

Democracy and State Repression:
What we Don't Know, Can Kill Us

by

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Chapter 1

Introduction

For nearly a quarter of a century, scholars have been concerned explicitly with the empirical relationship between democracy and physical integrity rights.¹ The literature has progressed a considerable amount from Stohl and Lopez (1984, 1986), who aggregated a set of country and regional studies in an effort to gain some leverage on the extent to which contextual variation effected a state’s use of force against its citizens. In that time, a few factors have emerged as the prominent determinants of state repression – namely, democracy, GDP and population. Aside from cursory coverage, I leave the latter two of these as fodder for future academic endeavors and focus on the former – democracy.

The literature on what Davenport (2007*b*) dubs the “domestic democratic peace,” exists in a somewhat strange and seemingly unstable equilibrium. There is considerable and mounting evidence that democracy “causes,” state repression. In fact, this democracy has been found to be one of the most important factors across space, time,

¹Later in the manuscript, I go into great detail about how both democracy and repression have been defined in the literature and how I define them specifically. However, by way of relatively quick introduction to the topics under consideration, I refer to state repression as the state’s use of tactics that unduly deprive citizens of their life (through extrajudicial killing) or their liberty (through the use of torture, forced disappearances or political imprisonment). With respect to democracy, I mean, in general, a complex of institutions that permits citizens to be involved in choosing their leaders and that limits the power of the chief executive through a series of checks.

region and model specification. This robustness in empirical findings suggests a level of theoretical rigor that has been lacking. The literature has generated three basic hypotheses about how and why democracy should effect state repression.

1. Through the use of their power to elect and recall leaders, citizens are able to increase the cost to leaders when they transgress the preferences of their constituents.
2. Checks and balances (i.e., veto players) constrain the ability of leaders to make harshly repressive policy.
3. Democracy provides opportunities for would-be dissidents to have their concerns addressed in the political system.

Often, these have been offered together as three reasons why democracy *could* matter and an empirical finding that democracy *does* influence repression is found as corroboration for this set of hypotheses together. I argue throughout the course of the ensuing manuscript that none of these three hypotheses have been sufficiently “tested,” in empirical models. Some have made admirable strides in this direction (e.g., Davenport 1996*a*, 1997, 2007*b*, Keith 2002), where either individual components of democratic pacification or the whole complex of explanations are investigated. Others, (e.g., Bueno de Mesquita et al. 2005) suggest that they have made strides in this direction, but as I argue below, the empirical analysis does not get us much closer to an understanding of how these various pieces of democratic pacification are related. Finally, some have not even really tried to investigate these various influences (e.g., Poe and Tate 1994). Thus, as a community of researchers, we are certain that democracy influences repression, we simply have little in the way of evidence for specific mechanisms. In the remaining pages of this manuscript, I directly test the extent to which each of these hypotheses is supported by data, both individually and in consort with the others.

Not only has the literature been insufficiently precise in its hypothesis tests, it has not taken measurement at all seriously. I take great pains to develop measures that closely approximate the theoretical quantities of interest. Further, I use these results of these measurement models in appropriate ways in further statistical analysis. With other colleagues (Armstrong, Duch and Bakker 2007), I propose a way to repurpose standard statistical tools for dealing with multiply imputed data to the task of estimating the parameters of predictive statistical models that include variables that are themselves estimates with corresponding uncertainty. I apply those methods to the data and measures developed here to provide the most rigorous investigation of the democracy-repression nexus to date.

Finally, the theories that are generally offered are meant not only to show how the dichotomous distinction between democracy and non-democracy works, but also to show how different types of democracies behave differently. I argue that many of the results that hope to provide evidence for these nuances have been mostly picking up the difference between democracies and non-democracies. Further, theories that suggest within-country relationships are often tested in models that leverage between-country variation, thus not providing a test of the more micro-level theories at all. By focusing primarily on a set of multiparty democracies, I take on both of these criticisms and show that, in fact, we are not particularly good at predicting within-country variation for two reasons - first there is often quite little variation on repression within democracies; and second, the types of explanations we offer provide very little within country variation. The only real exception to this is violent dissent, which has considerable within-country variance, but as I will show, has relatively little within-country explanatory power.

The findings are actually quite interesting and in some cases run almost directly counter to the conventional wisdom about why and how democracy influences repression. Briefly, I find that electoral accountability is the best democratic predictor of

repression. This confirms the findings of Davenport (2007*b*), but the reasoning runs almost directly counter to his. Rather than citizens “voting the bastards out,” there is considerable evidence that citizens may just as often be “voting the bastards in!” I also find little evidence for the pacifying effect of veto players. This is, however, more likely due to the fact that the literature has largely mis-interpreted Tsebelis (2002) and that when appropriately assessed, there is actually very little variation to leverage either for or against this hypothesis. Finally, I find that violent dissent has an enormous impact on state repression and that violence, itself is negatively affected by political inclusion. Further, I find that both electoral accountability and violence have independent, and counter to the literature unconditional, effects on a state’s repressive behavior.

The manuscript proceeds in six substantive chapters. In the first chapter, I outline the progress of the research on the domestic democratic peace. The second chapter provides a rigorous treatment of measurement for the variables of interest to the investigation. Here, I also estimate models as that replicate past research with the new measures to show that they behave in the expected fashion. In the subsequent three chapters, I offer detailed analysis of the three hypotheses mentioned above - starting with electoral accountability, then veto players and finally conflict. Next I take the best results from each of the three chapters and combine them to assess the support for each hypothesis in competition with the others. As I will show, variables tend to fail on their own without help from collinearity, so the resulting final model retains a number of interesting features of the models in each chapter.

Chapter 2

The Domestic Democratic Peace

The theoretical underpinnings of the domestic democratic peace could be framed as the tension between Hobbes (1997) and Madison (1787). On the one hand, the most fundamental charge of any state is the protection of its citizens. By relinquishing some power to the sovereign, citizens are able to secure a modicum of security and freedom. When security is threatened, Hobbes would suggest that it is the State's duty to step in and secure its citizens at all costs. On the other hand, democratic governments are generally operating within an institutional structure that was expressly designed to prevent just this sort of governmental activism to ensure security at all costs. The respect for minority rights is one of the hallmarks of modern liberal democracy. The research on the domestic democratic peace, then asks – is democracy up to the task of preventing governments from running roughshod over the rights of their citizens in the face of security threats? And, if so, how?

The robust relationship between democracy and state repression has been termed the “domestic democratic peace,” by Davenport (2007*b*). Here, Davenport is transplanting the language of the international Democratic Peace Theory to the domestic realm. While this is not a thesis concerning the international democratic peace, it is worth setting forth its major tenets in order to match them with their domestic counterparts. The main finding of the democratic peace literature is that generally, democracies do not fight wars with other democracies (Russett 1993). A secondary

claim, and one that is perhaps more relevant here, is that democracies are more pacific in general. The explanations for these findings range from structural to normative, but empirical investigations have tended to focus on structures as they are much more easily observed. There are a number of structural explanations, but they can be grouped as follows according to Gelpi and Griesdorf (2001):

- Liberal leaders are subject to institutional constraints that make it impossible to re-position state resources and efforts toward war mobilization without large-scale public approval. Further, these constraints are common knowledge to other states and as such, allow credible signals to be sent regarding willingness to fight. Two states with similarly well-known constraints will be much more likely to negotiate peaceful outcomes.
- Democratic government decreases the capacity of government leaders to extract rents. As such, territorial expansion becomes less attractive.
- The fact that wars are public allow citizens to enforce “large and transparent costs on leaders.” Thus, threats are taken seriously and the credibility of threats between liberal states generally leads to a diplomatic solution.

These explanations all suggest that leaders are constrained by institutions and public opinion.

The domestic democratic peace refers to a similarly robust empirical regularity - democracies are less *internally* violent than non-democracies (Davenport 1996*a*, 1999, 2004, Fein 1995, Mitchell and McCormick 1988, Poe and Tate 1994, Poe, Tate and Keith 1999, Regan and Henderson 2002). That is to say, violent state repression has been found to be lower in democracies across a wide range of time-periods, contexts and operationalizations (of both democracy and repression). In a very similar fashion, explanations tend to be structural in nature and tend to focus both on institutions and public opinion.

There are, however, some important differences between the domestic and international democratic peace. Negotiation may be a less viable strategy in internal conflicts. Repression has been found to be a response to violent dissent Davenport (2007*b*). If the violent dissent is aimed at the destruction of the government either from extremist forces within the polity or, more problematically from outside, then it seems quite clear that the range within which an acceptable compromise could be found is vanishingly small. Some degree of repression may be required to maintain the security of the polity. It is often not discussed, but all repression is not equally bad. There are more and less legitimate uses of repression and it is quite possible that public opinion will be in favor of the legitimate use of repressive force. In fact, as Davis (2007) and Davis and Silver (2004) suggest, citizens may even be in favor of quite illegitimate forms of coercive force if the security environment is sufficiently uncertain. Further, institutional constraints that are a function of the policy position of other political actors (e.g., legislative leaders) could also be less relevant as political leaders across ideological lines come together in favor of measures intended to increase the security of the polity. Public opinion about an international war in the face of violent action could be overwhelmingly in favor of action to security the country's territory.

Another major difference between the international and domestic democratic peace is the obvious power asymmetry. Incomplete information in the international realm can lead to unclear expectations about which side will win a conflict. This is not technically the same internally except in extreme cases of state weakness. Generally, governments monopolize the state's military capabilities. Thus, both sides know who is stronger and what the ultimate outcome of a "war," would be.

There are three broad theories that explain the mechanisms whereby democracy is thought to reduce state repression. These are - veto (institutions), voice (electoral accountability), and conflict resolution. It is this last that is the oldest in the empirical

literature. Henderson (1991) suggested that democracies provide effective means for the non-violent resolution of conflict and that in democracies, conflict should never get sufficiently bad that repression is necessary. Poe and Tate (1994) used this as their main argument for democracy's pacifying effect. Despite being quite reasonable theoretically, the empirical support for this proposition has been inconsistent. As Lichbach (1987) suggests, aggregate studies are unable to model the strategic interaction between dissidents and governments and, as such, aggregate models are unable to offer much in the way of consistent predictive power.

The second method through which democracies could influence repression is through the checks they place on the chief executive. A considerable amount of theoretical work has been done on the extent to which the number and position of veto players makes policy change more or less likely (Tsebelis 2002, Keefer and Stasavage 2002). If repression is viewed as a policy decision, then these results should apply equally to this dimension as well. Unfortunately, it seems as though the literature has misinterpreted this finding and has found considerable support for its mis-interpretation. Tsebelis suggests that analysis of this kind can only be conducted profitably within each country independently. There is no necessary between-country relationship with respect to the number of veto players. In general the literature has used statistical techniques that leverage both between and within country variation in ways that do not allow them to be separated (e.g., Armstrong and Davenport 2003, Bueno de Mesquita et al. 2005, Davenport 2007*b*, Davenport and Armstrong 2004, Poe and Tate 1994).

Thirdly, as Davenport (2007*b*) suggests, electoral accountability affects the state's willingness to use repression. The logic goes as follows – leaders want to maximize the time they spend in office. As such, they need to be constantly seeking the favor of their constituents. Citizens, always prefer less repression and thus leaders will always be trying to minimize their use of repression to maximize their popularity. The basic

premise that citizens prefer less repression is a dubious one at best (Davis 2007) as is the implicit converse that regimes always prefer more repression (Olson 2000). While this particular logic may have some flaws, the general idea is a sound one – leaders should seek to maximize the extent to which they follow the preferences of their constituents. Rather than their constituents holding them back, however, it could be that constituents are pushing for more repression (i.e., increasing the demand). Either way, electoral accountability should influence the state’s use of repression. Whether the public opinion is stable and opposed to repression or variable (and occasionally supporting of repressive action) is an empirical question that will be addressed below.

That is it. There are no complex formal models; there are no game trees. There are simply three propositions that seem reasonable. Further, given the nature of the analysis that has been done already, many would say that all three have been confirmed. I however, disagree. It is rare to find scholars attempting to adjudicate *between* these different explanations and those who have, reach differing conclusions. For example, Bueno de Mesquita et al. (2005) find that many different aspects of democracy must be in place before integrity rights are expected to improve. This confirms the work of Davenport and Armstrong (2004) who found similar things, though they did not attempt to address the question of which explanation was most effective. Davenport (2007*b*) assess the extent to which voice and veto, in different security environments affect a state’s use of repression. He finds that voice is relatively more powerful than veto, though both work to decrease repression in various contexts and both are less effective toward that end in the face of violent dissent.

The investigations that have taken place tend to consider not whether the specific mechanism is or might be at work, rather investigations focus on conditions that would allow that mechanism to function. For example, Davenport (2007*b*) considers the extent to which voice (the ability of citizens to remove governments through regular elections) is a feature of the political system. The assumption is that if the

practice exists, citizens *will* use it as hypothesized. Whether citizens *actually* punish leaders for increasing repression is an empirical question that is yet unanswered. Further, research considering the extent to which more constrained executives adopt less repressive strategies tends to focus only on the number of constraints. I will consider the effect on observed human rights practices of differing positions that political factions take on the issue. Through a deeper empirical investigation of these theoretical ideas, I can begin to assess the claims made by each explanation.

2.1 A Brief History of the Domestic Democratic Peace

Studies of state repression proceeded largely on country-by-country or regional basis until the mid-1980's (e.g., Dallin and Breslauer 1970, Duff and McCamant 1976, Fruhling and Woodbridge 1983, Gibson 1988). Lopez and Stohl (1989) and Stohl and Lopez (1984, 1986) aggregated these works in an effort to give some coherence to the subject as a global research agenda. As a discipline, the need to tie these studies together with cross-national research was clear.

In 1994, Poe and Tate wrote what would become the seminal article in the field. There, they hypothesized that either through democratic norms, democratic behavior or democratic institutions, democracy makes states less likely to violate the physical integrity rights of their citizens. Using both the Freedom House political rights measure (Piano and Puddington 2008) and Vanhanen's polyarchy measure (Vanhanen 2000), they found that democracy was one of the most important factors in curbing a state's repressive tendencies. Most of the literature in the ensuing decade would confirm this general finding.

Democracy has continued to receive virtual primacy in the explanations for state repressive activity. In fact, as Davenport and Armstrong (2004) state, "Repeatedly,

democratic political systems have been found to decrease political bans, censorship, torture, disappearances, and mass killing, doing so in a linear fashion across diverse measurements, methodologies, time-periods, countries and contexts.”¹ This key finding was probed and pushed in different directions. Davenport (1996*b*) and Zanger (2000) considers the temporal pattern of respect for civil liberties (censorship and sanctions). Still others, suggested a different functional form of the relationship such as a second-degree polynomial (Fein 1995, Regan and Henderson 2002) or a threshold (Davenport and Armstrong 2004).

Scholars have tended to focus on physical integrity rights to the exclusion of other potentially interesting human rights. The reasons for this generally focus on endogeneity, though there are substantive reasons to privilege these particular rights, which I discuss briefly below. The overwhelming majority of work in this area has used the Political Terror Scale (Gibney and Dalton 1996). This is a five-point scale where increasing values refer to increasing severity and scope of the activities covered - torture, political imprisonment, extrajudicial killing and forced disappearance. Recently, Cingranelli and Richards (2004*a*) released a dataset that disaggregates these various components.

There have been disagreements about dimensionality on both sides of the equation. McCormick and Mitchell (1997) suggest disaggregating the political terror scale into two distinct dimensions, though in a more convincing paper, Cingranelli and Richards (1999) show that the observed physical integrity rights indicators are well-explained by a single dimension. The more interesting discussion of dimensionality occurs on the democracy side. Both Bueno de Mesquita et al. (2005) and Armstrong and Davenport (2003) suggest disaggregating the Polity IV variable into individual

¹There are few studies that say democracy does not matter, but one in this vein is Franklin (1997) who shows that IMF conditionality matters for state repression, but that democracy (in the form of Political Rights) does not have a significant effect on state repression.

components, though they each take slightly different approaches to this. Neither approach was theoretical in nature (i.e., there was no attempt to test a disaggregate theory of democratic pacification). Davenport (2007*b*), in the most rigorous and empirically ambitious piece of work on the subject attempted to tease out the differential effects of *voice* (electoral accountability of democratic leaders) and *veto* (institutional constraints on policy choice) on repressive behavior. Here, all models were estimated with only one democratic concept in them. The model statistics do not offer convincing advice on which model is best, so there is no real ability to consider the extent to which different types of democracy are substitutes for each other, or whether they are necessary conditions for each other.

The remainder of this work will be aimed at pushing Davenport's (2007*b*) work forward by estimating measurement models for voice, veto and repression and using a full structural model to predict the extent to which repression responds to democracy.

Chapter 3

Measurement

Measurement is a fundamentally important step in the data analysis process. Measurement is the process whereby numerical values are assigned to observations Stevens (1946, 1959). These numerical values are the ones used to create correlations, cross-tabulations and considerably more complicated estimates of the nature of statistical relationships between variables of interest. The argument that the way numbers are assigned to objects can effect inferences is not a very difficult one to make. I argue that the lack of careful consideration of measurement has had a deleterious effect on the domestic democratic peace research.

As suggested above, there are two concepts of particular interest in this literature – democracy and repression. Of the two, much more attention has been focused on the measurement of democracy. This is not surprising, given that democracy has been and will continue to be a subject of intense scrutiny and intellectual curiosity for the foreseeable future. While in this particular literature, democracy is seen as the main explanatory variable, it also serves this role in many other sub-disciplines (e.g., the inter-state democratic peace, Oneal and Russett (1999)). Democracy also serves as the dependent variable in a number of prominent studies, including Przeworski (1991) and Przeworski et al. (2000). I will detail my measurement strategy for the concepts of interest here below, but first, I offer a brief history of the measurement of democracy and repression.

3.1 Measuring Democracy

Since Plato, scholars have been debating the merits of various regime configurations. It is not surprising that over the past two thousand years, the definition of democracy and its various sub-forms has evolved. These new definitions do not replace old ones, rather they sit alongside those definitions and conceptualizations that have been either forgotten or found wanting on some level. Thus, scholars find themselves facing a long series of “democracy with adjectives,” (Collier and Levitsky 1997). In the interest of parsimony and relevance to the endeavor here, I start the investigation with the early attempts at measuring democracy, as all attempts at measurement should have in mind an operational definition of the concept (obviously, the converse need not hold). Tracing the evolution of the measurements will permit the simultaneous tracing of concepts and definitions. There are a number of ways that this task could be accomplished. For instance, it would be possible to look at the chronological development. I do this to some degree as I start with those engaged in the developments at the mid-Twentieth Century, but will move forward more thematically than chronologically.

I start with what Held (1996) calls “Competitive Elitism,” which was popularized by Weber and Schumpeter. I start here as many of the early efforts at democratic measurement relied on this work for their operational definition of democracy. Specifically, Schumpeter (1962) argues:

the democratic method is that institutional arrangement for arriving at political decisions in which individuals acquire the power to decide by means of a competitive struggle for the people’s vote.

This definition provides only that people are able to compete for votes. Obviously, this definition has been criticized on a number of grounds. Schumpeter wanted to make explicit the disconnect between democratic process and the aims to which democratic

decision-making might be put, for example “social welfare,” or the “common good,” (Held 1996). Weber (1978) suggested that democracy was, in essence, an elected dictatorship. The requirements are similar to those of Schumpeter - elites vie for votes in periodic, competitive elections. Weber saw the right of citizens to dismiss ineffective leaders as the only real right of citizens and as the only piece of political power worth surrendering to them. In the long run, this would lead to administrative and political competence in the highest offices of the government.

There were a number of attempts aimed more or less at measuring this minimal conception of democracy. Among these, the most notable might be Lipset (1959). In general terms, democracy is defined here as free and fair, periodic, competitive elections. This definition suggests certain conditions: “(a) a ‘political formula,’ a system of beliefs, legitimizing the democratic system and specifying the institutions - parties, a free press, and so forth - which are legitimized, *i.e.*, accepted as proper by all; (b) one set of political leaders in office; and (c) one or more sets of leaders, out of office, who act as a legitimate opposition attempting to gain office,” (Lipset 1959, p.72). Harkening back not only to Weber and Schumpeter, but to Madison (1787), Lipset sees democracy as requiring those in power consider and respect the rights of those out of power. The exercise is to categorize countries in either “more,” or “less,” democratic categories.

Cutright (1963), in a rebuttal to Lipset, developed a more sensitive measure of democracy (he called it “Political Development,”) that depended largely on the characteristics of the party system in the legislative and executive governing bodies. He is less careful about the theoretical definition of democracy focusing more on the empirical indicators. He suggests that “a politically developed nation has more complex and specialized national political institutions than a less politically developed nation,” (Cutright 1963, , p. 255), but this is the closest he gets to a theoretical definition. Despite statements to the contrary by Lipset, Cutright observes high correlations be-

tween political development and economic development measures. This is, perhaps, the genesis of the debate between continuous and dichotomous (or more generally, categorical) measures of democracy - something that still remains unresolved today.

