels of statistical significance, and continuous covariates are mean-centered per standard practice.

	Model 1b	Model 2b	Model 3b	Model 4b	Model 5b
Intercept	0.29***	0.29***	0.29***	0.23***	0.07***
	(0.02)	(0.02)	(0.01)	(0.03)	(0.02)
Political competition	0.15***	0.15***	0.16***	0.14***	0.15***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Bureaucratic quality	0.16***	0.15***	0.03	0.17***	0.02
	(0.03)	(0.03)	(0.02)	(0.03)	(0.02)
Openness to trade	−0.00			0.00	
	(0.00)			(0.00)	
Foreign direct investment	−0.01			−0.05	
	(0.05)			(0.05)	
Years in office	−0.00***	−0.00***	−0.00***	−0.00***	−0.00***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Common law heritage	0.08**	0.08**	0.10***	0.08**	0.26***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)
Economic growth	−0.00			−0.00	
	(0.00)			(0.00)	
log(GDP/capita)	0.02***	0.03***	0.03***	−0.00	0.01*
	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)
Conflict	−0.01*			−0.01**	
	(0.00)			(0.00)	
Random effect: country	0.02	0.02	0.02		
	(0.13)	(0.13)	(0.13)		
Random effect: year	0.00	0.00	0.00		
	(0.03)	(0.03)	(0.02)		
Random effect: residual	0.00	0.00	0.00		
	(0.06)	(0.06)	(0.06)		
BIC	−3980.69	−4004.12	−5621.51	−3670.91	−5253.81
N	1659	1659	2280	1659	2280

Similar to the results in Table 4.1, the various models in Table 4.2 show significant agreement, with the differences in coefficient estimates minimal. Even more persuasive

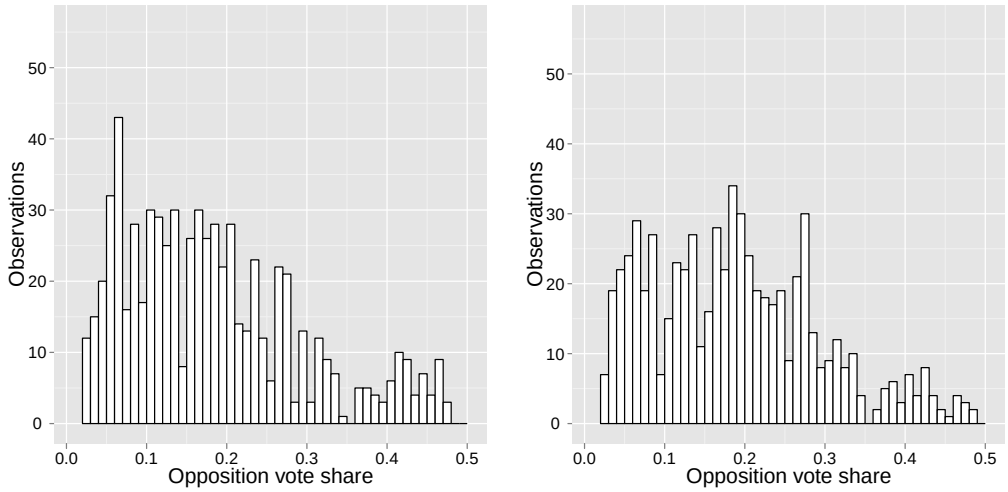
is the overwhelmingly overlap in the results not only within but also *between* the two data sets: the estimate for political competition between models within and across the two data sets are nearly identical. Although there is significant overlap in the observations within each data set, they are not totally identical. Despite this fact, the estimated coefficients show substantial similarities, and there is very little change in levels of statistical significance between the data sets. The only significant differences are with regards to the bureaucratic quality covariate, which does not always attain statistical significance in the models in Table 4.2, and the covariate for openness to trade, the exclusion of which produces better-fitting models in the models using the Freedom House data.

4.3 Model fit and robustness

The similar results across model specification and between data sets that are presented in Tables 4.1 and 4.2 offer persuasive evidence for my contention that political competition affects the level of de facto judicial independence in non-democratic contexts. In addition to this, however, in this section I present a variety of alternative specifications and robustness checks to offer more support; those interested primarily in the substantive implications of the models reported in Tables 4.1 and 4.2 might want to skip to the subsequent section of the chapter.

4.3.1 Alternative measures of competition

As noted above, the results of the models presented in Tables 4.1 and 4.2 are robust not only to the use of multiple measures to determine what is and isn't a democracy, but also to the use of alternative measures of political competition. I employ two other measures of political competition here, to illustrate that my findings that competition is associated with more judicial independence in non-democracies is not merely a function of using the Henisz (2000, 2002) measure of political competition.



(a) Cheibub and Gandhi measure (b) Freedom House measure

Figure 4.8: **Largest opposition party.** Two histograms showing the distribution of the percentage of legislative seats held by the largest opposition party in the data used for the models in Table 4.4, the former containing 1,740 observations and the latter 1,668. *Note:* In plot (a), 7 observations (0.4%) are above the upper limit of the x-axis, and 55% of observations are a situation in which no seats are held by an opposition party. In plot (b) 11 observations (0.7%) are above the upper limits of the x-axis, and 56% are those with no seats held by the opposition. These are not included to improve interpretability.

The first of these alternative measures is the percent of legislative seats controlled by the largest opposition party, drawn from the Database of Political Institutions (Beck et al. 2001). This measure of competition is used to serve as a check on the Henisz measure, as it's construction looks not at the partisian make-up of the legislature overall, but rather just the single largest opposition party. Figure 4.8 plots two histograms, each showing the distribution of the share of legislative seats held by the largest opposition party in the respective data set. It should be noted that like instances of the histograms in many figures presented earlier, because a large number of observations score 0, these were excluded from the histograms for the purposes of interpretability. In the case of plot (a) in Figure 4.8, this is 55% of the observations.

In plot (b) the level is roughly the same, at 56%.

The second alternative measure of competition used is the degree of legislative checks on the executive, also drawn from the Database of Political Institutions (DPI). Although the idea behind this measure is to determine the degree of policy flexibility at any given point in time, it should serve as an effective alternative measure due to the fact that this flexibility is determined by just how much dominance the executive has over the legislature, although such dominance is conceived of very differently than in the other measures.

The legislative checks measure is constructed by aggregating a number of other measures in the DPI. First, in instances when a legislature is not elected, only a single party is able to contest, or only a single party wins votes, the measure takes a value of one. In other cases, the measure is "is incremented by one if there is a chief executive, by a further one if the chief executive is competitively elected, and by a further one if the opposition controls the legislature." (Teorell et al. 2010, 86) Furthermore, in presidential systems, it is further incremented by one for every party in the legislature that is aligned with the president but whose ideological orientation is closer to the main opposition party. In parliamentary systems, it is increased by one for every party in the governing coalition if that party is required to maintain a majority, and a further one for every party in the coalition that has an ideological position closer to the largest opposition party. This conception of competition is different than the one I advance, and as such should provide a rather robust check on my argument. The legislative checks measure captures competition to the degree that the executive requires other parties to maintain power: in cases where a party is not required for governance, the measure is not increased.

Table 4.3 reports the distribution of this measure in the two data sets used here. It illustrates what we might expect: in the majority of observations, the level of legislative checks on the executive are effectively non-existent. In the remaining instances, the number of checks are low, but show some degree of variation, with most

Table 4.3: **Legislative checks.** This table reports the breakdown of (discrete) values for the alternative competition measure noting the number of legislative checks on the executive (drawn from the Database of Political Institutions) in both the Cheibub and Gandhi and the Freedom House Political Rights data sets.

Legislative checks	Cheibub and Gandhi	Freedom House
1	1,756	1,665
2	176	177
3	230	198
4	78	82
5	13	26
6	1	2
7	0	6
8	0	2
10	0	1
11	1	1
N	2,255	2,160

of these scoring 2, 3, or 4, and only a small number scoring above this.

Table 4.4 presents hierarchical linear regression models using these two alternative measures of political competition. The results provide further support for the argument forwarded in this chapter: even when using alternative, potentially weaker, measures of political competition, competition is strongly associated with the level of judicial independence in non-democracies. Model 6 presents the results of the opposition vote share measure of competition when using the Cheibub and Gandhi classification of non-democratic observations, and Model 7 presents the same measure when using the Freedom House Political Rights classification mechanism. Model 8 uses the legislative checks measure on the Cheibub and Gandhi data, and Model 9 uses the legislative checks measure on the Freedom House-derived data. In every single instance political competition is highly statistically significant, and is associated

Table 4.4: **Alternative competition measures.** Four models examining the relationship between competition (and other potentially-relevant covariates) and de facto judicial independence in non-democracies, using the share of legislative seats controlled by the opposition (Models 6 and 7) and the degree of legislative checks on the executive (Models 8 and 9). Models 6 and 8 use the Cheibub and Gandhi (2010) classification of non-democracies, and Models 7 and 9 use the classification of the Freedom House Political Rights measure. All are linear mixed effects (hierarchical) models with intercepts allowed to vary by country. Asterisks denote the standard 0.05, 0.01, and 0.001 levels of statistical significance, and continuous covariates are mean-centered per standard practice.

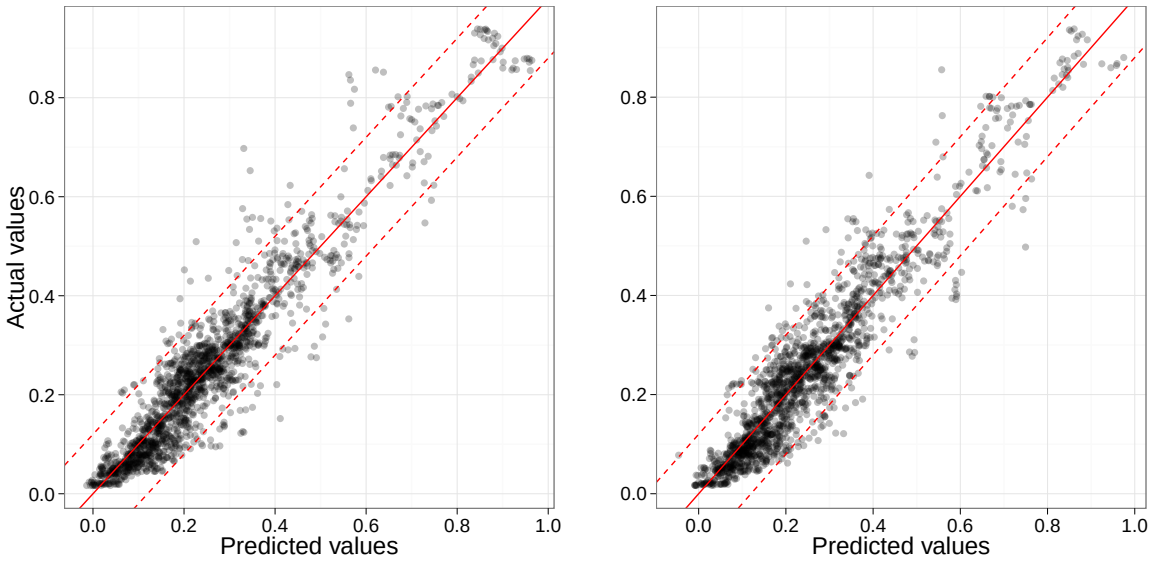
	Model 6	Model 7	Model 8	Model 9
Intercept	0.26***	0.28***	0.27***	0.28***
	(0.02)	(0.02)	(0.02)	(0.02)
Opposition vote share	0.05**	0.07**		
	(0.02)	(0.02)		
Legislative checks			0.01***	0.02***
			(0.00)	(0.00)
Bureaucratic quality	0.16***	0.18***	0.15***	0.16***
	(0.03)	(0.03)	(0.03)	(0.03)
Openness to trade	−0.00***	−0.00	−0.00***	−0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Foreign direct investment	0.09	0.01	0.05	−0.03
	(0.06)	(0.06)	(0.06)	(0.05)
Years in office	−0.00***	−0.00***	−0.00	−0.00***
	(0.00)	(0.00)	(0.00)	(0.00)
Common law heritage	0.12***	0.09**	0.12***	0.09**
	(0.03)	(0.03)	(0.03)	(0.03)
Economic growth	−0.00	−0.00	−0.00	−0.00
	(0.00)	(0.00)	(0.00)	(0.00)
log(GDP/capita)	0.05***	0.03***	0.05***	0.03***
	(0.01)	(0.01)	(0.01)	(0.01)
Conflict	−0.00	−0.00	−0.01	−0.01
	(0.01)	(0.01)	(0.00)	(0.00)
Random effect: country	0.02	0.02	0.02	0.02
	(0.13)	(0.13)	(0.13)	(0.13)
Random effect: year	0.00	0.00	0.00	0.00
	(0.01)	(0.03)	(0.02)	(0.03)
Random effect: residual	0.00	0.00	0.00	0.00
	(0.06)	(0.06)	(0.06)	(0.06)
BIC	−3020.70	−2805.76	−3923.02	−3733.52
N	1304	1250	1664	1568

with substantial changes in the expected value of judicial independence (the seemingly small coefficient of legislative checks measure is unstandardized, and as the measure ranges from 0–11, rather than 0–1, the effect is still large). Similarly, the estimates and errors of the other important covariates remain unchanged, suggesting that their effects are not conditional on the inclusion of specific other covariates, but rather driven by the data themselves.

Overall, the results of Tables 4.1, 4.2, and 4.4 provide extremely persuasive evidence that political competition is of vital importance in understanding variation in the level of judicial independence in the non-democratic states of the world. Furthermore, they suggest that the results are not a function of specific modeling choices made. Both fixed and random effects models produce highly similar results, and despite the measure of competition used or the scheme used to classify observations as non-democratic the results are comparable.

4.3.2 Cross validation

Cross validation is a technique that examines model fit based on one of the gold-standard means of model testing: out-of-sample predictive accuracy. Rather than collect new data (often extremely difficult in some fields, including comparative political economy) and test models on these data, a researcher employing cross validation partitions the data she has into two separate data sets (decisions as to the size of each are often a result of the total size of the data in her possession). The first data set is used as a 'training' or 'fit' set. Here, a model is created in the same manner as traditional quantitative analysis. The estimates from this model are then used on the second 'test' set of data. As the data in this test set were themselves not used to derive the model estimates, the model is thus being tested out of sample. A researcher can then use the differences between the model fit in the training and test sets to determine the degree to which the model is capturing the underlying data-generating process Ward, Siverson and Cao (2007). If the model fit out-of-sample is comparable



(a) In-sample fit

(b) Out of sample fit

Figure 4.9: **Cross validation.** Two plots showing the in and out of sample fit of a slightly modified version of Model 3a in Table 4.1. The 3,399 observations used in Model 3a are partitioned into training and test data sets, and model estimates drawn from the training set are applied to the data in the test set to produce out of sample predictions. The RMSE (root mean squared error) of the in-sample fit of 0.059 increases only slightly to 0.061 in the out-of-sample predictions.

to the model fit in-sample, persuasive evidence exists that the model fits the underlying, empirical process of interest that has generated the data, as opposed to just being a collection of statistically significant effects that fail to accurately capture this process.

Figure 4.9 shows the in and out-of-sample fit for a modified version of Model 3a from Table 4.1, plotting in the actual observed level of judicial independence against the predicted level.[8] Similar to Figure 4.11, each observation is plotted with an alpha

[8]The modification was that year fixed, rather than random, effects were employed. This was done because the cross-validation procedures requires the use of the `nlme` package in *R*, which cannot handle crossed (i.e. non-nested) random effects specifications. Estimates using this hybrid fixed/random effects specification are, predictably, nearly identical to those in either pure fixed or

(transparency) parameter of 1/4 to improve interpretability. Plot (a) illustrates the predictive accuracy of the model in-sample, while plot (b) does so out-of-sample. The solid diagonal line in each is where $x = y$, in other words when the model fits perfectly. The dashed lines are each twice the root mean squared error (RMSE), effectively showing 95% confidence intervals around the line of best fit. As can be seen, almost all of the 1,700 observations in each plot fall within these lines.

The RMSE of the in-sample model shown in Figure 4.9 plot (a) is 0.059, whereas the RMSE of the out-of-sample fit (it is not a model, but rather predicted values using the model estimated on in the 'training' data) shown in plot (b) is 0.061. In other words, there is no discernible difference between how good the model fits on the data used in its creation and new, out-of-sample data not used in its creation. Out-of-sample fit is a gold-standard in assessing model fit, and such remarkable out-of-sample fit provides the strongest evidence yet that the models I am presenting effectively capture the underlying process generating the data. In other words, the models presented are capturing the phenomenon being examined incredibly effectively.

4.4 Exploring substantive significance

The previous two sections of the chapter demonstrated that the models presented fit the data exceptionally well and are robust to alternative specifications. They do not, however, illustrate the substantive significance of the models. In other words, the question remains: do the models actually show that political competition is *important* in understanding judicial independence, and not just statistically significance but irrelevant empirically?

Figure 4.10 plots the expected value of de facto judicial independence as political competition is allowed to vary. Plot (a) shows the results of Model 1a, plot (b) shows the results of Model 3a. Plots (c) and (d) show the results of Models 1b and

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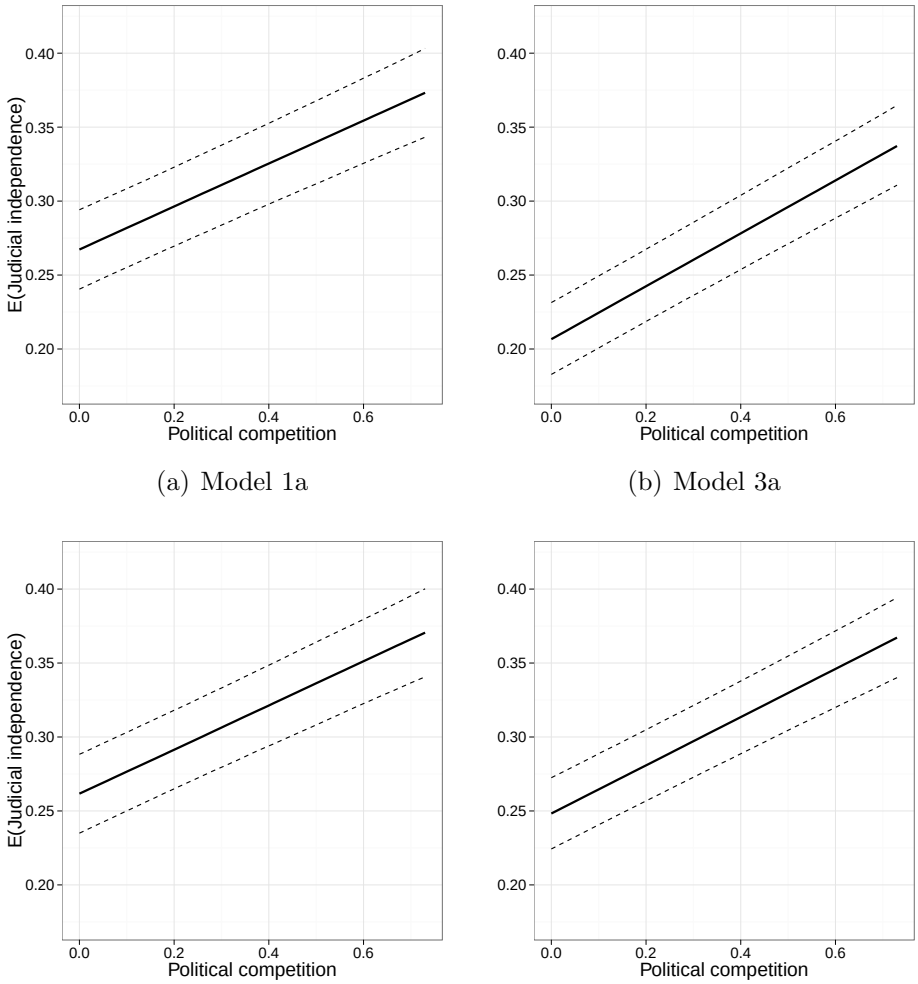
random effects specifications.

3b, respectively. Each is created according to the simulation procedure described in previous chapters: the estimates of the model are treated as the mean of a multivariate normal distribution with a variance equal to the variance-covariance matrix of the model. 10,000 observations are created via simulation with different quantities of interest. Here, political competition is allowed to vary while the other covariates are set to their mean values (common law heritage and conflict are both set to zero). The result is a distribution of predicted values of independence for each possible value of judicial independence, and overall expected values thus containing the fundamental uncertainty in the model rather than just the error surrounding the point estimates.

