Judicial Independence and the Democratic Order

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Abstract

Do legal institutions that limit governmental power promote order in democracies? Although the worldwide rule of law project predicates its reform efforts on knowing the answer to this question, we are aware of no systematic empirical test. Further mainstream literatures on democratic survival and civil conflict simply ignore the role of law in promoting order. In this paper we suggest how models of law can be linked to political economies of order. On this account, independent courts help police fundamental democratic compromises by addressing problems of monitoring and coordination. Empirically, we find that independent courts encourage order, but especially so when macroeconomic conditions favor inter-class compromise and when natural resources incentivize predation. The key implication is that though judicial reform may ultimately help stabilize regimes in some contexts, in others, its effect on order will be small and indirect at best.

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In Bosnia, we thought that democracy was the highest priority and we measured it by the number of elections we could organize. In hindsight, we should have put the establishment of rule of law first, for everything else depends on it: a functioning economy, a free and fair political system, the development of civil society, and public confidence in police and courts. We should do well to reflect on this as we formulate our plans for Afghanistan, and, perhaps, Iraq.
　　　--Paddy Ashdown, High Representative for Bosnia and Herzegovina, 2002-2006

Introduction

Does judicial independence promote order under democracy? Scholars, international organizations, non-governmental organizations, and the large number of states that promote judicial independence and the rule of law predicate their efforts on knowing the answer to this question.[2] And the answer is, of course, that judicial independence does promote order, and it does so in a substantial way. Despite the uplifting implications of this answer, it is derived largely from anecdotal evidence, and there are several research design challenges, which raise questions about its accuracy.

The primary challenges in our view are theoretical. First and foremost, mainstream scholarship on order in democratic states basically ignores judges and the courts on which they sit.[3] Democratic regime failures are explained primarily by models of economic redistribution, where exogenous features of the macro-economy (e.g. development, growth, asset specificity) influence the feasible tax burdens states can place on their citizens and thus affect incentives for regime challenges (e.g. Acemoglu and Robinson 2006; Przeworski 2005); or, by models of sociocultural conflict (Lijphart 1969, 1977, 1999), where the

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[2] Among many examples, see the American Bar Association Rule of Law Initiative (http://www.abanet.org/rol/) and the United Nations High Commissioner for Human rights, Special Rapporteur on the Independence of Judges and Lawyers (http://www2.ohchr.org/english/issues/judiciary/index.htm).

[3] Democracy scholars have acknowledged the theoretical importance of the "rule of law" for democratic stability and have considered its role in a variety of conceptual and case study treatments (e.g. Elster and Slagstad 1993; Linz and Stepan 1996; Maravall and Przeworski 2003; O'Donnell 1998). Yet there is a curious absence of published, large-N studies of democratic regime survival or political disorder that incorporate the quality of legal institutions in their analysis. Indeed, we are only aware of two other working papers on the subject. Gibler, Randazzo and Walsh (2010) find that established independent judiciaries reduce the likelihood of democratic reversals. Esarey and Sarkari's (2010) results suggesting that the rule of law attenuates the negative relationship between ethnic heterogeneity and instability. We return to these papers and the methodological challenges associated with their designs below.

distribution and structure of religious or ethnic cleavages influence conflict propensities.[4] In so far as the regime survival literature considers institutions, it centers on rules that aggregate votes and divide law-making power (e.g. Bernhard, Nordstrom and Reenock 2001; Cheibub 2007; Linz 1994). What is more, civil conflict scholarship almost completely ignores the law. Indeed, Nixon's (2009) review of forty-six published studies and over 200 possible causal variables, reports literally nothing about the influence of legal institutions on civil war.

The theoretical inattention to judicial independence in the mainstream literature is alarming, but it is not as if the rule of law project is completely unhinged from theory. There is considerable agreement among neoinstitutionalists that courts, which protect property rights, encourage economic development by rendering rights credible (Acemoglu, Johnson and Robinson 2001; Barro 1997; North 1990). Extending this logic, North, Summerhill and Weingast argue that before a state can attend to how its predatory instincts influence growth, it must focus on how predation undermines political order (North, Summerhill and Weingast 2000, 17). The reason is that order, by which they mean a state of society in which individuals do not fear for their lives and their sources of livelihood, is a precondition for growth. If holding political power is the only means of ensuring the safety of one's assets, and worse, if power offers significant opportunities for enrichment, then a democratic regime will be unstable. But here again, judiciaries that enforce limits on the state, render governmental promises to respect assets credible, lower the stakes of holding power and thus induce order.

Thus, the institutional tradition provides the rule of law community with a natural theoretical basis for the promotion of judicial reform. Independent courts solve governmental commitment problems by rendering rights credible. Nevertheless, North, Summerhill and Weingast's account does not offer a precise mechanism by which credibility is linked to the maintenance of order. It is certainly convincing that inducing the credibility of fundamental limits on power would lower the stakes of controlling the state, but what is not obvious is how independent courts induce credibility. To put a fine point on it, how

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[4] It is worth noting that the common result in these studies linking economic development to regime survival involves an implicit role for legal institutions, since institutions that protect property rights promote growth (see text).

is it exactly that independent judges render state promises self-enforcing? This is especially problematic in light of the considerable work in judicial politics, which reminds us that judicial decisions themselves are not self-enforcing, so that even if judges resolve conflicts independently of state interests, it is far from clear that their decisions will be implemented faithfully (e.g. Rosenberg 1991; Vanberg 2005).

Beyond these theoretical questions, two difficult empirical challenges remain. First, assuming that there is a satisfactory theoretical account linking judicial independence to order in democracy, we will require a measure of judicial independence that is both available over a decent time series and conceptually and empirically distinct from the concept of order itself. As we describe below, these conditions are not easily met, and recent research on the connection between judicial independence and democratic stability (Gibler and Randazzo 2010), while encouraging in a number of ways, does not satisfy them. Second, even if we can identify valid indicators, we confront the obvious possibility that judicial independence is endogenous to political order – specifically, judges are more likely to behave independently under conditions of order.

In summary, mainstream theoretical scholarship on order under democracy raises important questions about whether courts are anything more than epiphenomenal; and, research in judicial politics, which highlights the essential weaknesses of courts, only serves to underline the point. If we are to develop a satisfactory theoretical account of the link between judicial independence and order, we are likely going to have say something about how judicial independence might influence mainstream accounts, and we must do so in a way that addresses the judicial enforcement problem directly. Further, once we identify a satisfactory argument, which suggests clear empirical implications, we will have to address a number of measurement and inference challenges. As we discuss below, solutions to these challenges are not exactly straightforward or without their own concerns. That said, the challenges can be met, and in so far as they are not overcome perfectly, the process of addressing them highlights a number of fruitful goals for future scholarship.

We divide the remainder of this paper as follows. We begin by articulating how existing models of judicial politics can compliment a mainstream model of order. We suggest that courts, via their

monitoring function and their related capacity to coordinate group behavior, can facilitate the enforcement of precisely the kind of political agreements that sustain order in standard political economy models. Importantly, we do not assume that courts directly enforce major political compromises. Judges in the accounts we discuss are incapable of enforcing their own decisions. What they can do is influence how actors with the power to enforce manage to do so. We frame this discussion around Przeworksi's (2005) model of political conflict. We do so because Przeworski provides a tight theoretical explanation for one of the most well-known empirical findings in the literature on democratic regimes – the positive relationship between economic development and democratic survival (Przeworski et al. 2000). We then evaluate the empirical implications of our argument against data on regime survival and violent political conflict, confronting the measurement and endogeneity challenges directly. Across a very wide array of statistical tests, we find that judicial independence enhances order in democracy, but that this effect is strongest in both 1) states that have reached a minimal threshold of development and 2) states that have highly specific assets. Importantly, in very underdeveloped states, there is no evidence that judicial independence stabilizes democratic regimes. We conclude by considering the implications of our results for reform.

Development, Order, and Credible Commitments

Why are some democracies more stable than others? The literatures' primary empirical finding suggests a positive relationship between economic development and order (Lipset 1959; Przeworski et al. 2000). Nevertheless, there has been considerable debate over the explanation (e.g. Acemoglu and Robinson 2006; Boix 2003; Boix and Stokes 2003; Powell 1982; Przeworski 2005; Rueschemeyer, Stephens and Stephens 1992). Providing a precise mechanism linking development to democratic survival is Przeworski's (2005) goal, and so we begin our analysis there. As it turns out, the model offers a useful platform for wedding scholarship on legal institutions to a mainstream model of order. To appreciate how so, it is useful to summarize the model in detail.

Democracy as an Equilibrium

Przeworski considers a two party election in which opponents compete for the support of voters, who fall within one of three social classes (wealthy, middle, poor). Party platforms reflect promises over future redistribution, which will be implemented in the event of a victory.[5] Members of the poor and middle classes have pre-transfer income levels, which are below average, and thus prefer some degree of redistribution; the wealthy have pre-transfer incomes above average, and thus do not. No class represents a majority of the population, so parties must build coalitions in order to win elections.