There have also been more recent attempts at measurement of this minimalist conception in various forms. Alvarez et al. (1996) propose a binary measure of democracy as, “a regime in which some governmental offices are filled as a consequence of contested elections.” Vreeland (2003) attempted to construct a continuous measure of democracy that directly operationalized Schumpeter’s view of democracy. These measures all depend solely on indicators of competition or contestation and participation.

Dahl (1971) suggests that real democracy requires more than just periodic competitive elections. He states, “a key characteristic of democracy is the continuing responsiveness of the government to the preferences of its citizens, considered as political equals.” The entire complement of First Amendment-type rights should be available to a large number of people (e.g., freedoms of expression, association, press) along with the right to vote, the right for leaders to compete for support and elections that are free and fair. It seems as though Dahl is enumerating the factors required for individuals to have a legitimate vote in competitive elections. A potential divergence with previous scholars in this field is, perhaps, the responsiveness Dahl requires of the government to the will of citizens as political equals. Vanhanen (2000) relies on Dahl and Lipset (1959) for his continuous measure of democracy based on participation and competition (contestation). Though the Freedom House organization does not rely explicitly on Dahl’s operationalization of democracy, it is easy to see this conceptualization in the indicators used to create its political rights indicator (Piano and Puddington 2008), though this organization tends to define democracy more broadly.

An outgrowth of Dahl’s (1971) formulation of democracy is what a number of authors refer to as “liberal democracy.” Liberal democracy, is a Madisonian concept (Madison 1787) wherein government is designed to balance the rights of the few

against the will of the many. Bollen (2009) defines liberal democracy as, “the extent to which a political system allows political liberties and democratic rule.” These systems are characterized by all of the attributes mentioned by Dahl, but explicitly include the rule of law. There is some debate here whether suffrage/enfranchisement (Bollen and Paxton 2000, Paxton 2000) or actual participation Moon et al. (2006) is operative here. As Bollen (2009) suggests, it may be the case that participation, while important to understand the effectiveness of democratic government on some level, is different and distinct from liberal democracy. Liberal democracy has been the subject of the most rigorous measurement to date, thanks to Bollen and colleagues (e.g., Bollen 1980, 1990, 1993, Bollen and Grandjean 1981, Bollen and Paxton 2000)

A related literature has emerged recently considering how rational actors with veto power can shape policy production (Tsebelis 1995, 2002). Systems with democratic institutions may have actors that can constrain the set of decisions and policies over which leaders may choose. This was in some ways operationalized well before the literature came into its own with the executive constraints measure in the Polity dataset Gurr (1974), Gurr, Jagers and Moore (1990), Marshall and Jagers (2001). These data were based on earlier theoretical work by Eckstein and Gurr (1975). Here, the authors cast the relevant political relationship as one between super-ordinates and sub-ordinates. The result is the most commonly used dataset used to describe the nature of democracy in regimes.¹ Others have focused more narrowly on the placement and power of these veto players. Henisz (2000, 2002) generates empirical measures of the extent to which the policy space is constrained by veto players. Beck et al. (2000) also develop a measure of checks and balances in the political system.

There is no doubt that many have presented various strategies for measuring democracy, both as an overarching concept and as a series of quite specific indicators.

¹Recently Treier and Jackman (2008) estimated a rigorous measurement model for the Polity IV data, making them much more defensible.

Below, I draw on this literature to inform my measures of democracy as it pertains to the democracy-repression nexus.

3.2 Measuring State Repression

Human rights violations and state repression are often used interchangeably. Rather more likely is that state repression refers to violations of a subset of human rights. The idea that individuals have a set of rights by nature of existence is not a new idea. In published work, it dates back to philosophers like Locke (2004) who enumerated several rights of individuals including “life, liberty and property.” These natural rights were the basic principles on which the Constitution of the United States was based. The ratification of the Universal Declaration of Human Rights (United Nations 1948) was the first widespread, politically relevant acknowledgement that individuals everywhere are born with a set of rights of which they cannot be deprived legally. These include “life, liberty and security of person,” (Article 3), “equality before the law,” (Article 6) and a series of First Amendment-type rights - freedoms of movement (Article 13), religion (Article 18) and association (Article 20). Also included are the rights to participate in government (Article 21), a reasonable standard of living (Article 25) and the right to an education (Article 26). Few would disagree that this set of rights represents a laudable goal. However, the rights here are so broadly defined as to include what are generally considered social, economic and political outcomes, rather than a set of rights. This makes human rights as defined by this document less useful from an analytical point of view. In fact Goldstein (1986) warns against just this sort of definitional complexity in quantitative studies of human rights.

The empirical study of human rights is not as deep or broad as the empirical study of democracy. It has proceeded in a number of different directions. First, as Landman (2004) notes, there is a distinction between rights in principle and rights in

practice. Research on rights in principle focus on the extent to which countries are legally committed to protecting human rights. This can be done either by considering the extent of protections provided by constitutions (e.g., Foweraker and Landman 2000, Keith 2002, Maarseveen and Tang 1978) or by consider the extent to which countries have ratified international human rights treaties (e.g., Davenport 1996*a*, Hafner-Burton and Tsutsui 2007, Hathaway 2002, Keith 1999, Landman 2005).

Scholars have also employed measures based on rights in practice. These methods of measuring human rights are based on actual practices by the government regardless of the international or domestic legal arrangements that may exist. These are themselves split into three categories - events based, standards based and survey based. Events-based data try to chronicle each event that happened. These are often done for a single country or a small set of countries (Ball 1996, Ball and Davenport 2002) or what Christian Davenport and Allan Stam have done with the GenoDynamics project (<http://www.genodynamics.com>). These efforts provide an incredibly fine-grained look at human rights in a particular place over a relatively short period of time. These data are difficult to collect, so they are generally not available for a world-wide sample over a relatively long period of time. Survey-based methods randomly sample some subset of the population to see either their perceptions or experience of human rights violations.

Standards based measures are those that code qualitative information gleaned either from newspapers or country reports into ordinal scales. These are the most widely used in cross-national studies of human rights violations. Examples of these are the Freedom House Freedom in the World Indicators (Piano and Puddington 2008), the CIRI data (Cingranelli and Richards 2004*b*) and the Political Terror Scale (Carleton and Stohl 1985, Gastil 1980, Gibney 2005, Gibney and Dalton 1996, Mitchell and McCormick 1988, Poe and Tate 1994). The Freedom House measure casts its net quite widely considering characteristics that might be labeled either democracy or human

rights. The Political Rights measure is widely used as a measure of democracy and the Civil Liberties measure is widely used as a measure of human rights compliance. The CIRI data are meant to be a large-scale, publicly available extension of Cingranelli and Richards (1999). This is the only one of the standards-based measures that takes measurement seriously at all. Here, the authors assess the extent to which the data they produce conforms to a cumulative scaling model. Finally, the PTS data (that use information from both the US State Department and Amnesty International) are measured by a five-point ordinal scale. Here, state repression is generally used to refer to the violation of life integrity rights - including torture, killing, forced disappearance and political imprisonment.

Repression and human rights violations have not seen the same amount of debate about rigorous measurement as in the democracy literature. However, there have been a number of studies that attempt to understand the measurement properties and structure of data on human rights violations. Banks (1985) shows that a range of indicators cluster into only a few categories. However, he takes a rather maximalist conceptualization of human rights. Bollen (1986) considers a number of indicators of state repression and suggests that these should form a relative low-dimensional space, though does not go so far as to estimate these models. McCormick and Mitchell (1997) argue that personal integrity is a multi-dimensional concept, though they do so without any explicit measurement model to test the hypothesis. Cingranelli and Richards (1999), in a pre-cursor to their large-scale data collection project and in response to McCormick and Mitchell (1997), showed that the political terror scale items conformed to a unidimensional cumulative scale. I find the unidimensionality persuasive. Given the desire for parsimonious explanations of political phenomena, if one-dimension fits the data nearly as well as two (or more), that privileges the explanation in a single dimension.

As a departure from the convention, Davenport (2007*b*) uses a state repression

variable that combines elements of the Freedom House Civil Liberties indicator (Piano and Puddington 2008) with the Political Terror Scale (Gibney 2005, Gibney and Dalton 1996). This creates a nine-point ordered scale with the ordering as follows (FH/PTS): Low/Low, Med/Low, High/Low, Low/Med, Med/Med, Med/High, Low/High, Med/High, High/High. This is supposed to capture both non-violent and violent state repressive behavior. While Davenport is to be lauded for combining various measures to obtain an operational variable more closely related to his underlying theoretical definition, the properties of this variable from a measurement standpoint were never investigated. He assumes ordinality in a case where it is relatively ambiguous. For example, is the combination medium violence / low restrictions really “worse,” from a human rights stand-point than high restrictions / low violence? It is certainly a reasonable hypothesis, but there is nothing that would logically necessitate that relationship.

As a very simple analysis of this proposition – that the variable created by Davenport is ordered in the manner suggested, I simply found all permutations of the numbers 1-9 which generated 362,880 possible combinations. I winnowed these down to combinations that kept one as the first value and nine as the last value, leaving around 5400 combinations. Then, for each combination I estimated an ordered logit with the categories of the dependent variable ordered as suggested by the permutation with a number of appropriate controls including a dummied-out version of the lagged dependent variable. Of the 5400 models, 65 predicted more observations correctly than the ordering suggested by Davenport. To be fair, the increase in predictive power is marginal even for the best models, resulting in roughly 30 (or about 1%) more observations correctly predicted. While this is not a considerably larger number, it does suggest that other orderings could be at least equally good, if not better.

Another similar test allowed any reordering that kept values in either their original place or either adjacent place. This generated 55 unique combinations. I performed

a similar set of operations as above. Of these 55 orderings, only three produced more correctly predicted observations than the original ordering and only by, at most, four (about a tenth of one percent). This suggests that the orderings that do better than this are significant re-orderings of the original categories. The best of the previous set of models (with 30 more correctly predicted observations) had an ordering of: 1, 2, 5, 6, 8, 3, 4, 7, 9. Using the convention Freedom House low, medium, high / PTS low, medium high (e.g., H/H represents high on freedom house and high on PTS), the new ordering is: L/L, M/L, M/M, H/M, M/H, H/L, L/M, L/H and H/H. There does not seem to be an obvious pattern here, but this is at least suggestive that something else might be going on than originally thought.

3.3 Operationalizing Voice, Veto and State Repression

As suggested above, the democracy-repression nexus proposes a quite specific relationship between democracy and repression. This effectively limits the scope of democracy. While a number of the projects discussed above propose measures of democracy that are intellectually interesting, none, on its own, provides exactly what is needed. Rather than implying a deficiency with any particular measure, though, I am simply suggesting that it is possible to get “better,” estimates using more data. That is to say, for example, using different measures of veto players can lead to a better measure of the underlying concept of constrained executives than any individual measure. I employ a Bayesian measurement model to aggregate the data in a rigorous fashion, which will permit the calculation not only of all model parameters, but also the standard errors of model parameters including the latent variable scores (Congdon 2003, Lee 2007, Treier and Jackman 2008). Below, I discuss the operationalization and measurement of voice, veto and repression.

3.3.1 Voice

Davenport (2007*b*) uses two indicators of voice. The first is the measure of suffrage developed by Bollen and Paxton (2000). This simply records the proportion of the adult population with the right to vote. He also uses the modification to the Vanhanen (2000) polyarchy measure discussed by Gates et al. (2006), which I also use below. Rather than combining these two measures, Davenport estimates different models. Any difference in the results will presumably not be owing to a difference in the country's *true* voice, rather it will be due to a difference in the measurement strategies for these particular variables and measurement error. The measure I present below mitigates the effect of measurement error.

I use five indicators to operationalize voice. The first two come from the Polity IV project (Marshall and Jaggers 2001) - competitiveness of executive recruitment (`xrcomp`) and the competitiveness of political participation (`parcomp`). Competitiveness of executive recruitment is a three-point ordinal scale that ranges from hereditary or similar selection (1) to election (3). Competitiveness of political participation is a five-point scale, ranging from repressed competition (1) to competitive (5). I also use the democracy measure offered by Alvarez et al. (1996), which is a binary measure coded 1 if legislative and executive offices are filled via election, there is more than one party and there has been at least some history of the alternation of power among parties. Also included is the dichotomous measure developed by Bernhard, Nordstrom and Reenock (2001) which involved an original coding effort to operationalize the definition of polyarchy set out by Dahl (1971). Finally, I use the data developed by Vanhanen (2000) and modified by Gates et al. (2006). This measure is multiplicative in competition (the percentage of legislative seats held by all but the largest party) and participation (the percentage of adults who voted). However, countries with competition scores less than 30 automatically score zero on the composite indicator.

This indicator is continuous.²

It is not especially difficult to see how each of these variables operationalizes, at least a part of, the underlying idea of voice. They all focus to some extent on competition and participation - the two necessary conditions for citizens to have a voice.

3.3.2 Veto

The theoretical idea behind the “veto,” argument is, again, relatively straightforward. Tsebelis (2002), suggest that there are a number of political actors with the ability to veto legislation (either beforehand in the case of the legislature) or after the fact (in the case of an independent judiciary). The preferences of these relevant actors and the rules governing the votes necessary for legislation to pass are sufficient to partition the policy space into a set of policies preferable to the status quo and the set of policies not preferred to the status quo. It is only policies in the former set that would pass. Applying this to the democracy-repression nexus, Davenport (2007b) suggests that changes in repressive policy would have to be preferred to the status

²The original Vanhanen indicator was simply multiplicative in competition and participation. I use a version (G) that is multiplicative in the proportions as follows, where c is the proportion of seats won by all but the largest party and p is the proportion of adults voting in the population:

$$G = c \times I_{30}(c) \times p \tag{3.1}$$

Here, $I_{30}(c)$ is an indicator function that takes value one if $c > 0.3$ and zero otherwise. Since the resulting measure is a product of proportions, it will also have the range $[0,1]$, though the observed upper range will generally be much less than one. I use the logit transform to put the variable G theoretically on the whole real number line:

$$lG = \log \left(\frac{G + .01}{1 - (G + .01)} \right) \tag{3.2}$$

It is the lG measure I use in the measurement model.

quo by a number of relevant actors. Thus, chief executives would be generally unable to make unilateral changes in repressive policy. Further, any changes that are made would necessarily be relatively small, therefore it would be difficult for democracies to choose harshly repressive strategies.

There are a number of datasets that contain some measure of veto players. I will look at three specific datasets here. First, is Polity IV's executive constraints (XCONST) measure (Eckstein and Gurr 1975, Gurr, Jagers and Moore 1990, Marshall and Jagers 2001). This measure does not explicitly measure veto players, it measures constraints on executive authority. Specifically, executive constraints, "refers to the extent of institutionalized constraints on the decision-making power of the chief executives, whether individual or collective." Marshall and Jagers go on to say, "[t]he concern is therefore with the checks and balances between various parts of the decision-making process," (Marshall and Jagers 2001, 23). The scale ranges from one to seven; brief definitions for each category are listed in Table 3.1. There is a considerable amount of ambiguity here, in that there are nearly as many ambiguously defined "intermediate categories," as there are substantively defined categories. The executive constraints variable strays a bit from the more traditional veto-players variables as it is possible that there are powerful groups in one-party states that count as constraints that may not count as veto players, *per se*.

Another measure of veto players is that included in the Database of Political Institutions (DPI) (Keefer 2002). This measure increases with constraints on the executive. Specifically, one point is added for a chief executive, another point is added if that chief executive is competitively elected. Another point is added if the opposition controls the legislature. Further, checks are added for legislative diversity, generally if the chief executive is not thought to be in full and complete control of his party and his party is a single-party governor. The range of the checks variable is one to 18, with the bulk of the data in single-digits. The mean is 2.5 and the median is 2.

Table 3.1: Categories of Executive Constraints

Category	Definition
1	Unlimited Authority
2	Intermediate Category
3	Slight to Moderate Limitations
4	Intermediate Category
5	Substantial Limitations
6	Intermediate Category
7	Executive Parity or Subordination (to legislature)

As Keefer and Stasavage (2002) suggests, the checks measure in the DPI was designed to capture both what Tsebelis (2002) called “partisan,” and “constitutional,” veto players. For the countries with multiparty elections, the mean is 4.6 and the median is 4. The range, however, remains about the same, one to 16.

Another set of measures of checks and balances include those developed by Henisz (2009). The first is political constraints III (POLCONIII). This variable, “estimates the feasibility of policy change (the extent to which a change in the preferences of any one actor may lead to a change in government policy),” (Henisz 2002, 363). The feasibility of policy change is measured by the alignment of branches of government with veto power. The feasibility of policy change is greater in countries where the interests and preferences of the legislative house(s) and the chief executive are aligned. The author suggests that these capture a similar underlying process to those of Tsebelis (1995) and presumably to Tsebelis (2002). The logic of political constraints V (POLCONV) is similar to the of POLCONIII, but includes two other veto players - an independent judiciary and sub-federal units, as suggested in Henisz (2000).

The models here require that the variables be conditionally independent from

each other. This means that once the latent variable is accounted for, the remaining measurement error is assumed to be *i.i.d.* A necessary, though not sufficient, condition for this to be true is that the variables are not related by construction. Thus, I only use a subset of the variables mentioned above. Specifically, I use the `polconiii` variable from Henisz (2002) rather than `polconv` because the latter uses the Polity IV executive constraints measure in its construction (as a proxy for judicial independence, actually). I also use the other variables, unrelated to Polity IV, that operationalize additional veto points that were included in `polconv` - namely the Law and Order variable from the The PRS Group (2009) and the sub-federal veto players identified in the newest version of the POLCON database. I also use Polity IV's `xconst` variable as well as the log of the `checks` measure from Keefer (2002).

With the possible exception of Polity's executive constraints variable, all of these explicitly try to measure the feasibility of policy change as envisioned by Tsebelis (2002). These provide relatively similar information. The smallest correlations are between the checks variable and the two POLCON variables at around 0.63 each. The highest correlations are among the two POLCON variables at around 0.92. The Polity IV executive constraints measure is correlated at around 0.7 with the other variables.

3.3.3 Repression

The literature has been basically unanimous in its use of physical integrity rights as the dependent variable in models of the democracy-repression nexus. As Carleton and Stohl (1985) note - these rights were explicitly singled out as important by the US Congress in the International Development and Food Assistance Act of 1975 and the International Security Assistance and Arms Export Control Act of 1976. The argument here is that repression cannot cause a set of institutional arrangements. I also use physical integrity rights by using the data developed by Cingranelli and

Richards (2004a). These data are a publicly available extension of the earlier work done by Cingranelli and Richards (1999). Here, the authors found that the four aspects of physical integrity rights were well-described by a unidimensional cumulative scale. I use a parametric version of that model here.

Each aspect of physical integrity rights - torture, political imprisonment, forced disappearances and extrajudicial killing, is measured by one variable. Each of the four variables ranges on a three point scale: no incidents (0), 1-49 incidents (1), ≥ 50 incidents (2). These directly operationalize the four aspects commonly mentioned in the literature and measured by the more commonly used Political Terror Scale (PTS) (Gibney and Dalton 1996).³

3.4 Models and Results

The empirical modeling proceeds in two parts. First, I estimate the measurement models with variables described above. Then, I use these in a predictive model to re-estimate the model suggested by Davenport and Armstrong (2004).⁴

3.4.1 Measurement Models

Each measurement model will take roughly the same form, so I develop the model in its most general form here and apply it to each set of indicators mentioned above.

³Since the model assumes conditional independence of the indicators given the latent variable, I choose not to use the PTS codings of Amnesty International and State Department country reports. The reason is because the CIRI variables use the same source material so conditional independence is unlikely. While the aims of the two indicators are not perfectly overlapping (the PTS also codes some violence by non-state actors), they are sufficiently close to warrant not using the PTS variables.

⁴While this model is not the most recent one, it is the most recent one focusing on this core idea using the dependent variable of interest. Further, these authors present an interesting modification to the traditional understanding of the effect of democracy.

Here, let \mathbf{Y} refer to the $N \times k$ matrix of observed variables (think of this as the four repression variables if you like). The underlying measurement model, then suggests:

$$\mathbf{Y} = f(\text{latent}) \quad (3.3)$$

Thus, the idea is not especially difficult to grasp. However, the particulars get a bit more complicated. First, the nature of $f(\cdot)$ changes with the observed variables. Continuous variables will be a linear function of the latent variable, binary variables will be a logistic function of the latent variable and ordinal variables will be an ordered logistic function of the latent variable.⁵ All three types of data exist in the models mentioned here. Further there are two different measurement models - one where the voice and veto indicators are separately predicted by their own latent variables and one where both sets of indicators are predicted by a single democracy latent variable.