The results of the four models are highly similar, with only the plot from Model 3a showing anything more than a slight difference. In this case, the expected effects of political competition on judicial independence are slightly more than the other three models: at the lowest levels of political competition, the value of judicial independence is expected to be 0.04-0.07 lower than what the other models. Despite this slight difference, the implications suggested by the four models are all highly similar: as political competition increases, the expected value of judicial independence rises significantly. This is all the more obvious when one considers the fact that in each of the two data sets analyzed (Cheibub and Gandhi and Freedom House Political Rights) the third quartile value of the Linzer and Staton (2011) measure of de facto judicial independence is 0.36 and 0.35, respectively. In other words, for the data being analyzed an expected level of judicial independence of between 0.34–0.37 is not a low level. This is the range of the expected value of judicial independence in each of the plots in Figure 4.10 when political competition is at its highest. The mean value of judicial independence in the data sets is 0.27, which is approximately what the models predict when political competition is at its lowest value (and all other covariates are at their mean values). This suggests that political competition can account for a significant amount of the variation in the level of de facto judicial independence, taking it from it mean (or median, which is 0.25) value to its third



(a) Model 1a

(b) Model 3a

(c) Model 1b

(d) Model 3b

Figure 4.10: **Expected independence as competition varies.** Four plots showing the expected value of judicial independence as political competition is allowed to vary from its observed minimum to observed maximum. Each black line represents the point predictions, and the black dashed lines are 95% confidence intervals around these values. All results are drawn from 10,000 simulations and convey the fundamental uncertainty around the modeling process.

quartile value.

4.5 Conclusions

The results of this chapter provide significant support for the first argument presented in Chapter 1, that the political competition-based insurance model of judicial independence used in the existing literature to explain variation in democracies should also apply to non-democracies. It does by analyzing competition in two data sets of non-democracies, each using a different classifying scheme to determine which country-year observations qualify as non-democracies. The overwhelming similarly between the results of the two different data sets offers evidence that the models presented effectively capture the underlying data-generating process. Further evidence is provided by the similarity of the results within the data sets: both random effects (hierarchical) and fixed effects models produce nearly identical estimates of the relationship between competition and independence, and do so regardless of what other covariates are included and the data used to generate the models.

Results find further support in the robustness and model fit procedures presented: even when using alternate measures of political competition—the seat share of the largest opposition party in the legislature and the degree of legislative checks on the executive—the statistical significance of the measure for political competition remains. Most importantly, the results of the cross-validation procedure conducted persuasively demonstrate that the models accurately capture the data-generating process, as the out-of-sample fit is nearly identical to the in-sample fit. In other words, the model is so apt that it produces the same results even when fit to data not used in its creation.

The empirical analyses presented in this chapter further bolster the conclusions presented in the previous chapter about the relevance of political competition in explaining authoritarian judicial empowerment. The two chapters address related but distinct questions: the onset of judicial independence when independence is measured dichotomously, and variation in independence when the phenomenon is measured

continuously. Together they provide extensive support for my argument about the relevance of the insurance model outside of the democratic context. The second argument, that competition is even more salient in non-democracies as compared to democracies, is the subject of the next two chapters.

4.6 Appendix

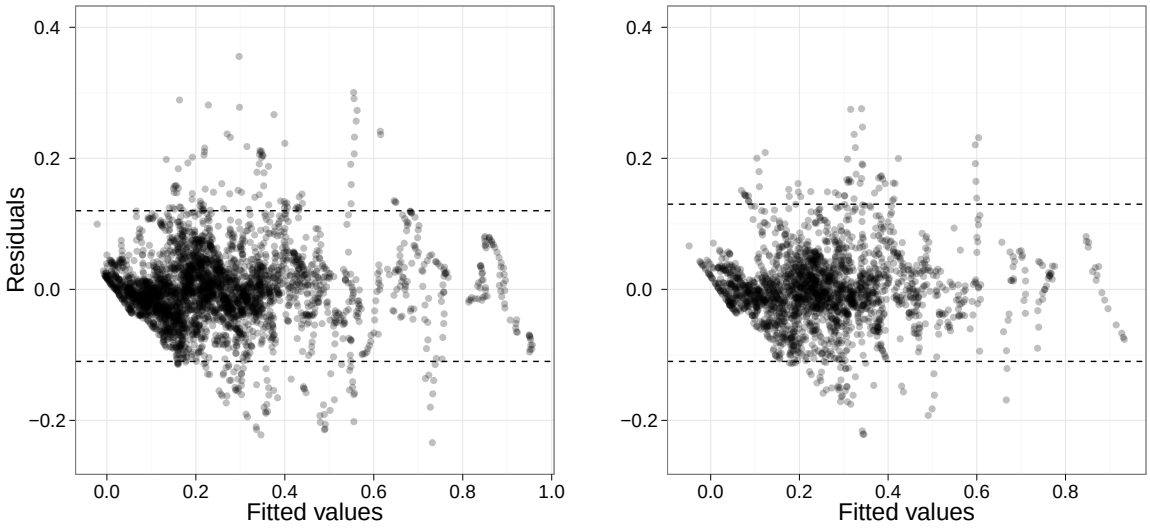
This section presents even more information on model fit than provided in the corpus of the chapter. Residual plots of the main models are presented, as well as multiple imputation to demonstrate that the findings are not merely the result of systematically biased data.

4.6.1 Residual plots

Figure 4.11 presents two scatterplots showing the residuals of Models 3a and 3b plotted against the fitted values of each model. To better improve interpretability, the points of the residuals are set with an alpha (transparency) parameter of 1/4, meaning that four overlapping observations produce one black circle. In each scatterplot the horiztonal dashed lines demarcate the area along the y-axis where 95% of the residuals fall (i.e. two standard deviations from the mean). The scatterplots shows no obvious signs of hetereoskedasticity, as there appears to be significant dispersion across all fitted values, which is even more the case for those observations falling within two standard deviations of the distribution of the residuals. Obviously, this is controlling for the number of observations, as most are predicted to have lower levels of judicial independence, along the x-axis; there are simply fewer observations along the right hand side of each x-axis.

4.6.2 Multiple imputation

The same procedures used to conduct multiple imputation in the previous chapter are used here: the `Amelia` package in the statistical program *R* (Honaker, King and



(a) Model 3a

(b) Model 3b

Figure 4.11: **Residuals of Models 3a and 3b.** Two scatterplots showing the residuals in Models 3a and 3b plotted against the fitted values from each model. Each residual plotted has an alpha (transparency) parameter of 1/4, so four overlapping observations produce one black circle. The black dashed lines in each plot bound the interval where 95% of the observations are found. Plot (a) shows the residuals of 3,399 observations, and plot (b) 2,280.

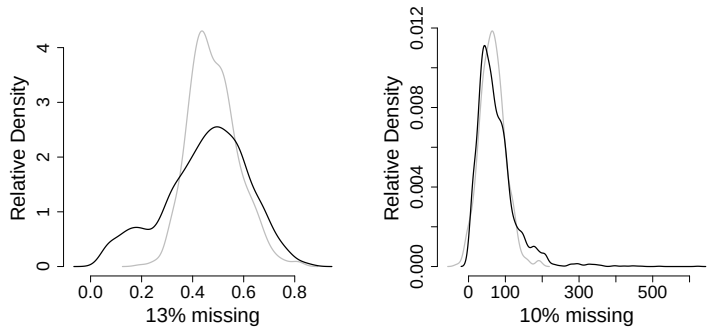
Blackwell 2011). Table 4.5 presents the results of two fixed effects models averaged over 15 imputed data sets (fixed effects rather than mixed effects models are presented due to the significantly easier averaging procedure over the 15 data sets when using ordinary least squares as compared to hierarchical models). As can be seen, the imputed data sets show very little differences when compared to the data used in Table 4.1 that uses listwise deletion of observations with missing variables.

Figure 4.12 presents the distribution of six observed and imputed variables across the 15 multiply-imputed data sets used in the models in Table 4.5. Black lines show the observed distributions, while the gray lines show the distribution of the imputed observations. As can be seen, the imputation model does a good job

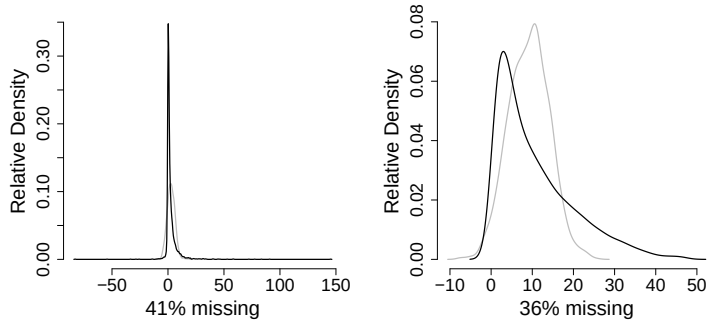
Table 4.5: **Multiple imputation model results.** The averaged results of five linear regression models performed on five imputed data sets, each with country and year fixed effects (not shown), equivalent to Model 5 presented in Table 4.1. Asterisks denote the standard 0.05, 0.01, and 0.001 levels of statistical significance.

	Model 4c	Model 5c
Intercept	-0.09^{***}	-0.09^{***}
	(0.02)	(0.02)
Political competition	0.20^{***}	0.20^{***}
	(0.01)	(0.01)
Bureaucratic quality	0.06^{***}	0.06^{***}
	(0.01)	(0.01)
Openness to trade	-0.00^{***}	-0.00^{***}
	(0.00)	(0.00)
Foreign direct investment	0.01	
	(0.03)	
Years in office	-0.00^{**}	
	(0.00)	
Common law heritage	0.29^{***}	0.29^{***}
	(0.01)	(0.01)
Economic growth	0.00	
	(0.00)	
log(GDP/capita)	0.02^{***}	
	(0.00)	
Conflict	-0.00	
	(0.00)	
N	4321	

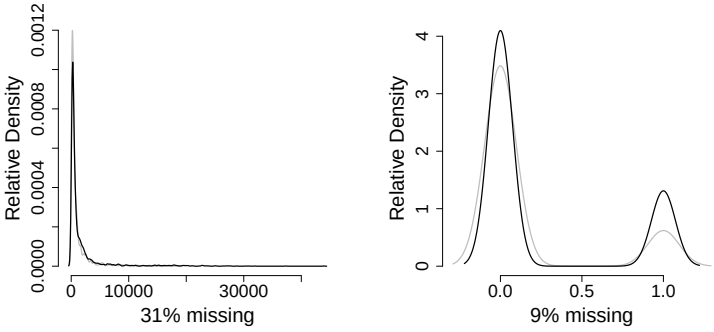
matching the missing and observed distributions (although we do not know if these two distributions should be the same). The common law variable has no missing data, and as such there was no need to impute any information. Unfortunately, imputation model is unable to impute missing values for the political competition covariate to produce a distribution that is similar to the observed data (it imputes too few zeroes); thankfully, the Henisz political competition measure has only 3% of observations with missing data, so the fact that so few of the observations in imputed data sets have imputed data for competition should not produce problems for inference.



(a) Bureaucratic quality (b) Openness to trade



(c) Foreign direct investment (d) Years in office



(e) GDP per capita (f) Conflict

Figure 4.12: **Distribution of imputed variables.** Four plots showing the relative distributions of actual (black lines) and imputed (gray lines) values for the four covariates in Model 5 from Table 4.1 with missing data.

Chapter 5

POLITICAL COMPETITION AND REGIME TYPE

The previous two chapters provided empirical tests the first key argument forwarded in the dissertation, that the insurance model of judicial independence, predicated on the incentivizing effects of electoral competition, also applies in non-democratic contexts. This and the following chapter test the second argument: electoral competition—when present—should be of greater salience in autocracies than democracies due to the greater risks of being a former leader in an autocracy.

In this chapter, I examine the argument that competition should matter more in non-democracies in a global context, looking at over 160 countries for more than 40 years. A variety of models are presented, and results are robust to an exhaustive array of alternative covariates, specifications, and goodness of fit tests. This chapter proceeds as follows. First, I offer a brief discussion of the question of democracy vs. non-democracy, and then present data on competition and independence by regime type. Sections concerning data analysis, robustness and model fit, and quantities of interest are presented sequentially, and conclusions are offered. A chapter appendix presents further robustness checks.

5.1 Democracy and non-democracy

In Chapter 1 I argue that the risks of competition are potentially greater in non-democracies. There are, in brief, four reasons. First, there is a generally greater potential for policy reversals in non-democracies. Autocrats are, on balance, significantly less hampered by other veto players (both institutional and non-institutional), public opinion, and institutionalized bureaucracies than leaders in democracies. As

the case of post-Kuchma Ukraine demonstrates, a former leader can even witness wholesale shifts in the geopolitical orientation of the country, something that has been observed elsewhere in the postcommunist region as well.

Second, the risk of expropriation of property rights are much more significant in non-democracies than they are in democracies, due to the often-intertwined nature of political and economic power in non-democratic polities. Adding to this is the reality that those in power in non-democracies have access to administrative resources that insulate them from the risks of electoral competition more effectively than those in power in democracies. As a result, if one loses power, one hands access to these resources over to one's opponents, often along with access to significant rent-seeking opportunities.

The third reason is that the physical integrity of former autocrats is at much greater risk than former leaders in democracies: ex-autocrats are imprisoned, exiled, and killed at much greater rates than their democratic counterparts, as the Archigos data presented in Chapters 1 and 2 demonstrated (Goemans, Gleditsch and Chiozza 2009). In the 1960–2000 period, more than 40% of former autocrats faced punishment in the year immediately after losing power, and less than half of those punished suffered the comparably-pleasant face of exile. Rather, almost 60% were either imprisoned or killed. This is 25% of *all* former autocrats in this 40 year time span. In other words, a former autocrat has a 1 in 4 chance of being imprisoned or killed after leaving office. However, as Chapter 2 demonstrates, independent courts do in fact provide insurance against such a fate, both in democracies and non-democracies.

Finally, it is possible that competition is more salient in non-democracies because it is less expected, and thus signals that greater levels of competition exist than are actually observed, as well that the uncertainty around outcomes is greater than in democratic regimes. It signals greater competition than it would in democracies because of the extensive access autocrats have to mechanisms for minimizing the electoral performance of their opponents (Levitsky and Way 2010). It signals greater

uncertainty due to the fact that in democratic contexts, a baseline expectation that electoral turnover removing those in power is possible exists, an expectation that is significantly weaker or non-existent in autocracies.

5.2 Data

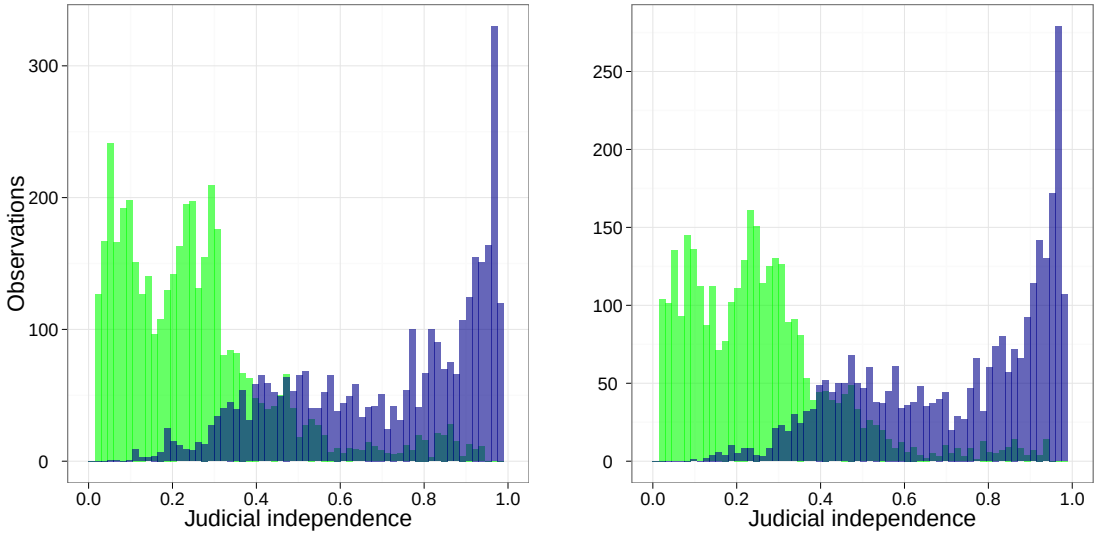
The covariates being analyzed in this chapter should be familiar to readers by now, as all but one have been employed in the preceding chapters. As such, rather than justify the inclusion of these covariates and provide descriptive statistics for them, I will focus specifically on the key dependent and key independent variable (judicial independence and political competition), and then briefly discuss the one new covariate introduced in this chapter's analysis.

A key question for data analysis across regime type is whether there exists significant variation both within and across regime type. From the descriptive statistics provided in the previous chapter, it is clear that both judicial independence and political competition show substantial variation within non-democracies. This variation extends across regime type, as well. Similar to the previous chapter, I present data and conduct analyses using two different classification schemes for regime type to more thoroughly demonstrate the robustness of the results.

5.2.1 Judicial independence

Figure 5.1 plots two histograms, each showing the distribution of Linzer and Staton's (2011) de facto judicial independence measure by regime type in the Cheibub and Gandhi data (a) and the Freedom House Political Rights data (b).[1] The green bins show the distribution of non-democratic observations and the blue bins show the distribution of democratic observations. The darkest bins are where the two overlap (if printed in grayscale, non-democracies are lightest, democracies are darker, and the

[1] In the Cheibub and Gandhi data, 3,871 country-years are non-democratic, while 3,392 are democratic. In the Freedom House data, these numbers are 3,244 and 2,874, respectively.



(a) Cheibub and Gandhi

(b) Freedom House Political Rights

Figure 5.1: **Distribution of judicial independence by regime type.** Two plots showing the in distribution of Linzer and Staton's (2011) de facto judicial independence measure by regime type in the Cheibub and Gandhi data (a) and the Freedom House Political Rights data (b). In both plots, green bins show the distribution in non-democracies while blue bins show the distribution in democracies. The darkest overlapping area is where the two distributions overlap (if printed in grayscale, non-democracies are lightest, democracies are darker, and the overlapping area is the darkest gray).

overlapping area is the darkest gray). It should be noted that the overall distribution of the covariate is not bimodal: neither plot shows a histogram of the overall distribution of judicial independence in either data set. This is because the overlapping areas contain twice the number of observations as the other areas and thus—if the goal is to ascertain the overall distribution—under report the number of total observations at these levels of judicial independence.

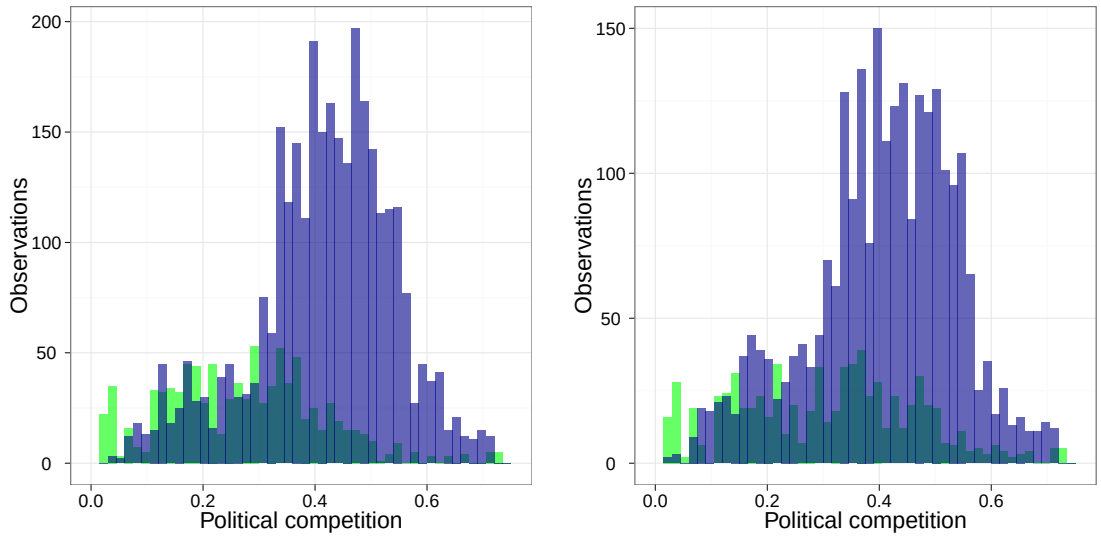
The plots in Figure 5.1 illustrate what we would expect: higher levels of judicial independence are typically found in democracies, and lower levels in non-democracies. There is, however, significant variation within and across regime type: instances of

high judicial independence are found in non-democratic country-years in both data sets, and instances of low independence in democratic observations. The area between 0.4 and 0.5 on the x-axis in each plot further illustrates the variation across regime type, as almost the same number of observations at this level of independence are found in each regime type.