Following the outcome of an election, both parties choose whether to accept the result or challenge the regime violently. If both accept the election, production ensues, the redistribution promise of the winning party is implemented, and income is realized. If either party rebels, there will be conflict, the outcome of which is determined probabilistically.[6] A conflict results either in dictatorship, where future income is shared among the victors only, or in the survival of the democracy. In any equilibrium in which democracy is respected (a democratic equilibrium), the redistribution platforms of the two parties converge on the same level, a level which will be higher than that preferred by the wealthy and lower than that preferred by the poor.[7] Given these promises, both parties confront a choice between accepting a suboptimal level of redistribution and challenging the regime. Although imposing a dictatorship offers considerable spoils, conflict is risky and costly. For this reason, the core tradeoff in the model asks the parties to consider whether life under democracy and its inherent compromise is valuable enough to prevent them from taking a gamble on instituting an autocracy.

Two constraints must be met in order for a democratic equilibrium to hold. Redistribution policies must be 1) low enough to constrain the party of the right from rebelling, yet 2) high enough to

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[5] Voters within a class share identical preferences and are assumed to select the party whose redistribution level provides the highest income. Personal income is a function of individual resources and the net transfer from the government, which is itself determined by the redistribution rate and the shadow cost of public funds.

[6] This is true because if one party engages in conflict, it is always a best response for the other to reciprocate.

[7] If the median voter's optimal level of redistribution lies within the wealthy and poor's rebellion constraints, discussed in the subsequent paragraph, convergence will be at the median. Otherwise, the parties will converge on one of the two rebellion constraints, in order to keep the polity from falling into conflict. If it is impossible to simultaneously satisfy the wealthy and poor (i.e. the highest level the wealthy will accept is lower than the lowest level the poor will accept), conflict ensues.

constrain the party of the left from doing the same. In some contexts, it will be impossible to satisfy these constraints simultaneously. The rich will require too little redistribution and the poor too much. In other contexts, the parties can find a level that will be acceptable to both extremes. Any promise within this interval reflects an implicit compromise between the right and the left. The key theoretical result, for our purposes, is that *as development increases, it becomes increasingly easy to satisfy the rebellion constraints of both parties* (Prezworski 2005, 261-262). At higher levels of development, the poor (rich) can be satisfied with lower (higher) levels of redistribution. Conflict simply risks losing too much for both parties as the overall economic pie increases. This is the explanation for the link between development and democratic regime survival.

The Leader's Commitment Problem

Przeworski's democratic equilibria[8] all assume the resolution to a commitment problem over the adoption of campaign promises. The relevant questions are as follows. What keeps a victorious party from violating the democratic compromise once the losing party has accepted the election result and it has taken over control of the state's resources? Why not wait, impose a dictatorship, redistribute optimally, and capture the personal spoils of autocratic rule? In some settings the incentives to do so may be especially strong. This is most obvious is where factor endowments (e.g. large oil or mineral reserves) offer leaders relatively easy access to considerable rents (Boix 2003; Collier and Hoeffler 2000; Fearon and Laitin 2003; Humphreys 2005).

History certainly affords examples of considerable deviations from seemingly fundamental campaign promises. In 1989, Carlos Menem promised on the campaign trail a massive upward shock to wages. Once in office, he included former right-wing competitors in his cabinet, took on labor unions, and generally embraced the Washington Consensus. Alberto Fujimori ran his 1990 campaign on the basis of the "antishock" against the conservative novelist Mario Vargas Llosa, who advocated financial reform. But then "less than two weeks after [Fujimori's] inauguration . . . [he] imposed one the largest packages

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[8] Technically, there are several cases in which democracy can be sustained as an equilbrium. The difference across each involves whether the equilibrium transfer rate is bound to one of the rebellion constraints (because the median voter's ideal level is outside the feasible set) or finds the median voter's ideal.

of price adjustments in Latin American history, followed by the full menu of neoliberal reforms (Stokes 1999, 98-99). Indeed, Stokes (2001) reports that 41% (12 out of 29) of winning presidential candidates in Latin America between 1982 to 1995, who had campaigned against neoliberal reforms nevertheless implemented them once in office! More ominously, experiences in Germany, India, Peru, Venezuela among many other states, remind us that democratically elected leaders sometimes simply impose dictatorships from above.

Although not modeled explicitly, one natural answer to the leader's problem is that losing parties continuously enforce campaign promises via the implicit threat of rebellion. A simple revision to Przeworski's model might allow the losing party to rebel after the government has actually implemented a policy.[9] This would induce faithful implementation whenever the electoral outcome is mutually acceptable. By allowing the parties to enforce the implementation of their own bargains, credibility is created, and thus we have an explanation for order that excludes the judiciary, independent or otherwise.

Common features of policy implementation politics, however, complicate the story in ways that suggest why judicial independence might matter. Not all major deviations from democratic compromises are directly observable or free from contention. Given a large enough population and a large enough bureaucracy, it is possible to subvert stated commitments to major policy initiatives in ways that are not immediately obvious. Further, minorities might even support some deviations. It is certainly plausible to imagine that deviations occasionally reflect sensible reactions to exogenous changes in the underlying bargaining context, the implications of which might only be transparent to members of the state. Indeed, Fujimori's central explanation for the Peruvian neoliberal reform was that he did know the full extent of the state's financial crisis or the international lending community's position until he took office (Stokes 2001, 69-71). Finally, even if violations are unambiguous and perfectly observable, the model assumes

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[9] This is exactly the kind of dynamic Acemoglu and Robinson (2006) evaluate (in particular pp. 231-246). Unfortunately, they build their models so that the key results will be independent of economic development, thus rendering the model unhelpful for understanding the process Przeworski considers. They do consider a model (p. 290) in which the capital intensity of economic activity lowers feasible democratic tax burdens and the corresponding threats from the wealthy against democracy. In so far as capital intensity and development are related, then Acemoglu and Robinson generate nearly the same theoretical result as Przeworski. Nevertheless, monitoring of democratic policies is perfect in their model, and so the problems we discuss emerge there, as well.

(as Przeworski acknowledges on p. 268) that parties are both unitary actors and capable of mobilizing constituents. If we instead model parties as collections of individuals, who have to mobilize others, then the account of endogenous enforcement runs into the pathologies of group mobilization (Wood 2003; Weingast 1997).

Consequences for Order

What are the consequences of these salient features of the policy implementation process for the logic of democratic stability in the Przeworksi model? In answering this question, we are benefited greatly by the fact that two extant models of law address precisely the problems the model does not. Indeed, they take off exactly from the point Przeworksi ends. Carrubba (2005) considers a non-cooperative game of political compromise, which is in effect a repeated, prisoner's dilemma, in which the players imperfectly observe the costs of cooperation paid by their opponents. These costs are drawn independently and identically each period, so that in some periods mutual cooperation is efficient, whereas in other periods it is not (2005, 672). In turn, Weingast (1997) considers a game in which citizens confront a coordination problem, the solution to which is necessary in order to keep the sovereign from violating fundamental limits on its power. Although these models do not define precisely what it would mean for a government to "deviate from a cooperative strategy" or "violate a fundamental limit," at a very profound level, they seem to have in mind exactly the kind of choice that is available to the winning party in the Przeworski model after it takes office and the election has been accepted. As long as we are willing to conceptualize the faithful implementation of a redistribution policy within the feasible set of democratic compromises as a special case of "cooperative behavior" or "actions within the fundamental limits of state power," then these authors are writing about exactly the same phenomenon. For this reason, Carrubba and Weingast separately identify the consequences for order when imperfect monitoring and coordination dilemmas complicate the endogenous enforcement democratic compromises.

Carrubba's analysis suggests that if defections cannot be observed perfectly, then states may confront threats to order when they would otherwise not (p. 678). The reason is that in the absence of good information about the nature of an opponent's defection, parties are forced to use inefficient

punishment strategies – courses of action that prevent opponents from violating agreements opportunistically but induce serious conflict when agreements are violated for good reasons (e.g. when mutual cooperation would be inefficient). More alarmingly, the Weingast model suggests that if individuals cannot be successfully coordinated, the entire logic of endogenous enforcement of democratic bargains unravels, the result of which will be that democratic regimes will involve significant transgressions on fundamental state limits.

Courts as Monitors and Focal Points

Carrubba and Weingast also suggest how independent courts might resolve these problems. Carrubba argues that courts can serve as monitors of cooperative agreements by offering the parties definitive declarations on the nature of alleged breaches. By so doing, courts allow parties to implement efficient punishment strategies, which ensure that opponents do not opportunistically violate cooperative agreements (i.e. violate them when the costs of implementation are not particularly high) yet allow violations when the costs of implementation are too high. This mechanism has two relevant effects for our purposes. First, conflict is avoided because courts allow the parties to more carefully design their enforcement strategies.[10] Second, by allowing for more efficient punishment, courts expand the conditions under which cooperative equilibria can be sustained. Having said that, it is important to note that judicial monitoring will not induce mutual cooperation in all contexts. As in the Przeworski model, there are some contexts in which a set of cooperative strategies simply cannot be sustained in equilibrium, regardless of the monitoring provided by a court (pp. 673-677). These results hold even under the assumption that judges are strategic and engage in some form of pragmatic decision-making, indeed, they hold precisely because courts behave in this way. Yet, *at least a minimal degree of independence from the parties is essential*. This must be so in order for the information judges provide to be a credible signal of the alleged violator's actual performance. The implications of this argument are that independent courts can

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[10] The spirit of this argument is captured Landes and Posner's (1976) explanation for the existence of independent courts. They suggest that judicial independence ensures the credibility of legislative deals, which increases the value of rents legislators distribute. A similar logic of monitoring for the maintenance of efficient trading agreements underlies the Milgrom, North and Weingast (1990) model.

encourage order by providing a monitoring function, which eases the process by which parties enforce their own agreements; yet, in so far as some contexts forestall any cooperative agreement, there are limits to what independent courts can provide.