The latent variables all have the same structure. In the first period (i.e., the first year the independent state appears in the dataset), the country's latent variable score (θ_{ijt}) is drawn from a normal distribution with mean zero and precision τ_{j1} , where i indexes country, t indexes time and j refers to the latent variables; $j = \{\text{veto, voice, democracy, repression}\}$. In the subsequent periods, autoregressive

⁵For continuous indicators,

$$Y_i^{\text{continuous}} = \lambda_0 + \lambda_1 \text{Latent}_i + e_i \quad (3.4)$$

for binary indicators,

$$Pr(Y_i^{\text{binary}} = 1) = \frac{1}{1 + \exp(-(\lambda_0 + \lambda_1 \text{Latent}_i))} \quad (3.5)$$

and for ordinal indicators,

$$Pr(Y_i^{\text{ordinal}} = m) = \frac{1}{1 + \exp(-(\kappa_m - \lambda_1 \text{Latent}_i))} - \frac{1}{1 + \exp(-(\kappa_{m-1} - \lambda_1 \text{Latent}_i))} \quad (3.6)$$

priors are used such that the country's latent variable score is drawn from a normal distribution with mean $\mu_{ijt} = \rho_j \theta_{ijt-1}$ and precision τ_{j2} . The precisions are all given Gamma(1,1) priors and the various autoregressive parameters are given uniform priors over the range [-1,1].

In the measurement model, the coefficients relating the latent variable to the observed variables (i.e., the λ 's) are given normal priors with mean zero and unit variance and are constrained to be positive, to prevent an equally good mirror image solution. To set the scale of the latent variable, the coefficient on the first variable is set to 1 and its corresponding intercept is set to zero. It will be obvious from the results which coefficients are fixed for identification purposes. In the ordered logit models, the threshold parameters (κ 's) are drawn from truncated normal distributions in such a way as to preserve the appropriate ordering. For example, for a variable with four ordered categories, κ_0 is set to $-\infty$ and κ_4 is set to ∞ . κ_1 is drawn from a distribution that has an upper bound of κ_2 . κ_2 is drawn from a distribution that has a lower bound of κ_1 and an upper bound of κ_3 and so on. In the repression model, the first set of cutpoints also has to be set and they are set in such a way that when the latent variable is zero, the marginal distribution of the first indicator is produced.

Democracy and Dimensionality

To address the dimensionality of democracy, I estimate two different measurement models. One where the voice and veto indicators are predicted by their own latent variables (of voice and veto, respectively). I also estimate a measurement model that treats both the voice and veto indicators as measures of a single democratic variable. This is essentially a test of dimensionality of the democratic concept. The details of the measurement models remain the same, so I do not discuss them in detail again. The parameter estimates should be sufficient to permit the evaluation of the models and I discuss the results with respect to the different dimensionality below.

At this point, it is worth making a small digression about dimensionality. Despite a firm grounding in measurement theory (e.g., the collected works of Bollen and colleagues cited throughout), it seems there is considerable resistance to this approach. The resistance comes from the laudable goal of estimating the effect of specific aspects of democracy (Bueno de Mesquita et al. 2005, Keech N.d.).⁶ These two ideas are seen as inextricably linked by the proponents of “causal specificity.” For this group of individuals the benefits of adopting this strategy outweigh the costs – modeling measurement error as potentially interesting. The argument that assessing causal mechanisms can be done *only*, or at least most profitably, by modeling the response as a function of individual variables suffers from two flaws that are not often discussed.

First, the idea that single variables are better than the products of measurement models assumes that the concept of interest is perfectly measurable with a single variable. Further, it assumes that the variable employed in the model is this perfectly measured realization of the phenomenon of interest. I think this assumption is generally untenable. Proponents of the causal specificity point of view, perhaps do not understand the causal mechanisms could be tested with measurement models estimated on quite specific domains (like voice and veto in the specifications suggested below). If voice really is a complex of issues related to the ability of individuals to periodically choose their leaders, then, there are a number of variables that might be related to those concepts. Not using a measurement model would require the inclusion of all of these to “test the mechanism.”

This brings me to the second main flaw. This need not necessarily be a flaw, but generally arises in practice. This is the fact that these various measures of the same underlying concept are very closely related. Let’s suppose, for the sake of argument that three variables x_1 , x_2 and x_3 are all measures of the underlying concept ξ and that they are relatively highly correlated, as measures of the same concept often are.

⁶I have even found myself (Armstrong and Davenport 2003) on this side of the debate.

You might think of these as the measures of voice and veto from above. When estimating $y = \beta_0 + \beta_1x_1 + \beta_2x_2 + \beta_3x_3 + \gamma\mathbf{Z}$, where \mathbf{Z} is a set of other controls, the eye is drawn to the marginal effect of each of the x variables and often inferences are made about what happens when x_1 , for instance, is moved holding the other variables, including the other x variables, constant. The problem is that x_1 probably only varies over a relatively small range when x_2 and x_3 are held constant at some value. To put this in context, consider competitiveness of participation (PARCOMP), executive constraints (XCONST) and competitiveness of executive recruitment (XRCOMP) from the Polity IV data. Bueno de Mesquita et al. (2005) would suggest that to understand how these variables influence repression, we simply look at the marginal effects in the model. Would we expect the effect of slightly more competition in executive recruitment to matter when there are no executive constraints and no competition of participation? Perhaps not. The interrelations of these variables suggest that multiplicative effects might be more appropriate than additive effects. Once this is done, the simple picture envisioned by the causal specificity folks is a distant memory. Further, when considering data density, the conditional predictions become even more difficult to calculate and interpret because valid inferences are only made over relatively narrow ranges of the conditioning variables.

All of this is not to say that we should not be considering causal mechanisms and the effect of specific concepts on our response variables, rather it is to say that there is a better way. Using measurement models does not preclude consideration of the original indicators, quite the contrary. Consider the veto latent variable for example and its indicators checks, sub-federal veto and executive constraints. These variables are related to the latent variable in the following ways:

$$\log(\text{checks}) = \lambda_{01} + \lambda_{11}\text{Veto} + \varepsilon_1 \quad (3.7)$$

$$\text{XCONST} = \lambda_{02} + \lambda_{12}\text{Veto} + \varepsilon_2 \quad (3.8)$$

$$\log\left(\frac{\Pr(\text{Sub-Fed} = 1)}{1 - \Pr(\text{Sub-Fed} = 1)}\right) = \lambda_{03} + \lambda_{13}\text{Veto} \quad (3.9)$$

This means that for any given value of *Veto*, it is possible to get the most likely value (i.e., the predicted value) of either checks, executive constraints or the probability of sub-federal veto existing. For example, a change in the veto latent variable from 1 to 2 would generate the following expected changes in the indicators:

$$\begin{aligned} \text{checks} &: e^{\lambda_{01} + \lambda_{11} \times 2} - e^{\lambda_{01} + \lambda_{11}} \\ \text{xconst} &: \lambda_{12} \\ \Pr(\text{Sub-Fed} = 1) &: \frac{1}{1 + e^{-(\lambda_{03} + \lambda_{13} \times 2)}} - \frac{1}{1 + e^{-(\lambda_{03} + \lambda_{13})}} \end{aligned}$$

I demonstrate this below on the predictive models.

3.4.2 Measurement Model Results

The results of the measurement model are presented in Table 3.2. In all of these models, the coefficients are significantly different from zero, suggesting that the latent variable is at least a moderately good predictor of the observed variables. At first glance, the coefficients, especially for the democracy model versus voice and veto look quite different. However, this is reasonable because the variance of these variables is quite different. You can see, the coefficients on the veto variable are bigger than they are on the democracy variable by a factor of about 6, which is roughly the ratio of the variances of the democracy latent variable to the veto latent variable, so this seems quite reasonable. The posterior means of voice and veto correlate at roughly 0.9, so there is definitely a strong relationship there. However, they are not perfectly

related. The posterior means of the democracy latent variable is correlated with voice at 0.98 and veto at 0.94, so there is clearly some strong inter-dependence here.

Despite the quite high inter-correlations here, the DIC for the overall structural models is unambiguous. DIC, in a similar fashion to AIC, is an information theoretic measure that penalizes model fit for model complexity. As Spiegelhalter et al. (2002, 584) suggest of more conventional penalized fit measures, “... in complex hierarchical models parameters may outnumber observations and these methods [referring to AIC and BIC] clearly cannot be directly applied.” The DIC clearly favors the less parsimonious model here, suggesting that the added complexity of estimating another latent variable is compensated by increased model fit. This suggests that the two-dimensional conceptualization of democracy is superior to the unidimensional conception of democracy.

3.4.3 The Predictive Models

As suggested above, I use the measurement model results (i.e., the posterior estimates of the latent variable scores for each country-year) to estimate predictive models for repression based both on voice and veto and on the overall democracy measure. While I spend the remainder of the manuscript moving beyond these particular model specifications, I find this useful to provide a benchmark for these results. As you will see below, the variables employed here behave as expected given previous empirical work. Toward this end, I estimate two empirical models. The first keeps voice and veto as distinct concepts:

$$\begin{aligned} \text{Integrity Rights} = & \beta_0 + \beta_1 \text{Integrity Rights}_{t-1} + \beta_2 \text{Voice} + \beta_3 \text{Veto} & (3.10) \\ & + \beta_4 \text{voice} \times \text{veto} + \gamma \mathbf{X} + e \end{aligned}$$

where \mathbf{X} refers to a set of control variables that will be discussed in greater detail

below. I also estimate another model that treats both voice and veto indicators as being caused by the same underlying latent factor, called democracy:

$$\begin{aligned} \text{Integrity Rights} = & \beta_0^* + \beta_1^* \text{Integrity Rights}_{t-1} + \beta_2^* \text{Democracy} & (3.11) \\ & + \beta_3^* \text{Democracy}^2 + \beta_4^* \text{Democracy}^3 \gamma^* \mathbf{X} + e \end{aligned}$$

Both model specifications are meant to operationalize the idea discussed in Davenport and Armstrong (2004). This is the idea that increases in democracy will only effect changes in repression when there is already a modicum of institutionalized democracy in place. That is to say as a full autocracy becoming slightly less autocratic (e.g., through sham elections or token political opposition) this should do very little to change a state's repressiveness.

The control variables used here in \mathbf{X} are GDP/capita and the natural log of population, both coming from Gleditsch (2002). Also included are dichotomous measures of interstate and civil war from Gleditsch et al. (2002).

Now, I investigate how these two different measurement models interact with the predictive statistical models mentioned in equations 3.10 and 3.11. Table 3.3 presents the coefficients from the predictive part of the model. All of the variables are significant here, save interstate war which is equally insignificant in both models. The coefficient for voice is not statistically significant, but due to the interaction, this is only the coefficient for voice when veto is equal to zero. As I show below, there is a big range of veto for which the conditional coefficients for voice are statistically significant. The results here are consistent with previous results in the literature (e.g., Davenport and Armstrong 2004, Poe and Tate 1994).

I started the investigation by suggesting that the models here would bring new insight into how interdependent pieces of democracy work together to lower a country's expected level of integrity rights violations. As has been well established, with

interaction terms, especially interactions between continuous variables, the conditional coefficients should be plotted rather than trying to interpret the coefficients in the model directly (Brambor, Clark and Golder 2006, Braumoeller 2004, Kam and Franzese 2007). Panels (a) and (b) of Figure 3.1 show the conditional coefficients and 95% credible intervals for voice and veto, respectively. As you can see here, the conditional effects of both variables are only significantly different from zero when the other takes on higher values. This provides a more nuanced picture of what Davenport and Armstrong (2004) suggest. Namely, that a number of aspects of democracy must be in place before further democratization has any significant marginal effect.

It is possible, using the parameters of the measurement model, to recover the values of the observed variables that correspond to the point where at which the marginal effect becomes significant. For voice, in panel (a), the critical value of veto is around 0.16. For values of veto bigger than 0.16, the effect of voice is negative and statistically significant. A value of 0.16 on veto, corresponds to values on the observed veto indicators approximately equal to the values listed in column 1 of Table 3.4, in the rows labeled “voice.” That is to say, when veto equals 0.16, we would expect checks to be around 1 (the lowest value on this variable, but one occupied by about 45% of the observations). Political constraints (`polconiii`) would be around 0.123, or roughly at its 45th percentile.⁷ This can be interpreted to mean that the effect of voice is not significant until political constraints are greater than those in roughly 45% of the country-years in the dataset. When veto = 0.16, the model would predict executive constraints at 3, meaning, “slight to moderate limitation on executive authority,” (Marshall and Jaggers 2001, 23).⁸ A value of 0.16 on veto corresponds to a value of 3

⁷Examples of countries with values around 0.123 on the political constraints variable are Brazil throughout the 1990’s, Poland (1991-1993) and Botswana in the early 1980’s and early 1990’s.

⁸Examples of countries with executive constraints equal to 3 are China throughout the 1980’s, Kenya throughout the 1980’s and 1990’s, and South Korea in the early 1980’s.

on the law and order variable. This would be a country with either nonzero, but still incomplete, impartiality of the legal system or nonzero, but still incomplete, popular respect for the law.⁹

The other half of column 1 (Table 3.4) refers to the predicted value of the voice indicators for the mean value of voice (roughly - 2.5) when veto is at least 0.16.¹⁰ The values in the “voice,” portion of column 1 are the predicted values of the voice indicators when the voice latent variable is - 2.5. The top three indicators are essentially probabilities whose range is [0,1]. The range of competitiveness of executive recruitment is [1,4] and for competitiveness of participation it is [1,5]. This suggests that movement from Gates values of [0,0.074] should not result in significant changes in repression. The others can be read in a similar fashion. Countries whose human rights could be expected to benefit immediately from even small amounts of democratization on the voice dimension are those with veto values bigger than 0.16 and with voice values bigger than -2.5. The countries that fit this bill (with $-2.5 < \text{voice} < -2$), are Kenya (1993-1995), South Africa (1982-1986) and Zimbabwe (1987-1988).

The second column of Table 3.4 can be read the same way, though the interpretation is flipped. Here, those values in the “voice,” section of column 2 refer to the values of the voice indicators above which a change in veto will be statistically significant. The values of veto in column 2 represent the values of the veto indicators at the expected value of veto at the critical value of voice (i.e., the value of voice at which the veto conditional coefficients turn negative and significant). There are similarly a number of countries poised at the edge of productive democratization on the veto

⁹Examples of countries in this situation with respect to law and order are Argentina in the late 1980's and early 1990's, Mexico in the late 1980's through the late 1990's and Turkey in the mid-late 1980's.

¹⁰To find this, I simply ran an OLS regression of the posterior means of voice on the posterior means of veto and generated the prediction of voice when veto = 0.16. The relationship between the two variables is sufficiently linear to make this a reasonable strategy.

dimension. These are countries whose values on voice are sufficiently high that the veto coefficient is significant and negative *and* have values on veto that are at the expected value for the critical value of voice (- 1.38). A few examples are Albania in the early 1990's, Georgia (1991-1999) and Honduras through the late 1980's and early 1990's.

Panel (c) of Figure 3.1 shows the interactive surface ($\beta_2\text{Voice} + \beta_3\text{Veto} + \beta_4\text{Voice} \times \text{Veto}$). The lightest color represents the entire surface. The next darkest shade represents the areas where 1) the predicted surface is significantly different from zero and 2) the density evaluated at the center corner of the lower-right grid cell is in the highest 75% of all of the densities at the lower-left corners of all the grid cells. The darkest region is similar to the above, except only the highest 50% of the densities are used. The biggest effects can be seen when both veto and voice take on high values. There is no significant effect when either veto or voice takes on low values. This is completely reasonable given that there are very few data points in the asymmetric extremes (i.e., high voice-low veto or high veto-low voice).

It is also possible to find the countries here that are poised to start to feel the effects of democratic pacification. These are countries whose voice and veto values are both less than their respective 65th percentile values, but whose values are sufficiently high that they are at least in the medium-gray shaded area in panel (c) of Figure 3.1. A few examples are Albania in the late 1990's, Georgia in the late 1990's and Honduras in the late 1980's and late 1990's.

Column 2 of Table 3.3 shows the effects when we consider democracy as one single variable rather than two. Its effect is modeled with a third-degree polynomial to allow for effects similar to those found by Davenport and Armstrong (2004). The results are roughly similar, though they depart on one important point. Figure 3.2 shows the conditional effect of democracy on repression. Here, there is some modest level of democratic pacification gained as the most autocratic countries begin to democratize.

Then, there is a significant plateau at which point no gains are felt to democratization. Then, as countries reach roughly the upper quarter of the democracy scores, democratization starts to bear fruit again with respect to integrity rights. There are a number of countries roughly on the verge of democratic pacification here as well. However, they are a set of countries quite different from the ones mentioned above. Of the 147 country-years found to be poised on the brink of democratic pacification above when considering voice and veto, none of them reach the critical value of democracy required by model 2. All of the countries found to be poised on the brink of pacification here (e.g., Argentina (1984-1989), India (1984-1991) and South Korea (1989-1995)) have either higher voice or veto scores than those countries found in the previous discussion.

Finally, what of Davenport’s (2007*b*, 179) claim that, “... results consistently reveal that *Voice* ... exceeds the influence of *Veto*.” Because the effects are conditional here, it is a more difficult task to assess this claim than it might seem. To get a sense of what this might look like, I did the following. I will discuss this in terms of the overall effect of veto, but I also performed the mirror image of this process for voice. First, make a sequence of 100 evenly spaced points from the minimum to the maximum of voice, call this $voice_0$. Then, for each point in $voice_0$, I found the nearest 50 posterior means of the voice latent variable.¹¹ I then calculated the standard deviation of *veto* for each of these 50 points. I repeat this for each of the points in turn, which results in a column vector of 100 standard deviations, call it σ^{veto} . Then, I took the last 5000 chain values for the predictive model coefficients β_3 and β_4 from equation 3.10, call this β_0^{veto} , which is a 5000×2 matrix. Next, I made the matrix:

¹¹The basic result holds regardless of the number of points chosen.

$$x_0 = \begin{bmatrix} 1 & \text{voice}_{0,1} \\ 1 & \text{voice}_{0,2} \\ \vdots & \\ 1 & \text{voice}_{0,100} \end{bmatrix}$$

Then, calculate the total effect as $\theta_{\text{veto}} = (\beta_0^{\text{veto}} x_0') \sigma^{\text{veto}}$, which is now a 5000×1 vector of effects, the quantiles of which can be taken to form a 95% credible interval. The idea here is to acknowledge that in places where the conditional coefficients are extreme (e.g., in the extreme values of voice for the conditional effect of veto and in the extreme values of veto, for the conditional effect of voice), there may be very little data or the data that is there might have very low variance. This is an attempt to account for the various amounts of variance at the different levels of the conditioning variable. This was done for both variables with the following results - the total effect of veto is -2.56 with a 95% interval of (-4.87, -0.45) and the total effect of voice is -4.06 with a 95% interval of (-6.76, -1.62). Davenport's claim is then borne out, though with the caveat that the difference in the two total effects calculated this way is not statistically different from zero.

The results above have important implications on both substantive and methodological grounds. From a methodological point of view, this research is a rather large step forward for the democracy-repression nexus. Until now, researchers have not been taking full advantage of the wealth of data that exists on the various aspects of democracy. By pooling these measures together, it allows the strengths of each to shine through while eliminating or at least mitigating the measurement errors. I demonstrated above that even though one measure for voice and one measure for veto are obtained, it is possible to look back at the values of the indicators that would be expected by certain values of the measurement model. Often when dimension reduction techniques are used, we abstract from the underlying substance that generated

the latent variable. This practice is clearly to our detriment. It is not necessary that the specifics of the substantive indicators completely give way to a more abstract, conceptual definition. Both the abstract and the concrete are useful here.

Substantively, this is also an interesting step forward. Though others have considered this type of strategy in a less sophisticated way (e.g., Bueno de Mesquita et al. 2005, Davenport 1997, 2007*b*, Davenport and Armstrong 2004), they have often done so without investigating or even acknowledging the inter-relatedness of these various aspects of democracy. This work shows that these two concepts are not independent of each other. It does not make sense to think inferentially about a country with low voice and high veto because such countries are very rare if not completely unobserved. When considering which countries are most likely to benefit from democratization either on voice, veto or both, it is important to consider which countries have values likely to be in the range where democratization can help.

The results above suggest that the operationalization of democracy does matter. Even though the substantive effects are quite similar (i.e., that democratic pacification happens only when multiple aspects of democracy already exist), the implications for which countries are most likely to benefit from democratization is quite different. The threshold when considering both voice and veto seems considerably lower than the threshold when considering a single democracy variable. Below, I try to pull apart these effects by considering more carefully how the relationships should exist and how the models should be constructed.