5.2.2 Political competition

Figure 5.2 plots two histograms, each containing the non-zero values of the Henisz (2000, 2002) measure of political competition in the Cheibub and Gandhi and Freedom House-derived data sets. Non-zero values are presented because their inclusion masks the variation within regime type by increasing the height of the y-axis to a degree that the size of the other bins are indistinguishable from one another. This is due to the fact that in Figure 5.2 (a), 78% of non-democratic and 10% of democratic observations score 0 on the competition measure; in plot (b) these percentages are 77% and 11%, respectively. In other words, because the bin for a 0 score would reach to approximately 3,300 on the y-axis in plot (a) and 2,500 in plot (b), the other bins would all be indecipherable. As the goal is to show that the values of the covariate are found throughout the range of possible values across regime-type, doing so would be problematic.

Figure 5.2 illustrates that, predictably, the average level of political competition is significantly higher in democratic polities, as can be seen from the distribution of the blue histogram, which peaks at roughtly 0.4 (in fact, the mean scores for competition in democracies in the two data sets are 0.38 and 0.4). While the mean score for democracies is much higher than in non-democracies (which clearly is to be expected), there is still significant variation within democracies across the different levels of political competition, with the shoulders of the roughly-normally distributed covariate including a large number of observations: exactly one-third of democratic observations in plot (a) are below 0.35, and a quarter are above 0.5. And at some



(a) Cheibub and Gandhi

(b) Freedom House Political Rights

Figure 5.2: **Distribution of political competition by regime type.** Two plots showing the distribution (non-zero values) of the Henisz measure of political competition by regime type in the Cheibub and Gandhi data (a) and the Freedom House Political Rights data (b). In both plots, green bins show the distribution in non-democracies while blue bins show the distribution in democracies. The darkest overlapping area is where the two distributions overlap (if printed in grayscale, non-democracies are lightest, democracies are darker, and the overlapping area is the darkest gray. In plot (a), 10% of democratic and 78% of non-democratic observations have the value 0 and are thus not shown. In plot (b) these percentages are 11% and 77%, respectively.

levels of competition there is striking similarities in the number of observations that are democratic and non-democratic. In both plots, for example, between 0.1–0.3, the number of observations are similar for each regime type.

What Figures 5.1 and 5.2 suggest is that there is good reason to think that comparisons across regime type are possible. Additionally, they suggest that competition might matter differently depending on regime type: obviously, higher levels of competition are to be expected in democracies given the relative frequency distributions of the covariate in democracies and non-democracies. Figure 5.2 demonstrates

that for a non-democracy, 0.4 is a very high level of competition, while for democracies this is not the case. This lends some preliminary support to the possibility that competition might be more salient in non-democracies, tests of which will be presented in the next section of this chapter.

5.2.3 Presidents vs. parliaments

One new covariate not used in previous analyses is presented in this chapter: the relationship between the executive and the legislative branch. It could very well be the case that the nature of this relationship affects the level of judicial independence due to the nature of the powers that the executive has in a presidential system.[2] In other words, it could be the case that a less independent judiciary is another one of the "perils of presidentialism," the existence and extensiveness of which scholars have so thoroughly debated (Horowitz 1990; Linz 1990).

The inclusion of such a covariate might be important if something about the nature of political competition is different in presidential as opposed to parliamentary systems, or if there is something about the nature of democratic vs. non-democratic regimes that is associated with the nature of the executive. That is, if competition is different in parliamentary as opposed to presidential systems, not including a covariate to account for this could lead to omitted variable bias. Given that research ion postcommunist democracies has shown that presidential systems have higher levels of electoral volatility than non-presidential systems, there is reason to worry about potential effects of the nature of the executive on competition (Epperly 2011). Similarly, research in the region has shown that there is a relationship between the openness of a regime and the nature of the executive (Easter 1997; Frye 1997), which opens up the possibility that any effect found for regime type could vary well instead be epiphenomenal to the nature of the executive.

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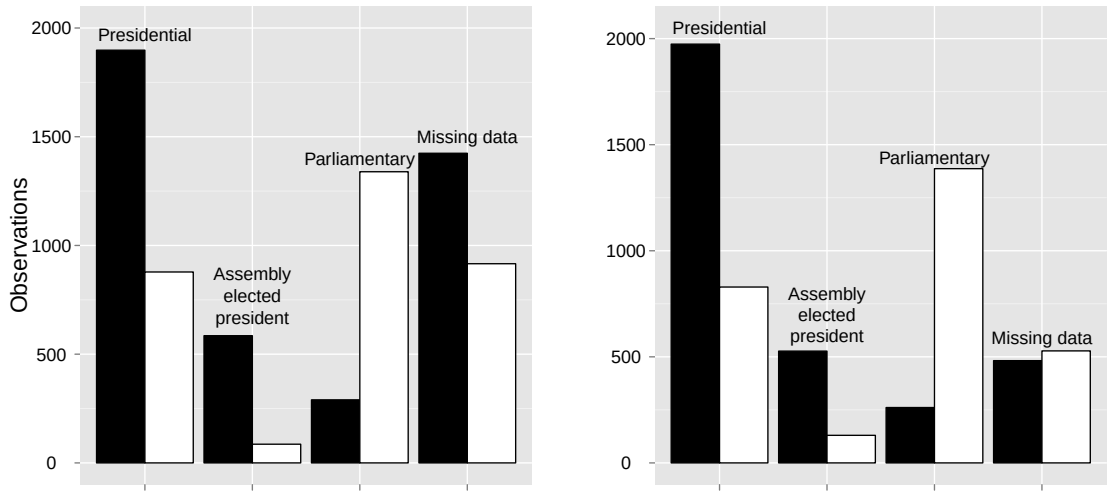
[2]The implications of models presented in the previous chapter do not change if a presidential/parliamentary covariate is included.



(a) Cheibub and Gandhi

(b) Freedom House Political Rights

Figure 5.3: **Nature of executive by regime type.** Two plots showing the distribution of the covariate for the nature of the executive by regime type in the Cheibub and Gandhi data (a) and the Freedom House Political Rights data (b). Black bins show non-democratic country-years, white bins democratic observations. Plot (a) shows the distribution of 7,416 observations, while plot (b) shows 6,118 observations.

Figure 5.3 plots two histograms, each showing the distribution of the covariate capturing the nature of the executive by regime type (the measure is drawn from the Database of Political Institutions, Beck et al. 2001) for the total number of observations in Cheibub and Gandhi and the Freedom House Political Rights data sets where political competition, judicial independence, and the respective regime type covariate are not missing.[3] In each plot the black bins show non-democratic country-years and white bins show democratic observations, and the "missing data" bins show those observations where the three key covariates (independence, competition, and regime

[3]Out of the 7,833 country-years between 1960–2008 in the overall data set, competition is only missing in 376 observations, and judicial independence not at all. With regards to regime type variables, in this time span 311 country-year observations are missing data for the Cheibub and Gandhi measure, and 1,714 are missing for the Freedom House Political Rights measure.

type) are present but the nature of the executive covariate is missing. As Figure 5.3 clearly illustrates, there are in fact systematic differences in the nature of executive across regime type: democracies are much more likely to be parliamentary systems, and presidential systems (straight presidential systems or those with assembly-elected presidents) are much more likely to be non-democratic. Comparing missing data binsacross the two histograms illustrates one of the key differences between using the Cheibub and Gandhi and the Freedom House measure to classify regimes as either democracies or dictatorships: while the Cheibub and Gandhi measure has significant more coverage (mostly temporally), that coverage is often in country-years when other covariates of interest also show significant missingness.

5.3 Analysis

This section aims to test the second argument forwarded in Chapter 1, that the effects of political competition on judicial independence should be greater in non-democracies, and in it I present the results of a number of models using a variety of specifications and alternative covariates. Similar to the previous chapter, most models presented are hierarchical linear models with random intercepts for both country and year, although linear models (ordinary least squares) with fixed effects terms for country and year are also presented to demonstrate the robustness of the random effects models to alternative model specification. The overall results of the various models provide significant support for my argument: in every instance the coefficient for political competition (no matter how it is measured) reaches the $p < 0.001$ level of statistical significance, and in every instance the effects of competition are attenuated in democracies, as the coefficient for the interaction effect between competition and democracy is always negative and always significant at the $p < 0.001$ level of significance.

Table 5.1 reports the results of four models of de facto judicial independence in which the effect of political competition is modeled as conditional on regime type.

Table 5.1: **Competition, regime type, and independence.** Four models hierarchical linear models of judicial independence. Models 1 and 2 use the Cheibub and Gandhi measure of democracy, and Models 3 and 4 use the Freedom House Political Rights measure. Models 2 and 4 are restricted, best-fitting versions of Models 1 and 3, respectively. Asterisks denote the standard 0.05, 0.01, and 0.001 levels of statistical significance.

	Model 1	Model 2	Model 3	Model 4
Intercept	-0.18^{***}	-0.17^{***}	-0.18^{***}	-0.18^{***}
	(0.04)	(0.04)	(0.04)	(0.04)
Political competition	0.16^{***}	0.16^{***}	0.20^{***}	0.20^{***}
	(0.01)	(0.01)	(0.01)	(0.01)
Democracy	0.16^{***}	0.16^{***}		
	(0.01)	(0.01)		
Democracy (FH)			0.13^{***}	0.14^{***}
			(0.01)	(0.01)
Pol. comp. × Democracy	-0.13^{***}	-0.13^{***}		
	(0.02)	(0.02)		
Pol. comp. × Democracy (FH)			-0.14^{***}	-0.14^{***}
			(0.02)	(0.02)
log(GDP/capita)	0.06^{***}	0.05^{***}	0.06^{***}	0.06^{***}
	(0.00)	(0.00)	(0.00)	(0.00)
Common law heritage	0.11^{***}	0.10^{***}	0.09^{***}	0.09^{***}
	(0.02)	(0.02)	(0.02)	(0.02)
Years democratic	0.00^{***}	0.00^{***}	0.00^{***}	0.00^{***}
	(0.00)	(0.00)	(0.00)	(0.00)
Openness to trade	-0.00^{*}		-0.00	
	(0.00)		(0.00)	
Bureaucratic quality	0.08^{***}	0.07^{***}	0.07^{***}	0.07^{***}
	(0.02)	(0.02)	(0.02)	(0.02)
Assembly-elected president	-0.01	-0.01	-0.01^{*}	-0.01^{*}
	(0.01)	(0.01)	(0.01)	(0.01)
Parliamentary system	0.03^{***}	0.03^{***}	0.03^{***}	0.03^{***}
	(0.01)	(0.01)	(0.01)	(0.01)
Interstate conflict	0.00		-0.00	
	(0.01)		(0.01)	
Internal conflict	-0.02^{***}	-0.02^{***}	-0.01	
	(0.00)	(0.00)	(0.00)	
Internationalized internal conflict	0.01		0.01	
	(0.01)		(0.01)	
Random effect: country	0.02	0.02	0.02	0.02
Random effect: year	0.00	0.00	0.00	0.00
Random effect: residual	0.00	0.00	0.00	0.00
BIC	-9421.74	-9440.70	-8972.06	-8998.06
N	3872	3872	3724	3724

Models 1 and 2 use the Cheibub and Gandhi regime type measure to classify observations as either democratic or non-democratic, and Models 2 and 3 do so using the Freedom House Political Rights measure. Models 2 and 4 are the restricted, best-fitting nested models of Models 1 and 3, in which extraneous covariates are omitted from the analysis. Regardless of which classification scheme is used for democracy, the estimated effect of competition is highly significant and positive, and the interaction with democracy is highly significant and negative, while the estimate for democracy itself is highly significant and positive. In other words, the coefficient for democracy informs us that democracy is associated with more judicial independence. The coefficient for political competition tells us the same.

The estimated effect of competition *in democracies*, however, is significantly less than in non-democracies, as the interaction effect between competition and democracy is negative.[4] Because the interaction effect is a conditional relationship between a continuous and a dichotomous covariate, interpreting it is rather straightforward: democracy is a dichotomous (dummy) variable coded 1 when a country-year is a democracy and 0 when a non-democracy. Looking at Models 1 and 2, the estimated coefficient for competition is 0.16, which is the effect of competition regardless of regime type. The estimated coefficient of the interaction effect for these two models is -0.13. Thus, according to Models 1 and 2, a one unit increase in competition is associated with a 0.16 higher judicial independence score in non-democracies, but only a 0.03 increase in democracies, because of the estimated effects (-0.13) of the interaction term. Put simply: competition matters, but it matters a lot more in non-democracies.

The results of the models in Table 5.1 further strengthen the findings presented in the previous chapters, as the estimated effects of most other covariates are highly

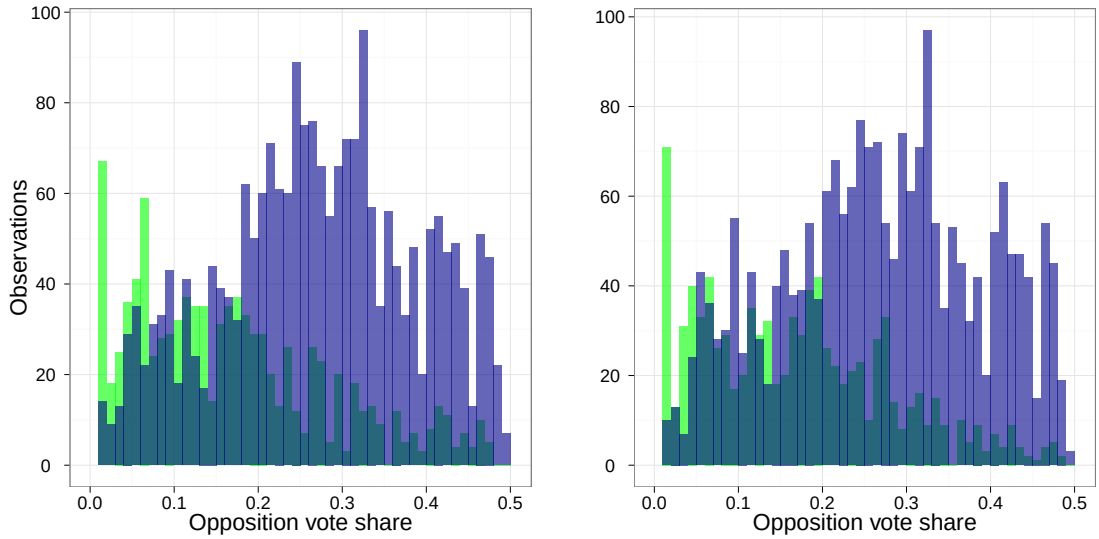
[4]Though not shown for reasons of space, models without the interaction effect show significantly poorer fit as measured by BIC, further suggesting that the nature of competition is in fact conditional: the the null hypothesis that both models show similar fit can be rejected with a test statistic of $p < 0.00000001$.

similar, despite the fact that these models use approximately twice as much data and include a vastly different universe of observations. Comparing these results to those of Table 4.1 in Chapter 4, one observes that all covariates that are statistically significant in the models only analyzing non-democracies remain significant in democracies, and that in many instances the estimates are nearly the same. The estimate of the coefficient for political competition, for example, range between 0.15–0.18 in the models in Table 4.1, and in the models in Table 5.1 this estimate ranges from 0.16–0.2. These similarities are striking given the significantly larger number and significantly different type of observations included across the two chapters. Similarly, the coefficients for common law and openness to trade are nearly identical. On the other hand, the estimated effect of bureaucratic quality is decreases somewhat when democracies are included in the analysis, while the effect of economic development (GDP per capita) is slightly larger; both remain highly statistically significant. Finally, the covariate indicating the nature of the executive distinguishes a very significant effect for parliamentary systems, finding that they have slightly higher (0.03) levels of judicial independence than presidential systems (the unreported baseline category). No statistically-distinguishable difference is found between popularly-elected and assembly-elected presidents.[5]

5.4 Model fit and robustness

The results of Table 5.1 provide preliminary evidence in support of my argument that competition is more salient in non-democracies than in democracies. In addition, in this section I present a variety of alternative specifications and robustness checks to offer more support; those interested primarily in the substantive implications of the models reported in Table 5.1 might want to skip to the subsequent section of the

[5]It should be noted that there is no evidence of a conditional relationship between the nature of the executive and either competition or regime type: in no instance does an interaction effect between the variables prove to be statistically significant, and if either or both interaction effects are added to a model, its fit decreases.



(a) Cheibub and Gandhi (b) Freedom House Political Rights

Figure 5.4: **Distribution of opposition share by regime type.** Two plots showing the distribution of the opposition share of legislative seats by regime type in the Cheibub and Gandhi data (a) and the Freedom House Political Rights data (b). In both plots, green bins show the distribution in non-democracies while blue bins show the distribution in democracies. The darkest overlapping area is where the two distributions overlap (if printed in grayscale, non-democracies are lightest, democracies are darker, and the overlapping area is the darkest gray. *Note:* In plot (a), 4% of democratic observations are 0 and 1% are greater than 0.5 and are thus not shown. For non-democracies these numbers are 55% and 0.3%. In plot (b) the respective numbers are 4% and 1% for democracies and 55% and 0.5% for non-democracies.

chapter.

5.4.1 Alternative measures of competition

Similar to the analysis presented in the previous chapter, I use the percentage of legislative seats controlled by the largest opposition party as well as the number of legislative checks on the executive as robustness checks on the main models that employ the Henisz measure of political competition. Figure 5.4 shows two histograms

Table 5.2: **Legislative checks.** This table reports the breakdown of (discrete) values for the alternative competition measure noting the number of legislative checks on the executive (drawn from the Database of Political Institutions) by regime type, in both the Cheibub and Gandhi and the Freedom House Political Rights data sets.

	Cheibub and Gandhi		**Freedom House**	
Legislative checks	Non-democracy	Democracy	Non-democracy	Democracy
1	1,509	118	2,038	159
2	220	257	218	258
3	307	650	267	672
4	102	735	113	708
5	19	350	33	316
6	2	94	3	93
7	0	51	6	44
8	0	13	3	10
9	0	13	0	13
10	0	2	1	1
11	1	2	1	2
16	0	3	0	3
17	0	3	0	3
18	0	2	0	2
N	2,200	2,322	2,683	2,284

plotting distribution of the share of seats of the largest opposition party covariate by regime type, each plot showing the data of one of the two regime classification measures. Because of the large amount of observations where no seats are controlled by an opposition party, those observations scoring 0 are omitted from both plots, as are observations above 0.5 (of which there are very few, but which require significantly increasing the size of the x-axis and thus decreasing interpretability). Figure 5.4 illustrates that significant variation exists in the alternative measure of competition both within and across regime type.

Table 5.2 consists of cross-tabulations of the legislative checks on the executive measure. Each row of the table contains counts of the number of observations at that

discrete level of legislative checks, disaggregating both regime type and data set by column. It demonstrates that significant variation exist both within and across regime type for both data sets, and further shows that the two data sets evidence a large degree of agreement on the distribution of checks.

The results of models using these two alternative measures of political competition are reported in Table 5.3. Model 5 interacts the opposition vote share covariate with the Cheibub and Gandhi democracy measure, and Model 6 interactions the vote share covariate with the Freedom House measure. Models 7 and 8 use the legislative checks covariate, interacting it with the Cheibub and Gandhi and Freedom House Political Rights democracy measures, respectively. Again, each measure of competition is highly statistically significant in both data sets, as is the coefficient for the democracy measure as well as the interaction term between the two. In each of the four combinations the estimated coefficient of the interaction term is negative, meaning that in democracies the effect of the political competition estimate is attenuated by the value of the respective interaction coefficient. In Model 5, this means that the estimated effect of opposition vote share, 0.09, is decreased by the estimated effect of the interaction term, 0.08. In other words, the effect of competition within democracies is significantly less than it is in non-democracies, which is precisely the same relationship between competition, independence, and regime type found in the models reported in Table 5.1.

5.4.2 Alternative model specification

Figure 5.4 presents four additional models, each with an alternative specification to further examine the limitations and implications of the argument being tested in this chapter. As the implications of Model 9 are not straightforward and require thorough discussion, Models 10–12 will be discussed first. Model 10 aggregates the three different types of conflict—interstate, internal, and internationalized internal conflict—considered separately in other models into one overall measure of conflict.

Table 5.3: **Alternative measures of competition.** Four hierarchical linear models using measures of competition (country and year effects not reported). Models 5 and 6 use the share of seats controlled by the largest opposition party, and Models 7 and 8 use legislative checks on the executive. Models 5 and 7 employ the Cheibub and Gandhi measure of democracy, while Models 6 and 8 use the Freedom House Political Rights measure. Asterisks denote the standard 0.05, 0.01, and 0.001 levels of statistical significance.