Carrubba's account suggests how judges might influence the monitoring of cooperative agreements. But monitoring alone cannot solve the order problem, because once a violation is observed, it will be necessary for aggrieved parties to respond, and this will require the solution of a significant coordination problem. Famously, Weingast suggests that constitutions can serve as focal points, selecting equilibria in which individuals successfully mobilize to redress transgressions from among the set of possible solutions. Though such a formal legal mechanism is plausible, courts endowed with constitutional jurisdiction, and which exercise that power independently, can also serve exactly the function proposed by Weingast. Such courts identify and report publicly violations of core limits on state powers. There is evidence that courts around the world promote publicly their decisions in the media, especially highly salient resolutions, which would, per Weingast, make it easier for judges to coordinate citizen responses (Staton 2010). But for courts to serve this kind of coordinating function, it seems highly likely that they would have to be at least minimally independent of the parties; otherwise, it will be difficult to interpret the decision as anything other than reflecting the interests of their co-partisans. In this sense, independent courts not only can provide monitoring to help enforce democratic compromises, they can help parties coordinate their constituents on appropriate reactions to unfaithful leaders. Yet again, independent courts are unlikely to advance order under all conditions. As before, if the underlying conditions for political compromise are not met, there will be no fundamental limits on state authority to enforce and thus no losing party constituents to coordinate.

Empirical Implications

Independent courts lower the stakes of holding power by helping political coalitions enforce their own bargains, and by so doing they both ensure order and expand the conditions under which cooperation can be sustained. Still, since courts cannot ensure that groups in society will always find cooperation attractive, the remaining question, is when will independent courts really matter?

If Przeworski is ultimately correct, then independent courts should be particularly useful when underlying macroeconomic conditions allow for great redistributive compromises, and also when governments confront strong incentives to break deals after the fact. The kind of democratic compromises Przeworski envisions are increasingly possible as economic development increases, precisely because extreme groups have so much more to lose from conflict as the size the economic pie is grows. In this sense, independent courts should be especially useful for the reasons Carrubba and Weingast suggest as development increases. Further, we know that the presence of natural resources, which are easily predated upon and which provide a valuable source of rents, provide strong incentives for governments violate limits on their powers (e.g. Boix 2003). For this reason, enforcing democratic deals should be especially problematic when a state's assets are highly specific. With these ideas in mind, three straightforward empirical implications emerge.

- *Proposition 1: Independent courts should encourage political order, but especially so as economic development increases.*

- *Proposition 2: Independent courts should lower the level of economic development that is necessary to insulate democratic regimes from disorder*

- *Proposition 3: Independent courts should encourage political order, but especially in states with highly specific assets*

We now turn to an empirical analysis of these claims.

Empirical Analysis

To test our expectations, we require data that reflect political disorder in democratic societies as well as data on those democracies' legal systems. And, of course, we will require measures of development and the specificity of a state's assets. To determine whether a country is a democracy we use the Democratic Regimes dataset (Bernhard, Nordstrom and Reenock 2001), which codes democratic states between 1919-2000. This database provides dichotomous classifications of democratic regimes, conceptualizing democracy as a fundamentally distinct organization of state authority compared to autocratic regimes. Its

coding of democracy is highly congruent with other existing datasets.[11] As we note below, our main

independent variable is only available beginning in 1960, and so the data frame begins in 1960 and ends

in 2000. The democratic states included in this analysis are shown in Appendix A.

Assessing Disorder in Democratic Regimes

To assess the level of political disorder in democratic states, we use three indicators: democratic regime

survival, violent domestic political events and armed intrastate conflict. Democratic regime breakdown

reflects the likelihood of observing a transition from a democratic regime to an autocratic one. Breakdown

is a ubiquitous indicator of democratic disorder that has been the object of investigation for a wide range

of scholars (Bernhard, Nordstrom and Reenock 2001, Bernhard, Reenock and Nordstrom 2003, 2004;

Cheibub 2002, 2007; Gasiorowski 1995; Gasiorowski and Power 1998; Power and Gasiorowski, 1997;

Przeworski, Alvarez, Cheibub, and Limongi 1996; Przeworski and Limongi, 1997). Most critically

breakdown reflects the final and perhaps most salient consequence of disorder within a democracy – its

complete collapse. For this analysis of democratic breakdown, the unit of analysis is the democratic

episode, where an episode represents a distinct period of democracy in a country's history. The portion of

the original dataset that we use runs from 1960-2000 and includes 2323 country-years with 140 episodes

of democracy and 44 breakdowns.[12]

  We also use the occurrence and frequency of violent domestic political events as an alternate

indicator of political disorder. This indicator reflects the likelihood of observing a violent political event

that includes such acts as assassination, rebellion or regime crisis within a democratic regime (Banks

1979) and has been the focus of several investigations on domestic conflict (Powell 1982; Small and

Singer 1982; Sarkees 2001). To measure the incidence of violent domestic conflict we rely on data from

Banks (1979), which includes yearly event counts of domestic conflict for each country. We use the

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[11] As a robustness check, we also use an alternative coding of democratic regimes using the Cheibub and Gandhi (2004) dataset; we present the results of the analysis with the Cheibub and Gandhi codings in Appendix C.

[12] We correct for this left-truncation by setting the count variable's starting value for the event history analysis and the splines at the cumulative number of years that a state was democratic prior to 1960 (or the years that had passed without a violent event)(Guo 1993).

indicators of the most intense levels of violence reported including assassinations, guerrilla warfare, revolutions and coups (for definitions, see Banks 1979, 14). We utilize two indicators of these occurrences. The first measure is a dichotomous indicator of the occurrence of any single event of violent political disorder. When any of the individual indicators (assassinations, guerrilla warfare, revolutions and coups) were non-zero, we coded this variable as '1,' when all of the individual indicators were zero we coded this variable as '0.' The second measure is an additive sum of these events that reflects the frequency of disorder present in a given democracy for a given year. For both of these variables the analysis of violent domestic political events, the unit of analysis is the democratic country-year. The distribution for the dichotomous indicator of violent events consists of 434 events out of 2240 county-years or approximately 19.4% of the cases. The distribution for the frequency of violent domestic events in these data ranges from 0 to 17 for a given year, with an average occurrence rate of .47 and a standard deviation of 1.42.

Last, we use the armed intrastate conflict data from the International Peace Research Institute, Oslo (PRIO) covering the period 1946-2001 (Gleditsch et al. 2002). The data set reports all instances of armed intrastate conflict that produce at least 25 annual battle deaths. We use a dichotomous indicator of the onset of armed intrastate conflict. When a conflict was initiated with at least 25 annual battle deaths, we code the variable as '1' and zero otherwise. The distribution for the dichotomous indicator of armed intrastate conflict consists of 44 events out of 2082 county-years or approximately 2.1% of the cases.[13] Descriptive statistics for all variables used in this analysis are shown in Appendix B.

Assessing Judicial Independence

To assess the independence of the judiciary within a democracy, we require a measure that identifies the extent to which the judiciary produces decisions that are sincere reflections of the underlying preferences of its judges (Kornhauser 2002). To estimate the causal effect of judicial independence on democratic

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[13] In addition to the casualty threshold, the PRIO onset data also allows the analyst to specify how many years of inactivity there must be between two different observations of active conflict before a new onset is coded 1. In the paper, we report the findings for the minimum intermittency period of two years. However, our results are robust for minimum periods of two, four and six years.

order, requires that any measure of judicial independence satisfy at least two properties: 1) it must be exogenous to order and 2) it must be available over a wide temporal and spatial range with any missingness being nonsystematic to our key variables of interest. Failing to satisfy the first condition would result in potentially both biased and inefficient parameter estimates and failing to satisfy the second condition would prevent us from estimating the most important statistical models in the research design.