Table 3.2: Measurement Model Results

	Model 1			Model 2	
	Voice	Veto	Repression	Democracy	Repression
log(Gates)	1.000 NA			1.000 NA	
Alvarez et al	2.754 (2.418, 3.144)			2.239 (2.021, 2.480)	
Bernhard et al	1.252 (1.135, 1.378)			1.169 (1.068, 1.274)	
Competitiveness of Exec. Recruitment	1.503 (1.405, 1.600)			1.788 (1.677, 1.903)	
Competitiveness of Participation	2.184 (2.046, 2.339)			1.927 (1.819, 2.040)	
log(Checks)		1.000 NA		0.249 (0.239, 0.260)	
Political Constraints (III)		0.506 (0.466, 0.547)		0.083 (0.081, 0.085)	
Executive Constraints		6.637 (6.142, 7.131)		0.959 (0.938, 0.981)	
Law & Order		2.186 (1.982, 2.398)		0.357 (0.337, 0.379)	
Sub-Federal Veto		3.181 (2.618, 3.794)		0.559 (0.463, 0.659)	
Torture			1.000 NA		1.000 NA
Extrajudicial Killing			1.885 (1.626, 2.186)		1.896 (1.698, 2.114)
Forced Disappearance			1.587 (1.357, 1.856)		1.663 (1.473, 1.877)
Political Imprisonment			1.330 (1.140, 1.545)		1.331 (1.194, 1.484)
SD(Posterior Means)	2.225	0.346	1.718	2.215	1.758
DIC		47285.9		48108	

Note: Main entries are the coefficients on the latent variables (think factor pattern coefficients or “loadings”). Below are 95% credible intervals. The intercepts and threshold parameters are omitted in the interest of space, but are available from the author upon request.

Table 3.3: Predictive Model Results

	Model 1	Model 2
Intercept	-0.429 (-0.654, -0.223)	-0.396 (-0.565, -0.243)
Repression _{t-1}	0.836 (0.802, 0.867)	0.865 (0.837, 0.891)
Voice	-0.015 (-0.047, 0.016)	
Veto	-0.306 (-0.522, -0.102)	
Voice×Veto	-0.087 (-0.144, -0.035)	
Democracy		-0.075 (-0.106, -0.046)
Democracy ²		-0.046 (-0.065, -0.027)
Democracy ³		-0.008 (-0.012, -0.004)
GDP/capita (in \$10,000)	-0.116 (-0.177, -0.059)	-0.055 (-0.102, -0.008)
log(Population) (in 1,000)	0.053 (0.033, 0.075)	0.042 (0.026, 0.059)
Civil War	0.385 (0.219, 0.572)	0.269 (0.127, 0.416)
Interstate War	0.086 (-0.065, 0.237)	0.086 (-0.04, 0.215)

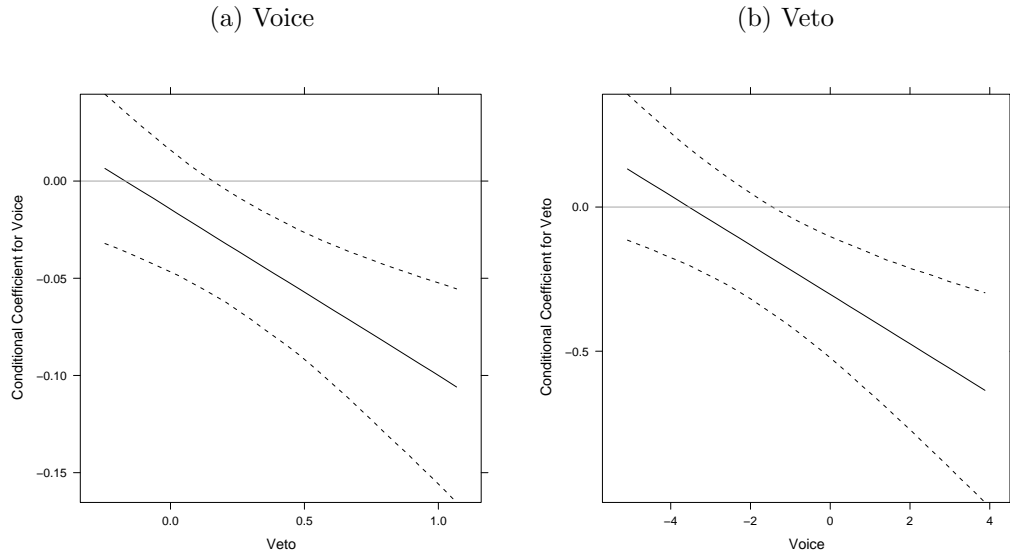
Note: Main entries are posterior means of the regression coefficients in the predictive piece of the structural model. The entries below are 95% credible intervals.

Table 3.4: Substantive Thresholds

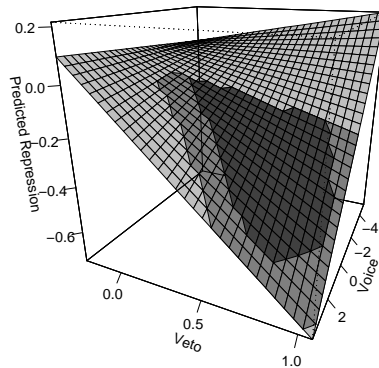
		Voice	Veto
Voice	Gates	0.074	0.202
	Pr(Alvarez et al = 1)	0.015	0.261
	Pr(Bernhard et al = 1)	0.032	0.122
	Competitiveness of Exec. Recruitment	2	3
	Competitiveness of Participation	2	3
Veto	Checks	1	1
	Political Constraints (III)	0.123	0.218
	Executive Constraints	3	4
	Law & Order	3	4
	Pr(Sub-Federal Veto = 1)	0.025	0.044

Note: Entries in this table represent the values of the observed variables at the thresholds of democratic pacification. These represent the expected values of the observed values at the point where the conditional coefficient for voice (first column) and veto (second column) become statistically significant.

Figure 3.1: Conditional Effects of Voice and Veto

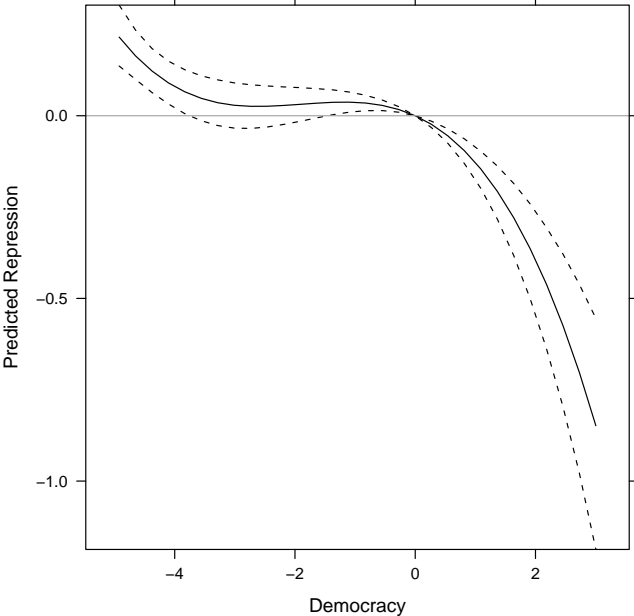


(c) Predicted Surface



Note: Panels (a) and (b) show the conditional coefficients and 95% credible intervals for voice and veto, respectively. Panel (c) shows the interactive surface ($\beta_2 \text{Voice} + \beta_3 \text{Veto} + \beta_4 \text{Voice} \times \text{Veto}$). The lightest color represents the entire surface. The next darkest shade represents the areas where 1) the predicted surface is significantly different from zero and 2) the density evaluated at the center corner of the lower-right grid cell is in the highest 75% of all of the densities at the lower-left corners of all the grid cells. The darkest region is similar to the above, except only the highest 50% of the densities are used.

Figure 3.2: Predictions as a function of Democracy



Chapter 4

Voice

It is suggested that polities where citizens have the right to sanction government leaders will see lower levels of state repression. The argument hinges on the accountability of government officials to voters. Assume that citizens have single-peaked preferences along the repression dimension and that they vote based on their preferences. If this is so, the leader's repressive policies will have to be such that a plurality of the citizens are closer to this policy choice than the policies offered by any of the leader's competitors in the election. If this is not the case, then the leader will be voted out of office. Otherwise, the leader will retain his position. The logic here is the same as for any unidimensional spatial voting model [e.g., Downs (1957)].

Davenport (2007*b*) suggests that for citizens to exercise their voice they must have the ability to participate in the political process and they must be able to choose among a set of diverse alternatives. The logic of the first proposition is straightforward - when citizens are able to participate in the political process, leaders are accountable for their decisions. As for the second case, the more distinct viewpoints are represented the less likely it is that even a smaller minority can be repressed without generating some social and political backlash. The logic for the second proposition seems a bit shakier than the first.

When are citizens able to sanction the chief executive directly? Only in presidential systems are citizens able to vote up or down on the one person who will lead the

government. The connection between votes and leadership of the government is a bit more tenuous in parliamentary systems, though often the choices are obvious. Often the only real difference is the inability to vote independently for a local representative and the national leader. However, if the vote is simply to send a message to the ruling party, then there is often a clear ability to do just that.

Work designed to “test,” this mechanism has been, I feel, generally unsuccessful. The lack of success is not with generating significant and seemingly interesting findings; rather, the lack of success has been in fleshing out the extent to which the underlying mechanism is operative. Much of the work has shown that conditions exist where this mechanism *might* be working, but it fails to demonstrate convincingly that it *does* operate (i.e., that electoral accountability drives repression). Davenport (2007*b*) attempts to test this hypothesis by using the Vanhanen (2000) data on competition and participation. The suggestion here is that as voter participation increases and one-party hegemony decreases, leaders should be *more likely* to be sanctioned by voters. It is not obvious that these represent sufficient conditions for the mechanism to work. The underlying mechanism does not imply that a greater level of competition in free, multi-party elections ought to lead to leaders being removed for repressive policy. Rather, it suggests that leaders will modify their behavior based on the existence or potential of an electoral sanction. Whether 50% or 60% of the population participates in a free and fair election should have no real bearing on this question, unless one assumes the electorate is unevenly distributed along the dimension based on likelihood of voting. If the electoral constraint is latent (i.e., it only really matters that it *could* be exercised), then suffrage (or potential participation) matters more than realized participation in any election. Rather than considering these variables that will, generally, separate non-democracies from democracies, I focus only on democracies to tease out the effects of different democratic arrangements on repression. While it might seem like this poses a selection problem, it seems rea-

sonable for two reasons. First, I am not selecting on the dependent variable, rather on one of the independent variables. Further, this practice follows the advice of both Achen (2002) and Clarke (2005) who suggest testing theories in more homogenous domains.

4.1 Salience of Political Repression

As I suggested above, a considerable amount of focus has been placed on global investigation of the concepts of interest here. However, many of the theories we have propose interesting within-country differences for different types of democracies. To leverage some interesting data that has, until now, been unused by this literature, I restrict the focus in the manuscript to a set of democracies covered by the comparative manifestos project (Budge et al. 2001, Klingemann et al. 2007). These are all, for at least some of the time under investigation, multi-party democracies with more or less free and fair elections. Due to the temporal domain of the repression data, I focus on the years 1980-present. This focus on only democracies restricts the sample to about 40% of the independent countries which is a relatively small subset. Further, the inferences made here will provide unprecedented detail in the working of repression within democracies. By concentrating on this particular group, it is possible to test theories about the differences between democracies, rather than the differences among democracies and non-democracies.

I first consider the extent to which voters may use potential repressive policy as a means for casting their votes. To do this, I used data from the Comparative Manifesto Project. The CMP has provided a coding of party manifestos across 25 countries from the years 1945-2003 and a further 24 countries from 1990-2003 (Budge et al. 2001, Klingemann et al. 2007). The manifestos are coded into 56 exhaustive and mutually exclusive categories. The final numbers represent the percentage of

each manifesto devoted to the each of the 56 topics. These topics are divided across seven areas - external relations, freedom and democracy, the political system, the economy, welfare and quality of life, fabric of society and social groups. There is also one category for sentences that did not fit neatly into any other category. While these have been criticized for the way they've been aggregated to form a scale of overall left-right party placement (See Bakker, Edwards and Netjes 2006, Laver, Benoit and Garry 2003, Laver and Garry 2000, for some critiques), they nonetheless do present valuable information on what parties found important.

For my specific purposes, there are two items in the manifesto project's coding that seem useful. The variable *National Way of Life: Positive* (per601) is defined as, "appeals to patriotism and/or nationalism; suspension of some freedoms in order to protect the state against subversion; support for established national ideals," (Volkens 2005, 13). The other is *Law and Order: Positive*, which is defined as, "enforcement of all laws, actions against crime, support and resources for police, tougher attitudes in course," (Volkens 2005, 13). I use these two measures to calculate the percentage of each party's manifesto devoted to these two measures combined. While I grant that these are not perfectly related to repression *per se*, they appear as reasonable proxies for the concepts of interest. As you will see in the ensuing investigation, these perform as expected.

The percentage of each party's manifesto devoted to these two items can be thought of in a couple of different ways. First, it can be thought to convey the salience of that issue for that party. That is to say, a party that concentrates 50% of its manifesto on these issues ought to be more interested in policy on this issue than a party which spends only 10% of its manifesto on these items. Another way to think about this is in terms of position. That is to say, we can think of these issues as providing a measure of each party's placement on the repression dimension of political competition. To enable this measure to be used with country-level data, I

execute the following transformation. First, I define s as a column-vector of the seat shares of each party (as a proportion) and m as a column-vector of the percentage of times each party's manifesto mentions either the law & order or national way of life (positive) questions as a percentage of the total number of statements made. Then $s'm$ is the country's measure of the salience of repression. This amounts to a set-share weighted sum individual party repression salience.

As suggested above, conventional wisdom proposes two relevant hypothesis: 1) citizen opinion with respect to repression should be relatively low and as such, 2) government repression will generally be unrelated to public opinion on the subject (because there will be very little variance). That is to say, when governments repress, they will be doing so against the will of the people - taking a calculated risk that ensuring security will eventually pay electoral benefits. By way of description, Figure 4.1 shows the unconditional distribution of the repressive salience scores. The scores tend, on average, to be between zero and ten. However, there are certainly some larger numbers as well. The highest values are around 20% - suggesting an average of around 20% of manifestos in that country-election were devoted to these two issues. Figure 4.2 shows the salience of repression over time across the 49 countries under investigation here (i.e., the 49 countries covered by the Comparative Manifesto Project). A simple linear latent trajectory model suggests that the trajectories are

generally positive.¹ This suggests that over time, repression is becoming more salient. This is not surprising given the rise in high-profile international terrorist events in the early 2000's.

4.2 Salience and Security

As an intermediate step to the ultimate goal, I consider the extent to which the salience of repression tends to follow security-relevant events. The suggestion Davernport (2007*b*) makes is that repression can be used as means of security territory against threats from within and without. As such, in the wake of revealed vulnerability, parties should generally increase the amount of manifestos devoted to law & order and preserving the national way of life. Using data from the Memorial Institute for the Prevention of Terrorism (Memorial Institute for the Prevention of Terrorism 2006) as well as ITERATE (Mickolus et al. 2003) I found the number of fatalities

¹The latent trajectory model is simply a multi-level linear model with random intercepts and random slopes on a linear year term.

$$\text{Salience}_{ij} = \alpha_j + \beta_j \text{year}_{ij} + \varepsilon_{ij} \quad (4.1)$$

where

$$\alpha_j = \gamma_{00} + \omega_{1j} \quad (4.2)$$

$$\beta_j = \gamma_{01} + \omega_{2j} \quad (4.3)$$

The errors of the random intercepts and slopes are assumed to be multivariate normal:

$$\begin{pmatrix} \omega_1 \\ \omega_2 \end{pmatrix} \sim \mathcal{N}(\mathbf{0}, \mathbf{\Sigma}), \quad \mathbf{\Sigma} = \begin{bmatrix} \sigma_1^2 & \\ \sigma_{12} & \sigma_2^2 \end{bmatrix} \quad (4.4)$$

The results of the model are $\gamma_{00} = -133.99$, $\gamma_{01} = 0.070$, $\mathbf{\Sigma} = \begin{bmatrix} 0.336 & \\ -0.213 & 0.001 \end{bmatrix}$.

Figure 4.1: Histogram of Saliency of Repression

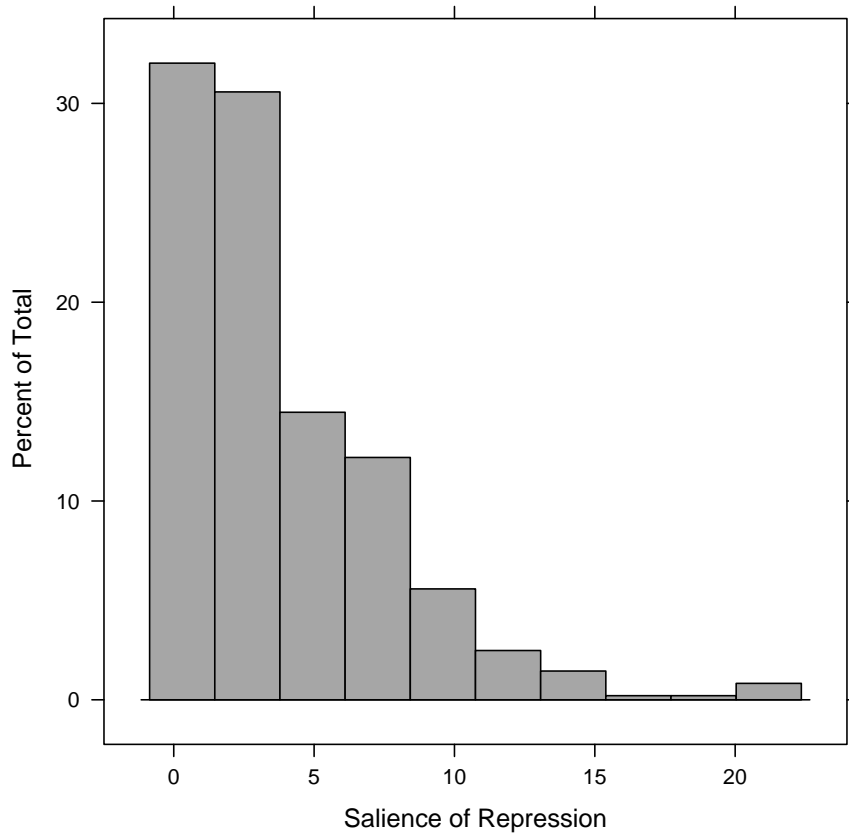
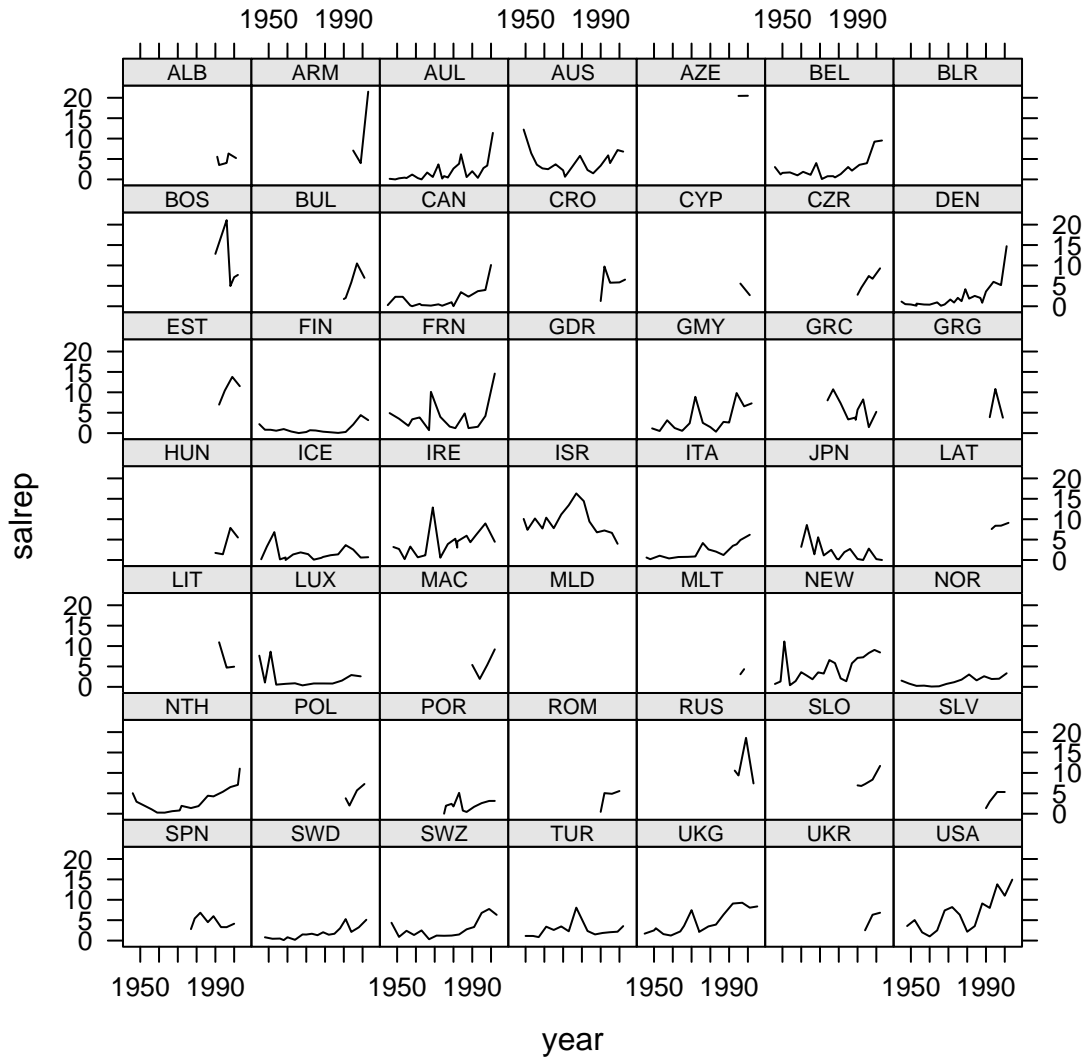


Figure 4.2: Salience of Repression in 49 Countries



Note: The figure represents the seat-share-weighted sum of salience scores for items per601 and per605 in the Comparative Manifesto Project data. These refer to law & order and national way of life (positive), respectively. Numbers exist for each country-election and are connected with a straight line between elections.

due to terrorist incidents in each year (and election cycle).² To do this, I simply took the maximum reported in each country in each year by both sources. The resulting measure is then used as a proxy for security threats.