	Model 5	Model 6	Model 7	Model 8
Intercept	-0.22^{***}	-0.24^{***}	-0.20^{***}	-0.21^{***}
	(0.04)	(0.04)	(0.04)	(0.04)
Opposition vote share	0.09^{***}	0.12^{***}		
	(0.02)	(0.02)		
Legislative checks			0.02^{***}	0.02^{***}
			(0.00)	(0.00)
Democracy	0.17^{***}		0.19^{***}	
	(0.01)		(0.01)	
Democracy (FH)		0.13^{***}		0.16^{***}
		(0.01)		(0.01)
Opp share × Democracy	-0.08^{***}			
	(0.02)			
Opp share × Democracy (FH)		-0.10^{***}		
		(0.02)		
Leg. checks × Democracy			-0.02^{***}	
			(0.00)	
Leg. checks × Democracy (FH)				-0.02^{***}
				(0.00)
log(GDP/capita)	0.06^{***}	0.06^{***}	0.05^{***}	0.06^{***}
	(0.00)	(0.01)	(0.00)	(0.00)
Common law heritage	0.10^{***}	0.08^{***}	0.10^{***}	0.09^{***}
	(0.02)	(0.02)	(0.02)	(0.02)
Years democratic	0.00^{***}	0.00^{***}	0.00^{***}	0.00^{***}
	(0.00)	(0.00)	(0.00)	(0.00)
Bureaucratic quality	0.12^{***}	0.12^{***}	0.10^{***}	0.10^{***}
	(0.02)	(0.02)	(0.02)	(0.02)
Assembly-elected president	0.01	-0.01	-0.01	-0.02^{**}
	(0.01)	(0.01)	(0.01)	(0.01)
Parliamentary system	0.05^{***}	0.05^{***}	0.03^{***}	0.02^{***}
	(0.01)	(0.01)	(0.01)	(0.01)
Interstate conflict	-0.00	-0.01	-0.00	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)
Internal conflict	-0.02^{***}	-0.00	-0.02^{***}	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Internationalized internal conflict	0.00	0.01	0.00	0.01
	(0.01)	(0.01)	(0.01)	(0.01)
BIC	-7789.06	-7335.82	-9210.02	-8688.17
N	3256	3136	3803	3658

The result is a statistically significant covariate with no substantive significance, as conflict is only associated with a 0.01 decrease in judicial independence. Looking at the BIC scores of Model 10 and comparing it to Model 1, it is impossible to assert that one fits the data better than the other, as one cannot reject the null hypothesis in a test of the differences in their BIC scores (the test statistic has a p-value of 0.15).

Instead of using the common law heritage covariate like all other models, Model 11 disaggregates the non-common law category into four categories: French, Communist, German, and Scandinavian legal heritage. The estimated coefficients are similar. In other models a common law country has approximately a 0.10 higher level of judicial independence; in Model 11 French and communist legal heritages are associated with a judicial independence score -0.13 and -0.11 lower than the common law baseline category. There is no statistically significant association between German or Scandinavian legal heritage and judicial independence, which is likely a function of the relative lack of data in these cells (the British and French were, after all, much "better" at colonization than the Germans or the Swedes). Regardless of how the legal covariate is measured—a common law dummy or a categorical measure—the fit of the models is the same: comparing the BIC scores of Models 1 and 11, a statistical test of the null hypothesis that they fit the data the same cannot be rejected (the test statistic has a p-value of 0.13).

Instead of using a hierarchical (multilevel) model specification (with random intercepts for country and year) like Models 1–11, Model 12 is a linear regression model (ordinary least squares) with country and year fixed effects. The estimated coefficients of the covariates in Model 12 are nearly identical to those in Model 1 (which analyzes the same data and uses the same covariate structure, only fixed vs. random effects), suggesting that the results presented are robust to alternative specifications of the any country or year-specific omitted variables. Comparing the model fit of Model 12 to Model 1, it is clear that the random effects specification found in Models 1–11 is preferable to the fixed effects structure used in Model 12.

Table 5.4: **Alternative model specification.** Model 9 omits the nature of the executive covariate to analyze an addition 1,916 observations. Model 10 collapses the three forms of conflict into one measure. Instead of a dichotomous common law variable, Model 11 uses the categorical legal origins covariate. Model 12 is a linear regression model with fixed effects (country and year), using the same covariates and data as Model 1. Models 9–11 are linear mixed effects (hierarchical) models with intercepts allowed to vary by country and year; estimates are omitted for reasons of space. Asterisks denote the standard 0.05, 0.01, and 0.001 levels of statistical significance.

	Model 9	Model 10	Model 11	Model 12
Intercept	-0.12^{***}	-0.18^{***}	-0.07	-0.25^{***}
	(0.02)	(0.04)	(0.04)	(0.04)
Political competition	0.21^{***}	0.16^{***}	0.16^{***}	0.15^{***}
	(0.01)	(0.01)	(0.01)	(0.01)
Democracy	0.15^{***}	0.16^{***}	0.16^{***}	0.16^{***}
	(0.01)	(0.01)	(0.01)	(0.01)
Pol. comp. × Democracy	-0.07^{***}	-0.13^{***}	-0.13^{***}	-0.12^{***}
	(0.02)	(0.02)	(0.02)	(0.02)
log(GDP/capita)	0.05^{***}	0.06^{***}	0.06^{***}	0.04^{***}
	(0.00)	(0.00)	(0.00)	(0.01)
Common law heritage	0.13^{***}	0.10^{***}		0.21^{***}
	(0.03)	(0.02)		(0.02)
French legal heritage			-0.13^{***}	
			(0.02)	
Communist legal heritage			-0.11^{***}	
			(0.03)	
German legal heritage			0.07	
			(0.06)	
Scandinavian legal heritage			0.10	
			(0.06)	
Years democratic	0.00^{**}	0.00^{***}	0.00^{***}	0.00^{***}
	(0.00)	(0.00)	(0.00)	(0.00)
Bureaucratic quality	0.11^{***}	0.07^{***}	0.08^{***}	0.07^{***}
	(0.01)	(0.02)	(0.02)	(0.02)
Openness to trade		-0.00^{*}	-0.00	-0.00
		(0.00)	(0.00)	(0.00)
Assembly-elected president		-0.00	-0.01	-0.01
		(0.01)	(0.01)	(0.01)
Parliamentary system		0.03^{***}	0.03^{***}	0.03^{***}
		(0.01)	(0.01)	(0.01)
Conflict		-0.01^{**}		
		(0.00)		
Interstate conflict			0.00	0.00
			(0.01)	(0.01)
Internal conflict	-0.02^{***}		-0.02^{***}	-0.02^{***}
	(0.00)		(0.00)	(0.00)
Internationalized internal conflict			0.01	0.01
			(0.01)	(0.01)
BIC	−12940.74	−9425.83	−9417.89	−8936.25
N	5788	3872	3872	3872

As illustrated in Figure 5.3, the covariate for the nature of the executive has severe problems of data missingness, which might affect the estimates presented when the covariate is included. To ascertain the degree to which this might be the case, Model 9 excludes this covariate (as well as the insignificant openness to trade covariate). The finding here is interesting: the coefficient estimate of political competition increases, while the estimate of the interaction term decreases (or, rather, as it is negative, increases more towards zero).[6] That is, when the nature of the executive is not included in the model and the number of observations increases by nearly 2,000, the effect of political competition is even larger.

This is *not* the case if the analysis is restricted to the 3,872 observations used in Model 1. Using those restricted data, the excluding the nature of the executive covariate produces no change whatsoever in the coefficients of political competition, democracy, or the interaction term. In other words, it appears that what is driving the difference between the estimates for competition and the interaction term between Models 1 and 9 is *not* omitted variable bias introduced by not including the covariate for nature of the executive, but rather driven by the significant amount of information being excluded from the analysis in Model 1. Again, this is because of the significant temporal restrictions introduced in the analysis when including the nature of the executive covariate, as it is unavailable before 1980, whereas competition and judicial independence are available from 1960 on, with very little missingness.

This presents a conundrum: does one use the estimates produced by models that include the covariate for the nature of the executive, despite the fact that doing so clearly biases the results due to what appears to by systematic differences in the data being excluded? Or does one omit a covariate that is consistently statistically

[6]If a model is conducted only regression the dependent variable of judicial independence on the interaction between political competition and democracy (plus the random effects), the number of observations included is 7,416, and the values of the three estimated coefficients are almost identical to those in Model 9: competition increases from 0.21 to 0.23, the effect of democracy is the same, and the interaction term increases from -0.07 to -0.08.

significant and increases model fit, potentially introducing omitted variable bias? Given that doing the latter means increasing the number of observations by two thirds, and that the estimated effect of the nature of the executive covariate is substantively very small (0.03 in Models 1–4), I am inclined to omit the covariate and use the vastly larger amount of information available.[7] In the following section, however, when I explore the substantive significance of competition, I will present results from multiple models to best assess the implications of alternative model specifications.

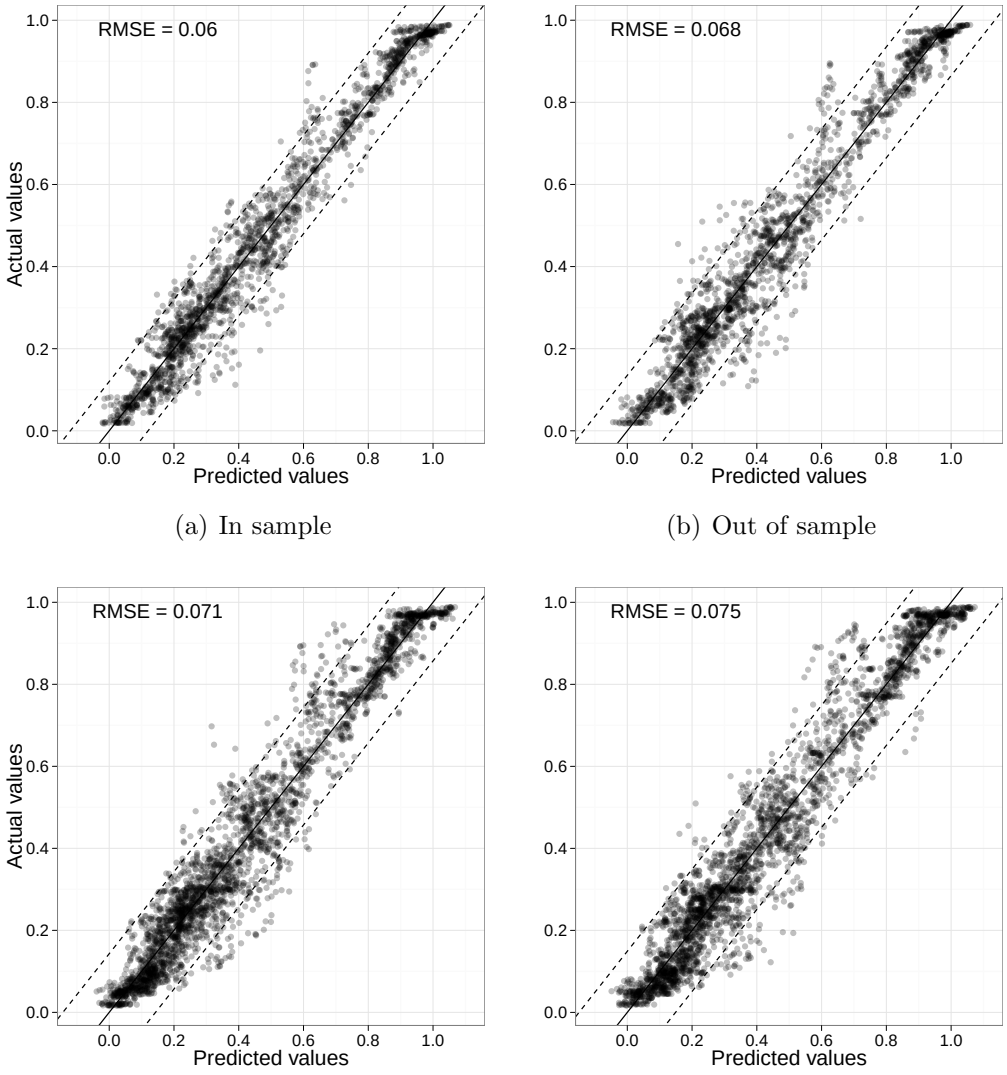
A critical point to stress is that the difference in estimates between Model 9 and previous models in absolutely no way affects the test of my argument about the conditional nature of political competition and regime type. In all models, including Model 9, the effects of competition are estimated to be greater in non-democracies than in democracies. If anything, settling on Model 9 as the best inferential model attenuates the difference in the effects across regime type: while competition is still a lot more salient for judicial independence in non-democracies than in democracies according to Model 9, it is also important in democracies, whereas the other models would lead one to conclude that it is not of much substantive import in democratic contexts.

5.4.3 Cross validation

Similar to the previous chapter, I employ cross validation techniques to determine the out-of-sample fit of Models 2 and 9. Again, out-of-sample fit is one of the most stringent standards of model fit, and one can have substantial confidence that models that fit well both in and out-of-sample are doing an effective job at capturing the underlying data-generating process.

Figure 5.5 presents the results of cross validations of Models highly similar to

[7]The results of multiple imputation, presented in the chapter appendix, provide further support for thinking Model 9 is the best model.



(a) In sample

(b) Out of sample

(c) In sample

(d) Out of sample

Figure 5.5: **Cross validation.** Four plots showing the in sample (a) and out-of-sample (b) predictive accuracy of Model 3 from Table 5.1, and in sample (c) and out-of-sample (b) predictive accuracy of Model 9 from Table 5.4. The solid lines show $x = y$, when the predicted and actual values would be identical. The dotted lines show what are analogous to 95% confidence intervals around this line, as the value of the RMSE is effectively one standard deviation from the line of best fit. Observations are plotted with some translucency to better show where large amounts of observations cluster.

2 and 9.[8] Plots (a) and (b) each contain 1,936 observations. This is half the number of of observations used in Model 2, as the data used are randomly split into two data sets, one to estimate the coefficients and the other to test the predictive accuracy of the model in out-of-sample data. Plots (c) and (d) contain 2,894 observations each, or half the number of observations used to create Model 9.

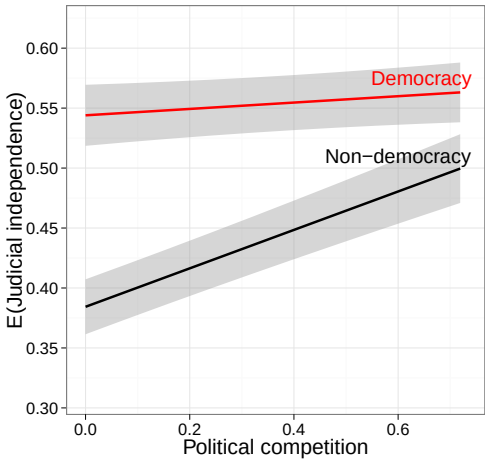
Figure 5.5 illustrates that both Models 2 and 9 demonstrate impressive out-of-sample fit, with only very small increases in the root mean square error (RMSE) as compared to the in-sample fit. Again, this is how well the estimates of each model fit data that were not used to generate them. Such out-of-sample predictive accuracy is one of the most generalizable and rigorous methods of assessing model fit, and much like the cross validation conducted in the previous chapter, this analysis suggests that the models presented in Tables 5.1, 5.3, and 5.4 are doing an impressive job modeling the underlying empirical phenomena.

5.5 Exploring substantive significance

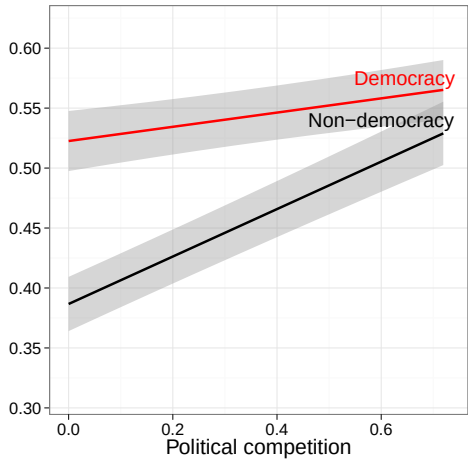
The previous two sections of the chapter provided persuasive evidence supporting the second key argument advanced in this dissertation: the effects of political competition on de facto judicial independence are conditional on regime type and much more salient in non-democracies. They furthermore offered a variety of information to show that the models presented fit the data exceptionally well and are robust to alternative specifications. In this section I will show the substantive implications of the model and illustrate just to what degree competition matters across regime type.

Figure 5.6 contains three plots, each showing the expected value of judicial independence in both democracies and non-democracies as political competition is allowed to vary from its observed minimum to its observed maximum value in each

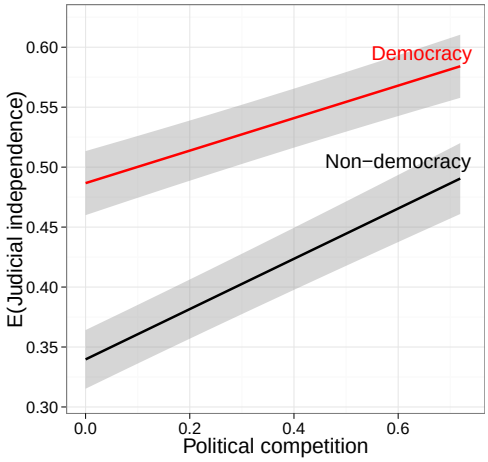
[8] As was discussed in the previous chapter, the only difference is the use of year fixed rather than year random effects, due to the predictive estimating procedures of the linear mixed effects commands found in the *R* packages `lme4` and `nlme`.



(a) Model 2



(b) Model 4



(c) Model 9

Figure 5.6: **Expected effects of political competition.** Three plots showing the expected value of judicial independence as political competition is allowed to vary, conditional on regime type. Each solid black line is the point estimate for judicial independence when political competition is at the corresponding value on the x-axis, and the gray shaded areas are 95% confidence intervals. Estimates and intervals are drawn from 10,000 simulations when all other covariates are held constant at their mean values.

of the enumerated models. The plots are created via the simulation procedure explained in the preceding chapters, using the fundamental uncertainty in the model rather than simply the uncertainty contained in the standard errors of each coefficient. Each plot further demonstrates that significant support exists for the hypothesis that competition should matter more in non-democracies.

Plots (a) and (b) in Figure 5.6 show the results when Models 2 and 4 are used. These are the best-fitting nested models of the full models (1 and 3) which use all covariates, using the Cheibub and Gandhi and Freedom House Political Rights classification schemes for democracy, respectively. Model 2 omits the covariates for openness to trade, interstate conflict, and internationalized internal conflict, while Model 4 omits openness to trade and all three conflict covariates. These show that the expected value of judicial independence in democracies only increases almost imperceptibly, whereas it rises sharply in non-democracies. Model 2 predicts that the point estimate (drawn from 10,000 simulations) of judicial independence in democracies when political competition is non-existent is 0.54, and when competition is at its maximum observed value of 0.72 this increases to only 0.56. In other words, competition has no effect in democracies. Model 4 predicts a slightly greater change in democracies, from 0.52 to 0.57. To contrast, in non-democracies, Model 2 predicts that as competition ranges from its minimum to maximum the expected value of judicial independence increases from 0.38 to 0.5; Model 4 predicts this change is slightly larger, from 0.39 to 0.53.

As discussed in the previous section, there is reason to suspect that the inferences in Models 1–8 (and Models 10–12) are biased due to the systematic exclusion of observations before 1980, which arises because of the inclusion of the nature of the executive covariate drawn from the Database of Political Institutions. As such, plot (c) in Figure 5.6 uses the estimates of Model 9, which omits the nature of the executive covariate and thus is fit to data with 66% more observations than Model 2. Results here are more consistent with the existing literature's consensus on the

importance of political competition in democracies: moving from the lowest to the highest level of competition in a democracy, the expected value of judicial independence increases from 0.49 to 0.58 (which, unlike the expected values from Model 2 and 4, is a statistically distinguishable difference). This is not to say that plot (c) fails to provide support for the argument that competition matters more in non-democracies. The expected value of judicial independence in non-democracies when competition is at its minimum is 0.34, which raises to 0.49 when competition is at its maximum observed value. Thus, the effect of competition in non-democracies is 64% larger than in democracies.

5.6 Conclusions

This chapter presents the first tests of the argument that political competition should be associated with higher levels of judicial independence not in democracies as is argued in the existing literature, but instead in non-democracies. Analyzing up to 162 countries over 44 years, I find that political competition is consistently associated with higher levels of judicial independence, and that this effect is substantially greater in non-democracies. This is true regardless of how models are specified, how a country is classified as a democracy or dictatorship, or what covariates are included. Model estimates are nearly identical regardless of model specification, and both hierarchical linear models with intercepts allowed to vary by country and year and ordinary least squares with country and year fixed effects produce similar results. As in the previous chapter, the substantive findings here remain the same regardless of which classification scheme is used for democracy, either the Cheibub and Gandhi measure or the measure derived from the Freedom House Political Rights score. Results also remain the same regardless of which covariates are included or excluded, or how these other covariates are measured.