Endogeneity

One obvious challenge to the research design is that our key independent variable, judicial independence, may be endogenous to our variables assessing disorder. We have sought several solutions. We began by considering potential exogenous instruments for judicial independence. The most likely candidate is the settler mortality variable introduced by Acemoglu, Johnson and Robinson (2001). There are, however, two difficulties in pursuing this instrument. First, there are no data for those states that were not colonies and there are significant levels of missing data on those states that were colonies. In fact, the use of the settle mortality data would result in 60% of the total country-years used in the democratic survival analysis and 61% of the total country-years used in the political events analysis to be list-wise deleted. Proceeding with the analysis in the presence of substantial listwise deletion would likely introduce selection bias and result in our drawing incorrect inferences (King et al. 2001). In particular, given the correlation between our key variables of interest (development and order) and the missingness of the settler mortality data there is reason to believe that we would introduce strong bias into our analysis by ignoring the missingness problem. Using common multiple imputation solutions (e.g. King et al. 2001) is equally implausible given that most of the missing data are missing for entire country series, which would require us to impute all information for all non-settler states, and many more states that were not colonies, but were nevertheless excluded in the Acemoglu, Johnson and Robinson study. As an alternative to settler mortality we also pursued biological and geographic instruments employed by Olsson and Hibbs (2005).[14]

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[14] The biological variable was drawn from the first principal component of two variables: the number of wild grasses exceeding 10mg mean kernel weight and the number of domesticable animals weighing more than 45kg. The

Using these instruments, Wald tests using a two-stage conditional maximum likelihood estimate, 2SCML, (see Alvarez and Glasgow 2000) for the additive models in our analysis suggested that we cannot reject the null hypothesis of exogeneity. While encouraging, there are two issues of concern with these instruments. First, while these instruments avoid the problems associated with missing data, our analysis suggested that they are weak instruments for our key judicial independence measure. Rather, as reported by Olsson and Hibbs, we find that they are fairly strong instruments of economic development. Second, and related to the first issue, given that our main analysis is a multiplicative model between judicial independence and development, we are left without an effective instrument for legal system effectiveness.

In the end, we believe that while there is evidence from the 2SCML analysis that endogeneity of judicial independence to disorder is unlikely, we treat for this possibility in the models that we present here. All reported models in this analysis are based upon our judicial independence measure lagged by one period. We also analyzed our results using two and three-period lags of independence as well as non-time-varying independence inputs, where we used only the first observation of judicial independence for a given country for that country's entire democratic episode. The results reported here were robust to these alternate specifications, increasing our confidence that we are not drawing incorrect inferences about the relationship between judicial independence and disorder by treating independence as being exogenous to disorder.

Measuring Judicial Independence: Data Availability and Missingness

As it turns out, it is extremely difficult to locate a measure of judicial independence that is both maximally available and yet nonsystematic in its missingness. Although there are a suite of *rule of law* measures that do seem to (at least partially) indicate the extent to which the judiciary is independent; these measures have insufficient temporal variation (e.g. Gwartney and Lawson 2007; Kaufmann, Kraay and Mastruzzi 2007) and often include an order component, which captures the extent to which individuals in society engage in crime and other disorderly behavior (and presents problems of

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geographical variable was drawn from the first principal component of four variables: climate for agriculture, latitude, the East-West orientation of a landmass relative to its North-West orientation and the land mass in sq. kilometers to which a country belongs.

endogeneity addressed in the previous section), including ostensibly anti-regime behavior (ICRG 2004).

Finally, recent comparative measures of judicial independence either have no temporal variation at all

(e.g. Feld and Voigt 2003; La Porta *et al*) or are limited to the 1990s (*e.g.* Cingranelli and Richardson

2008; Howard and Carey 2004; Tate and Keith 2007). The primary difficulty with many of these

measures is that the series for which there are no observations are often highly correlated with

development and order -- two concepts central to our analysis.

In light of these concerns, we turn to the contract intensive money score (*CIM*), derived by

Clague *et al*. *(*1996). *CIM* is "the ratio of non-currency money to the total money supply, or *(M₂-C)/M₂*

where M_2 is a broad definition of money supply and C is currency held outside of banks" (Clague, Knack,

Keefer, and Olson 1999, 188). This measure is particularly attractive in that it has significant temporal

and spatial coverage and it does not measure order directly. Although perhaps not immediately apparent,

CIM provides the kind of information we are looking for. This measure was conceptualized as a measure

of legal protections for *property rights*, and this is how it has been traditionally used in the literature (e.g.

Clague, Knack, Keefer, and Olson, 1999:186; Souva *et al.*, n.d.). Conceptually, high values of *CIM* reflect

a society's trust in legal protections for their assets. For this reason, it assesses the behavior that we would

expect to observe if the state's rights commitments were perceived to be credible because its judicial

institutions independently evaluated claims of rights violations. In addition, *CIM* correlates reasonably

well with the alternative measures of the concept described above.[15]

As a robustness check, however, we also include an alternative measure of judicial independence,

the Polity project's XCONST score, which indicates constraints on the executive. One of these constraints

is the presence of an independent judiciary. While the coverage of this variable is not as complete as CIM,

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[15] Correlations between the *CIM* and the measures noted in the text are as follows: ICRG Law and Order (.51); Gwartney and Lawson Judicial Independence (.42); Kaufmann, Kraay and Mastruzzi Rule of Law (.61); La Porta *et al* Judicial Independence (.05); Feld & Voigt *de facto* Judicial Independence (.40). Correlations between the *XCONST* and these measures are: ICRG Law and Order (.39); Gwartney and Lawson Judicial Independence (.44); Kaufmann, Kraay and Mastruzzi Rule of Law (.61); La Porta *et al* Judicial Independence (.30); Feld & Voigt *de facto* Judicial Independence (.30).

we believe that it offers a reasonable robustness check.[16] For our data, the CIM measure ranges from a low of .246 (Chad 1962) to a high of .999 (Luxembourg 1992), with an average of .838 and a standard deviation of .119. Moreover, for our data, Polity's XCONST variable ranges from a low of 1to a high of 7, with an average of 6.46 and a standard deviation of 1.35.

Independent Variables and Controls

Macroeconomic Context

Above we highlighted two specific conditions under which legal institutions ought to be more helpful in reducing conflict within democracies: in the presence of greater economic development and when a state has natural resources that are easily predated upon. To assess the former, we use the natural log of *Gross Domestic Product* measured as GDP per capita (purchasing power parity) computed in 2000 constant prices from the World Development Indicators (WDI 2008). As an additional control for endogeneity, we use the lag of ln(GDP) for a given year.

To evaluate whether certain factor endowments heighten the incentive for leaders to violate their promises, we use two measures of specific assets: annual diamond and oil production. We use data from Humpherys (2005) to measure the annual oil and diamond production assets for a given democracy. Specifically, for diamond production we include the variable, *Diamonds*, which yields the annual diamond production for a given year in hundreds of thousands of carats. For oil production, we include the variable, *Oil*, which yields the average amount of oil extracted per day in a given year measured in millions of barrels per day.

Control Variables

We include several variables in both the democratic survival and disorder event count models to decrease the possibility of observing a spurious relationship between judicial independence, socioeconomic context and our dependent variables of interest. We include a *Presidential Regime* dummy variable that indicates

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[16] In addition to XCONST, we also included recently developed alternative measures of judicial independence from three sets of scholars (Cingranelli and Richardson 2008; Howard and Carey 2004; Tate and Keith 2007). We report the results of these robustness tests in our Table 5. Each of these variables has high missingness that is systematically correlated to several key variables in our analysis including: development, order and CIM.

the presence of an executive, who was elected under a separate mandate to an office with fixed terms and who does not posses the ability to dismiss the legislature (Linz 1994). *Growth* is the annual percentage change in GDP (Penn World Tables 2005). We also control for *Ethnic and Religious fractionalization* as measured by Rae and Taylor's (1970) fractionalization index as well as *party fractionalization* using the Laakso-Taagepera index (1979) to calculate the effective number of parties in the legislature. In the breakdown models only, we also include *Previous Experiences* to measure the number of times a democracy has previously experienced democratic failures (Huntington 1991). We include *Prevdem,* which measures the number of previous democratic episode that a current episode has experienced. We also include two dichotomous variables: *BritCol* as a control for British colonial legacy and *Micro* as a control for microstates with populations lower than one million citizens. Last, we include, in the violent event models only, the natural log of *Population* (Auvinen 1997; Flanagan and Fogelman 1970; Gurr and Duval 1973; Helliwell 1994; Powell 1982; Weede 1981) and the natural log of the difference in elevation between the highest and lowest elevation points in the country as a proxy for the country's mountainous terrain (Fearon and Laitin 2003, 81). These data were gathered from the CIA's World Factbook.

Model Estimation

For our democratic breakdown models, we estimated a proportional hazards Weibull model, to estimate a parameter for duration dependence, along with clustered standard errors to correct for non-independence of observations within episodes. We estimate the effect of legal institutions on the number of violent domestic events in a democracy using a population averaged negative binomial estimator that is appropriate for pooled event count panel data. We estimate the effect of legal institutions on the occurrence of violent domestic events (Banks) and the onset of armed intrastate conflict (PRIO) with a logit estimator with temporal splines to account for temporal dependence between observations (Beck, Katz and Tucker 1998). Each of the models uses clustered standard errors on the democratic episode to correct for non-independence in the error structure within democratic episodes.

Results

Does Judicial Independence Influence Political Order?