By way of a short digression, the terrorism measure will serve the same purpose here that civil and interstate war experience serve in traditional models in this field. The 49 countries under consideration here are all democracies with some degree of multi-party competition. As such, civil and interstate war experience do not do a particularly good job of distinguishing countries on the level of internal and external security threats. Most countries in this subset are free of both internal and external wars over the time-period in question.

To estimate the effect of terrorist events on the salience of repression, I aggregate the data by election cycle. Specifically, I sum the total number of fatalities in the previous election cycle. For example if election 1 is held in 1995 and election 2 is held in 1999, then I predict salience in the 1999 platforms with the sum of terrorist fatalities in that country from 1995-1999. While it may be reasonable to suggest that parties change their position on repression continuously between elections in response to the ever-changing security environment, manifestos only change at election times. Thus, these are not a particularly good tool for picking up intra-election change. As such, the results are preliminary and instructive in nature. Findings here will likely be underestimates of the party's responsiveness to terrorism.

The findings here confirm what Davenport (2007*b*) suggested - parties tend to focus more on law & order and preserving the national way of life following terrorist attacks that generate fatalities. The general finding is not changed by the inclusion of

²Exploratory analysis suggested that fatalities were the most important predictor of salience of repression. That is to say, injuries or the raw number of terrorist attacks did less well. This suggests that citizens are responding, in general, to serious threats to national security. There is little evidence of knee-jerk reactions to terrorist incidents that claim no lives.

the lagged dependent variable, though the absolute magnitude of the instantaneous effect decreases. The statistical results are in Table 4.1

Table 4.1: Predicting Salience of Repression with Terrorist Fatalities

	MM	OLS	MM	OLS
Intercept	3.963	4.211	1.608	1.796
	(0.242)	(0.238)	(0.292)	(0.292)
Salience of Repression _{t-1}			0.658	0.630
			(0.059)	(0.059)
Terrorist Fatalities _{t-1}	0.030	0.026	0.018	0.019
	(0.005)	(0.005)	(0.004)	(0.003)
R^2		0.147		0.496

Note: The main entries are coefficient estimates, with parentheses in standard errors. The dependent variable is the salience of repression at time t . Due to the presence of outliers, I present not only the OLS results, but also the robust regression results. These were calculated using an MM estimate with a Huber weight function Andersen (2008).

The substantive effects are not overwhelming here. Fatalities range from zero to 2756 (US in 2001-2004). The substantive effects suggest that 100 fatalities should increase the salience of repression by somewhere from two to three percent. About three percent of the cases have fatality figures bigger than 100. Thus, this is instructive, but is not a result that demands more attention. Simply stated, when terrorist events generate large levels of fatalities, it appears that parties move in the direction of providing more security, perhaps by (temporarily) eliminating some freedoms in an effort to secure the country's territory.

Thus far, two pieces of information have been generated. First, there *is* variance

in the seat-share weighted sum of party positions on repression. It is a relatively short jump to conclude that there is variance in public opinion about repression, both across countries and over time. These two pieces of evidence suggest that the idea that citizens universally prefer lower levels of repression is, quite simply, false.

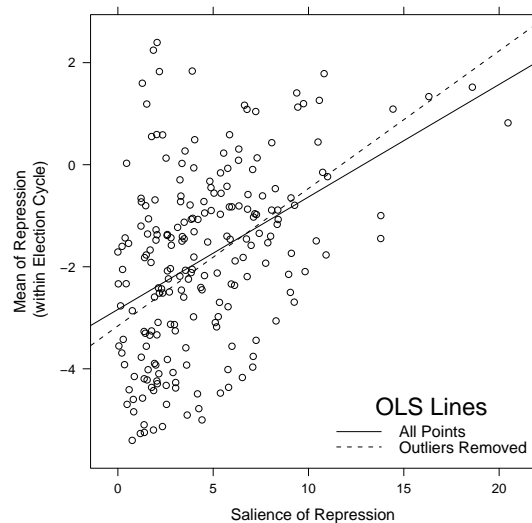
4.3 Salience and Observed Repression

Now, I move to understanding the effect that salience has on actual, observed repression. The idea here is that as repression becomes more salient across the board, repressive policy should become more harsh as repressive policies also respond to security-relevant events. To accomplish this goal, I use the same data as above that was aggregated by election cycle. First, consider the bivariate scatterplot between the mean of repression and the salience of repression in Figure 4.3. The relationship here seems relatively strong and linear. However, there are some interesting points. First, there are a number of countries with relatively high repression, where repression plays a relatively small part of electoral competition. These are the points in the upper left-hand corner of the plot. Of these six points, four of them belong to Turkey. In fact, these are the four election cycles in Turkey's post-war electoral history with the greatest number of political parties running for seats - indicating more contentious electoral politics. I address contentious politics directly later on, so I leave a deeper discussion of these possibilities to that chapter. The other two points represent one election-cycle each for Georgia and Croatia. The other two were the first elections in both countries, so the higher levels of repression were likely legacies of autocracy.

There is also another set of countries with unusually high salience of repression. Of the six points with salience of repression greater than 13, two belong to Israel, marking the period from 1981-1988. There were certainly conflicts in Israel at this time including the South Lebanon Conflict over most of this period, but nothing out

of the ordinary for Israel. One of the points belongs to Russia, from 2003-2007. Over this period, relationships with Chechnya were certainly tumultuous, to the extent that Chechans engaged in a series of terrorist attacks against Russia from 2002-2004. The level of repression indicated by the CIRI data are certainly consistent with reports of repression around this time. Another belongs to Azerbaijan's first election. Finally, the last one belongs to the US from 2000-2004. This is obviously the time of the terrorist bombings in New York and Washington, DC. It is no surprise that the US shows up here given its policies on torture and rendition directly following the bombings.

Figure 4.3: Bivariate Scatterplot of Mean of Repression versus Salience of Repression



Both groups of points mentioned above are unusual (i.e., outliers) on one dimension. They are not regression outliers (i.e., they do not make big differences to the regression line) because the first group has big residuals and almost no leverage and the second group has big leverage, but quite small residuals. The figure suggests that the line describing all of the points is not all that much different from the line describing all of the points except the ones mentioned explicitly above. If anything, the outliers tend to dampen the strength of the relationship. The trend seems to

be relatively clear, as repression becomes more salient, actual repression tends to increase. This suggests an addendum to Davenport’s (2007b), “vote the bastard’s out,” idea. In fact, sometimes, citizens might be, “voting the bastards in!” When security is threatened, citizens may, indeed, have a preference for repressive leaders.

A simple statistical model confirms that the relationship is statistically significant (see Table 4.2).³ Here, I estimate the equation:

$$\text{Mean Repression} = \beta_0 + \beta_1 \text{Salience} + \varepsilon \quad (4.8)$$

The results presented in Table 4.2. Repression ranges from roughly -5 to 5, a 10-point increase in the salience of repression should result in an increase in actual repression of about 2.5 points (or around 25% of the scale). This is both substantively and

³As repression is a latent variable (i.e., an estimate with uncertainty), a simple model that employs the posterior mean will tend to under-estimate the model uncertainty. Following Armstrong, Duch and Bakker (2007), I treat the latent variable as a severe case of missing data - where all observations are missing. Then I employ standard multiple imputation technology (see Rubin 1987, Schafer 1997) to propagate the latent variable uncertainty through the second-stage predictive model. Specifically, I use the last 100 iterations of the markov chain for each value of repression, making a matrix that is $N \times 100$. Then, I run 100 predictive models with coefficient vectors $\beta_1, \dots, \beta_{100}$ and variance-covariance matrices $\Sigma_1, \dots, \Sigma_{100}$. The overall coefficient estimate, denoted \bar{Q} , is simply the mean of the 100 coefficient estimates. The total variance of those coefficients is a function of both the inherent sampling variability in each model (\bar{U}) and the variance of the repression scores across iterations of the markov chain (B).

$$B = \left(\frac{1}{m-1} \right) (\alpha_j - \bar{Q})(\alpha_j - \bar{Q})' \quad (4.5)$$

$$\bar{U} = \left(\frac{1}{m} \right) \sum_{j=1}^m \mathbf{V}_j \quad (4.6)$$

$$T = \bar{U} + \left(1 + \frac{1}{m} \right) B \quad (4.7)$$

statistically meaningful. Looking at the observed ranges across all the countries here, the average range of the salience of repression is around 5. That is to say, the salience of repression in the average country has a range of around 5 (i.e., the maximum is 5 points higher than the minimum in the average country). This should generate about a 1.25 unit change in actual repression (roughly 10% of the range).

Table 4.2: Regression Results for Observed Repression as a Function of Salience of Repression

	OLS	MM
Salience of Repression	0.242 (0.050)	0.248 (0.050)
Intercept	-2.955 (0.415)	-2.992 (0.416)
N	210	210
$R^2 \approx$	0.168	

Note: Main entries are coefficients resulting from the process described above in footnote 3 (page 59), with country-clustered standard errors in parentheses. The dependent variable is the mean of repression. The R^2 here is approximate because the R^2 for each randomly drawn repression point is slightly different. The range is roughly (0.15, 0.19).

The first-order concerns have thus been satisfied - actual repression seems to be a function of the salience of repression. Now, to aid in the interpretation of what this actually means, I consider what would be the expected change in the indicators of repression for the changes in salience seen in each country. These are shown in Figure 4.4. First, a bit of explanation about how the figure was made. You will remember from 3.4.1 that the measurement model relates the latent variable to the

observed indicators through a regression-type model (here an ordered logit). As such, it is possible to get predictions for each of the indicators based on any particular level of repression. Also, the discussion above suggests that actual repression can be expressed as a stochastic linear function of the salience of repression. Thus, it is possible to get from the salience of repression (through two equations) to the indicator values expected for that level of salience. Now, which values of salience would be most instructive? Here, I use the minimum and maximum in each country.⁴ Most countries do not change. Even though they experience changes in salience, the changes are not enough to induce predicted changes on any of the indicators. However, there are some interesting patterns. Torture is by far the variable with the greatest number of changes (16 of the 42 countries would be expected to experience a change). Only 4 of the 42 countries change on killings and political imprisonment and none change on disappearances. What can be said, then, is that in this set of democracies (remember, all of these countries have multi-party elections), when repression becomes most salient, torture is almost as likely as not and killings and political imprisonment are not out of the question.

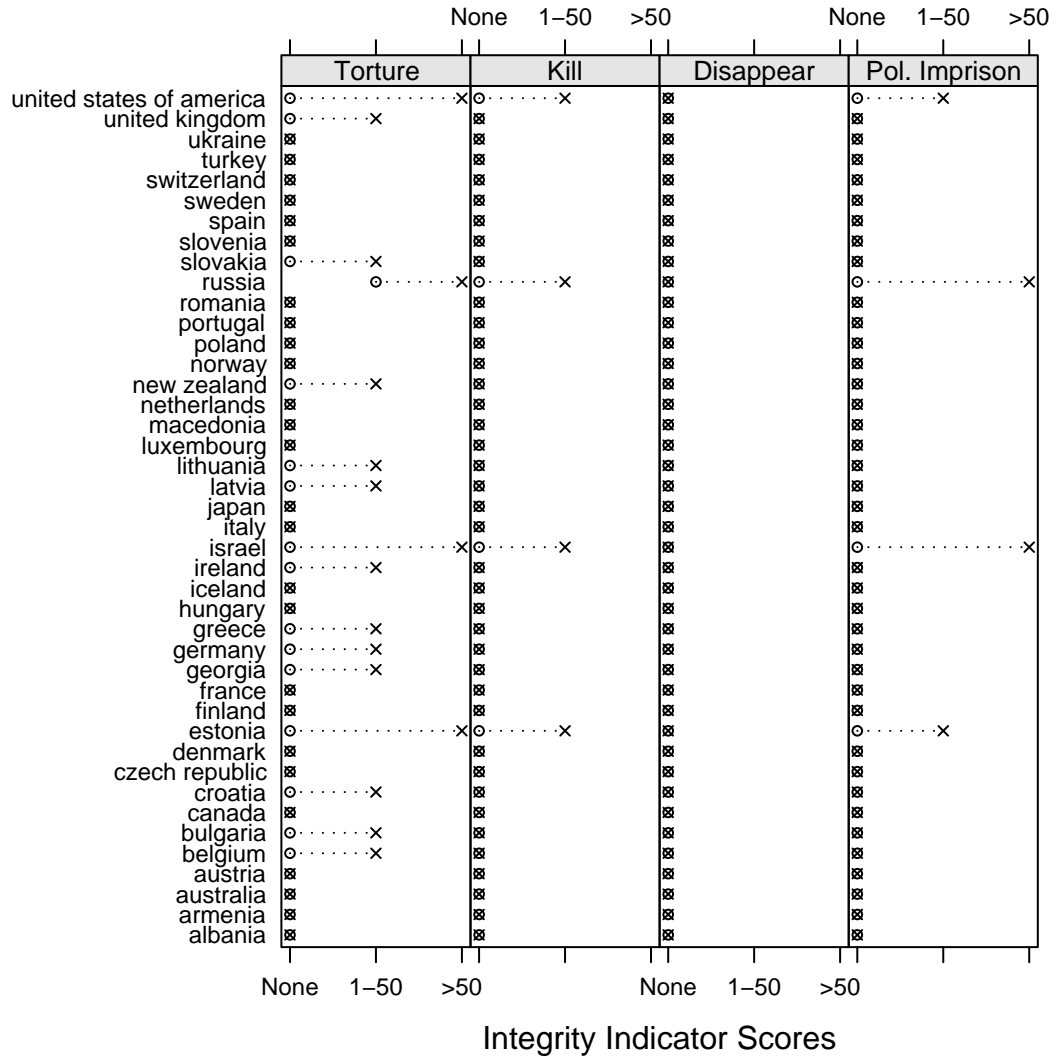
Theory, such as it is, has suggested other possible causes of repression. Namely, GDP/capita and population have been shown to mitigate and exacerbate the use of repression, respectively. Money is thought to buy options – with greater resources, authorities have avenues whereby they can placate potential dissidents. Countries with greater populations are thought to have more strain on their limited resources,

⁴Technically, the measurement model equation suggests that:

$$\Pr(Y_{ij} \leq m) = \frac{1}{1 + \exp[-(\kappa_{mj} - (\gamma_j - \text{Repression}_i))]} \quad (4.9)$$

where i refers to the N country-years and j refers to the each of the four observed indicators of repression. In the above, I simply replace Repression_i with: $\beta_0 + \beta_1 \text{Salience} + \varepsilon$, estimated in equation 4.8.

Figure 4.4: Predicted Changes in Repressive Indicators for Changes in Saliency of Repression



Note: Predicted categories when saliency takes on its lowest values are represented by an open circle “o” and predicted categories when saliency takes on its highest values are represented by an “x”.

all else equal. This should breed more discontent and in turn increase the demand for repression. As argued above, threats to domestic tranquility can also increase the demand for repression. As such, all of these are included as controls in the

model. Table 4.3 shows the multiple regression of the mean of repression on salience, and the controls mentioned above. In essence, the salience measure is a proxy for the demand of repression. The fact that both population and GDP/capita are statistically significant means either A) they act on repression in some other way than influencing factors that increase or decrease the demand for repression or B) salience is a poor proxy for the demand of repression. These continue to go in the right direction, so I suspect that a bit of both A and B are happening here. Salience is probably not a perfect measure of demand for repression and it is likely that both demographic variables effect repression through yet unexplored theoretical avenues.

For the current work, the important result is that of salience. Salience remains an important predictor, both substantively and statistically, of repression. Further, the effect of the security variable (fatalities from terrorist events) is not statistically different from zero. This suggests that whatever effect terrorist events have - they have through increasing the salience of repression. This seems a reasonable result and argues that the salience variable has at least some value as a proxy for the demand for repression. Figure 4.5 provides information similar to that provided by Figure 4.4, though for the multiple regression model. This figure provides the predicted changes for each of the repression indicators for a change in salience over the observed range in each country holding the other variables constant at their median value in each country. There are a number of similarities between the two graphs, especially in Western Europe and North America. However, in Central/Eastern Europe and Russia, the demographic pressures may repression more likely at the highest salience values in those countries. That is to say, the relatively low GDP/capita in these countries will lead to increased repression for similar values of repressive salience.

Perhaps these demographic indicators, especially GDP/capita, are also capturing regime stability. In stable regimes, small demands for repression could be dismissed more easily. Low, but non-zero, demands for repression are unlikely to result in the

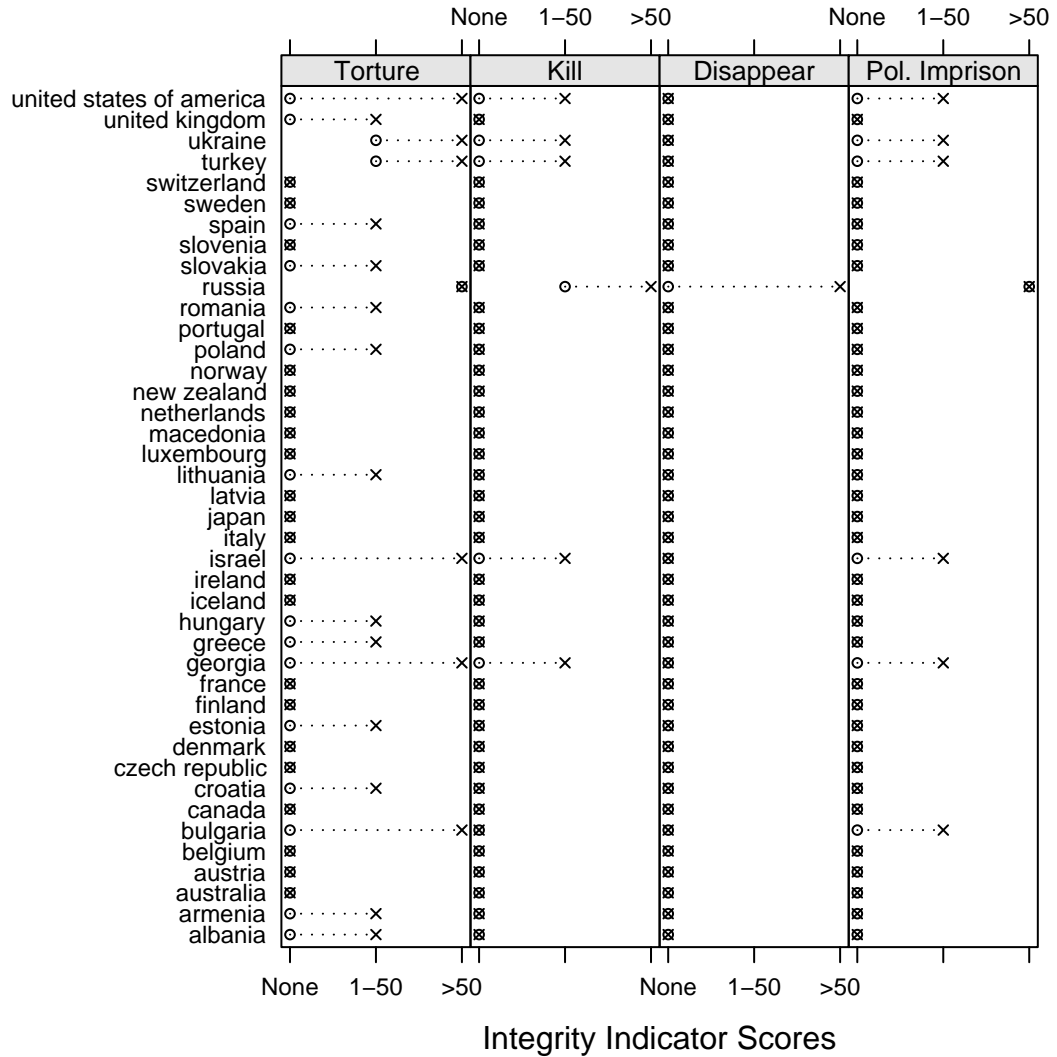
Table 4.3: Multiple Regression Results for Observed Repression

Intercept	-6.039 (0.278)
Salience of Repression	0.222 (0.009)
Fatalities (in 100's)	0.044 (0.027)
GDP/capita (in 1000's)	-0.104 (0.006)
Log(Population)	0.504 (0.029)
N	206
$R^2 \approx$	0.455

Note: Main entries are coefficients arrived at by the method suggested in footnote 3 (page 59), with their corresponding standard errors in parentheses. The dependent variable is observed repression. The R^2 here is approximate because the R^2 for each randomly drawn regression point is slightly different. The range is roughly (0.40,0.50).

catastrophic end to a stable democratic regime. The same may not be true of a less stable, perhaps newer, democracy. Leaders in these regimes may feel as though protecting the security of the regime is of paramount importance, even at the expense of liberty. One very simple way to investigate this proposition is to consider, for each country, the GDP/capita and the year of the first entry into the Manifesto data. The first date of entry into the Manifesto data marks the first multi-party election in the country. These two are correlated at -0.61, suggesting that newer countries have, on average, lower GDP/capita figures. There were basically three waves of inclusion

Figure 4.5: Predicted Changes in Repressive Indicators for Changes in Saliency of Repression Controlling for Other Variables

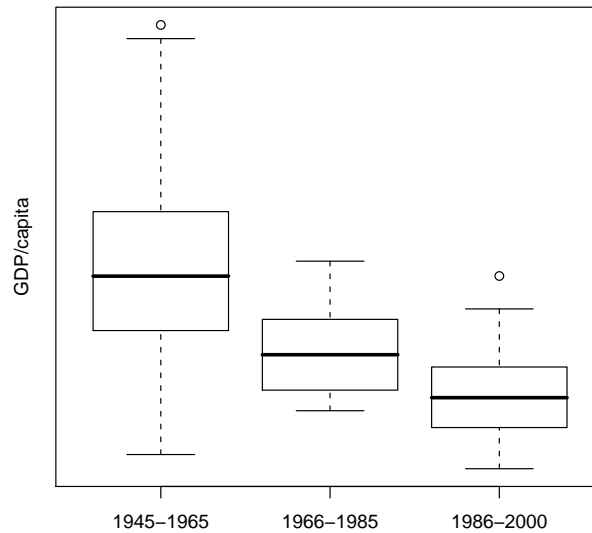


Note: Predicted categories when saliency takes on its lowest values are represented by an open circle “o” and predicted categories when saliency takes on its highest values are represented by an “x”. All other variables are held at the median value for each country.

in the data. Many appeared in the late 1940’s or early 1950’s. A few other countries appeared in the mid 1970’s and finally another group appeared in the late 1980’s and

early 1990's. Figure 4.6 shows the box-plot of GDP/capita by these three waves of inclusion in the data.