The one exception to this is the manner in which the results change as a result of the inclusion or exclusion of the nature of the executive covariate. As noted, these

changes do not affect the test of my main argument: whether or not the covariate is included, competition is still predicted to have a greater effect on judicial independence in non-democracies. The implications for the existing literature and our understanding of how competition is associated with independence in democracies, however, is greatly affected by this modeling choice. Model 9 excludes the covariate and is thus able to be fit to 66% more observations than the models presented in Table 5.1. As a result of this, the estimated effect of competition on judicial independence is greater, and the effect of the interaction term, which attenuates the effects of competition in democracies, is lessened. This is demonstrated visually in Figure 5.6. In Models 1–8, the interaction term is strong enough to almost entirely attenuate the effects of competition in democracies. Model 9, however, suggests that this is not in fact the case, and that competition does affect independence in democracies, though it still does so to a significantly smaller degree than in non-democracies.

Given that the covariate capturing the nature of the executive has minimal substantive importance, and that excluding it allows for a large increase in the number of observations, I tentatively concluded earlier in the chapter that its exclusion is justified. This would suggest that the evidence presented in this chapter offers support to the overall consensus that competition matters in democracies. This conclusion is further bolstered by the multiple imputation results presented in the chapter appendix. These results show that after multiple imputation is conducted—and thus the full range of data is available with the nature of the executive covariate—the results of models including the nature of the executive covariate look similar to Model 9, rather than Models 1–8. In other words, the imputation results suggest that the listwise deletion of observations that is required by including the nature of the executive covariate introduces greater bias in model results than the omitted variable bias introduced as a consequence of excluding the covariate.

The overall results presented in this chapter are bolstered by the robustness checks and model fit procedures presented. Even when using the two alternative

measures of competition, the effects of competition on independence are still greater in non-democracies. Most importantly, the results of the cross validation procedures conducted provide strong support for the models and suggest that they are accurately capturing the underlying data-generating process, as they fit nearly as well on data not used in their generation. In other words, even when applying the models presented here to out-of-sample data, political competition still matters more in non-democratic contexts.

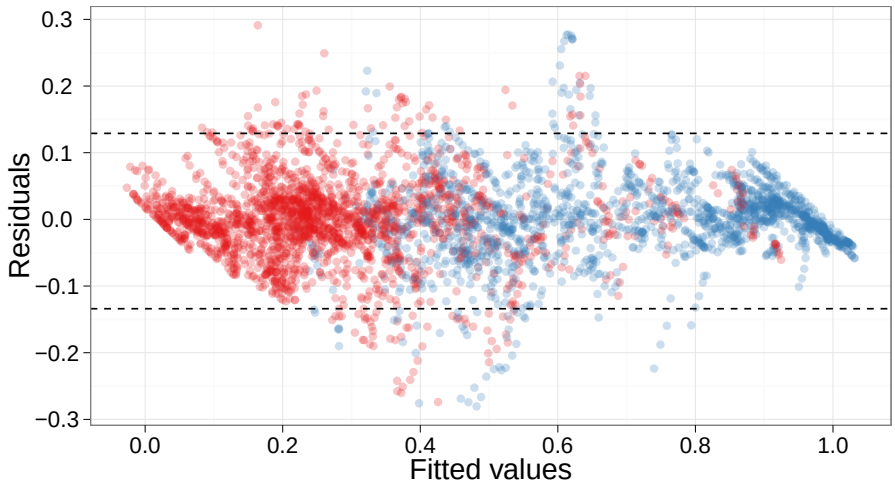
5.7 Appendix

This section presents even more information on model fit than provided in the corpus of the chapter. Residual plots of the main models are presented, as well as multiple imputation to demonstrate that the findings are not merely the result of systematically biased data. Finally, models using three and five-year averages of competition are presented to demonstrate that my findings are robust to even further alternative measures.

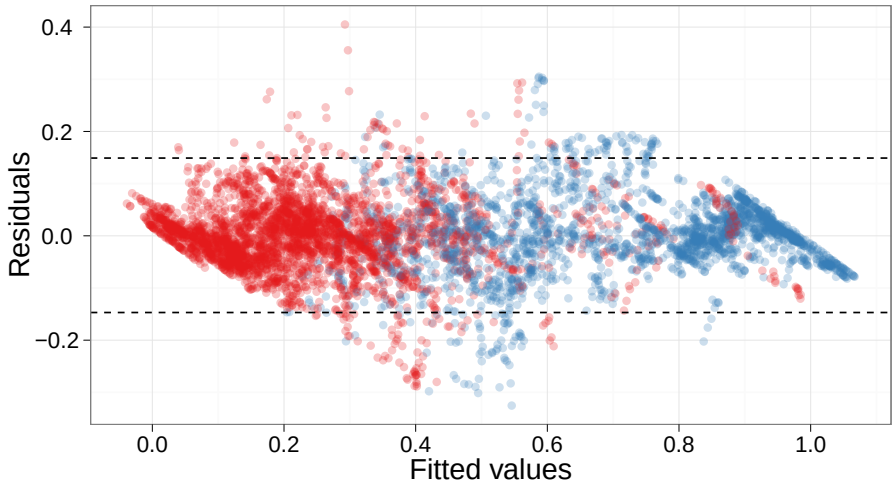
5.7.1 Residual plots

Figure 5.7 presents two scatterplots showing the residuals of Models 2 and 9 plotted against the fitted values of each model. To better improve interpretability, the points of the residuals are set with an alpha (transparency) parameter of 1/4, meaning that four overlapping observations produce one fully colored circle. In each scatterplot the horiztonal dashed lines demarcate the area along the y-axis where 95% of the residuals fall (i.e. two standard deviations from the mean). The scatterplots shows no obvious signs of hetereoskedasticity, as there appears to be significant dispersion across all fitted values, which is even more the case for those observations falling within two standard deviations of the distribution of the residuals.

Indeed, that the residuals appear to show diagonal skew at each end of the x-axis provides further evidence of model fit, not hetereoskedasticity; although the



(a) Model 2



(b) Model 9

Figure 5.7: **Residuals of Models 2 and 9.** Two plots showing the residuals of Models 2 and 9 from Tables 5.1 and 5.4 (y-axis) plotted against the fitted values of each model (x-axis). Red points show non-democratic observations, while blue points show democratic observations, and the black dashed lines show the 95% bounds of the distribution of the residuals (along the y-axis).

dependent variable is bounded by 0–1, the nature of the hierarchical linear models do not provide these bounds on the fitted values, and theoretically allow for the dependent variable to vary from negative to positive infinity. That there are no negative residuals when fitted values are 0 means that when the fitted values of the models are "missing" the actual values at the extreme lows, they are doing so by predicting higher fitted than actual values. The reverse is true when considering positive residuals when the fitted values are at 1: here, each model is fitting models slightly less than those actually observed. In fact, less than 2% of the fitted values of Model 2 are above 1 or below 0, and only 2.4% of the fitted values of Model 9 are beyond the 0–1 range.

5.7.2 Multiple imputation

The same procedures used to conduct multiple imputation in the preceding chapter are used here: the `Amelia` package in the statistical program *R* (Honaker, King and Blackwell 2011). Table 5.5 presents the results of one mixed effects and one fixed effect model, each averaged over 15 imputed data sets. Model 13 is a linear regression (ordinary least squares) model with country and year fixed effects, and Model 14 is a hierarchical linear model with intercepts allowed to vary by country and year.

As can be seen, the imputed data sets show very little differences when compared to Model 9 in Table 5.4. This provides significant support for the contention that Model 9 is the best-fitting model, and that the relationship between competition and judicial independence in democracies is positive and that the substantive effects of competition are statistically distinguishable from zero, while also providing evidence for the argument that competition matters more in non-democracies. The coefficient for competition in both Models 13 and 14 is 0.20, exceptionally close to the estimate in Model 9, which is 0.21. Similarly, the interaction term in Models 13 and 14 is estimated to be -0.06, also exceptionally close to the -0.07 estimate in Model 9.

Table 5.5: **Multiple imputation models.** Two models of de facto judicial independence. Each model is averaged over 15 multiply-imputed data sets. Model 13 is a linear model (ordinary least square) with fixed effects for country and year. Model 14 is a hierarchical linear model with intercepts allowed to vary by country and year (random effects not reported). Asterisks denote the standard 0.05, 0.01, and 0.001 levels of statistical significance.

	Model 13	Model 14
Intercept	-0.12^{***}	0.04
	(0.02)	(0.03)
Political competition	0.20^{***}	0.20^{***}
	(0.01)	(0.02)
Democracy	0.13^{***}	0.13^{***}
	(0.01)	(0.01)
Pol. comp. × Democracy	-0.06^{**}	-0.06^{***}
	(0.02)	(0.02)
log(GDP/capita)	0.03^{***}	0.03^{***}
	(0.00)	(0.00)
Common law heritage	0.25^{***}	0.18^{***}
	(0.02)	(0.04)
Years democratic	0.00	0.00
	(0.00)	(0.00)
Openness to trade	-0.00	-0.00^{***}
	(0.00)	(0.00)
Bureaucratic quality	0.05^{***}	0.05^{***}
	(0.01)	(0.01)
Assembly-elected president	0.00	0.00
	(0.00)	(0.00)
Parliamentary system	0.03^{**}	0.03^{***}
	(0.00)	(0.00)
Interstate conflict	0.01	0.01
	(0.01)	(0.01)
Internal conflict	-0.01^{**}	-0.01^{***}
	(0.00)	(0.00)
Internationalized internal conflict	0.00	0.00
	(0.00)	(0.01)
BIC	$-16,086.28$	$-16,817.28$
N	8,024	8,024

5.7.3 Competition averaged over multiple years

It is possible that the effects of competition are not the result of competition in just the previous year, as is modeled in Models 1–14. Rather, it might be the case that the

overall competitiveness of a system in the previous few years is important. The idea here is that leaders might implicitly "average" competition, disregarding potential outliers, be they positive (an overwhelming victory) or negative (a resounding defeat) for those in power. In this sense, leaders are performing a form of Bayesian updating, not looking just at the results in the previous year, but at those results in the context of their priors.

To capture this potential alternative conception of how competition might affect the calculus of those in power with regards to judicial independence, I use three and five year averages of political competition. Table 5.6 reports these results. Models 15 and 16 use the same covariate structure as Model 2 in Table 5.1, while Models 17 and 18 use the same covariates as Model 9 in Table 5.4. Models 15 and 17 use the average of political competition in the previous three years, while Models 16 and 18 use a five-year average. The results of Table 5.6 provide further support for the idea that my models capture the underlying data-generating process, and that the arguments forwarded in Chapter 1 have robust empirical support.

Table 5.6: **Competition averaged over previous years.** Four models of de facto judicial independence. Models 15 and 16 use the same covariates as Model 2, and Models 17 and 18 use the same covariate structure as Model 9. Models 15 and 17 average the level of political competition in the three previous years, while Models 16 and 18 use a five-year average. Asterisks denote the standard 0.05, 0.01, and 0.001 levels of statistical significance.

	Model 15	Model 16	Model 17	Model 18
Intercept	-0.17^{***}	-0.18^{***}	-0.12^{***}	-0.13^{***}
	(0.04)	(0.04)	(0.02)	(0.02)
Political competition	0.20^{***}	0.21^{***}	0.25^{***}	0.25^{***}
	(0.02)	(0.02)	(0.01)	(0.02)
Democracy	0.16^{***}	0.16^{***}	0.15^{***}	0.15^{***}
	(0.01)	(0.01)	(0.01)	(0.01)
Pol. comp. × Democracy	-0.14^{***}	-0.14^{***}	-0.06^{**}	-0.05^{**}
	(0.02)	(0.02)	(0.02)	(0.02)
log(GDP/capita)	0.05^{***}	0.06^{***}	0.05^{***}	0.05^{***}
	(0.00)	(0.00)	(0.00)	(0.00)
Common law heritage	0.10^{***}	0.10^{***}	0.13^{***}	0.13^{***}
	(0.02)	(0.02)	(0.03)	(0.03)
Years democratic	0.00^{***}	0.00^{***}	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Bureaucratic quality	0.07^{***}	0.07^{***}	0.10^{***}	0.10^{***}
	(0.02)	(0.02)	(0.01)	(0.01)
Assembly-elected president	−0.00	0.00		
	(0.01)	(0.01)		
Parliamentary system	0.03^{***}	0.03^{***}		
	(0.01)	(0.01)		
Internal conflict	-0.02^{***}	-0.02^{***}	-0.02^{***}	-0.02^{***}
	(0.00)	(0.00)	(0.00)	(0.00)
Random effect: country	0.02	0.02	0.02	0.02
	(0.14)	(0.14)	(0.15)	(0.15)
Random effect: year	0.00	0.00	0.00	0.00
	(0.01)	(0.01)	(0.01)	(0.02)
Random effect: residual	0.00	0.00	0.01	0.01
	(0.06)	(0.06)	(0.07)	(0.07)
BIC	−9466.26	−9469.68	−13036.16	−13028.83
N	3875	3875	5794	5794

Chapter 6

JUDICIAL INDEPENDENCE IN THE POSTCOMMUNIST WORLD

The previous chapter examined the relationship between political competition and de facto judicial independence in a global context over forty years, finding that the salience of competition is conditional on regime type: the effects of competition are greater in non-democracies than in democracies, as is hypothesized in Chapter 1. This chapter addresses the relationship with much more focused data, both temporally and cross-sectionally, examining how competition among political parties relates to de facto levels of judicial independence in Eastern Europe and the former Soviet Union. Whereas the goal of the previous chapter is to determine whether the theorized conditional effects of regime type hold globally, the goal of this chapter is to narrow the focus to a specific group of countries that share significant political, economic, and social legacies (Ekiert and Hanson 2003).

The aim is twofold: first, to ascertain whether the patterns found globally hold up in a specific time and place. The second aim is to marshal area knowledge to collect and analyze potential confounding variables that scholars of the region would think might play an important role, an insurmountable challenge at the global level. This chapter furthermore presents an example of testing the broader arguments in a region possibly least amenable to the argument: to date, the only analysis of competition in non-democracies focuses on a paired comparison from the postcommunist world, and it argues that competition inhibits independence (Popova 2010). Though an analysis of 15 years of data drawn from 27 postcommunist countries, I show that competition is associated with an increased likelihood of de facto judicial independence, and that this

relationship is conditional on regime type: as theorized, competition is more salient in non-democracies. The level of economic development as well as the time since a state began its transition to democracy are also consistently associated with judicial independence, with economic development demonstrating a strong positive relationship, and the time since transition a strong negative relationship. A striking finding is that neither bilateral democracy and governance assistance nor the influence of EU candidacy show any relationship with judicial independence. The results of this chapter have important implications not only for the popular "insurance" model of judicial independence, but also for the literatures on the rule of law and economic growth, transitions from communism, European integration, and democracy promotion.

This chapter proceeds as follows. First, I discuss the temporal scope of the chapter, highlighting work on courts in the postcommunist region and arguing that the region is an ideal venue for the study of political competition and judicial independence. I then present data on the levels of competition and independence in Eastern Europe and the former Soviet Union (FSU), and in doing so address questions of measurement. After this I discuss other covariates of interest that should be relevant to an examination of de facto independence in the specific context of postcommunism, and present ordered probit regression models employing a trichotomous measure of judicial independence over the first decade and a half of the postcommunist era. I then discuss results and provide visualizations of model predictions across various quantities of interest, and conclude by discussing the broader implications of the findings.

6.1 The postcommunist context

In the two decades since the collapse of communism, much attention has been paid to the development of judiciaries in the region, resulting in rich descriptive studies of both constitutional and lower courts in individual countries and, occasionally, comparatively (Holmes 1999; Scheppele 1999; Hendley, Murrell and Ryterman 2000;

Solomon and Foglesong 2000; Trochev 2008). Far less attention has been paid to the determinants of variation in the independence of these courts: there is almost no reference to explanations found in the law and courts literature, and no work looks at the effects of competition comparatively across the region.

In an excellent study of seven postcommunist constitutional courts, Herron and Randazzo (2003) show that constitutional guarantees of independence (*de jure* judicial independence) fail to predict whether a constitutional court rules against the state. Instead, economic growth, presidential power, and issue characteristics better predict judicial review. Unfortunately, their measure of judicial review, cases in which the constitutional court "overruled, invalidated, or declared unconstitutional a statute or governmental action," fails to address the strategic nature of judicial decision–making, which Helmke (2005) has shown to be of critical importance (Herron and Randazzo 2003, 428–9). More importantly, weighing the strength of courts by the number of times they overturn statutes or actions fails to take into account the fact that the courts are agents of government principals that are often divided, either by coalitional politics or executive–legislative competition. The often-delayed nature of judicial decisions coupled with high turnover in office and electoral volatility creates a further problem in inferring just how much de facto independence is demonstrated by overturning statutes (Birch 2003; Epperly 2011). In their 2002 study, Smithey and Ishiyama also use rulings against policies to capture de facto independence, and face the same problems. They find broadly comparable results, with political and economic factors best explaining the outcome. Unsurprising, as Herron and Randazzo (2003) use their measure of de jure independence, Smithey and Ishiyama (2002) find little evidence to suggest that constitutional protections have any relationship with judicial activism.

While these studies include measures of the effective number of parties, they fail to account for political competition, thought to be a key determinant of judicial independence Helmke and Rosenbluth (2009). The large legislative fractionalization

accompanying high numbers of parties can signal either significant or weak political competition, as a legislature with many small and divided opposition parties offers little credible threat of replacement to those in power. Surprisingly, given the focus on competition in securing judicial independence in the law and politics literature, almost no attention has been paid to competition by those studying the postcommunist region, regime type aside. The lone example is Popova; Popova's (2010; 2012) work, discussed in more detail in Chapter 1. Examining court rulings and media freedom disputes, she finds that Ukrainian courts are more likely to rule for the government, attributing this to more competitive Ukrainian politics.[1] If her "strategic pressure" explanation of judicial independence in non-democracies with electoral competition is correct, that more competition means less independence, the results presented in the previous three chapters and later in this chapter should be the exact opposite as they are found.

The lack of attention paid to the study of judicial independence in the postcommunist region as compared to East Asia or Latin America is unfortunate, as a number of features of the postcommunist transition make it an ideal context in which to study the question of the development of judicial independence. As noted previously, in the half-decade after the collapse of state socialism, every country save for two adopted new constitutions, the majority of which explicitly addressed judicial independence in multiple ways.[2] The contemporaneous nature of the transitions away from communism solves a number of problems of comparability that scholars have expressed with regards to judicial independence.

First, some have noted that the international environment and the "normative value" of certain types of institutional arrangements possess different salience at different times (Finnemore and Sikkink 2005). With specific reference to judicial inde-

[1]For a critique of the limits of this analysis, see the discussion Chapter 1.

[2]These two are Hungary and Latvia. The former retained a thoroughly-amended version of its communist-era constitution, and the latter reinstated the bulk of its 1922 inter-war constitution.

pendence, this can be most clearly seen in the experience of Western Europe and the post-war expansion of independent courts of the variety forwarded by Hans Kelsen, which had little purchase when first proposed in the inter-war years (Hirschl 2004). In a more contemporary context, Helmke and Rosenbluth (2009) note that the international community has in recent years advocated and pushed for the establishment of strong courts in emerging democracies, while Tate and Keith (2007) demonstrate that the average level of judicial independence has fluctuated widely over the past three decades.[3] While the general and diffused influence of the international environment (as opposed to specific influences such as trade, multilateral pressure, etc.) is a contested idea, for the purposes of this study it can be bracketed aside as a result of the postcommunist transition occurring contemporaneously in over two dozen countries. One specific aspect of the international environment that is of extreme relevance for the study of postcommunism and thus must be addressed, however, is the expansion of the European Union and its resulting accession requirements, which scholars suggest strongly affects democratization in candidate countries (Dimitrova and Pridham 2004). Whether it has strongly affected judicial independence, however, is a question this literature has left unanswered; the results of this chapter strongly suggest that there is no discernible EU influence on this important aspect of governance.

This chapter examines the time period following the adoption of new constitutions in 27 postcommunist states to 2005.[4] Beginning with the adoption of new constitutions is appropriate for two key reasons. The first of these is that each of these constitutions to varying degrees laid out the extensiveness (or lack thereof) of de jure judicial independence in its respective polity. Prior to the adoption of these constitutions, each state was operating in a fluid institutional environment. While

[3]A key reason why year random effects and/or year trends were used in the models presented throughout previous chapters.