Table 1 reports the results for several additive models of political order, and tests the basic hypothesis that judicial independence encourages order. Models 1 through 4 display the results using the Contract Intensive Money measure of legal institutions while Models 5 through 8 display the results using the Polity XCONST measure. The results suggest that the judicial independence (*CIM)* generally behaves as we would expect. In most models, democracies with greater judicial independence, as measured by our two indicators, experience both longer-lived tenures as well as lower likelihood of violent domestic conflict and armed conflict. For example, according to Model 1, an increase in contract intensive money from a low of .245 (e.g. Chad in the 1960s) to a high of .99 (e.g. Luxembourg and New Zealand in the 1990s) would increase a democracy's expected duration by a factor of about 6.17 times. In other words, with their level of judicial independence, Luxembourg or New Zealand democratic regimes are expected to last about 6 times longer than Chad's, ceteris paribus. This is a fairly substantial effect when compared to the long-held finding that enhanced development provides protections to democracies. For this model, an increase in real GDP per capita from Chad's 1960s level of $252 to Luxembourg's 1999 GDP of $38,407 would enhance Chad's expected duration by a factor of approximately 21.45.

(Table 1)

Models 2 through 4 and 6 through 8 in Table 1 display the results of the additive models of various our three indicators of violent domestic conflict. The estimates using CIM as an indicator of judicial independence, while in the expected negative direction, do not exceed their standard errors and are not statistically significant. This would suggest that, across all socioeconomic contexts, the average effect of judicial independence with this measure is essentially null for our indicators of domestic political disorder. However, it is useful to recall that these reported effects are the *average* effects produced from an additive model, where judicial independence is unconditioned by socioeconomic context. It is possible,

if the logics of the Carrubba and Weingast models are correct, that judicial independence may matter more in some states and less, or not at all, in others.[17]

The results for the XCONST measure suggest that more effective legal institutions afford democracies greater protection from the conflict likely to arise in disputes over contract failure. In each model, the XCONST variable is in the expected negative direction and is statistically significant, suggesting that democracies with more independent judiciaries on average experience a lower likelihood of the occurrence of domestic acts of violence, fewer numbers of violent domestic acts and lower likelihood of the onset of armed conflict than those with more politicized judiciaries. In fact, with respect to the actual number of violent domestic events, the expected count for a democracy with an XCONST score of 1 decreases by a factor of .267 compared to a democracy with an XCONST score of 7 (the same relative change from 1960s Chad to 1990s Luxembourg). In other words, a democracy with an independent judiciary (XCONST of 7) decreases the expected the expected number of violent domestic events by approximately 73%, holding all other variables constant.

In summary, the additive models provide convincing evidence that democracies benefit from the supply of independent judicial institutions. With both domestic political violence and the ultimate culmination of such political violence, the collapse of the democratic state, independent judiciaries appear to offer some protection against the fear of winning parties violating their promises. The analysis above, however, assumes that every democratic state in the dataset is equally likely to posses the desired political context within which compromises over macroeconomic disagreement is possible. As a result, the effects estimated in the additive models above are essentially the *average* effects of such judicial institutions across all macroeconomic contexts. In the following section, we consider whether the effect of legal institutions on democratic survival and political disorder are conditioned by a democratic state's macroeconomic context.

The Conditional Effects of Legal Institutions on Order

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[17] The robustness analysis, using the Cheibub and Gandhi data reported in Appendix C.1 does suggest that CIM has a significant additive effect for the set of democracies classified by Cheibub and Gandhi's codification.

Our first empirical expectation suggested that *independent courts should encourage political order, but especially so development increases.* This is because economic development widens the space within which it is possible for competing classes to reach an acceptable compromise over economic policy. And when this is possible, the independence of the judiciary becomes relevant as a means of locking-in the deal. At sufficiently low levels of development, there simply may be no compromise for courts to enforce, and in those contexts, judges are unlikely to influence order. The results of multiplicative models, which test this expectation, are shown in Table 2 (and Appendix C.2.).

(Table 2)

The results in Table 2 suggest that the benefits of judicial independence for democratic order are conditioned by the opportunity for compromise provided by enhanced development. The coefficients on the interaction terms are negative and statistically significant in nearly every model, suggesting that the beneficial effects of legal institutions for democratic order are greater in the presence of enhanced development.[18] Of course, the full measure of an interaction term is difficult to evaluate in a table of results. It is always possible that the marginal effect of legal institutions may not have a statistically significant impact on democratic order over the entire range of development. To more precisely evaluate the conditional relationship, we present one set of the results graphically below in Figure 1. Figure 1 displays two panels each reflecting the effect of an increase of one standard deviation in either CIM or economic development on democratic survival.[19] The figures display the marginal effect of the independent variable of interest along with its 95% confidence interval plotted around it. In addition, each of the figures superimposes a plot of the histogram of the conditioning variable on the chart. Given that both the marginal effect and its associated standard error varies over the level of the conditioning variable, these figure more accurately test the precise relationship between our interactive variables and the dependent variable (Brambor, Clark and Golder 2006).

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[18] Taking all the models into consideration, those with the BNR data and those with the C&G data, out of the 16 total models, only in two, the dichotomous Banks models with the XCONST measure of effective legal systems, does the interaction not achieve statistical significance.

[19] For each panel, we estimated the effect of moving the variable of interest from one standard deviation below the mean up to the mean.

The results in Figure 1 conform to our expectations. Panel A demonstrates that the effective legal systems are most helpful for developed democracies. CIM has a positive and statistically significant effect on democratic survival, and this effect is increasingly more beneficial for more economically developed democracies. However, not all democracies reap the benefits that legal institutions may provide. At sufficiently low levels of development, the 95% confidence interval straddles the x-axis, suggesting no benefit of legal system effectiveness for those democracies below economic development of 6.65 ln(GDP) or approximately $772 per capita. Moreover, the histogram reveals that the number of democracies that fall below this lower limit is non-trivial, with nearly 15% of the cases in the data having GDPs below this level. In our data, this set of cases consists of modern-day democracies including countries like Malawi, Mozambique, Nepal and Madagascar. Our analysis suggests that, in the absence of sufficiently high development, these democracies are not likely to benefit from greater attention to judicial reform. Turning our attention to the right hand side of Panel A in Figure 1, we should also mention that we have truncated the Figure at ln(GDP) of 9.0, or approximately $8103, due to the fact that democratic regimes do not experience breakdown above sufficiently high GDP (Przeworski et al. 2000). Therefore, while the expected benefits of legal institutions continue to grow for regime survival beyond this level, there are no cases of breakdown above this level of economic development.

Panel B in Figure 1 reports the effect on the survival of a democratic regime of an increase in one std. deviation of ln(GDP) across legal system effectiveness. Here we see what has been widely reported in the previous literature, increases in economic development have beneficial effects for democratic regime survival. But the benefits of enhanced development are actually greater in the presence of more effective judicial systems. It is also interesting to note that at sufficiently low judicial independence, an increase in development does not seem to benefit regimes. However, the histogram suggests that the number of regimes for which this lower condition would hold is quite small and may actually explain the estimated null effect of development at this level of CIM.

(Figure 1)

The substantive results for the other indicators of political order, violent conflict and armed conflict, display patterns similar to those in Figure 1. For each of the other indicators of disorder, the benefit of legal institutions in discouraging violent disorder grows as economic development increases. Moreover, for each indicator, a similar lower level economic development threshold exists, below which legal institutions have no impact on political disorder. In these models, however, this threshold is slightly higher compared to the regime survival models, with democracies below approximately $1808 experiencing no benefit from independent judicial systems.

In summary, the analysis above provides evidence for our expectation that independent courts should encourage political order, but especially so as it becomes increasingly likely that competing coalitions can reach a democratic compromise. Independent judiciaries enhance the durability of democratic regimes on average and they reduce the likelihood and intensity of domestic political conflict. Yet, the benefits of such systems are critically and increasingly important as the necessary conditions for compromise between political interests expands.

Expanding the conditions of compromise?

What about our second expectation, that i*ndependent courts should lower the level of economic development that is necessary to insulate democratic regimes from disorder?* Do we have evidence for this claim? In Figure 2, we present the effect of a one standard deviation increase in CIM on that regime's expected duration. However we present two specifications in the figure. In the first, we present the expected benefit of an increase in CIM for an additive model, in which economic development does not condition the effect of judicial effectiveness. This estimated effect is the dashed line. In the second, we present the expected benefit of an increase in CIM for a conditional model, in which development conditions the effect of judicial effectiveness. This estimated effect is represented by the dot-dashed line. For the sake of illustration, we also include a horizontal line positioned at an expected duration of 37 years. The precise location of the reference line is irrelevant but this position is useful to illustrate our findings.

(Figure2)

Imagine that this line represents the abstract amount of expected duration that we would need to observe before we declared a democracy to be 'insulated' from breakdown. As the horizontal line moves up on the figure, we would require additional time before we declared a given regime to be impervious from breakdown.[20] If we focus on the dashed line in the figure, the line produced from the additive models, we can see that as we move along the horizontal axis, increasing a democracy's economic development, the expected duration represented by the dashed line increases and approaches our 'ideal' duration threshold represented by the horizontal line. In fact, in this figure, when ln(GDP) is equivalent to approximately 8.9, or $7300, the estimated benefit of an increase in CIM up to the mean is sufficient to produce an expected duration at our ideal threshold, thus 'guaranteeing' democratic survival. What then is the effect of legal system effectiveness on this ideal threshold?