Figure 4.6: Boxplot of GDP/capita by Year of Entry into the CMP Dataset



By considering these three different waves, it is possible to parse the effect of GDP/capita and stability. For example, it is possible that poorer, older countries see less repression than newer, richer countries. Further, it is possible that the effect of GDP/capita is different in these three groups. It seems reasonable that GDP/capita could make a big difference in newer countries. These resources may provide a level of stability that cannot be provided by longevity.

Table 4.4 replicates the results in Table 4.3 but allows the effect of GDP/capita to be conditional on the entry group into the Manifesto data. The reference group for the entry variable is 1945-1965. The table shows that the coefficient for GDP/capita is negative, but not significant for the reference group. The conditional coefficient for the second entry group is 0.050 with a t -statistic of 1.14 and the conditional coefficient for the third entry group is -0.274 with a t -statistic of -4.917. Thus, GDP/capita

matters only in the newest set of democracies. This lends some credibility to the argument that the stability that cannot be provided by longevity, may be provided by resources.

4.3.1 Between- vs. Within-Country Salience and Political Repression

Finally, I take this result one step further. Throughout this investigation, I have treated these observations as though they were independent. This was useful to get a sense of the global relationship, but it has overlooked an important aspect of the structure of the data – namely that these data are years nested within countries. To this end, I estimate the following multilevel model that acknowledges the structure in the data and allows the estimation of unit effects.

$$\text{Repression}_{it} = \alpha_i + \beta_{1,i}\text{GDP/capita}_{it} + \beta_{2,i}\log(\text{Population})_{it} \quad (4.10)$$

$$+ \beta_3\text{Salience}_{it} + \beta_4\text{Fatalities}/100_{it} + \varepsilon_{it}$$

$$\alpha_i = \gamma_{0,0} + \gamma_{0,1}\overline{\text{Salience}_i} + \gamma_{0,2}\text{Entry} > 1980_i + \nu_{i,0} \quad (4.11)$$

$$\beta_{1,i} = \gamma_{1,0} + \gamma_{1,1}\text{Entry} > 1980_i + \nu_{i,1} \quad (4.12)$$

$$\beta_{2,i} = \gamma_{2,0} + \nu_{i,2} \quad (4.13)$$

Notice, here since the entry date was really identifying the late third-wave countries entering in the late 1980's, I simply include a dummy for entry into the dataset after 1980. The somewhat complicated second level effects represent both the product of a bit of model searching and advice from previous models. Both GDP/capita and population are variables that would seem obvious candidates for differential effects by country. I am particularly interested in whether the salience of repression retains its within-country predictive power when the country-mean is included as a predictor of

the intercept. Finally, since the date a country enters the sample is a country-specific effect, it does not have a level-one effect, rather its level-two effect is to predict one of the level-one parameters (Snijders and Bosker 1999).

The results in Table 4.5 show that many of the results remain significant when we consider the appropriate nesting structure at the individual country-year level. The salience of repression result seems to be generally a between-country effect, rather than a within-country effect. A bit of investigation showed that this was largely a function of allowing random slopes on GDP/capita (until this modification, the individual salience term remained significant). A statistical comparison of models both with and without variable slopes on GDP/capita provides clear evidence that the former is preferred. Further, the random slopes on population also survive a statistical model comparison, though seem to be less detrimental to the individual-level result of repressive salience.

I have argued throughout this chapter that salience provides a measure of demand for repression. Specifically, as parties tend to talk more about repression, that reflects an increased salience in repression among the general public. This demand for repression is often met with an increased supply of repression by governments. A considerable amount of evidence can be amassed to show that there is a within-country relationship, however, when a multilevel model is estimated which allows for different between- and within-country effects, I find that the salience measure effectively captures between-country variation. In countries where salience is, on average, higher, repression is also, on average, higher. Further to that point, it does seem as though citizens have variable tastes for repression and that governments, as they ought to be according to electoral accountability, are willing to accommodate.

The prospects for domestic democratic peace, are then somewhat tenuous. They depend, at least in part, on the preferences of the citizens. If citizens are moved to react sharply to their security environment, the results here suggest that governments

could very well respond with repression.

Table 4.4: Multiple Regression of Mean Repression including Conditional Effect of GDP/capita

Intercept	-7.462 (1.221)
Salience of Repression	0.192 (0.060)
Fatalities (by 100)	0.023 (0.030)
GDP/capita (by 1000)	-0.043 (0.044)
Entry: 1966-1985	0.052 (1.443)
Entry: 1986-2000	3.080 (1.022)
Log(Population)	0.530 (0.119)
GDP/capita \times Entry: 1966-1985	0.093 (0.090)
GDP/capita \times Entry: 1986-2000	-0.231 (0.065)
N	210
$R^2 \approx$	0.546

Note: Main entries are coefficients arrived at by the method suggested in footnote 3 (page 59), with their corresponding standard errors in parentheses. The dependent variable is observed repression. The R^2 here is approximate because the R^2 for each randomly drawn repression point is slightly different. The range is roughly (0.48,0.58).

Table 4.5: Multilevel Model of Repression as a Function of Salience

Intercept	-10.672 (1.898)
Salience of Repression	0.213 (0.088)
Entry > 1980	3.146 (0.830)
GDP/capita (by 1000)	0.092 (0.032)
GDP/capita × Entry > 1980	-0.114 (0.054)
Log(Population)	0.638 (0.197)
Salience of Repression	-0.005 (0.015)
Fatalities (by 100)	-0.004 (0.018)
$\bar{\sigma}_{\nu 0}$	3.807
$\sigma_{\nu 0}$: 95% CI	(0.000, 9.256)
$\bar{\sigma}_{\nu 1}$	0.136
$\sigma_{\nu 1}$: 95% CI	(0.098, 0.183)
$\bar{\sigma}_{\nu 2}$	0.469
$\sigma_{\nu 2}$: 95% CI	(0.229, 0.880)
$\bar{\sigma}_{\varepsilon}$	0.367
σ_{ε} : 95% CI	(0.333, 0.403)

Note: Main entries are multilevel model coefficients using the strategy described in footnote 3 (page 59). Entries in parentheses are standard errors. $\bar{\sigma}_{\nu 0}$, $\bar{\sigma}_{\nu 1}$, $\bar{\sigma}_{\nu 2}$, and $\bar{\sigma}_{\varepsilon}$ were obtained by taking the empirical mean of the parameters across $m = 100$ models. The 95% CI's for those parameters were obtained by computing the 2.5th and 97.5th percentiles of the parameters across the 100 models.

Chapter 5

Veto

The argument here is relatively straightforward, but has been generally misrepresented by the literature thus far. The literature suggests (at least implicitly) that checks alone are enough to bring about lower levels of repression. This line of reasoning is antithetical to the veto players literature. Tsebelis (2002) suggests that veto players analysis can point to times or contexts where policy *change* is rather more or less likely. It is, by definition, a dynamic theory of policy stability and change. Further, Tsebelis admits that often as researchers, we do not know the status quo policy. I think the status quo might be a bit more clear here, but the veto players analysis was designed explicitly to generate results without knowledge of the position of the status quo. In the current context, that means that veto players analysis should suggest when repressive policy is more or less likely to change regardless of the baseline level of repression. Thus, the argument that as the number of veto players increases we would predict a particular policy or even a range of policies be chosen seems invalid.

If Tsebelis (2002) is the foundation of the veto players argument forwarded by Davenport (2007*b*), then what can we learn from veto players with respect to political repression? The first, thing to realize is that Tsebelis (2002) suggests that comparisons across countries are not valid – “it would be [equally] inappropriate to compare two different systems, one with three veto players and one with four and

conclude that the second produces more policy stability than the first” (p.25). Thus, the current practice of including the level of veto players in a model of repressive behavior is, technically speaking, not appropriate. As suggested above, analysis on this level is about policy *change*. The literature has tended to consider the number of veto players to generate an absolute result – more veto players equals lower repression. Rather, what we should consider be looking for is a relative result – changes in repressive policy over time should be relatively smaller when there are more veto players, rather than fewer, within a single country.

While Tsebelis has a number of different proposition that include the number and position of veto players as well as sequencing and agenda setting, there are a couple that are of particular interest. Specifically, I consider the extent of support in the data for two propositions. First, that increasing the number of veto players should increase the stability and second, that as the distance between veto players increases, so should policy stability. The application to repressive policy should be obvious, though I discuss it with respect to both propositions below.

5.1 Number of Veto Players

Consider Tsebelis’ proposition 1.1: “[t]he addition of a new veto player increases policy stability or leaves it the same (either by decreasing the size of the winset of the status quo, or by increasing the size of the unanimity core, or by leaving both the same).” (Tsebelis 2002, 25). This suggests that within each country, a change in the veto players should make repressive policy more stable (i.e., policies should have lower variance). This proposition requires that there be within-country variance on the veto measure. Figure 5.1 shows the variance of veto over time within countries. As you can see, there are many countries that do not have interesting variation on this measure. The consolidated, relatively older democracies seem to be relatively

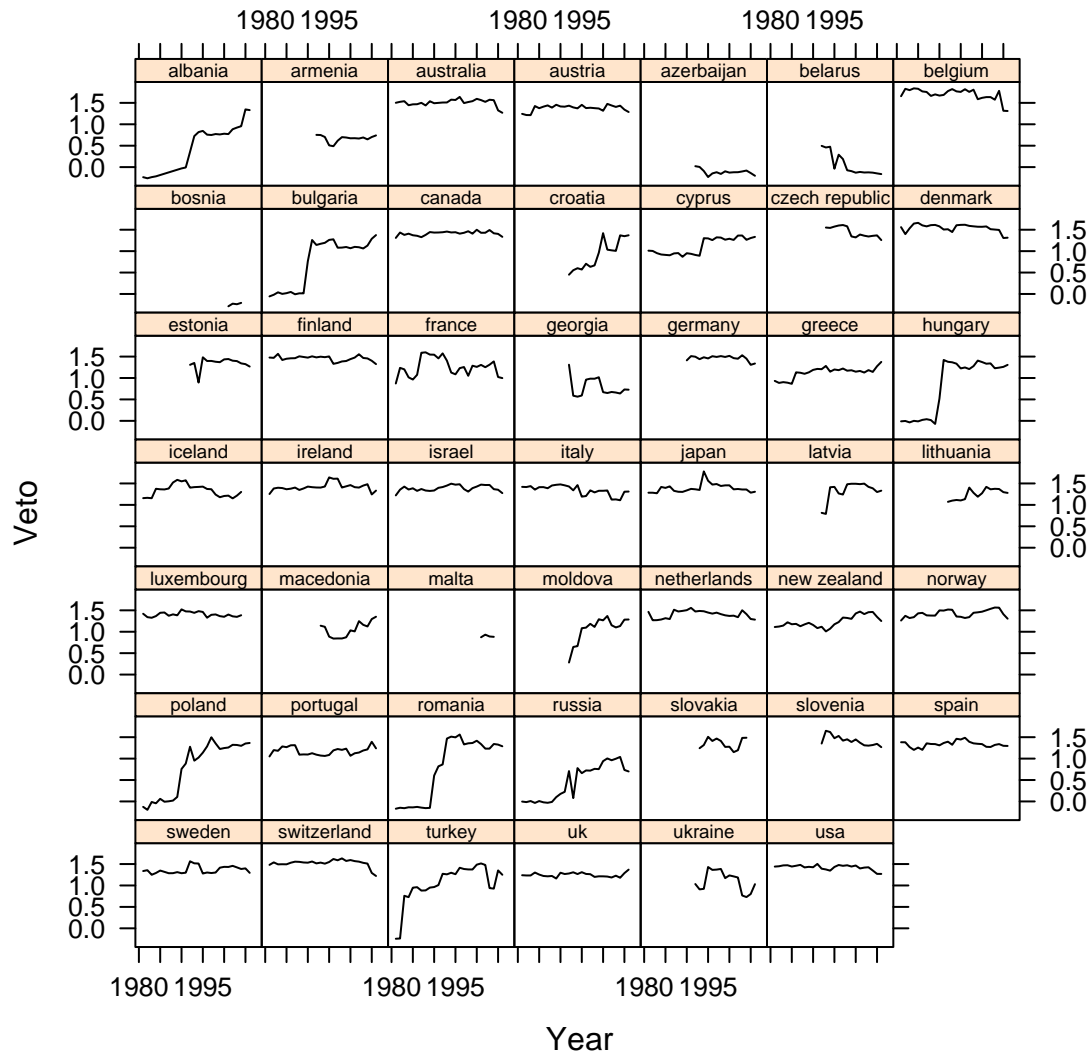
stable with respect to veto players. On the other hand, newer democracies seem to have a considerable amount of variance. Thus, one would expect veto players to make a difference in repressive strategy in the late third-wave democratizers, but changes in veto players simply don't exist in the other countries as a general rule and as such, should be unable to influence repressive policy in the way suggested by Tsebelis.

Despite this general pattern, there are a couple of outliers – France and Turkey. This makes sense as in France in 1986, Jacques Chirac came to power in an election that brought about an enormous change in the seat shares held by the main parties which led to a switch from single-party majority government to a coalition government. Turkey, on the other hand, appears to exhibit a secular trend in most of the indicators over the time-period of interest here. The only exception is the sub-federal veto, which continues to obtain a value of zero over the entire period for Turkey.

To evaluate the hypothesis - that within a country, more veto players should lead to policy stability, I use the veto variable calculated above in chapter 3. I calculate the difference in repressive policy choice between years t and $t + 1$. Then, I regress the absolute value of that difference on the value of veto at time t . The expectation, if repressive policy follows the model suggested by Tsebelis and assuming the existing actors stay in the same places over time, higher values on the veto variable should lead to smaller absolute changes in the next period. Again, rather than doing this globally, I estimate a fixed effects model that includes country dummy variables and an interaction between the country dummies and the veto variable. I also include the difference in the salience of repression from time t to time $t+1$. This is to allow for the fact that preferences may change over time. As the salience of repression increases, generally I would expect repressive behavior to follow.

The results are less than encouraging here. Figure 5.2 shows the conditional intercepts and coefficients for each country. The black points represent conditional intercepts and coefficients that are statistically different from zero and the gray points

Figure 5.1: Variance in Veto Latent Variable by Country



Note: The values in the plots represent the posterior means of the veto latent variable in each country in each year. In the interest of easing the identification of patterns, I have omitted measures of uncertainty here. However, countries that appear to have big changes do and countries that appear to have simply random variation over time, generally do.

represent conditional coefficients and intercepts that are not statistically different from zero. In general, most points are not different from zero. This is not especially

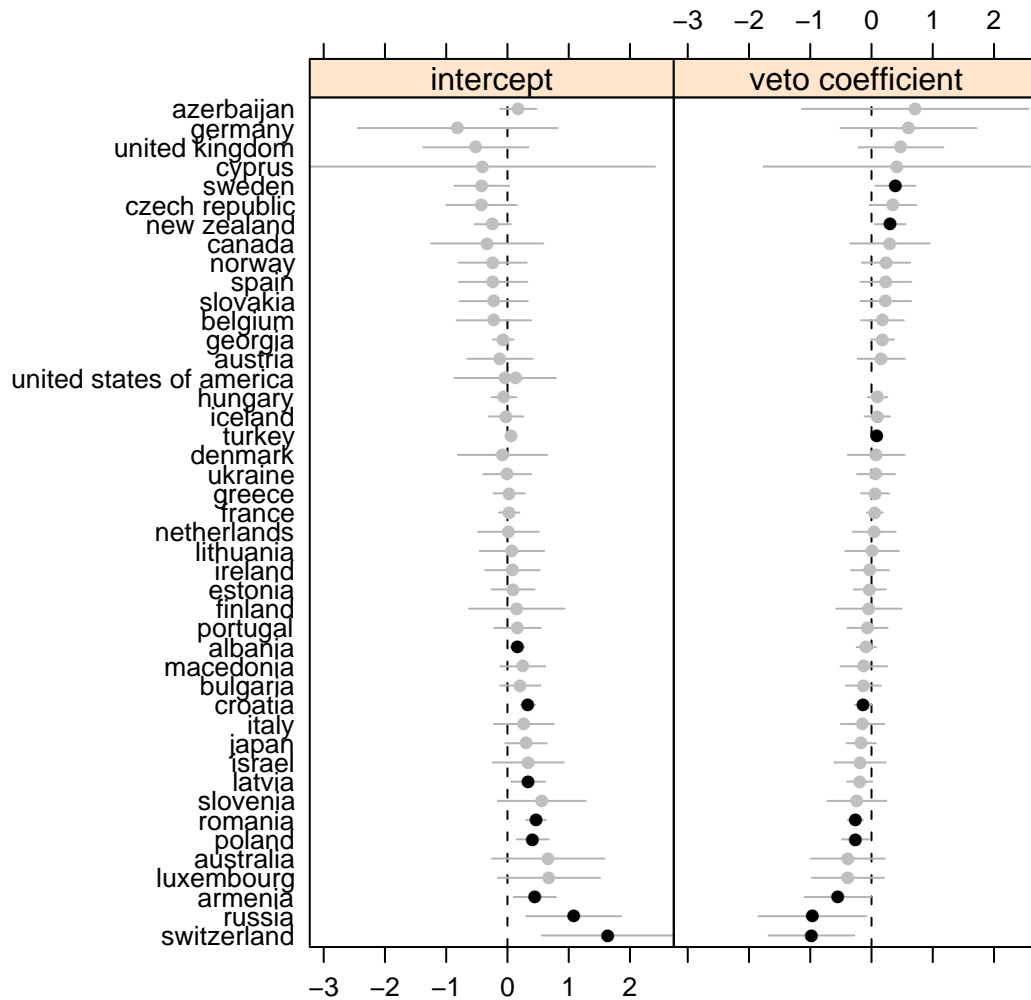
surprising given that many countries had no real variance on the veto variable over time. In absolute terms, there are some interesting findings. There are actually some significant, positive coefficients. This suggests that net of changes in salience of repression, as veto power gets more dispersed, changes in repressive policy increase. This happens in Sweden and New Zealand.