[4]Or, in the case of Latvia, the reinstatement of the 1922 constitution. With regards to Hungary, the time period chosen is after the constitutional amendment dealing with the issue of judicial independence.

many communist-era constitutions were still technically valid, the degree to which they were in force on the ground was in question, and at times an issue of serious contestation, as the Russian constitutional crisis of 1993 aptly illustrates (Ellison 2006). In some cases, emergency laws or interim constitutions were put into place even before the actual collapse of communism, but even in these cases, the resulting judicial structures were neither new nor possessing any legitimacy vis-à-vis the public or other political actors (Pabriks and Purs 2001; Smith 2001). The second reason for this decision is that not only did the new constitutional orders determine the new judicial institutions, they also determined the new rules under which political competition would occur. As political competition is a key variable of interest in the literatures on both postcommunism and judicial independence, allowing at the very least for the formal rules to be set allows for a better comparison across time over the period in question. In addition, during the constitutionalization phase the nature of political competition was different; it was often disparate factions coalescing around mutual opposition to communism rather than political parties that were the relevant political actors.

6.2 Concepts and measurement

There are a number of covariates that might be associated with judicial independence but for which measures possessing extensive temporal coverage do not exist, and were as a result not included in models presented in previous chapters. The more restricted time frame analyzed in this chapter, however, allows for their inclusion, and in this section I discuss why they are included and how they are measured. In addition, I present measures of both competition and judicial independence with significant validity, but with either limited temporal or cross-sectional availability.

6.2.1 De facto judicial independence

This chapter employs the trichotomous measure of de facto judicial independence developed by Tate and Keith (2007) .To craft their measure, Tate and Keith explicitly follow the general scheme employed by Howard and Carey (2004), creating a trichotomous measure of judicial independence based on the State Department's *Country Reports on Human Rights Practices*. The difference between the two measures has to do with how each set of authors operationalizes independence, primarily with regards to rights protections; one of Howard and Carey's components specifically includes whether or not the state "afford[s] basic criminal due process to criminal defendants," which Tate and Keith note is problematic, as it conflates both judicial independence and fair and the impartial treatment of citizens before the judiciary as well as elements of de jure and de facto independence.[5] Instead, they follow the language of the State Department reports, noting when outside influence or corruption affect the judiciary, irrespective of impartiality or rights provisions. The scorings for the trichotomous measure presented in Table 6.1 are drawn directly from Tate and Keith (2007), and illustrate how each country-year is scored.[6]

For the purposes of examining judicial independence in the postcommunist world, the measure created by Tate and Keith is the most appropriate of the existing cross-national measures of de facto judicial independence, in both terms of construct validity and the validity of measurement itself. With regards to construct validity, the measure directly captures whether the judicial system is unduly influenced by other state or non-state actors, and if this is the case whether such influence is pervasive or occasional in nature. Though as a three-category ordinal measure it lacks fine granularity, this is true of all direct measures of judicial independence, and more

[5]Comparing the two measures, Ríos-Figueroa and Staton (2009) find a Kendall's *tau-b* coefficient of 0.72.

[6]For examples of the wordings of the State Department reports and their codings, see the appendix of Tate and Keith (2007).

Table 6.1: **Tate and Keith Measure Criteria.** The scoring criteria for the trichotomous measure of de facto judicial independence presented in Tate and Keith (2007, 26).

Low independence	The judiciary is reported as not being independent in practice; is reported to have significant or high levels of executive influence or interference; or is reported to high levels of corruption.
Moderate independence	The judiciary is reported to be somewhat independent in practice with reports of (some) pressure from the executive "at times" or with occasional reports of corruption.
High independence	The judiciary is reported as "generally independent" or is independent in practice with no mention of corruption or outside influence.

importantly this can also be seen as a strength rather than a weakness; complex scales can create gulfs between concept and measure, and also result in codings that lack face validity. Given the limited temporal and cross-sectional focus of this chapter, the measure is the best choice.[7]

The measure possesses three characteristics that bolster its measurement validity when compared to many alternative measures discussed in Chapter 2. First, because it is constructed from a template and uses easily-accessible public documents, it is eminently replicable. Second, its temporal coverage (1981–2000) is more than adequate for assessments of judicial independence in the postcommunist context[8]; I extended the coverage of the measure for the postcommunist countries, coding the first half of the 2000s after establishing a baseline by recoding the 1990s data to ensure validity with Tate and Keith's coding. Third, the measure shows no miss-

[7]Note, however, that in the chapter appendix I present models using the Linzer and Staton (2011) measure used in previous chapters.

[8]The Howard and Carey measure, by comparison, only covers the 1992-1999 period. A concern for longer time-series applications raised by Tate and Keith (2009) is the potential lack of comprehensiveness of the State Department reports as one goes back into the 1980s. Obviously, this is not a concern here.

Table 6.2: **De facto independence.** The distribution of levels of de facto judicial independence across the postcommunist world. The universe of cases is all postcommunist states after post-transition constitutionalization of *de jure* judicial independence. Percentages may not total 100 due to rounding.

	Low independence	Moderate independence	High independence	*N*
Total	179 (50%)	79 (22%)	99 (28%)	357
1994	9 (38%)	7 (29%)	8 (33%)	24
2005	17 (63%)	5 (19%)	5 (19%)	27

ingness (which as discussed produces serious concerns with regards to bias) for any postcommunist country-years, unlike many other measures.

Table 6.2 contains descriptive statistics illustrating the distribution of such scores across the postcommunist world. Looking at the total for the 357 post-constitutionalization country-years (up to 2005), a full half have judiciaries with low independence, with the remaining half almost split between moderately and highly independent judiciaries; while half the observations may fall into the same category, large degree of variation is still observed. Also noted is the breakdown of scores for two years: 1994 and 2005.[9] The possibility of a general temporal dimension (rather than case-specific temporal trends) is suggested by these descriptive statistics, as the percentage of states with low independence jumped over the decade in question, while both those with moderate and high independence show a sharp drop. Modeling the correlates of judicial independence after communism should therefore take this temporal feature of the data into account, which will be further discussed later.

[9]The number of countries in each analysis is not a function of missing data, but rather the conceptual relevance of post-constitutionalization discussed above. In 1994, Armenia, Bosnia, Georgia, and Ukraine had not yet adopted new constitutions.

6.2.2 Political competition

As one of the key goals of an analysis with more narrow scope was to test the arguments forwarded in the dissertation with more fine-grained data, I created a measure of political competition for the postcommunist observations analyzed in this chapter. The concept of political competition forwarded in this dissertation focuses on the distribution of power between the executive and opposition parties in the legislature, and the measure used in this chapter is the share of seats controlled by the largest opposition party (how this differs from the opposition seat share measure used in previous chapters will be addressed). The executive is considered the president in a presidential system, and the prime minister in parliamentary systems; opposition parties are those parties not members of the ruling coalition (or not the ruling party in instances of single-party government), unless they are a puppet party supporting the government in an autocratic state. In the case of presidential systems, those presidents who were prior to election members of a political party are considered to remain affiliated with said party, and in cases where the president is successful in creating a "party of power" (such as Putin and United Russia), the party of power is not considered an opposition party. The strength of this competition is measured as the share of seats in the legislature.

The coding of shares of seats and the relationship of parties to the executive was conducted with reference to three secondary sources, and with reference to the electoral commissions of each country, as available.[10] Determining the results of elections and the distribution of seats in legislatures, I primarily relied on the Comparative Political Data Set II, which focuses on a variety of institutional and electoral factors in 28 postcommunist countries, and the the European Election Database, which has a wealth of information on elections in EU member states (Armingeon

[10]Seats rather than the percentage of votes were used because the former is not as widely available across postcommunist elections. Where it does exist, however, the two values are correlated at over 0.95 (Frye 2010).

et al. 2008; Norwegian Social Science Data Services 2010). Freedom House's yearly *Nations in Transit* reports, which document the political and economic developments in postcommunist states, were used to determine the partisan makeup of governments and the opposition, including changes in coalitions between elections (Freedom House 2010).

Conceptualizing the level of political competition as a function of the share of legislative seats held by the largest opposition party makes sense: in those instances when the opposition controls a large percentage of the legislature it can be seen as presenting a credible electoral threat to the party in power, and thus induce more judicial independence (or perhaps more accurately better restrain infringements on such independence). The opposition is the largest party that is not part of the government. In cases of presidential systems such as Russia, this is the largest party in the legislature that is not the party of the president in office. [11] As an example, during the Yeltsin years, the opposition is coded as simply the party with the most seats in Duma (typically the Communist Party of the Russian Federation). During the Putin years, however, the electoral dominance of United Russia means that the opposition is coded as the largest party in the Duma, excepting the case of United Russia.[12] An implication of this is that there are situations where the legislature is dominated by a party that the president is not a part of, and as a result the measure for political competition results in a high score. This is appropriate, as such situations demonstrate the clear potential of the opposition taking control after the following election, as well as demonstrate a situation in which there should be a check on the power of the executive to infringe on the rights of the courts.

In cases of parliamentary systems, the opposition is coded as being the largest party not part of government. The resulting score can take a wide variety of values,

[11] A system with an elected but purely ceremonial presidency, such as Estonia, is treated as a parliamentary system as is common in the literature.

[12] Had earlier attempts by Yeltsin to create a viable "party of power" been successful, such a party would also have not been counted as the opposition.

dependent on the partisan structure of the legislature. In cases where two roughly equal factions dominate, political competition is high. Similarly, in cases of minority governments, it is also very likely that political competition scores will (appropriately) be high. On the other hand, in cases where the opposition is fractured and divided into a number of small parties, the score will be low. The measure in this case also has construct validity; when no single opposition party is able to capture a decent share of the legislature, it is unlikely to create any concern on those in power that they may soon find themselves in the opposition. The same logic holds for instances of encompassing coalitions; when the largest parties in the political system share power (though this is a rare occurrence in the postcommunist context, it is not unheard of), political competition is low.

The coding scheme I employ in this chapter is, predictably given its specificity, more accurate than using existing global data on the distribution of power within legislatures, the most common of which is the Database of Political Institutions (Beck et al. 2001). These types of data present two two problems to those studying postcommunism. First, they routinely fail to account for changes in the legislature between elections, coding years between elections as having the same make-up. While this is not a serious problem for first-past-the-post countries such as the US or the UK, or established proportional systems such as Germany. It is a problem in the parliamentary systems of Eastern Europe and the former Soviet Union, as significant changes take place in the make-up of coalitions between elections, with parties joining and leaving governing coalitions. Second, these data universally code any party not in government as an opposition party. While this may not be an issue in consolidated democracies, it is critical in less-democratic contexts, where often opposition parties exist merely to provide a facade of multipartism. Often, such parties are led by by key advisors to executives, and supported both implicitly and explicitly by the executive (Levitsky and Way 2010). Table 6.3 illustrates just such problems: the "opposition" parties coded as having 1/5 of the legislatures of both Tajikistan and Uzbekistan,

Table 6.3: **Measuring competition.** A table showing five examples of differences between the share of seats measure of political competition when using the data coding employed here and that of the Database of Political Institutions, which does not consider between-election changes in governing coalitions of the facade of multipartism common in non-democracies.

Country	Year	My estimate	DPI
Latvia	1994	15	60
Lithuania	2000	51.1	8.5
Moldova	2000	39.6	25.7
Tajikistan	2004	3.3	20.6
Uzbekistan	1996	0	18.8
Correlation = 0.44			

for example, were parties supportive of and supported by the autocrats Emomalii Rahmon and Islam Karimov.[13]

Following the above discussion, the first measure I employ is continuous, and is merely the percentage of seats controlled by the opposition. Summary statistics of these scores are provided in the Table 6.4. Similar to the summary statistics for de facto judicial independence presented above, these show a (slighter) temporal dimension, with the median and mean levels of political competition higher in 2005 than in 1994 or in the full set of country-years.

It is also possible, however, that the relationship between political competition and judicial independence may more resemble a threshold effect; when the opposition presents itself as a credible alternative and garners enough popular support to win a decent share of the legislature, the restraining effects of competition on judicial independence obtain. This coding scheme challenges the idea that there is a direct linear

[13]Though the Henisz (2000, 2002) measure generally outperforms the DPI measure in these regards, it is not immune from these problems. Such problems are a worry of global studies, and one of the key reasons this chapter is of vital importance.

Table 6.4: **Political Competition.** Median and mean values and the standard deviation of a continuous measure of political competition, determined by the percentage of legislative seats controlled by the largest opposition party.

	Median	Mean	Std. Dev.	*N*
Total	17.65	19.2	14.4	307
1994	17.2	17.5	11.4	22
2005	24.0	21.4	14.7	23

relationship between the percent of seats and the salience of political competition, and instead suggests that the relationship is dichotomous, with the implication that it is the presence of plausible competition that is critical, rather than the exact degree of such competition. The exact choice of a threshold cannot be explicitly guided by theory, but certain levels suggest themselves as plausible in the light of postcommunist electoral history. Scholars looking at party polarization have employed both 15% and 20% of seats as signaling credible political competition (Haggard and Kaufman 1995; Frye 2010). The logic behind this choice is that, given the aforementioned high levels of volatility and seats controlled by independents, such percentages represent credible threats to the party in power. This is eminently plausible, given that the average electoral volatility on the Pedersen index for the postcommunist states in the decade and a half after the transition was over 27 (Epperly 2011).[14] To capture this potential alternative relationship between the presence of political competition rather than the degree, I also operationalize competition in this dichotomous sense to see if results vary across these specifications. Table 6.5 contains descriptive statistics for dichotomous measures of political competition, employing both 15% and 20% as cut-points in coding.

[14]To compare, Western Europe in the inter-war era—a time period widely considered to have witnessed high levels of electoral volatility—only averaged a score of 10 (Bartolini and Mair 1990).

Table 6.5: **Dichotomous competition.** Values for dichotomous measures of political competition, using 15% and 20% as cut-points for the determination of a given country-year as competitive or non-competitive.

Threshold		Competition	No Competition	*N*
15%	Total	176	131	307
	1994	13	9	22
	2005	14	9	23
20%	Total	142	165	307
	1994	9	13	22
	2005	12	11	23

While this is in no way a radical conceptualization of political competition, the contest for elected office does not naturally suggest a single method of measurement. Referencing back to the prior discussion of why competition is important for judicial independence, however, a clear goal of how to measure competition is foregrounded. The potential electoral threat posed by the opposition, as it is the likelihood of removal from office that has been theorized to influence political actors' preferences for judicial independence. One potential alternative is to use the alternation in power by competing groups as a proxy for the overall competition in a system. The logic here is that when the threat of losing office is a regular concern because it is a regular reality, those in power would be more likely to empower courts to protect them during their term in the minority; this argument has been persuasively employed to explain the relatively lack of judicial independence in Japan, most notably in a seminal article by Ramseyer (1994). As Frye (2010) notes, however, such an approach is problematic, because it fails to account for those situations that can be described as "near-misses," when parties in power are able to retain power.[15] Frye illustrates this idea with a

[15]Such an approach is also problematic when assessing the early years of postcommunism, when

reference to the 1996 Russian presidential elections. While Boris Yeltsin was able to secure reelection, this outcome was not only in question, but was in fact unexpected by many observers, including investors, who (fearing a communist victory) pulled out of Russia (Kiewiet and Myagkov 1998; Frye 2000).

As a result, such an approach would almost by definition result in an underestimation of the level of political competition when applied to shorter time slices such as those analyzed in this chapter, as well as in majoritarian systems where losing parties can still provide serious political competition even when losing successive elections. The United States from 2000–2008 is an apt example: George W. Bush won both the 2000 and 2004 elections, but in terms of the popular vote lost the first and won the second by only 1.5%, and in terms of the share of electoral college, only prevailed in the first with 50.047% and the second with 53%. Regardless of how political commentators or actors may have treated each election after the fact, it cannot be said that in either case the Democratic party failed to offer serious political competition.

6.2.3 Democracy

Narrowing the focus from a global analysis to an analysis of a particular region provides a number of benefits, and these benefits are exponentially greater if one is familiar with the particular region. Above, it allowed me to create an exceptionally accurate measure for political competition. Here, it allows me to use the best measure of democracy available, based upon the degree to which it captures what I know about the region, and what the consensus is with regards to the democratic nature of specific countries at specific times.

For this reason, in this chapter I exclusively employ the measure of democracy derived from the Freedom House Political Rights score. It has the previously discussed benefit (or, more accurately, requirement) of being one of the few measures of

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such a history of alternating parties in power was non-existent.

democracy that does not implicitly or explicitly include judicial independence in its conceptualization of democracy. It is not the only one, however, as my widespread use of the Cheibub and Gandhi (2010) measure makes clear. I exclusively use the Freedom House measure, however, because in the postcommunist context the Cheibub and Gandhi measure often lacks face validity, due to how it back-codes years preceding a successful or failed transfer of power. While this is not more than a minor nuisance in an analysis with 1,500, 3,000, or 6,000 observations, in an analysis of 308 observations it can have serious effects on any analysis conducted.

The two most obvious examples of how this affects outcomes are its coding results for Russia and Ukraine. It codes Russia as a dictatorship in every year after the collapse of communism, due to the lack of a transfer of power to the opposition. This lacks face validity according to most conceptions of democracy, which is underscored by the fact that scholars and policymakers have bemoaned the return to authoritarianism under the first presidency of Vladimir Putin. While it is certainly the case that the administration of Boris Yeltin was not a fully-consolidated, liberal democracy, the consensus among scholars is that Russia was a democracy during the 1990s (Ellison 2006; Colton 2008). Ukraine provides the opposite example: here, every year after the collapse of communism is coded as a democracy by Cheibub and Gandhi, presumably due to the success of the Orange Revolution and the election of Victor Yushchenko. Coding the preceding years under Leonid Kuchma as a democracy is highly problematic, however, which is most clearly articulated by the fact that it was only due to a long-standing popular uprising of the Orange Revolution that the election "won" by Victor Yanukovych was nullified and a new election was held (Way 2008).

6.2.4 Other covariates

Focusing on a narrower time span and cross-section of countries in this chapter allows me to offer a more nuanced series of models, as there exist covariates potentially important in the postcommunist context that with less or no relevance in a global

analysis. As such, in this section I present a variety of possibly-confounding relationships unique to the postcommunist region and the data used to capture them.

Economic development

In Chapter 4 I discussed how the direct causal relationship between economic development and judicial independence is unclear: scholars have asserted the key role playing by judicial independence in the protection of property rights and thus economic growth, but have done so on simple cross-sections of data, never considering the potential for reverse causality (Feld and Voigt 2003; La Porta et al. 2004)

The direction of the causal relationship in the context of postcommunism, however, should be clearer. Existing studies and the proposed causal mechanisms all focus on the determinants of long-run growth, and the short time span of the postcommunist experience to date makes inferring relevance problematic: independent courts did not have the requisite decades to protect private property rights. Similarly, there is little reason to expect that the variation in levels of judicial independence and its proposed link to secure property rights should be salient given the monumental undertaking of economic reform in the countries at hand. The (small) differences in long-run growth rates due to judicial independence are hypothesized under relatively stable economic conditions; the collapse of the planned economy, followed by large and small-scale privatization programs, price and currency liberalization, banking sector reforms, and a host of other economic transformations are so huge that it seems safe to assume these overwhelm the effects of judicial independence on economic outcomes in the first decade and a half of postcommunism (Berg et al. 1999; Heybey and Murrell 1999).

As such, the level of judicial independence should not be affecting short-term economic growth in the postcommunist countries of Eastern Europe and Eurasia. The level of economic development, however, may affect the degree of judicial independence in the region. Countries with more dynamic and advanced economies

require more effective institutions of regulation, and as economic resources increase, the resources able to be devoted to judicial systems increase as well (Herron and Randazzo 2003; Helmke and Rosenbluth 2009). Unfortunately, poor countries often lack the human and other capital resources to provide for effective judicial institutions (Buscaglia 1998). These historical realities should allow for us to assess the effects of growth on independence, rather than the reverse, with some degree of certainty that the relationship is not being heavily influenced by reverse causality.

Rule of law aid

In a series of articles, Carothers (1998, 2010) writes forcefully about the "revival" of the rule of law and the "temptations" on the part of policymakers and scholars, many who treat it as a panacea to the ills associated with transition, democratization, and development. Large amounts of time and money are spent on projects to strengthen the police, courts, and a host of other governmental agencies in the postcommunist states, and similar efforts are put into building civil society in the region, as well (USAID 2009). There is, however, little to no work examining systematically whether such efforts are associated with positive outcomes; as Carothers notes, instead of a consensus as to what works that is well-grounded in critical examination, there are assumptions that these efforts must have positive effects, and that we know more than we do about how to achieve results. In a similar vein, Holmes (1999) offers a critical assessment of Western attempts at supporting and inducing legal reform in Russia, suggesting that the disjointed manner in which Western attempts are made, combined with a failure to integrate reforms with key domestic actors, leads predictably to failure. On the other hand, a group of scholars analyzing whether USAID democracy and governance assistance is effective at strengthening democracy worldwide found that it has a consistent and robust relationship with democracy, although the temporal span of the analysis was severely limited (Finkel et al. 2006). While we cannot assume for the above reasons that these results when measuring democracy obtain when

looking at judicial independence, such findings suggest at the minimum that targeted assistance might produce desirable outcomes.