To answer this, we now focus on the dot-dashed line in the figure, the line produced from the conditional models. We can see that as we move along the horizontal axis, increasing a democracy's economic development, the expected duration represented by the dot-dashed line increases and approaches our 'ideal' duration threshold represented by the horizontal line more rapidly than the additive model. In fact, in this figure, when ln(GDP) is equivalent to approximately 7.6, or $1998, the estimated benefit of an increase in CIM up to the mean is sufficient to produce an expected duration at our ideal threshold, thus 'guaranteeing' democratic survival. Again, we do not mean to place too much emphasis on the precise values in this illustration. We do believe, however that this exercise is useful in demonstrating the potential of effective legal institutions to enhance and expand the acceptable boundaries of peaceful resolution of democratic conflict. With sufficiently high legal system effectiveness, democratic regimes can enhance their prospects of survival at effectively lower minimum levels of economic development. This analysis suggests support for our second expectation that independent courts should expand the conditions under which compromise is possible.

The Conditional Effect of Legal Institutions on Order in the Presence of Specific Assets

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[20] The exact number on the y-axis associated with this abstract expected duration is irrelevant and is for illustration purposes only. We do not mean to suggest that an expected duration of 37 years is sufficient to insulate a democracy against breakdown.

Last, we consider whether the effect of independent courts is *especially strong in the presence of specific assets*. Table 3 and 4 report our findings for the analysis of our two indicators of specific assets diamonds and oil, respectively.[21] We expect that judicial independence should be provide increasingly beneficial effects on order as the production of oil and diamonds increase. On the whole, the analysis suggests support for our expectation. In 12 of 16 total models of political disorder, the coefficient on the interaction between diamonds and our indicators of an independent judiciary is statistically significant and negative. The results for oil are less conclusive but still positive for our expectation, with 9 of 16 models producing a statistically significant coefficient on the interaction. Even in the models where the interaction coefficient is not statistically significant, the sign of the coefficient is always in the expected direction.

(Table 3)

(Table 4)

While the previous literature has highlighted specific assets as a particularly attractive resource for parties contemplating anti-democratic actions, there has been little attention to the whether certain institutions might effectively constrain leaders from being tempted by such incentives. The results here suggest that when leaders have particularly strong incentives to violate a compromise, (i.e. when their democracy produces high quantities of assets that are easy to loot or expropriate), independent judiciaries offer democracies a strong benefit for maintaining order. Either by providing monitoring to help enforce a deal or by helping parties coordinate their constituents on appropriate reactions to unfaithful leaders, independent judiciaries may reduce the risks that some democracies face when they possess large pools of lootable resources.

Conclusion

The rule of law community is deeply invested in judicial reform, at least in part because good courts are supposed to strengthen democracies. Upham (2002, 8) reports that during the 1980s and early 1990s, over a billion dollars was spent by USAID and the World Bank alone. There is no evidence that

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[21] The robustness analyses using the Cheibub and Gandhi data are reported in Appendix C.3 and C.4.

this kind of commitment is slowing down. Yet, the community's sense of the value of good courts has been driven largely by anecdotal evidence. And the mainstream literature on domestic conflict has almost entirely ignored the law.

In this paper, we have suggested that there are natural connections between models of judicial politics and political economy models of regime survival, which provide a rationale in general for rule of law reform. Critically, however, the idea is not that with independent courts, order follows *ipso facto*. Independent courts do not produce order simply because, by definition, independent courts are veto players, which constrain the state and lower the stakes of holding power. Instead courts can be useful devices for monitoring underlying democratic deals and for helping parties coordinate their efforts to punish breaches. We considered how this can be so in the context of Przeworski's model of democracy, which provides what we take to be the first fully developed model of the relationship between economic development and democratic stability. Although we have not done so in this paper, the implications for other accounts of democratic stability are clear. Specifically, Acemoglu and Robinson's (2006) models of regime transition also operate via the logic of constraining groups from rebelling with the strategic implementation of redistribution rates. Although they model the leader's commitment problem directly, monitoring of implementation is perfect and there is no coordination problem to solve. But if there were, independent courts might help ensure order. The precise empirical implications will change of course, but only in so far as they speak to the different underlying features of the economy that ensure cooperation. For Przeworski it is a sufficiently high level of development. For Acemoglu and Robinson, it is a moderate level of inequality (2006, 199). The upshot of connecting these literatures is the recognition that independent courts can help competing democratic coalitions enforce the terms of broad compromises over economic policy. On this account, courts are especially important where the conditions for compromise are likely to be met and where governments have strong incentives to violate limits on their power.

It is worth summarizing the empirical results in light of our reform goals. Empirically, we know that there are no instances of democratic breakdown above a sufficiently high threshold of development

(e.g. Przeworski 2005). Essentially, very wealthy democracies seem to be entirely resilient to disorder. Yet here, we have found evidence suggesting that independent courts seem to lower the level of development at which democracies become highly stable, suggesting that independent courts renders states relatively safe, which would be otherwise remain at high risk for breakdown. Also, and most relevant for reform efforts, though we have found that judicial independence increases order, the relationship is especially strong as economic development rises and when states produce considerable oil and diamonds. All of these results are consistent with the rule of law community's investment in judicial reform. Independent courts seem to nourish democracy, especially where the conditions for fundamental compromises are met and where powerful actors have incentives to undermine collective rule.

Still it is important to remember that we have found that there may be no direct relationship between independent courts and order when states are very poor. Unfortunately, these are the precisely states most vulnerable to breakdown. What this suggests is that judicial reform may be least effective where we believe it is needed most. In this sense, the results should be sobering for the rule of law community. They also remind us why mainstream literatures on conflict, which center empirically on under-developed states, have largely ignored the law. Simply put, there is reason to question whether building key rule of law institutions in Chad or Iraq is likely to matter in any substantial way. It may not be possible to induce order directly via judicial independence under conditions of significant under-development, precisely because the feasible set of democratic compromise is empty. Finally, these results are highly consistent with North, Wallis and Weingast (2009) argument that developing states are heavily invested in maintaining order through rent-distribution, and that for this reason, building strong rule of law institutions only undermines common strategies for holding power.

Lest we paint too dreary a picture for reform, it is worth considering an alternative causal path. If we recall the evidence linking effective legal institutions to economic growth and development (Barro 1997, Acemoglu, Johnson, and Robinson 2001) then we can infer that, if anything, we are underestimating the total effect of legal institutions on order, precisely because some of the effect of law passes indirectly through development. We have been careful to restrict our more sobering claims to the

direct effects of law on order. If independent courts breed efficient investment, and ultimately growth, then law might have an indirectly effect on order. Although this is plausible, it is important to remember North, Summerhill and Weingast's primary point: order is a necessary condition for development. Thus, if an underdeveloped state is subject to significant breakdowns in order, it is not obvious that judicial independence will breed order via its impact on development. Solving the order problem may be paramount. For this reason, in some democratic contexts, the right strategy for stabilizing regimes may need to center on critical concerns of security and perhaps involve development strategies that do not depend on judges to enforce market-based contracting.

Appendix A
Democratic Episodes in the Data Set from 1961 to 2000

Albania 92-97	Greece 75-00	Paraguay 93-00
Antigua and Barbuda 81-00	Grenada 78-**79**, 84-00	Peru 80-**92**
Argentina 84-00	Guatemala 96-00	Philippines 61-**72**, 87-00
Australia 61-00	Guyana 92-00	Poland 91-00
Austria 61-00	Haiti 95-**99**	Portugal 76-00
Bahamas 73-00	Honduras 90-00	Romania 92-00
Bangladesh 91-00	Hungary 90-00	Russia 93-00
Barbados 66-00	Iceland 61-00	St. Kitts and Nevis 83-00
Belgium 61-00	India 61-**75**, 77-00	St. Lucia 81-00
Belize 81-00	Indonesia 99-00	St. Vincent 79-00
Benin 61-**62**, 91-00	Ireland 61-00	Senegal 00
Bolivia 82-00	Israel 61-00	Seychelles 93-00
Botswana 76-00	Italy 61-00	Sierra Leone 62-**67**
Brazil 61-**64**, 86-00	Jamaica 67-00	Slovakia 92-00
Bulgaria 90-00	Japan 61-00	Slovenia 91-00
Burkina Faso 78-**80**	Kenya **66**	Solomon Islands 80-**00**
Burundi **93**	Latvia 93-00	South Africa 94-00
Canada 61-00	Lithuania 91-00	South Korea **61**, 88-00
Cape Verde 91-00	Luxemburg 61-00	Spain 78-00
Chad 61-**62**	Macedonia 91-00	Sri Lanka 61-**83**
Chile 60-**73**, 90-00	Madagascar 61-**71**, 93-00	Sudan 65-**69**, 86-**89**
Columbia 74-**98**	Malawi 94-00	Suriname 76-**79**, 88-**89**, 91-00
Congo 61-**63**, 92-**93**	Malaysia 61-**69**	Sweden 61-00
Costa Rica 61-00	Mali 92-00	Switzerland 71-00
Croatia 00	Malta 64-00	Thailand 97-00
Czech Rep. 92-00	Mauritius 81-00	Trinidad 62-00
Denmark 61-00	Mexico 00	Turkey 69-**71**, 73-**80**, 83-00
Dominica 78-00	Moldova 94-00	Ukraine 94-**99**
Dominican Rep. **63**, 78-00	Mongolia 92-00	United Kingdom 61-00
Ecuador 79-00	Mozambique 94-00	United States 61-00
El Salvador 91-00	Nepal 91-00	Uruguay 61-**73**, 85-00
Estonia 92-00	Netherlands 61-00	Vanuatu 80-00
Finland 61-00	New Zealand 61-00	Venezuela 61-00
Fiji 99-**00**	Nicaragua 90-00	Zambia 91-**96**
France 61-00	Niger 93-**95**	
Gambia 67-**94**	Nigeria 61-**66**, 79-**83**	
Germany 72-00	Norway 61-00	
Ghana 69-**72**, 79-**82**	Papua New Guinea 77-00	