One way to potentially reconcile this finding with the theoretical literature could be that countries with higher variance in their veto player scores are the ones with the biggest coefficients. While there certainly are countries with big variance that seem to have big negative coefficients, there is no particularly strong statistical relationship here. This suggests that while there are certainly some countries that would seem to follow the pattern suggested by theoretical work, it is not obvious that this relationship is widespread and interesting with respect to repressive policy.

The fact that salience here is a characteristic that varies only by electoral cycle, rather than by year (as the other measures) could be influential to the results. As such, I calculate the variance of repression in each election cycle and then calculate the mean of veto the veto latent variable. Then, I subset the data to include only those countries with sufficiently many observations (≥ 5), this leaves 23 countries. This model proves even less supportive as the adjusted R^2 is, on average across the 100 simulated draws from the posterior, negative. Thus, there is surprisingly little support for the idea that a changing constraint environment results in more stable repressive policies.

This seems anomalous given the work of Tsebelis (2002). However, there could be a couple of potential explanations. First, repressive policy might be, on average, pretty stable regardless of the level of veto players. The fact that the effects are insignificant certainly suggests that the results are essentially equally stable regardless of the constraint environment. It could also be that repressive policy is not a policy area that would see the situations required for these theoretical results to hold. Let

Figure 5.2: Conditional Effect of Veto on Change in Repression



Note: The points represent the conditional intercepts (left panel) and coefficients on the veto variable (right panel) from an OLS model predicting absolute change in repression with the veto latent variable. Points colored black are significantly different from zero while those colored gray are not. The bars represent the 95% confidence interval of the conditional intercept or coefficient.

me explain. As Olson (2000) suggests, rational leaders do not have a preference for repression, meaning they do not seek repression as an end in itself, rather they use it

as a means to an end, namely that of security.¹ When security is threatened, public opinion on repression and security can change quite rapidly and both citizens and governments who would normally be staunch proponents of civil liberties now see the tradeoff between liberty and security as a bit harder to evaluate (Davis 2007). At the point when repressive policy changes, it is preceded by relatively large changes in both the position of the veto players and public opinion.

All things considered, there seems relatively little support for the relationship between the number of veto players and the stability of repressive policy. Now, I move on to another important proposition from the veto players literature.

5.2 Distance Between Veto Players

Tsebelis (2002) also suggests that as veto players take positions more distant from each other, policy stability should increase, (p, 30). Specifically, regardless of the position of the status quo, as veto players move further apart, the size of the winset shrinks and the size of the unanimity core increases. In relation to repression, as veto players (e.g., coalition partners) have increasingly different positions on the use of repressive tactics, such as torture, they are less likely to agree on a change to the status quo policy and thus the status quo is likely to continue as the policy of choice.

It is less obvious how the empirical investigation of this hypothesis ought to proceed. The reason for the ambiguity in this case is that it is somewhat difficult to obtain the positions of each of the veto players. This is especially true for independent judiciaries and sub-federal veto players. Further, by incorporating different measures of constraints on different metrics, the resulting measure of constrained executives does

¹This is consistent with Davenport's (2007b) work suggesting that repression is generally used to security territory against threats from within or without.

not even really identify a number of veto players. Rather, it identifies a the relative constraints on leaders such that higher numbers represent more constraints. Below, I propose a few different ways of assessing support for this proposition.

First, I simply take the variance of party positions on repression in each election cycle and regress variance in repressive strategy on these values along with a set of country dummy variables. While the sign is in the right direction, negative, meaning that variance in repressive policy is lower when variance in positions is higher, but the coefficient is not statistically significant ($p \approx 0.7$). This might not be the best way to operationalize the dispersion of veto player ideal points as not all parties have a veto. In an effort to adjust these values, I multiply the election-cycle variance in party positions by the veto latent variable. This provides no additional leverage on the problem as the coefficient remains negative, but statistically insignificant ($p \approx 0.4$).

To properly address this hypothesis it would be necessary to know which parties are in each coalition. In the absence of such information, I find the repressive policy of the minimum winning adjacent coalition. This is the coalition of adjacent parties on the repression dimension that has closest to, but still greater than 50% of the legislative seats. Then, I take the seat-share weighted average of their position scores, in a similar fashion to the way the salience score was calculated. Finally, I regress the intra-electoral-cycle variance in repressive policy on the minimum winning coalition variance.² Unfortunately, this strategy does not change the results either. The results remain statistically insignificant ($p \approx 0.4$), though in the right direction.

Again, there is very little support for the veto players result with respect to repression. There is at least one potential reasonable explanations here as well. It is possible that I have not properly captured the “right,” variance in the veto players.

²Not every coalition will be a minimum winning coalition, but it does at least provide a slightly better measure. The result will generally be to discount the position of small, extremist parties. These may occasionally show up in the minimum winning coalition, but less often than not.

Having tried a few different methods with quite similar results, I suspect that a better operationalization of this measure would not yield much in the way of interesting new results.

The results here seem somewhat contradictory to the current state of the literature on constrained executives. Others have found this to be one of the most important pieces of the puzzle with respect to state repression (Armstrong and Davenport 2003, Bueno de Mesquita et al. 2005, Davenport 2007*b*). If this is true, then why are these results so paltry? It is largely from the fact that models using these concepts are capitalizing on between-country variation rather than within-country variation, which explicitly runs counter to the advice offered by Tsebelis (2002). A very simple variance decomposition of the veto variable shows that the between country variation (roughly 0.3) is about three times greater than the within-country variation. Thus, it is very likely that models incorporating veto players as a variable are capitalizing on this variation. Even more convincing on these grounds is the variance decomposition of the repression variable. As suggested above, 80% of the countries in this study do not have significant changes in their veto scores over the entire period. Here, the between country variance (roughly 3) is an order of magnitude bigger than its within-country counterpart. So, while the within country variance on the veto measure may be up to the task, there is generally relatively little variance in repression over time within each country. This suggests that our models might be good at identifying relatively long-standing stable features of countries that lead them, as a rule, to respect human rights or not, but that we may be, on average, less good at predicting changes within country than we would have thought.

Chapter 6

Conflict

As Davenport (2007*a,b*) suggests, one cause of repression is internal insecurity brought about by violent dissent. In essence, this raises the demand for repression and as the theory goes, governments are more than willing to increase the supply of repression. In this case, democracy works through its ability to decrease conflict. Rather than decreasing it in absolute terms, democracy channels would-be violent conflict into the political process. Davenport assumes that this relationship is operative. In fact, it would seem that the aspects of democracy important to this particular relationship are somewhat different from the sets of indicators considered above. Voice required indicators of electoral accountability. Further, the policy positions on the repression dimension were important because they suggested the extent to which the population was activated on this particular issue. Veto was concerned with the constraints on the executive. Here, positions on the repressive dimension are less important and the number of constraints on the executive is almost completely orthogonal in theory. Important to this hypothesis is that there are groups representing diverse political views that have power of real potential for power in the government.

Scholars often include civil and interstate war experience as controls in their models to “test,” this hypothesis (e.g., Davenport 2007*b*, Davenport and Armstrong 2004, Poe and Tate 1994). One potential problem is that in current times (given the data

only generally go back in time until 1980), Western liberal democracies countries are uncharacteristically free from civil and inter-state war. Thus, these variables may, in fact, be picking up some of the effects of democracy. If the goal is to understand why some *democracies* work better than others with respect to civil liberties, then some more sensitive indicators will need to be chosen to test this mechanism. Davenport (2007b) also includes measures of violent dissent, but others generally do not.

To show that this mechanism is at work requires two things. First, it requires that violence be significantly predicted by political inclusion. Secondly, it requires that violent dissent be a predictor of state repression. Whether inclusion ought to also have a direct effect on repression is unclear. I suspect that it would probably tap the same cross-national variation that veto players did. Thus, there will likely be an effect, but it will be difficult to attribute the effect to inclusion without a relatively clear theoretical direct link between inclusion and repression. If the mechanism proposed here seems to work, then there are some clear institutional changes that could be made to foster inclusion in the political system. Namely, things like proportional representation could increase the number of viable political parties and, thus decrease the demand for repression.

6.1 Measurement

To test the extent to which these mechanisms are operative, I need to estimate two new measurement models - one representing the inclusive nature of the electoral system and one representing the nature of violent dissent within the polity. These models will generally be estimated in a fashion similar to those above.

6.1.1 Measuring Inclusiveness

Indicators for inclusiveness must be ones that change as a function of the diversity in opinions represented in the political system. To this end, I include a set of both behavioral and institutional variables. First, I use a number of measures from Golder's (2005) database. The database houses an extraordinary amount of information pertaining to nuances of the institutional design in democracies. While many of these are outside the realm of interest for my particular purposes, there are a few that are worth exploring. First, I use two measures of the party system - number of electoral parties and number of parliamentary parties. These are basically Herfindahl indices (or rather inverses thereof) that increase as the dispersion of either votes in the former or legislative seats in the latter approaches equality across the parties receiving votes or seats. The particular calculations are done according to Laasko and Taagepara (1979). From this dataset, I also use the electoral system type which can take values – majoritarian, proportional, multi-tier, and mixed. The expectation here is that proportional systems are more inclusive than majoritarian and other mixed types. Finally, I also use the average district magnitude as increases here should also foster multi-party competition.¹

I also pull two variables from other datasets. First I use competition from Vanhanen's (2000) dataset, which simply provides the percentage of legislative seats held by all but the largest party. This should provide similar, though still slightly different information to the effective number of party indices above. Finally, I use the variance on the main left-right dimension within each country-election cycle from the Comparative Manifestos Project Budge and Bara (2001), Klingemann et al. (2007). This should provide information not on the number of parties, but on the relative

¹There are majoritarian systems with average district magnitudes greater than one, namely Japan, Mali, Mongolia and Thailand. However, the vast minority of majoritarian systems have single-member districts.

dispersion of political parties across the ideological space. Systems that have a wider spread of parties should be more likely to have parties that accommodate a wide range of political views. If this is true, then people who might otherwise be driven toward violent dissent could have vehicles to address their concerns within the political system.

I estimate the same type of model as above - a Bayesian measurement model. One interesting feature of these data are that they tend to basically be constant within country-election cycle. This has implications for statistical models that I will address later, but it also has implications for the measurement model. The measurement model should be carried out on the data using country-election cycle as the unit of observation. In the data at hand, this leaves roughly 203 observations covering 41 countries with an average of 5 elections in each country. Once computed, the posterior means can be merged back in to either country-year or country-election cycle data as required.

Five of these variables are essentially continuous - effective number of electoral parties, effective number of parliamentary parties, the average district magnitude, variance of the left-right dimension and the competition measure from Vanhanen. Further, these are all constrained to positive values, but do not necessarily take integer values. Thus, I use the natural logarithm of these variables in the measurement models to put their values, at least theoretically, on the whole real number line. Electoral system is an unordered categorical variable with four possible values. I estimate the following equations in the measurement model:

$$Y_i^C = \lambda_{1,j} + \lambda_{2,j} \text{Inclusion}_i + \varepsilon_{ij} \quad (6.1)$$

$$\Pr(Y_i^P = m) = \frac{e^{\gamma_{1,m} + \gamma_{2,m} \text{Inclusion}_i + \varepsilon_{ij}}}{\sum_m e^{\gamma_{1,m} + \gamma_{2,m} \text{Inclusion}_i + \varepsilon_{ij}}} \quad (6.2)$$

where Y_i^C denotes the natural log of the $j = 5$ continuous variables mentioned above

and Y_i^P represents the polychotomous, unordered electoral system variable. Some identifying restrictions must be placed on the model. Specifically, these are that $\lambda_{1,1} = 0$, $\lambda_{2,1} = 1$ and that $\gamma_{1,1} = \gamma_{2,1} = 0$. The first two constraints work to identify the scale of the latent variable and the last restriction identifies the reference category in the multinomial logit model.

The priors in the model are also as above. The coefficient estimates, the $\lambda_{1,j}$, $\gamma_{1,j}$ and $\gamma_{2,j}$ were all given unconstrained normal priors with zero mean and variance of 10. The $\lambda_{2,j}$ parameters were given similar normal priors that were constrained to be positive. The residual variances for the continuous variables were given $\mathcal{IG}(1, 1)$ priors. The model appeared to have converged rapidly² and the results below are from the last 1000 iterations of two chains after 10,000 burn-in iterations.

Table 6.1 shows the results of the measurement model. Some variables are reasonably good indicators – namely, average district magnitude and the number of political parties. The values in the PRE column show the proportional reductions in error owing to the measurement model. Most of these are reasonable (in the 0.2-0.3 range). While these do not show especially strong inter-relationships between these variables, there is certainly some common variation that the model is picking up.³ The one variable that really does not belong is the variance on the left-right dimension. Apparently there is little relationship between inclusion as measured by the other variables, and the spread of political parties on the main dimension of political competition. There is, however, a considerable amount of research criticizing the Manifesto Research Group’s method for estimating placements on the main dimen-

²Both the visual diagnostics and the Brooks, Gelman and Rubin diagnostic suggested convergence on all parameters.

³An eigen-decomposition of the correlation matrix of the five continuous variables has eigenvalues of 2.88 and 0.99 for the first and second dimension suggesting only one underlying dimension to these data.

sion of competition (e.g., Bakker, Edwards and Netjes 2006, Laver, Benoit and Garry 2003) suggesting that while some result may exist on the “true,” dimension, it fails to obtain on these estimates. Since the manifesto data are mostly noise with respect to this model, I estimate it again removing this variable. These results are in the right-most columns of Table 6.1. The results here are quite similar to those discussed above.

6.1.2 Measuring Violent Dissent

The data on violent dissent is considerably worse than data on any other single piece of the puzzle here. There are good country or regional datasets. For example, Ron Francisco has data on some European democracies, but truncates both the spatial and temporal dimensions of this study. The World Handbook of Political and Social Indicators IV Jenkins and Taylor (2002) has an interesting set of events data for a wide range of countries, but only for the years 1991-2000. In the interest of retaining as many observations as possible, I choose to use information from a couple of different datasets. First, I use data from the Cross National Time-Series Data Archive (Banks 2001) pertaining to the number of general strikes, riots and anti-government demonstrations. These are counts of the identified activities ranging from zero (the modal category for each) to generally somewhere in the low-teens. I recode these such that six represents values greater than or equal to six.⁴

There has been a growing interest with respect to the effect of terrorist actions on internal state politics and repression Enders and Sandler (e.g., 1993). I use a combination of the International Terrorism: Attributes of Terrorist Events (ITERATE) data (Mickolus et al. 2003) and data from the Memorial Institute for the Prevention of Terrorism (Memorial Institute for the Prevention of Terrorism 2006) to code terrorist events. Specifically, countries are coded as to whether they had terrorist events

⁴I did this to avoid having the findings driven by a relatively small set of outliers.

at all, whether they had terrorist events that generated injuries and whether they had terrorist events that generated fatalities. To incorporate both sources of data, I simply used the maximum value of events, injuries and fatalities across the two datasets for each year. Since I am more concerned with the presence or absence of events, rather than the aggregate counts of fatalities, injuries or events, this seemed a reasonable strategy.

To calculate the posterior means of violent conflict, I used the following equations:

$$Y_{i,j}^C \sim \text{Poisson} (e^{\lambda_{1,j} + \lambda_{2,j} \text{Violence}_i}) \quad (6.3)$$

$$Y_{i,j}^B \sim \text{Bernoulli} \left(\frac{1}{1 + e^{\lambda_{1,j} + \lambda_{2,j} \text{Violence}_i}} \right) \quad (6.4)$$

where $Y_{i,j}^C$ refers to the $j = 3$ count variables (riots, anti-government demonstrations and general strikes) and $Y_{i,j}^B$ refers to the $j = 3$ binary terrorist events. Just as above, $\lambda_{1,1} = 0$ and $\lambda_{1,2} = 1$ were imposed as identifying restrictions. The other $\lambda_{1,j}$ parameters were given unconstrained normal distributions with zero mean and variance of 10. The other $\lambda_{2,j}$ parameters were given normal priors with zero mean and variance 10, but were constrained to be positive to prevent an equally good mirror image solution from appearing. The model appeared to have converged relatively quickly, with the results below resulting from the last 1,000 iterations from two chains after 10,000 burn-in iterations.

The results of the measurement model are in Table 6.2. These results are relatively encouraging. They are encouraging because they suggest that the violent activities, such as riots and anti-government demonstrations are better predicted by this dimension than what might be seen as more legitimate, non-violent activity such as general strikes. Further, the violent terrorist activities are also all well-described by the latent variable with proportional reductions in error of around 50%. This provides strong evidence that higher values on the violence variable will relate to situations where

security is, indeed, in peril.

Figure 6.1 provides a sense of the variance of these data across countries and over time. As you can see, there is considerably more within-country variation here than on any of the other indicators. A simple variance-decomposition suggests that about 35% of the variance can be attributed to between-country variation and the remaining 65% is within-country variation.

6.2 Violent Dissent and Inclusiveness

Now, I can use the measures developed above to test theories about violent dissent and political inclusiveness. The simplest one suggests that inclusiveness suppresses violent dissent. To test this, I simply regress violent dissent on inclusiveness. The structure of the data, however, requires a multilevel model as inclusiveness only varies by country-election cycle. Thus, I estimate the following set of equations:

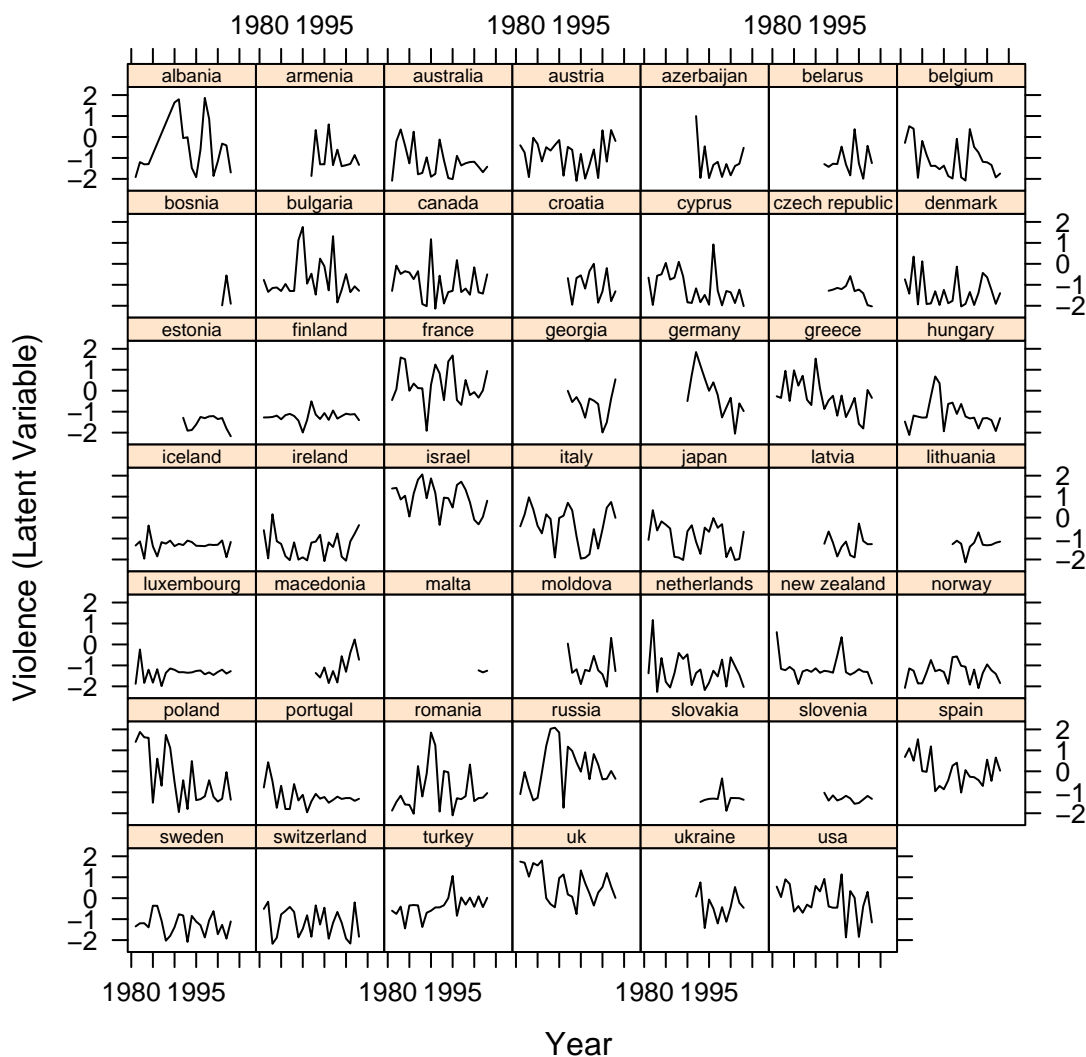
$$\text{Repression}_{ij} = \alpha_i + \varepsilon_{ij} \tag{6.5}$$

$$\alpha_i = \gamma_{00} + \gamma_{01}\text{Inclusion}_i + \nu_i \tag{6.6}$$

where j indexes country-years within country-elections and i indexes country-election cycle. This allows appropriate hypothesis testing of the inclusion variable given that there are only i independent pieces of information and not N . The model provides direct estimates of σ_ν and σ_ε as well as empirical Bayes estimates of the parameters ν_j . The expectation here is that as inclusion increases, violent dissent should be lower. Since violent dissent and inclusion are both unobserved, I apply the strategy discussed in footnote 3 (page 59) to propagate the latent variable uncertainty through the predictive model.