If rule of law, democracy, and governance aid is successful in achieving its objectives, we should expect to see improvement in de facto judicial independence, as judicial independence is one of the fundamental building blocks of the rule of law (Ríos-Figueroa and Staton 2009). Measuring total democracy and governance assistance is difficult, because, as Holmes notes, a multitude of state and non-state actors are engaged in the process in an uncoordinated and redundant fashion. Bilateral state aid, however, is easily measurable, as the number of actors is by definition constrained, and additionally because such funds are budgeted in dispersed in a more open and transparent fashion. To test and control for the potential relationship between democracy and governance assistance and judicial independence, the total bilateral state aid targeted for relevant projects is included.

European Union influence

A cottage industry has sprung up around questions of "Europeanization," examining the effects of EU influence on democratic and economic reform in Eastern Europe.[16] The effect of the EU can either be direct or indirect. The former involves aspects of conditionality and the fulfillment of specific requirements contained in the *acquis communautaire*, while the latter involves the more general (typically theorized as moderating) influence that the potential for membership contains. Vachudova (2005), for example, considers how these active and passive forms of leverage affected candidate states in a variety of ways, including the fostering of political competition. One broad consensus is that the political conditionality required as part of the accession process effectively aided democratization by strengthening pro-democracy domestic elites and

[16]For a review of this literature, see the recent special issue of *Europe-Asia Studies*, examining "The European Union and Post-Communist Eastern Europe: Conditionality, Legacies and Europeanisation." 62(3), 2010.

making democratic reform—necessary to acquire EU membership—the only game in town (Kubicek 2003; Pridham 2005; Schimmelfennig, Engert and Knobel 2006). In a pair of recent articles, Schimmelfennig and Scholtz (2008, 2010) scrutinized this relationship between Europeanization and democracy, and find that controlling for a number of other factors, EU influence is still an important factor (though not more important than domestic political factors) in accounting for variation in the European neighborhood.

A serious problem with this literature is the question of endogeneity that too often goes unexamined. Put simply, it is difficult to disentangle the direction of causality for the argument. Did the European Union effect positive change in its Eastern neighbors, or did the EU integrate these states because they were already making significant strides in the transition process? Some have argued that the relevance of the EU during the early and mid-1990s—a critical time period when it comes to divergence between differing postcommunist states—was negligible. This was a function of internal disagreement among EU members as to who should be viewed as a potential candidate, and a general inattention to questions of Eastern enlargement in the better part of the first decade of postcommunism (Mattli and Plumper 2002). Furthermore, in her study of institutional and regulatory reform in Eastern European democracies, Gryzmala-Busse (2007) demonstrates that the EU demands that were approximately equal across her countries of interest are ineffective at explaining the variation in these specific areas. The implications here might very well be that while the EU potentially has positively affected economic reform and democratic consolidation in Central and Eastern Europe in the sense of the general level of economic reform and political rights (the measure used in by Schimmelfennig and Sholtz), its ability to affect more specific policies and outcomes remains questionable. Such a reading when it comes to judicial independence is certainly bolstered by a close examination of the priorities of the European Union as determined by the content of their regular (yearly) reports from the Commission on each country's progress towards accession, which spend at

most a few sentences of their dozens of pages on the question of judicial independence and reform, and do so with the broadest of brushstrokes.[17]

To account for the theorized positive effects of Europeanization on institutional reform, democracy, and governance, I use a binary measure which takes a value of 1 if the state during the year in question was a formal applicant for membership in the European Union (roughly a third of the country-years in question). As the analysis here extends to 2004, and it was the middle of that year that the first wave of postcommunist applicants joined the EU, the more complicated question of whether or not the positive effect that conditionality had on candidate states remained after accession is not an issue (Epstein and Sedelmeier 2008; Sedelmeier 2008).

Presidential powers

Much work on institutions and political outcomes in the postcommunist world has found that presidentialism in the region is consistently associated with less-desirable outcomes (Herron and Randazzo 2003; Fish 2005). These prior findings and the findings of the previous chapter warrant the inclusion of a covariate assessing the nature of the executive. Rather than simply using a categorical measure as was done globally, however, in this chapter I use a measure that directly captures the extensiveness of the powers of the president. Presidents in postcommunist countries combine a substantial degree of executive power with an office much more loosely tethered to the moderating effects of political parties and powerful cabinets. Rather than needing to maintain support for their government and policies (the toppling of prime ministers is not uncommon in Eastern Europe and the former Soviet Union), presidents are forced only to occasionally seek popular approval of their general administration.

With regards to parliamentary systems, we observe that coalition governments are common in the postcommunist context, further moderating the potential influ-

[17]The specifics of economic reform, however, are covered thoroughly and in detail.

ence of the executive, which is forced to include a wider variety of viewpoints and preferences within the government. This is not to say that we should expect prime ministers in parliamentary systems to be more likely to take positive actions with regards to judicial independence, but rather than they are more restrained from being able to take negative actions, in that de facto judicial independence thrives when other governmental actors choose—or are induced to choose—restraint. Still, there are theoretical reasons to suggest the opposite, as well; if the reason that political competition matters is that the fear of later being out of power has a moderating influence, presidents, who are certain to sooner rather than later be out of office, might be more likely to empower the courts. As such, it is hard to ex ante predict the direction of the relationship between presidentialism and independence after communism, though the findings of much prior research and the previous chapter would lead us to assume negative effects.

While many of the states of postcommunist Eastern Europe and Eurasia possess parliamentary systems, all have presidents with some powers, even if the powers are merely ceremonial. And among both states with and without elected presidents, the actual constitutional powers of the office vary considerably, with some presidents having substantially more powers than others (though all postcommunist presidents in presidential systems unsurprisingly have more power than their counterparts in parliamentary systems). To capture this variation, an index of presidential power is employed. Armingeon et al. (2008) expand on the presidential powers index first created by Frye (1997), cataloguing 29 different potential powers enumerated in the constitution.[18] Figure 6.1 is a histogram of the scores for presidential power across the geographic and temporal space being analyzed (country-years are used, as constitutional changes during the period in question result in differing scores within

[18]Each power was coded 1 if the president holds exclusive power, 0.5 if it is shared, and 0 if the president does not hold the power in question. In instances of indirectly elected presidents, the scores were multiplied by 0.5, as the very selection of the president is determined by another branch of government.

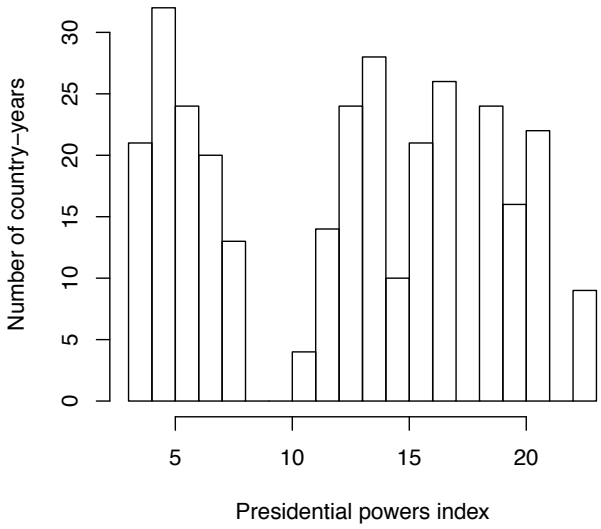

Figure 6.1: **Presidential powers index.** A histogram of the index of presidential powers in the postcommunist period being examined. The bimodality does not clearly differentiate parliamentary versus presidential systems; multiple countries with parliamentary governments are found in the grouping on the right of the x-axis. Index taken from Armingeon et al. (2008).

countries over time). The bimodal nature of the histogram does not clearly differentiate parliamentary versus presidential systems; multiple countries with parliamentary governments are found in the grouping on the right of the x-axis, thus lending further support to the use of a continuous, rather than dichotomous, measure.

Initial conditions and legacies

The literature on postcommunism has established that the legacies of communist rule have affected a number of aspects of the postcommunist institutional environment as well as popular attitudes, and the debate has now turned to how and under what conditions these legacies matter, rather than if they exist (Jowitt 1992; Ekiert and Hanson 2003; Howard 2003; Bernhard and Nordstrom 2010). Scholars have suggested a number of "legacy" factors that could potentially matter for institutional and eco-

nomic development after communism, including the time spent under communism, the nature of communist rule itself, and the degree of economic distortions caused by Stalinist economics (overindustrialization and the black market exchange premium, for example).[19] Other structural factors that relate to these legacies have also been discussed, including the level of resource dependence, the geographic location of the country, the level of economic development at the time of communist collapse, and urbanization(Sachs and Warner 1995; Kopstein and Reilly 2000; Hoff and Stiglitz 2004).

As many of these initial conditions are highly correlated, de Melo et al. (2001) used principal components analysis of 11 relevant initial conditions, including those listed above, as well as repressed inflation, trade dependence with other socialist economies, and prior experience with independent rule. These data have been used by many scholars studying postcommunism, including Fischer and Sahay (2004), who used the resulting index to revisit their prior empirical studies on economic transition. To account for the potential relationship between these structural factors, the initial conditions index is employed in the models below. Figure 6.2 maps these scores to provide an idea of their distribution across Eastern Europe and the former Soviet Union, with darker grays being higher and thus "better" initial conditions.

Time since democratic transition

If democratic consolidation is a process that occurs over the course of years, the time since a democratic transition could be related to the level of de facto judicial independence in a country. As actors become accustomed to the democratic process and the new institutional context, there are two possible outcomes. The first is that the political actors embrace the democratic order as time progresses and judicial independence deepens. Similarly, as other actors such as civil society organizations

[19]For a recent review of the literature to date, see Schimmelfennig and Cirtautas (2010).

Figure 6.2: **Initial conditions.** A map showing scores from a principal components analysis of 11 initial conditions created by de Melo et al. (2001).

develop and strengthen in the post-transition polity, they should be able to serve as a watchdog, checking potential infringements on the judiciary (USAID 2009). The other, less happy, scenario is that as time progresses and political actors solidify their bases of support and their control over new state institutions (as well as expand their economic activities) they instead are more able and willing to curtail the power of the courts. Similarly, as the luster of the new democratic institutions fades in the reality of unmet postcommunist expectations, it could be that other powerful actors are less inhibited and the potential for abuses increases (Urban 1997). To test these competing ideas, a covariate counting the number of years since the transition to democracy is included.

6.3 Analysis

To test the arguments presented in Chapter 1 with more fine-grained data, this section presents a number of models examining the association between political competition and de facto judicial independence after communism, conditional on regime type. Results show that while competition is both statistically and substantively important,

its effects are more salient in non-democracies, with results generally holding across different model specifications. The data cover the time period after the adoption of new constitutions in each postcommunist state until 2005, with no missing country-years, for a total of 308 observations.

Ordered probit is used as the Tate and Keith measure for de facto judicial independence contains multiple levels with no reason to assume that distances between each level are equal.[20] Two primary models are specified and reported in Table 6.6. In both models all covariates previously described are employed (each lagged one year to prevent simultaneity bias), and standard errors are robust, clustered by country.[21] The difference between these two models is how political competition is operationalized: Model 1 reports results of the continuous measure, the percentage of legislative seats held by the largest opposition party. Model 2 reports results of the dichotomous measure, where all instances of the largest party controlling more than 20% of legislative seats are coded 1.

Results show the direct relationship between political competition and de facto independence is consistently positive and statistically significant, whether a continuous or dichotomous measure is used (I present interpretations of the continuous models, as they lend themselves to more intuitive interpretation). Similarly, economic development is consistently significant, with wealthier countries having a higher probability of judicial independence than their poorer neighbors. Time since transition to

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[20] All analyses were conducted using both the `polr` command found in the `MASS` package of `R` (version 2.10.1) as well as the `oprobit` command in `Stata v12` unless specifically noted otherwise.

[21] The results are almost identical if non-robust standard errors are included. The only difference is that the coefficient for presidential powers reaches the 0.05 level of statistical significance. Robustness to alternative model specifications are included in the appendix. Results here are robust to multilevel modeling with intercepts varying by country, averages of multi-year lags of the key independent variable, using Freedom House's three-level measure of political rights (not free, partially free, free), the inclusion of a covariate indicating distance from Brussels (Kopstein and Reilly 2000), and a cubic polynomial of time to account for potential temporal dependence (Carter and Signorino 2010). Additionally, an ordered logistic regression model (with comparable results to the ordered probit employed in the analysis here) passes the Brant test, suggesting the parallel regressions assumption is not violated.

Table 6.6: **Ordered probit results.** Two ordered probit regression model with a trichotomous measure of de facto judicial independence. Errors are robust and clustered by country. Model 1 employs a continuous measure of political competition, the share of seats controlled by the largest opposition party. Model 2 employs a dichotomous measure, 1 when the largest opposition party controls at least 20% of the legislature. Asterisks denote the typical 0.05, 0.01, and 0.001 levels of statistical significance.

	Model 1	Model 2
Political competition	0.09^{***}	1.36^{***}
	(0.02)	(0.39)
Democracy	1.76^{***}	1.14^{**}
	(0.43)	(0.49)
Pol. comp. × Democracy	-0.08^{***}	-1.32^{**}
	(0.02)	(0.50)
log(GDP/capita)	1.47^{***}	1.42^{***}
	(0.42)	(0.42)
Time since democratic transition	-0.20^{***}	-0.15^{***}
	(0.05)	(0.05)
De jure independence	−0.11	−0.04
	(0.32)	(0.33)
Foreign aid	0.01	0.00
	(0.00)	(0.00)
Conflict	−0.19	−0.10
	(0.33)	(0.36)
EU applicant	0.37	0.16
	(0.38)	(0.38)
Presidential powers	−0.07	−0.07
	(0.04)	(0.04)
Initial conditions	−0.04	−0.02
	(0.09)	(0.09)
Cut-points:		
1 | 2	1.74^{***}	0.83^{***}
	(0.32)	(0.21)
2 | 3	3.14^{***}	2.16^{***}
	(0.35)	(0.24)
BIC	411.57	428.93
N	308	308

democracy shows a consistent negative relationship in both models.

The results provide support for the argument being tested here: that the relationship between political competition and judicial independence is conditional on regime type, and that the effects of competition are more pronounced in non-democracies.[22] Results here also bolster the findings of the global analyses presented in previous chapter, and do so with very different data. Political competition is positive across regime type, but attenuated in democratic states. That is, the relationship between competition and independence is stronger in non-democracies. Much like the analysis presented in the previous chapter, democracy is itself consistently associated with higher levels of judicial independence.[23]

An important implication of the non-significant covariates in Table 6.6 is that status as a EU applicant has no relationship with independence. This finding contrasts with the "Europeanization" literature, which argues that EU influence positively affects political outcomes in Eastern Europe, and bolsters the arguments of those who contend that EU leverage is of minimal influence compared to domestic factors (Gryzmala-Busse 2007). The results suggest that in cross-national tests, there is no consistent effect of a country's status, and that the influence of the EU is not without limits. This is not entirely surprising, however, given the EU's relative lack of attention to judicial independence (Pettai 2003).

Aid, the other international factor, also shows no significant effect. The results question whether democracy and governance assistance produces positive effects with regards to judicial independence. Results hold regardless of how aid is measured: total aid, as percentage of GDP per capita as done here, or by population size. Similarly, if aid is restricted to just rule of law aid (and no other form of democracy and governance aid is included) results are similarly indeterminate. In no specification

[22]Though not reported here for reasons of space, models without the interaction term show significantly poorer fit as measured by the Bayesian Information Criterion (BIC).

[23]Results hold when using the trichotomous Freedom House measure of political rights, which classifies states as free, partially free, or not free.

is aid statistically significant. This bolsters conclusions drawn from the lack of efficacy shown by EU applicant status, as even in cases of high amounts of democracy and governance assistance, it is unlikely that such assistance will effectively constrain powerful actors who otherwise seek to infringe on independence.

The lack of a relationship between initial conditions and de facto judicial independence should not be too surprising, as there are few clear reasons to think that such legacies would directly translate into variation in levels of judicial independence. Similarly, that there is no relationship between de jure and de facto independence is unsurprising, given the theoretical and empirical consensus on the literature that the former has little effect on the latter, and that measures of each show no relationship (Feld and Voigt 2003; Ríos-Figueroa and Staton 2009). That conflict is not associated with judicial independence is surprising, given that it has been suggested to be both theoretically and empirically. This is potentially because the total number of country-years where conflict is present is small. Similarly surprising is the lack of significance of the presidential powers covariate, as much work demonstrates the pernicious effects of overly powerful executives in the region.

6.4 Exploring substantive significance

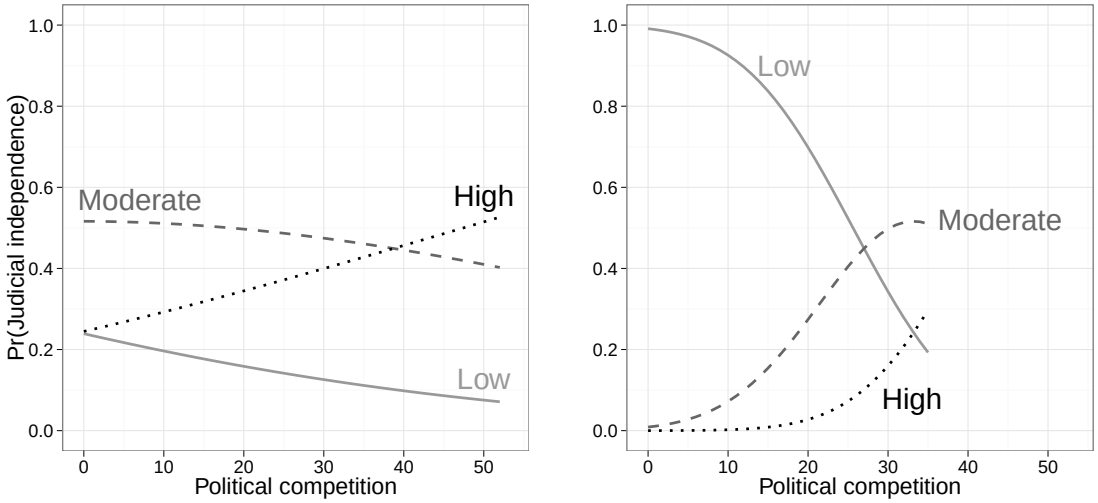
Unlike with many linear models, interpreting the *substantive* importance of coefficients reported in ordered probit models is not straightforward. To better ascertain how models predict changes in the probabilities of judicial independence, Figure 6.3 presents plots of the proportional probabilities (for Model 1) that a state falls into each of the three categories of de facto judicial independence, conditional on the value of political competition. At any point along the x-axis, the sum of the three lines (i.e. each of the three probabilities) is equal to 1.

Figure 6.3 plots the predicted proportional probabilities of each level of de facto judicial independence as political competition varies. In the left plot (where the covariate for democracy equals one), other covariates are held constant at the mean

observed for all postcommunist country-years where democracy equals 1. This is done because many economic and political outcomes travel alongside each other in the post-communist world, and there is no reason to assume that the mean level of economic development, for example, is constant across regime type: we know for a fact that the democracies of Central Europe are much more developed than the autocracies of Eurasia. Given the statistically significant and substantively important positive relationship between economic development and judicial independence estimated by the models, using the postcommunist mean would inflate the probability of independence in non-democracies. Correspondingly, in the right hand side plot of Figure 6.3, the other covariates are held constant at the mean value of non-democratic country-years. The plotted lines along the x-axes vary between regime type, to not extrapolate beyond the observed data: no postcommunist non-democracy has an opposition party with more than 35% of the legislature.

Differences between the predicted probabilities across regime type demonstrate that while the effect of political competition on judicial independence is positive, its effects are much stronger in non-democracies. The probability of having low judicial independence—the solid line—falls sharply as political competition increases in autocracies. In democracies it decreases more slowly. This demonstrates the effect of the coefficient for the interaction term: in democracies the interaction is attenuating the relationship between competition and independence.

Comparing plots in Figure 6.3, at no level of competition is the probability of low judicial independence greater in democracies. The proportional probability for high judicial independence category is similar. At the highest observed level of competition in non-democracies, the probability of having a highly independent judiciary is less than in democracies. Even though the relationship is attenuated in democracies, Figure 6.3 shows that it is still vitally important: when the largest opposition party holds 10% of the legislature, the predicted probability of high de facto independence is 0.31, whereas when it holds 40% it is 0.44. This demonstrates



(a) Democracy (b) Non-democracy

Figure 6.3: **Effects of political competition.** Predicted proportional probabilities (Model 1) of being in each category of de facto judicial independence conditional on the interaction of political competition and democracy, with other covariates held at mean values by regime type.

visually the effect of the democracy covariate, which is highly statistically significant and positive.