Appendix B

Appendix B. Descriptive Statistics

Survival Models

Dependent Variable	Number of Democratic Episodes		Breakdowns	Temporal Domain	N (# of country-years)
Democratic Episode (BNR data)	140		44	1961 - 2000	2323
Democratic Episode (C&G data)	156		46	1961 - 2000	2507

Independent Variables	Mean	SD	Range	Temporal Domain	N (# of country-years)
Contract Intensive Money (CIM)	0.838	0.119	.212 - .999	1961 - 2000	2231
Polity XCONST	6.460	1.052	1-7	1961 - 2000	1870
Diamond Production	0.489	3.501	0 - 43.8	1961 - 2000	2231
Oil Production	0.371	1.350	0 - 10.082	1961 - 2000	2231
Ln(GDP per capita)	8.367	1.412	4.820 - 10.742	1961 - 2000	2236
Economic Growth	2.163	4.167	-30.74 - 23.430	1961 - 2000	2206
Effective Number of Political Parties	3.150	1.798	1 - 23.673	1961 - 2000	2231
Religious Fractionalization	0.375	0.247	.004 - .84	1961 - 2000	2231
Ethnic Fractionalization	0.349	0.261	.006 - .91	1961 - 2000	2231
Previous Democratic Experience	0.107	0.353	0 - 2	1961 - 2000	2323

	# of Type		Percentage	Temporal Domain	N (# of country-years)
Presidential Regime	553		24.79	1961 - 2000	2323
Former British Colony	826		37.02	1961 - 2000	2323
Micro State	490		21.96	1961 - 2000	2323

Violent Events

Dependent Variables

	# of Events		Percentage	Temporal Domain	N (# of country-years)
Domestic Violent Events (1,0)	434		19.38	1961 - 2000	2240
	Mean	SD	Range	Temporal Domain	N (# of country-years)
Domestic Violent Events (count)	0.468	1.418	0 - 17	1961 - 2000	2240
Armed Conflicts (1,0)	12.437	11.158	0 - 40	1961 - 2000	2082

Independent Variables	Mean	SD	Range	Temporal Domain	N (# of country-years)
CIM	0.838	0.119	.212 - .999	1961 - 2000	2231
Polity XCONST	6.460	1.052	1-7	1961 - 2000	1870
Diamond Production	0.489	3.501	0 - 43.8	1961 - 2000	2231
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Economic Growth	2.163	4.167	-30.74 - 23.430	1961 - 2000	2206
Effective Number of Political Parties	3.150	1.798	1 - 23.673	1961 - 2000	2231
Religious Fractionalization	0.375	0.247	.004 - .84	1961 - 2000	2231
Ethnic Fractionalization	0.349	0.261	.006 - .91	1961 - 2000	2231
Ln(Elevation Difference)	7.484	1.093	3.97 -9.08	1961 - 2000	2321
Ln(Population)	15.466	2.105	10.6 - 20.739	1961 - 2000	2304

	# of Presidential Regime - Years		Percentage	Temporal Domain	N (# of country-years)
Presidential Regime	553		24.79	1961 - 2000	2323

Appendix C

Appendix C.1. Additive Models of Legal Institutions' Effect on Political Disorder (CNG data)

	Legal Instititions (CIM measure)				Legal Institutions (XCONST measure)			
	Survival	**Banks Logit**	**Banks Count**	**Prio Logit**	**Survival**	**Banks Logit**	**Banks Count**	**Prio Logit**
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8
Main Variables								
Legal Institutions	3.49***	-1.24*	-1.28**	-3.17***	0.01	-0.27***	-0.03***	0.00
	0.95	0.65	0.54	1.12	0.01	0.07	0.01	0.01
Ln(GDP per capita)	0.30*	-0.09	-0.17***	0.02	0.54***	-0.04	-0.17***	-0.09
	0.17	0.09	0.06	0.15	0.16	0.10	0.06	0.16
Diamond Production	0.02	0.01	0.00	0.06*	0.00	-0.01	0.00	0.05
	0.08	0.03	0.02	0.03	0.05	0.03	0.02	0.04
Oil Production	0.16	-0.05	-0.14***	0.05	0.10	-0.06	-0.15***	0.01
	0.20	0.06	0.05	0.11	0.15	0.05	0.05	0.11
Control Variables								
Economic Growth	0.00***	0.00*	0.00	-0.06***	0.00***	0.00*	0.00	-0.08***
	0.00	0.00	0.00	0.02	0.00	0.00	0.00	0.02
Presidential Regime	-0.49	0.52**	0.81***	-0.05	-0.69*	0.39*	0.88***	-0.21
	0.38	0.20	0.14	0.32	0.41	0.22	0.15	0.32
Effective Parties	0.05	-0.07*	-0.09**	-0.04	0.05	-0.09*	-0.11***	-0.06
	0.09	0.04	0.04	0.06	0.08	0.05	0.04	0.06
Religious Frac.	0.40	-0.21	-0.20	-0.20	0.27	-0.39	-0.56	-0.07
	0.77	0.41	0.32	0.80	0.93	0.48	0.34	0.83
Ethnic Frac.	-1.11	-0.01	-0.26	1.20*	-0.85	0.16	-0.15	1.34**
	0.76	0.36	0.31	0.67	0.60	0.41	0.30	0.68
Previous Dem.	-0.27*	---	---	---	-0.12	---	---	---
	0.15	---	---	---	0.19	---	---	---
Former British Colony	0.14	---	---	---	0.24	---	---	---
	0.43	---	---	---	0.37	---	---	---
Micro State	-0.09	---	---	---	11.21***	---	---	---
	0.60	---	---	---	1.76	---	---	---
Ln(Population)	---	0.23***	0.33***	0.44***	---	0.29***	0.38***	0.36***
	---	0.07	0.05	0.11	---	0.10	0.05	0.10
Ln(Elevation)	---	0.27*	0.39***	0.25	---	0.19	0.46***	0.36
	---	0.16	0.11	0.26	---	0.18	0.12	0.27
Time Since Last Event	---	-0.66***	---	-0.18	---	-0.65***	---	-0.20
	---	0.07	---	0.20	---	0.08	---	0.19
Constant	-0.64	-2.46**	-4.02***	-7.46***	0.18	-2.03*	-6.00***	-8.94***
	1.37	1.01	0.95	2.11	1.41	1.16	1.04	2.37
ln(p)	0.19	---	---	---	0.12	---	---	---
	0.13	---	---	---	0.14	---	---	---
Log pseudo-likelihood	-97.16	-853.65	---	-181.01	-43.22	-765.71	---	-171.34
Wald χ^2 (df)	228.69(12)	283.86(15)	234.02(11)	204.53(15)	170.25(12)	261.79(15)	259.11(11)	226.58(15)
N	2252	2197	1940	2056	1913	1860	1826	1870

Note: Robust standard errors clustered by country (survival models) or democratic episode (all other models)

$^{*}p<.10$, $^{**}p<.05$, $^{***}p<.01$, two-tailed tests

Appendix C.2. Multiplicative Models of Legal Institutions' Effect by GDP on Political Disorder (CNG data)