Table 6.3 shows the parameter estimates from the model. Here, inclusion is a significant predictor of violent dissent in the manner expected by the hypothesis. This

Figure 6.1: Violent Dissent Across Space and Time



Note: The lines represent violent dissent in each country over time. In the interest of presenting the information as clearly as possible, I have omitted measures of uncertainty.

confirms the first part of the analysis suggested in the introduction to this chapter - namely that violence must be a function of inclusion. Inclusion ranges from around -0.7 to 2.5. Repression has a range of around 11, so moving inclusion across its range will be accompanied by a change of around 8 or 9% of the range of repression. This

is not a huge effect, but is it respectable.

It is also worth estimating a model that assumes a two-level structure with country at the second level. Then it is possible to investigate whether there is any within country relationship between violence and inclusion. Thus, I estimate the following model:

$$\text{Repression}_{ij} = \alpha_{ij} + \beta \text{Inclusion}_{it}^c + \varepsilon_{ij} \quad (6.7)$$

$$\alpha_i = \gamma_{00} + \gamma_{01} \overline{\text{Inclusion}}_i + \nu_i \quad (6.8)$$

Here, Inclusion_{it}^c represents inclusion that has been demeaned at the country level. The results in Table 6.4 show that the relationship is largely cross-country in nature. The sign of the relationship remains the same, but it is not statistically significant within-country. The relationship is still statistically significant between countries.

6.3 Violent Dissent and Repression

Now, for the second piece of the puzzle - is repression effected by violence. Since the data here are arranged by country-year and, at least theoretically, both variables can vary over country and year, then a multilevel model seems appropriate here, too. I start by estimating a relatively simple model regression repression on violence. Specifically, I estimate the following:

$$\text{Repression}_{it} = \alpha_i + \beta_i \text{Violence}_{it} + \varepsilon_{it} \quad (6.9)$$

$$\alpha_i = \gamma_{00} + \gamma_{01} \overline{\text{Violence}}_i + \nu_{i,1} \quad (6.10)$$

$$\beta_i = \gamma_{10} + \nu_{i,2} \quad (6.11)$$

Here, the random intercept is predicted by the average level of violence in each country. This is basically allowing different between-country and within-country relation-

ships to reveal themselves. I also tested to see whether β_i varied as a function of the mean country-level violence, but there was no evidence in support of that specification.

Table 6.5 presents the results of this model. The evidence is less convincing on this front. While this might be surprising theoretically, the surprise fades quickly when looking back at the veto player results. There, it was made clear that repression was not all that variable over time within each country. The results here basically show that it is between variation that is causing the relationship. That is to say, the relationship between the mean of violence and the mean of repression is quite strong. However, there is relatively little evidence of within-country relationship between violent dissent and repression.

There is, then, some evidence in favor of the relationship suggested by the conflict hypothesis. Specifically, countries with more inclusion do, on average, have less violent dissent. Further, countries with higher levels of violent dissent do, on average, have more repression. Now, it will be interesting to see whether the effect of inclusion is truly through violent dissent, or whether it also works more in a cross-country fashion. To do this, I estimate the parameters of the following model:

$$\text{Repression}_{it} = \alpha_i + \beta_{1,i}\text{Violence}_{it} + \varepsilon_{it} \quad (6.12)$$

$$\alpha_i = \gamma_{00} + \gamma_{01}\overline{\text{Violence}}_i + \gamma_{02}\text{Inclusion}_i + \nu_{i,1} \quad (6.13)$$

$$(6.14)$$

This will show the extent to which the mean repression in each country is a function of the mean violence and mean inclusion and the within-country effect of violence. The results in Table 6.6 show that including both variables makes both statistically insignificant. This shows that whatever relationship exists between the means of these variables and country-level repression is fragile. Further, the within-country rela-

tionship between violence and repression remains both statistically and substantively weak. With the addition of political inclusion, both violence and political inclusion coefficients become insignificant. This provides at least some evidence to the contention that political inclusion’s effect (on top of being largely between-country) is through violence, rather than direct.

Finally, I can put these results back in the context of the bigger model as was done with voice. Again, I estimate a multilevel model with country-year as the level-one unit of analysis and country as the level-two unit of analysis. Specifically, I estimate the following model:

$$\text{Repression}_{it} = \alpha_i + \beta_{1,i}\text{GDP/capita}_{it} + \beta_{2,i}\log(\text{Population})_{it} + \varepsilon_{it} \quad (6.15)$$

$$\alpha_i = \gamma_{0,0} + \gamma_{0,1}\overline{\text{Violence}}_i + \gamma_{0,2}\text{Entry} > 1980_i + \nu_{i,0} \quad (6.16)$$

$$\beta_{1,i} = \gamma_{1,0} + \gamma_{1,1}\text{Entry} > 1980_i + \nu_{i,1} \quad (6.17)$$

$$\beta_{2,i} = \gamma_{2,0} + \nu_{i,2} \quad (6.18)$$

Here again, the second level effects are somewhat complicated. In general, they retain the structure of above, with one change. Instead of the country-mean of salience in the second level, I include the country-mean of violence. Aside from the exclusion of salience at the individual-level, the rest of the model is the same as in equation 4.10.

Table 6.7 shows the results from this regression. Unlike the ones undertaken with voice, many of the results seem to not hold in the new models. The main result of interest is the coefficient on violence. It remains positive, though it is not statistically significant when included with all of the other variables. Even the country-level result is essentially gone here. The only significant factor here is the entry variable which significantly increases the intercept. Substantively, it suggests that the new democracies are considerably more repressive than older ones. This result remains significant, just as it was before.

This chapter shows that there is certainly a relationship between the mean levels of inclusion and the mean level of violence, but it does not suggest that the relationship goes much further than that. Again, even with violence, a variable that really varies over country, and in a way that would be expected to influence repression, the relationships remain generally cross-country in nature. When the variable is included in a multilevel model which respects the structure of the data, the main finding of this chapter essentially disappears, though the coefficient remains in the right direction.

Table 6.1: Results of Inclusion Measurement Model

Variable	Coefficient	PRE	Coefficient	PRE
Effective Number of Electoral Parties	1.000 NA	0.20	1.000 NA	0.17
Effective Number of Political Parties	0.263 (0.189, 0.34)	0.33	0.257 (0.185, 0.332)	0.29
Average District Magnitude	1.712 (1.474, 1.975)	0.95	1.676 (1.451, 1.924)	0.96
Variance of Left-Right (CMP)	0.112 (0.005, 0.282)	0.01		
Competition (Vanhanen)	0.085 (0.053, 0.121)	0.24	0.085 (0.052, 0.117)	0.23
Electoral System:		0.24		0.25
Proportional vs. Majority	9.198 (6.13, 12.98)		8.914 (6.357, 11.67)	
Multi-tier vs. Majority	1.364 (-1.142, 3.766)		1.572 (-0.723, 4.168)	
Multiple vs. Majority	0.350 (-5.225, 5.642)		0.711 (-4.045, 5.682)	

Note: Main entries are coefficients from the models in equations 6.1 and 6.2. The entries in parentheses are the 95% credible intervals from the Bayesian model. Entries in the PRE column are proportional reductions in error. For the continuous variables this is simply the squared correlation between the observed variable and the measurement model predictions; for electoral system this is the proportional reduction in error owing to the latent variable model calculated as $\frac{E_{null} - E_{full}}{E_{null}}$.

Table 6.2: Results of Violence Measurement Model

Variable	Coefficient	PRE
Anti-Government Demonstrations	1.000	0.69
	NA	
General Strikes	1.306	0.22
	(1.182, 1.436)	
Riots	1.795	0.78
	(1.674, 1.936)	
Terrorist Fatalities	0.575	0.51
	(0.449, 0.709)	
Terrorist Injuries	0.8262	0.56
	(0.653, 1.011)	
Terrorist Events	0.8285	0.31
	(0.659, 1.019)	

Note: Main entries are coefficients from the latent variable model described by equations 6.3 and 6.4. Entries in parentheses are 95% credible intervals. Entries in the PRE column are R^2 for the count variables and for the binary variables this is the proportional reduction in error owing to the latent variable model calculated as

$$\frac{E_{null} - E_{full}}{E_{null}}$$

Table 6.3: Multilevel Model Results for Violent Dissent

Inclusion	-0.364
	(0.107)
Intercept	-0.508
	(0.108)
$\bar{\sigma}_\nu$	0.536
σ_ν : 95% CI	(0.416, 0.657)
$\bar{\sigma}_\varepsilon$	1.065
σ_ε : 95% CI	(0.982, 1.154)

Note: Main entries are multilevel model coefficients using the strategy described in footnote 3 (page 59). Entries in parentheses are standard errors. $\bar{\sigma}_\nu$ and $\bar{\sigma}_\varepsilon$ were obtained by taking the empirical mean of the parameters across $m = 100$ models. The 95% CI's for those parameters were obtained by computing the 2.5th and 97.5th percentiles of the parameters across the 100 models.

Table 6.4: Multilevel Model Results for Violent Dissent

Inclusion $_{it}^c$	-0.059
	(0.290)
$\overline{\text{Inclusion}}_i$	-0.365
	(0.147)
Intercept	-0.553
	(0.143)
$\bar{\sigma}_\nu$	0.484
σ_ν : 95% CI	(0.394, 0.577)
$\bar{\sigma}_\varepsilon$	1.094
σ_ε : 95% CI	(1.013, 1.171)

Note: Main entries are multilevel model coefficients using the strategy described in footnote 3 (page 59). Entries in parentheses are standard errors. $\bar{\sigma}_\nu$ and $\bar{\sigma}_\varepsilon$ were obtained by taking the empirical mean of the parameters across $m = 100$ models. The 95% CI's for those parameters were obtained by computing the 2.5th and 97.5th percentiles of the parameters across the 100 models.

Table 6.5: Multilevel Model Results for Repression as a function of Violence

Violence _{it}	-0.020
	(0.035)
$\overline{\text{Violence}}_i$	1.151
	(0.492)
Intercept	-0.528
	(0.449)
$\bar{\sigma}_{\nu 1}$	1.546
$\sigma_{\nu 1}$: 95% CI	(1.406, 1.700)
$\bar{\sigma}_{\nu 2}$	0.055
$\sigma_{\nu 2}$: 95% CI	(0.006, 0.132)
$\bar{\sigma}_{\varepsilon}$	0.564
σ_{ε} : 95% CI	(0.511, 0.625)

Note: Main entries are multilevel model coefficients using the strategy described in footnote 3 (page 59). Entries in parentheses are standard errors. $\bar{\sigma}_{\nu 1}$, $\bar{\sigma}_{\nu 2}$, and $\bar{\sigma}_{\varepsilon}$ were obtained by taking the empirical mean of the parameters across $m = 100$ models. The 95% CI's for those parameters were obtained by computing the 2.5th and 97.5th percentiles of the parameters across the 100 models.

Table 6.6: Multilevel Model of Repression as a function of Violence and Inclusion

Violence _{it}	-0.021
	(0.035)
$\overline{\text{Violence}}_i$	0.809
	(0.536)
$\overline{\text{Inclusion}}_i$	-0.621
	(0.413)
Intercept	-0.429
	(0.462)
$\bar{\sigma}_{\nu 1}$	0.568
$\sigma_{\nu 1}$: 95% CI	(0.512, 0.628)
$\bar{\sigma}_{\varepsilon}$	1.477
σ_{ε} : 95% CI	(1.378, 1.618)

Note: Main entries are multilevel model coefficients using the strategy described in footnote 3 (page 59). Entries in parentheses are standard errors. $\bar{\sigma}_{\nu 1}$ and $\bar{\sigma}_{\varepsilon}$ were obtained by taking the empirical mean of the parameters across $m = 100$ models. The 95% CI's for those parameters were obtained by computing the 2.5th and 97.5th percentiles of the parameters across the 100 models.

Table 6.7: Multilevel Model of Regression as a Function of Violence

Intercept	-2.297 (4.132)
$\overline{\text{Violence}}$	1.130 (0.839)
Entry > 1980	3.446 (0.939)
GDP/capita (by 1000)	0.044 (0.048)
GDP/capita \times Entry > 1980	-0.082 (0.056)
Log(Population)	-0.059 (0.407)
$\bar{\sigma}_{\nu 0}$	12.608
$\sigma_{\nu 0}$: 95% CI	(5.562, 21.762)
$\bar{\sigma}_{\nu 1}$	0.164
$\sigma_{\nu 1}$: 95% CI	(0.118, 0.210)
$\bar{\sigma}_{\nu 2}$	1.356
$\sigma_{\nu 2}$: 95% CI	(0.412, 2.657)
$\bar{\sigma}_{\varepsilon}$	0.399
σ_{ε} : 95% CI	(0.353, 0.451)

Note: Main entries are multilevel model coefficients using the strategy described in footnote 3 (page 59). Entries in parentheses are standard errors. $\bar{\sigma}_{\nu 0}$, $\bar{\sigma}_{\nu 1}$, $\bar{\sigma}_{\nu 2}$, and $\bar{\sigma}_{\varepsilon}$ were obtained by taking the empirical mean of the parameters across $m = 100$ models. The 95% CI's for those parameters were obtained by computing the 2.5th and 97.5th percentiles of the parameters across the 100 models.

Chapter 7

Conclusion: Putting it Together

Now, it is time to consolidate the findings from above. In each of the three main chapters, I focus on one particular relationship - voice, veto and conflict. I demonstrated beyond a reasonable doubt that the effects of each of these variables, to the extent that any exists at all, is in the form of between-country relationships rather than within-country relationships. The potential exception is with salience, though the multilevel regressions suggested that the within-country result was largely an artifact of between-country variation.

It is at this point that one has to think carefully about how these interdependent pieces - violence, voice and veto ought to fit together. Davenport (2007*b*) suggests that the effects of voice and veto should be conditional on violence. Though he notes that the variables representing voice and veto have variable effects in the presence and absence of 1) other democracy variables and 2) conflict, the general trend is that the pacifying effects of democracy decrease in the presence of large-scale conflict. This is consistent theoretically with the findings of Davis (2007), Davis and Silver (2004).

This suggests a statistical model where both the country and individual effects of salience and violence are in the model and only the individual effect of demeaned veto is in the model. However, after some preliminary estimation, only the country-level effects of violence and salience weather the tests of statistical significance. Further, there were problems estimating a random slope on veto that was a function of violence.

These models tended not to converge and I take this as a sign that this model, though theoretically interesting is not statistically interesting. I further allowed an interaction between violence and salience, but this, too, showed little in the way of statistical evidence justifying its inclusion. After the winnowing down, I estimate the following model:

$$\text{Repression}_{it} = \alpha_i + \beta_{1,i}\text{GDP/capita}_{it} + \beta_{2,i}\log(\text{Population})_{it} + \varepsilon_{it} \quad (7.1)$$

$$\alpha_i = \gamma_{0,0} + \gamma_{0,1}\overline{\text{Violence}}_i + \gamma_{0,2}\overline{\text{Salience}}_i + \gamma_{0,3}\text{Entry} > 1980_i + \nu_{i,0} \quad (7.2)$$

$$\beta_{1,i} = \gamma_{1,0} + \gamma_{1,1}\text{Entry} > 1980_i + \nu_{i,1} \quad (7.3)$$

$$\beta_{2,i} = \gamma_{2,0} + \nu_{i,2} \quad (7.4)$$

The results of this model are in Table 7.1. These confirm some major hypotheses and disconfirm others. Briefly, these results relate in the following way to the literature:

- These results confirm the idea that voice (electoral accountability) matters, however they suggest that citizens may be as often “voting the bastards in,” as they are “voting the bastards out.”
- Violence matters - in fact, violence has an enormous and significant effect on repression. However, it does not, as the literature suggests, condition the effects of voice.
- Violence is, itself, predicted by political inclusion, which suggests that political inclusion has an indirect effect on repression through its pacifying effect on political dissent. The investigation showed no evidence of a direct effect of political inclusion on repression.
- Veto has no real impact on repression – especially when analyzed as suggested by Tsebelis (2002).

- All of the effects of democracy are between-country effects. None of these had significant within-country effects.

I say something about each of these results in turn.

Davenport (2007*b*) found voice to be the most consistent democratic pacifier. These results confirm that conclusion, though in quite a different way than intended. The literature to date has generally implied that citizen preferences for repression will be lower than state preferences for repression. As such, electoral accountability will force leaders to choose less repression than they would left to their own devices. The results here suggest that repression rises and falls with popular support for the same. Leaders, rather than always existing on the boundary of acceptable behavior, follow the citizens' demands for repression. Those demands were shown to be a function of the security environment, so security has an indirect effect through salience.

Violence does, indeed have a significant effect on repression, both substantively and statistically. There are two particularly interesting things about this effect. First, the significant effect comes between countries rather than within them. This suggests that countries that are more violent on average have more repression on average. There is no evidence of within country variation. That is to say, it is generally not the case that changes in violent behavior generate changes in repression. Well, this statement may be a bit disingenuous. The fact is that repression, as measured by the CIRI variables (though there is no reason to believe that things would be significantly different using different aggregate data) is simply not sensitive enough to pick up over-time variation. In fact, of the 44 countries used across this analysis, there were only a handful that saw statistically significant over-time changes in their repression scores. So, rather than being completely busted, the jury remains out on this proposition.

The second important finding with respect to violence is that it does not condition the effects of the other democracy variable(s). Across this investigation, there was no evidence that democracy factors worked more or less in situations of threat. This is

Table 7.1: Multilevel Model of Regression as a Function of Violence

Intercept	-6.480 (2.504)
$\overline{\text{Salience}}$	0.165 (0.082)
$\overline{\text{Violence}}$	1.475 (0.487)
Entry > 1980	3.403 (0.765)
GDP/capita (by 1000)	0.093 (0.033)
GDP/capita \times Entry > 1980	-0.118 (0.056)
Log(Population)	0.323 (0.238)
$\bar{\sigma}_{\nu 0}$	3.914
$\sigma_{\nu 0}$: 95% CI	(0.000, 10.478)
$\bar{\sigma}_{\nu 1}$	0.136
$\sigma_{\nu 1}$: 95% CI	(0.096, 0.184)
$\bar{\sigma}_{\nu 2}$	0.496
$\sigma_{\nu 2}$: 95% CI	(0.196, 1.128)
$\bar{\sigma}_{\varepsilon}$	0.368
σ_{ε} : 95% CI	(0.333, 0.404)

Note: Main entries are multilevel model coefficients using the strategy described in footnote 3 (page 59). Entries in parentheses are standard errors. $\bar{\sigma}_{\nu 0}$, $\bar{\sigma}_{\nu 1}$, $\bar{\sigma}_{\nu 2}$, and $\bar{\sigma}_{\varepsilon}$ were obtained by taking the empirical mean of the parameters across $m = 100$ models. The 95% CI's for those parameters were obtained by computing the 2.5th and 97.5th percentiles of the parameters across the 100 models.

likely for different reasons for the different democracy variables. First, for veto, the lack of variation could be the culprit here. For salience, it could be because this is a

behavioral measure. This is in contrast to suffrage or competition or something like that. These measures do not change much over time. Thus, the effectiveness of these measures needs to be “activated,” by some external event, like violence. Salience tracks how activated the constituency is on the matter of repression, so to the extent that salience is a reflection of the security environment, there is no need to condition on the security environment again.

Violence is predicted by political inclusion. This proposition is one of the older ones in the empirical literature, relating back at least to Henderson (1991). Again, however, the effect of inclusion seemed to be across countries rather than within. Countries with inclusive policies see, on average, less violent dissent. This lower-than-average violent dissent translates directly into lower-than-average repression as suggested by the previous point. The lack of within-country relationships here is not surprising.

I argued above that veto player analysis as it is generally undertaken in this literature has been done inappropriately. Tsebelis (2002) suggests that only within-country comparisons can be made and that one country with three veto players should not necessarily have less policy stability than a country with 4. Or, that the policy stability may not be owing to the configuration of veto players. Thus, the investigation undertaken above showed that both with respect to the number and position of veto players that no interesting relationships emerged. As it turns out, in the final model even the cross-country variation in veto players came up insignificant. Here, I think the lack of cross-country variation is not a function of imprecise data, rather it is a function of the glacially slow changes in institutional structures in consolidated democracies. Thus, to profitably analyze the veto-player environment, we will need a much longer time-series with greater within-country variation.

I have shown through rigorous measurement and appropriate analysis that many of the things we thought we knew about the democracy-repression nexus were not

borne out by the data. Further, the logic behind some of the relationships is actually counter to the conventional wisdom. This work will provide a basis for further work with more sensitive data to test more nuanced theories about democratic pacification.

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