The plot for democracies in Figure 6.3 also illuminates an interesting facet of the relationship obscured by regression output: competition in democracies primarily increases the probability of high judicial independence. The plot for autocracies shows a much different relationship. Increased political competition strongly diminishes the predicted probability of having low judicial independence, but it does so primarily by increasing the chance of moderate independence. The plot for non-democracies demonstrates that even in non-democracies competition is important. Once the largest opposition party commands between 15% to 20% of seats, a marked increase in the probability of moderate judicial independence begins.

The results of Figure 6.3, however, still suggest that the substantive importance

of political competition for judicial independence is different than what many scholars have theorized or reported. Even at the highest observed level of competition, the chance of a postcommunist democracy having high de facto independence is still only 50%. And when the largest opposition party controls 20% of the legislature, there is only a 1 in 3 chance of high judicial independence. In autocracies, 20% of seats held by the largest opposition party is associated with a probability of low independence of 0.74. Were the insurance model as typically conceived sufficient in explaining judicial independence, these results would be unexpected. If holding all else equal the chance of a highly independent judiciary in a postcommunist democracy is only 50/50 under extreme levels of political competition, competition cannot be treated as the sole determinant of de facto independence as is common in the literature.

Looking at Figure 6.3, it is clear that competition is important in getting *some* degree of judicial independence. If the largest opposition party in a non-democracy controls 20% of the legislature, the probability of at least moderate independence is predicted to be 0.26. If it gains an additional 10%, the probability jumps to 0.55. At such levels in a democracy, the predicted probabilities for at least moderate judicial independence are 0.84 and 0.87, respectively. This finding has implications for the insurance model, offering a more nuanced and developed picture of how political competition relates to de facto independence: instead of a deterministic relationship where competition means judicial independence, this suggests that competition is important — but not the only factor. The relationship between competition and judicial independence is not universal, but mediated via democracy. Political competition is different under different regime types, and its relationship with judicial independence is similarly different.

6.5 Conclusions

This chapter tests the second main argument forwarded in this dissertation, that the relationship between competition and judicial independence should be stronger

in non-democracies, and does so in a vastly different way than the previous chapter. While the previous chapter looked at the question in 162 countries over 44 years, this chapter does so by using fine-grained data on 15 years of the postcommunist experience. The models presented in Table 6.6 further bolster the inferences drawn in the previous chapter, as I find a similar relationship between competition, independence, and regime type, despite the vastly different measures and universe of observations. Once again, competition is highly statistically significant (and in this case this fact is quite impressive given the small number of observations), and once again its effects are highly attenuated in democracies. But, as the coefficient for democracy and the two plots in Figure 6.3 make clear, democracy by itself has a strong positive association with judicial independence, similar to the findings in the previous chapter.

The results of models presented in this chapter also have implications for our understanding of the relationship between the rule of law broadly and judicial independence specifically and economic outcomes. While many scholars have contended that judicial independence is an effective predictor of economic performance over the long-run (Feld and Voigt 2003; La Porta et al. 2004), most of this work has been cross-sectional and plagued by issues of systematic missingness correlated with economic development (Ríos-Figueroa and Staton 2009). The postcommunist context offers us a chance to analyze the effect of economic development on judicial independence, rather than the reverse, in a situation where levels of economic development are effectively exogenous to independence; every postcommunist state started the transition lacking judicial independence, and it was starting conditions in play with factors such as privatization, liberalization, and other reforms that led to divergent economic outcomes as the transition progressed. The results presented in Table 6.6 are as striking as they are conclusive: the level of economic development matters—a lot—for the degree of judicial independence in postcommunist countries, both democracies and non-democracies alike. While over the long-term this relationship is undoubtedly reciprocal, here we can be relatively confident in the causal direction for the reasons

discussed previously, and this suggests that we carefully rethink our empirical and theoretical conclusions about the effects of judicial independence on economic outcomes, as the latter can be exogenous to the former, and in such cases can be highly relevant.

6.6 Appendix

This section contains supplemental information for the work presented in the chapter, including variable descriptions, descriptive statistics and visualizations of variables, and alternative model specifications offering robustness checks.

6.6.1 Descriptive statistics

Figure 6.4 shows the frequencies of the values of the dependent variable (de facto judicial independence). The left plot contains histograms showing the frequency of none, some, and full judicial independence across all 308 observations. The center plot contains histograms showing the frequency across the 166 democratic country-year observations. The right plot contains histograms for the 132 non-democratic country-years.

As could be expected, the relative frequency of each of the three ordinal categories is different according to regime type: in democracies, high judicial independence is the modal category, whereas in non-democracies low independence is the modal category. That being said, a large portion of democracies demonstrate low judicial independence, with 26% of the observations falling into this category. Similarly, 22% of non-democratic observations demonstrate some degree of judicial independence (either moderate or high). This demonstrates clearly the contention in the paper and the historical record that judicial independence and democracy cannot be treated as synonymous with one another.

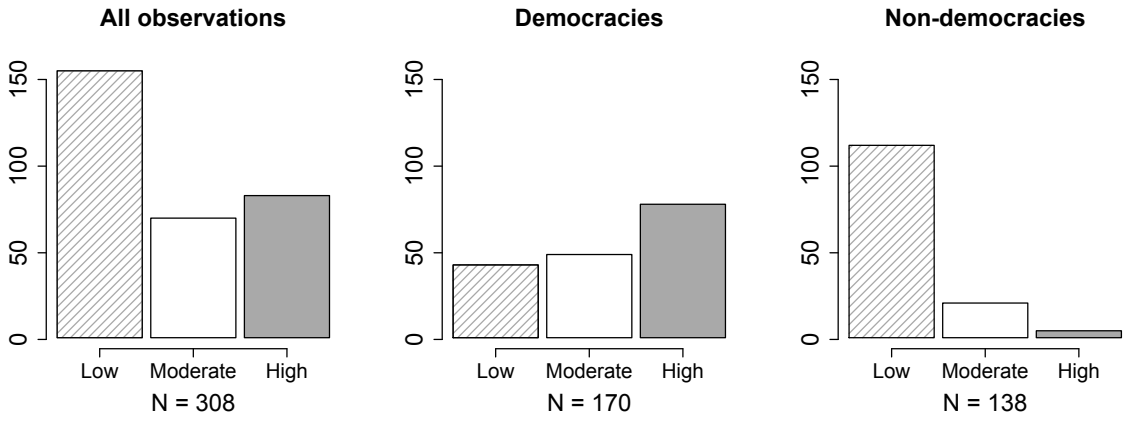


Figure 6.4: **De facto independence.** Thee plots with histograms showing the number of observations in each category of de facto judicial independence. The left plot shows all postcommunist observations, the middle only democratic observations, and the right only non-democratic observations.

Figure 6.5 contains density plots for four covariates whose variation is interesting. None of the binary variables nor the year trend were plotted, as the plots would show only minimal information. With regards to democracy, 166 observations are considered democratic, while 132 are not. 9% (28) of the observations experienced a conflict, while 91% (270) did not. The final dichotomous measure, EU applicant, has 32% (99) of the observations qualifying as candidates, while 65% (199) observations did not. Here the solid gray lines signify the density across all observations. The dashed black lines show the density for democratic observations, and dotted gray lines convey the density for non-democratic observations. While readers are obviously free to draw their own conclusions from these densities, three things are worth noting. First, for presidential powers, the distribution of scores within non-democracies is bimodal, primarily but not exclusively a function of the Central Asian states' extreme presidential systems. Second, for initial conditions, the distribution for democracies is bimodal. This is primarily but not exclusively a function of the Eastern European/former Soviet divide (a number of components of the index cap-

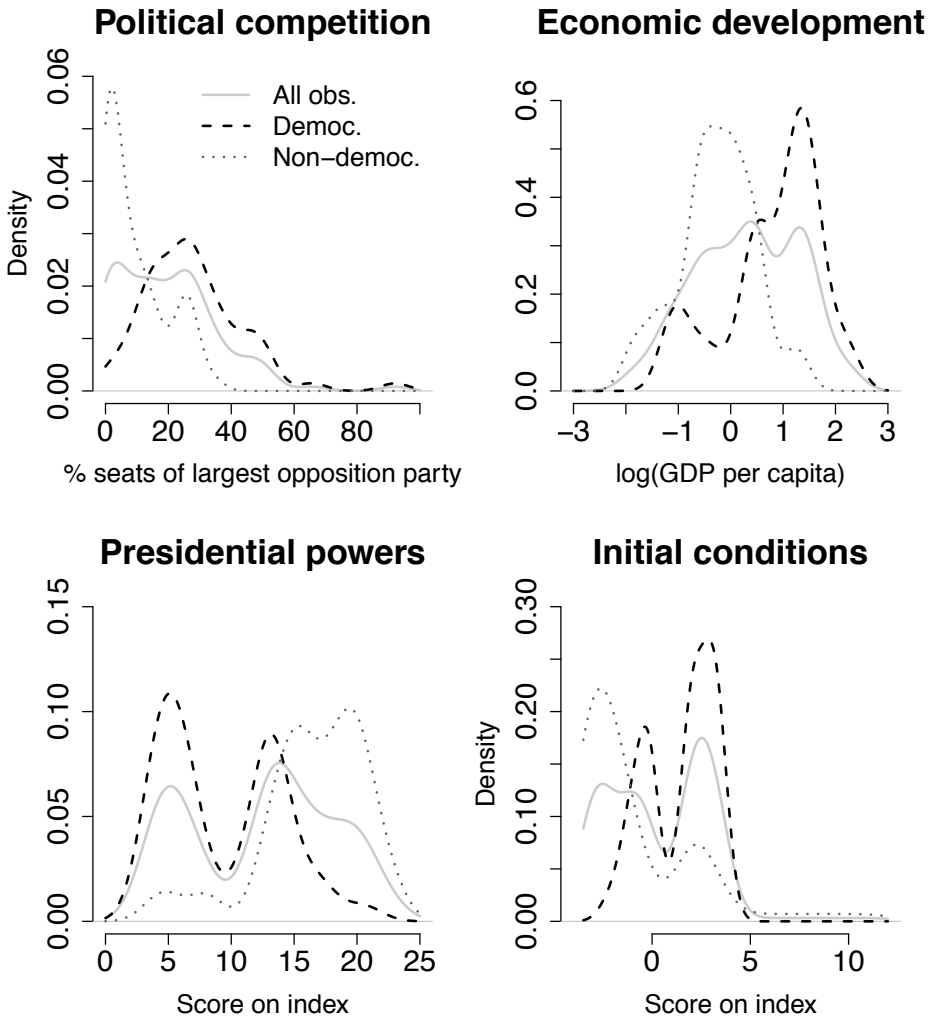


Figure 6.5: **Distribution of covariates.** Four plots showing the distribution of values of four covariates. The solid gray line is the density of all 308 postcommunist observations. The dashed black line shows the density of the 166 democratic observations. The dotted blgrayue line shows the density of 132 non-democratic observations.

ture this divide). Third, while there is a slight difference, the distributions of de jure judicial independence for democracies and non-democracies are highly similar. This is unsurprising, as de jure independence has no relationship with de facto independence: constitutional protections of independence are mere parchment barriers. Indicative

of this is the fact that every Central Asian state scores better on the index of de jure independence than Latvia, and all but Turkmenistan "outperform" Poland and Slovenia.

6.6.2 Alternative specifications

As noted in the chapter, the results hold up to a number of alternative specifications. Table 6.7 presents the results of three alternative specifications of political competition. Models 3 and 4 differ by using the averaged level of competition over the previous 3 and 5 years, respectively. This was done to ascertain whether the general level of competitiveness in the system in the preceding few years, rather than the specific level one year prior, showed a consistent relationship with de facto independence. The logic here is that it might very well be not only the exact level in the year before, but that leaders may take into account the level of competition that obtained in the recent past as a good judge of future expectations.

As can be seen, the general relationship of competition being associated with a higher probability of independence, but attenuated by democracy, remains. The only difference is that the relevance of competition—i.e. the coefficient–is slightly larger, suggesting that not only the year previous matters, but competition in the system in the preceding few years is critical.

Also mentioned was the fact that results are highly similar if the Freedom House measure for political rights was split into its common three-level scale of 'not free', 'partially free', and 'free'. Model 5 contains the regression output for a model using this alternative specification of democracy. The results are highly similar to the models presented in Table 6.6, with competition increasing the probability of judicial independence, but with this effect highly attenuated in democracies, both partially and fully free. The interesting difference between this model specification is that the coefficient for conflict reaches the $p < 0.05$ level of statistical significance. That being said, the actual substantive effect is much smaller than a one unit (in this data this is

Table 6.7: **Alternative specification.** Three ordered logistic regression models with a trichotomous measure of de facto judicial independence as the dependent variable. Models 3 and 4 average the degree of political competition over the previous 3 and 5 years, respectively. Model 5 uses the trichotomous Freedom House Political Rights measure.

	Model 3	Model 4	Model 5
Poltical competition	0.12***	0.13***	0.43**
	(0.02)	(0.02)	(0.13)
Democracy	1.74***	1.72***	
	(0.47)	(0.50)	
Pol. comp. × Democracy	−0.09***	−0.09***	
	(0.02)	(0.02)	
Partially free			4.11**
			(1.25)
Free			4.35***
			(1.23)
Pol. comp. × Partly free			−0.41**
			(0.13)
Pol. comp. × Free			−0.42**
			(0.13)
log(GDP/capita)	1.71***	1.80***	1.59***
	(0.22)	(0.22)	(0.21)
Time since democratic transition	−0.25***	−0.25***	−0.22***
	(0.04)	(0.04)	(0.03)
De jure independence	−0.14	−0.15	0.03
	(0.13)	(0.13)	(0.14)
Foreign aid	0.01*	0.01*	0.01
	(0.00)	(0.00)	(0.00)
Conflict	−0.55	−0.65	−0.75*
	(0.34)	(0.35)	(0.38)
EU applicant	0.39	0.40	0.33
	(0.26)	(0.27)	(0.30)
Presidential powers	−0.07**	−0.07**	−0.05*
	(0.02)	(0.02)	(0.02)
Initial conditions	−0.09	−0.10	−0.10
	(0.06)	(0.06)	(0.07)
1 | 2	2.17***	2.31***	4.16***
	(0.36)	(0.38)	(1.17)
2 | 3	3.65***	3.82***	5.62***
	(0.41)	(0.43)	(1.19)
BIC	396.74	392.47	407.34
N	308	308	308

also precisely one standard deviation) increase in the log of GDP. Similarly, moving from a non–democracy to a either a partial or full democracy has three times the effect of conflict, and a few more percentage points of competition outweigh the effect of conflict.

Chapter 7

CONCLUSION

The goal of this dissertation is to challenge the existing literature on the determinants of judicial independence to the degree that this literature has failed to account for empowerment of courts in non-democracies or variation in the levels of judicial independence outside of the democratic context. I offer two key arguments, and test each exhaustively, with multiple data sets and countless model specifications.

The first of the two key arguments is that the popular insurance model of judicial independence, predicated on the incentivizing effects of electoral-competition, should also be of use in explaining judicial independence in non-democracies. The basis of this argument is the fundamental logic of the insurance model itself: independence is a function of the risks of losing office and being in the political minority and the expectation that such an outcome is likely. While most non-democracies do not evidence much electoral competition, it is indisputable that there is a sizeable minority of autocratic regimes that do; these are the competitive authoritarian regimes thoroughly analyzed by Levitsky and Way (2010). Given the fact that in some non-democracies there is in fact a likelihood of losing office through electoral means, the question is then whether or not there are significant risks of losing office in these regimes. In Chapter 1 I offer a number of arguments why losing office in non-democracies presents the same risks as in democracies. If this is the case, and electoral competition *does* exist in some non-democracies, then there is no reason that the logic of the insurance model should not apply to these states.

Chapters 3 and 4 test this argument. Chapter 3 does so by looking at what I call judicial empowerment, or the establishment of judicial independence. Here, I

use a binary measure of judicial independence to create a covariate demarcating non-democratic country-years in which judicial independence first appears. I then use logistic regression to examine my argument about the relevance of political competition and existing arguments about what compels autocrats to empower their judiciaries. Results fail to provide any support for the existing explanations found in the law and politics literature, drawn from single-country case studies. Rather, they show that political competition is consistently and strongly associated with a higher probability of judicial empowerment.

Instead of looking at judicial empowerment, Chapter 4 examines variation in judicial independence in non-democracies, using a new measure of independence produced by Linzer and Staton (2011). The results of hierarchical linear models (with intercepts allowed to vary by country and year) on data sets created by using two alternative classifications of democracy and dictatorship show that political competition is strongly associated with higher levels of judicial independence in non-democracies. To examine the robustness of these findings to alternative specifications, I use linear regression models with fixed effects (country and year), finding no discernible differences. The findings are robust to alternative measures of competition, either measured by the largest share of the opposition party or the degree of legislative checks on the executive. To determine just how well the models fit the data I use cross-validation, and show that the models fit as well out-of-sample as they do in sample, persuasive evidence that the models are effective at capturing the underlying data-generating process. Finally, I use multiple imputation to demonstrate that the results are not the result of potential biases resulting from missing data.

Chapters 5 and 6 examine the second key argument forwarded in this dissertation: that political competition is not only important in explaining judicial independence in non-democracies, but that is in fact more important in explaining independence in non-democracies as compared to democracies. The logic forwarded in existing political insurance accounts of judicial independence is grounded in the

idea that courts provide insurance against policy changes and help protect minority political groups, and thus minimize the risks associated with losing power. In Chapter 1 I argue that if this is the case then competition should be more salient in non-democracies because the risks of losing office are greater than they are in democracies for a number of reasons, including greater potentials for policy change, the expropriation of political and economic rights, and the physical integrity of leaders.

In Chapter 5 I test this argument using hierarchical linear models on global data covering up to 162 countries over 44 years. I find that competition is associated with significantly higher levels of judicial independence, and that this effect is conditional on regime type: in democracies, the effects of competition are significantly less than they are in non-democracies. These results are robust to a variety of model specifications, alternative covariates, and data imputation procedures. Similar to Chapter 4, I demonstrate that the models I present are able to predict the level of judicial independence in out-of-sample data exceptionally well, thus meeting one of the most stringent and generalizable tests of model fit.

Chapter 6 shifts the focus to the postcommunist countries of Eastern Europe and the former Soviet Union. Here, I use primary and secondary sources to create a highly accurate measure of political competition, finding that estimates of the effects of political competition on judicial independence meet the highest levels of statistical significance even in a data set with only 308 observations. The results bolster the findings of Chapter 5, as they are highly similar despite being derived using different measures of judicial independence, political competition, and democracy.

Beyond forwarding two new arguments about the relationship between political competition and judicial independence, this dissertation also offers the first cross-national tests of the insurance model of judicial independence, and the first ever test of one of the fundamental assumptions of the insurance model, an assumption that until now has remained unexamined. This assumption is that judicial independence actually provides insurance to those formerly in power. Obviously critical to

the overall model, to date it has been taken as given rather than subjected to empirical analysis. Chapter 2 tests this assumption using data on the post-tenure fate of political leaders between 1960 and 2000. Using traditional, hierarchical, ordinal, and multinomial logistic regression I find that judicial independence is strongly associated with better outcomes for political leaders in both democracies and non-democracies. These results suggest two things: first, that one of the fundamental assumptions of the insurance model holds up to (preliminary) empirical examination, and secondly, that there is reason to suspect that the model should apply to non-democracies. Obviously, the subsequent chapters of the dissertation provide further evidence of the second implication of Chapter 2.

In this dissertation I present one of the most thorough examinations of the political insurance model of judicial independence, offering new insights into its scope conditions and the explanatory power of competition as compared to other factors. Most critically, I offer a theoretical extension of the electoral competition explanation to the non-democratic context and exhaustively tests this extension in multiple ways. The results demonstrate that the idea found in the existing literature that political competition doesn't matter in authoritarian regimes is highly problematic. I find that opposite is in fact the case. Political competition is both the most statistically significant and substantively important covariate in nearly every test of the arguments presented, and the relationship is robust to alternative measures and data as well as a myriad number different model specifications. Competition matters, and it matters more in non-democracies.

This dissertation has implications not only for our understanding of judicial independence and courts in authoritarian states, but also for our understanding of authoritarian politics more broadly. If independent judiciaries are better able to secure both property and civil rights, as many suggest, as well as increase the successes of other liberalizing policies, as others contend, then the role of political competition in authoritarian politics is critical. An important area for future research is to inte-

grate arguments for and against the insurance model and examine what institutional features of autocracy might allow it to be "immune," able to maintain dependent courts in the face of continued political competition. Finally, this dissertation helps integrate the study of courts in non-democracies with recent work on competitive authoritarian regimes, expanding upon the insight that the presence of real electoral competition changes the nature of authoritarian politics. One manner in which competition changes the nature of authoritarian politics is by increasing the likelihood of an independent judiciary.

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