	Legal Instititions (CIM measure)				Legal Institutions (XCONST measure)			
	Survival	Banks Logit	Banks Count	Prio Logit	Survival	Banks Logit	Banks Count	Prio Logit
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8
Main Variables								
Legal Institutions	-5.51	3.88	5.41**	9.81	-1.51***	-0.84**	0.40***	0.55***
	3.99	3.07	2.73	6.40	0.32	0.37	0.07	0.21
Ln(GDP per capita)	-0.58	0.49	0.57*	1.34**	-0.72***	-0.51	0.41***	0.56**
	0.43	0.34	0.31	0.61	0.22	0.33	0.10	0.22
Diamond Production	-0.02	0.01	-0.01	0.05	-0.02	-0.01	-0.01	0.03
	0.04	0.03	0.02	0.03	0.24	0.03	0.03	0.04
Oil Production	0.14	-0.04	-0.12**	0.08	0.21	-7.44	-0.13**	-0.02
	0.19	0.05	0.05	0.11	0.67	10.10	0.06	0.10
Legal Inst. X Ln(GDP)	1.28	-0.72*	-0.93**	-1.80**	0.30***	0.08	-0.09***	-0.11***
	0.54	0.44	0.37	0.88	0.06	0.05	0.01	0.04
Control Variables								
Economic Growth	0.00***	0.00*	0.00	-0.06**	0.00	0.00	0.00*	-0.08***
	0.00	0.00	0.00	0.02	0.00	0.00	0.00	0.02
Presidential Regime	-0.50	0.47**	0.72***	-0.12	-0.32	0.40*	0.70***	-0.38
	0.36	0.19	0.15	0.33	0.33	0.22	0.15	0.33
Effective Parties	0.03	-0.07*	-0.09**	-0.05	-0.02	-0.09*	-0.11***	-0.07
	0.07	0.03	0.04	0.06	0.04	0.05	0.04	0.06
Religious Frac.	-0.10	-0.06	-0.04	0.24	-0.07	-0.46	-0.44	0.41
	0.73	0.43	0.33	1.02	0.99	0.50	0.35	1.05
Ethnic Frac.	-0.98	0.01	-0.24	1.25*	-0.17	0.19	-0.10	1.53**
	0.70	0.36	0.31	0.68	0.78	0.42	0.30	0.78
Previous Dem.	-0.20	---	---	---	0.05	---	---	---
	0.16	---	---	---	0.22	---	---	---
Former British Colony	0.21	---	---	---	-0.10	---	---	---
	0.39	---	---	---	0.42	---	---	---
Micro State	-0.11	---	---	---	9.13***	---	---	---
	0.54	---	---	---	1.25	---	---	---
Ln(Population)	---	0.25***	0.34***	0.43***	---	0.29***	0.41***	0.36***
	---	0.07	0.05	0.11	---	0.10	0.05	0.11
Ln(Elevation)	---	0.27*	0.37***	0.27	---	0.23	0.32***	0.33
	---	0.15	0.11	0.26	---	0.19	0.12	0.29
Time Since Last Event	---	-0.66***	---	-0.13	---	-0.65	---	-0.19
	---	0.07	---	0.18	---	0.08	---	0.20
Constant	5.60*	-6.67**	-9.22***	-17.10***	6.18***	1.05	-8.18***	-12.43***
	3.25	2.74	2.37	5.11	1.72	2.44	1.10	2.35
ln(p)	0.24*	---	---	---	0.38***	---	---	---
	0.12	---	---	---	0.10	---	---	---
Log pseudo-likelihood	-94.25	-851.63	---	-178.44	-43.22	-764.07	---	-167.82
Wald χ^2 (df)	211.36(13)	284.68(16)	234.12(12)	235.11(16)	165.61(13)	264.14(16)	292.26(12)	224.82(16)
N	2252	2197	1940	2056	1913	1860	1826	1870

Note: Robust standard errors clustered by country (survival models) or democratic episode (all other models)

$^*p<.10$, $^{**}p<.05$, $^{***}p<.01$, two-tailed tests

Appendix C.3. Multiplicative Models of Legal Institutions' Effect by Diamonds on Political Disorder (CNG)

	Legal Instiitions (CIM measure)				Legal Institutions (XCONST measure)			
	Survival	Banks Logit	Banks Count	Prio Logit	Survival	Banks Logit	Banks Count	Prio Logit
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8
Main Variables								
Legal Institutions	-6.15	4.23	5.17 *	9.51	-1.49 ***	-0.82 **	0.38 ***	0.56 ***
	4.22	3.04	2.70	6.32	0.31	0.37	0.07	0.21
Ln(GDP per capita)	-0.64	0.52	0.55 *	1.33 **	-0.70 ***	-0.50	0.41 ***	0.60 ***
	0.46	0.34	0.31	0.60	0.22	0.32	0.10	0.23
Diamond Production	-0.39	1.13 ***	1.16 **	0.52	-0.41	0.27 ***	0.22 **	0.54 **
	0.74	0.21	0.46	0.52	0.25	0.09	0.09	0.22
Oil Production	0.16	-0.08 *	-0.18 ***	0.02	0.17	-0.12 **	-0.19 ***	-0.12
	0.23	0.04	0.06	0.14	0.68	0.06	0.06	0.13
Legal Inst. X Ln(GDP)	1.35 **	-0.73 *	-0.86 **	-1.75 **	0.29 ***	0.08	-0.08 ***	-0.10 **
	0.56	0.43	0.37	0.87	0.06	0.05	0.01	0.04
Legal Inst. X Diamond	0.43	-1.29 ***	-1.38 **	-0.55	0.14 ***	-0.06 ***	-0.05 **	-0.14 **
	0.83	0.25	0.56	0.62	0.03	0.02	0.02	0.07
Control Variables								
Economic Growth	0.00 ***	0.00 *	0.00	-0.06 **	0.00	0.00 *	0.00	-0.09 ***
	0.00	0.00	0.00	0.02	0.00	0.00	0.00	0.02
Presidential Regime	-0.50	0.54 ***	0.81 ***	-0.06	-0.36	0.50 **	0.81 ***	-0.23
	0.36	0.20	0.15	0.33	0.33	0.24	0.16	0.33
Effective Parties	0.03	-0.07 *	-0.11 ***	-0.06	-0.02	-0.09 *	-0.13 ***	-0.08
	0.07	0.04	0.04	0.06	0.04	0.05	0.04	0.06
Religious Frac.	-0.06	-0.05	-0.03	0.29	-0.12	-0.37	-0.36	0.64
	0.74	0.44	0.33	1.02	0.98	0.50	0.35	1.04
Ethnic Frac.	-0.99	0.06	-0.16	1.31 *	-0.16	0.24	-0.07	1.72 **
	0.71	0.36	0.31	0.70	0.78	0.43	0.30	0.81
Previous Dem.	-0.19	---	---	---	0.11	---	---	---
	0.16	---	---	---	0.20	---	---	---
Former British Colony	0.22	---	---	---	-0.07	---	---	---
	0.39	---	---	---	0.42	---	---	---
Micro State	-0.11	---	---	---	8.92 ***	---	---	---
	0.54	---	---	---	1.24	---	---	---
Ln(Population)	---	0.25 ***	0.34 ***	0.45 ***	---	0.30 ***	0.42 ***	0.38 ***
	---	0.07	0.05	0.11	---	0.10	0.05	0.12
Ln(Elevation)	---	0.27 *	0.36 ***	0.27	---	0.22	0.31 ***	0.33
	---	0.15	0.11	0.26	---	0.19	0.12	0.28
Time Since Last Event	---	-0.66 ***	---	-0.13	---	-0.65 ***	---	-0.21
	---	0.07	---	0.18	---	0.08	---	0.19
Constant	6.08 *	-7.19 ***	-9.35 ***	-17.27 ***	6.08 ***	0.73	-8.35 ***	-13.16 ***
	3.53	2.74	2.36	5.03	1.70	2.38	1.10	2.42
ln(*p*)	0.25 **	---	---	---	0.41 ***	---	---	---
	0.12	---	---	---	0.10	---	---	---
Log pseudo-likelihood	-94.17	-848.17	---	-178.12	-41.78	-761.38	---	-166.10
Wald χ2 (df)	214.26(14)	360.58(17)	241.84(13)	278.01(17)	175.26(14)	338.89(17)	294.46(13)	282.95(17)
N	2252	2197	1940	2056	1913	1860	1826	1870

Note: Robust standard errors clustered by country (survival models) or democratic episode (all other models)

$^*p<.10$, $^{**}p<.05$, $^{***}p<.01$, two-tailed tests

Appendix C.4. Multiplicative Models of Legal Institutions' Effect by Oil on Political Disorder (CNG data)

	Legal Instititions (CIM measure)				Legal Institutions (XCONST measure)			
	Survival	Banks Logit	Banks Count	Prio Logit	Survival	Banks Logit	Banks Count	Prio Logit
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8
Main Variables								
Legal Institutions	-4.98	3.16	4.23	8.44	-1.70 ***	-0.87 **	0.38 ***	0.45 **
	3.66	3.10	2.79	6.49	0.36	0.37	0.07	0.22
Ln(GDP per capita)	-0.47	0.40	0.43	1.22 **	-0.85 ***	-0.54 *	0.39 ***	0.48 **
	0.37	0.34	0.32	0.60	0.25	0.32	0.10	0.24
Diamond Production	-0.07 *	-0.02	-0.03	0.01	-0.10 *	-0.04	-0.05	-0.07
	0.04	0.03	0.03	0.05	0.05	0.03	0.04	0.07
Oil Production	-13.31 ***	1.00	1.05	1.27	1.38 *	0.26	0.22	0.64 **
	4.78	0.68	0.67	1.13	0.80	0.24	0.24	0.30
Legal Inst. X Ln(GDP)	1.13 **	-0.60	-0.74 *	-1.61 *	0.34 ***	0.08	-0.08 ***	-0.09 **
	0.49	0.44	0.39	0.88	0.07	0.05	0.01	0.04
Legal Inst. X Oil	17.82 ***	-1.16	-1.32 *	-1.38	0.25 *	-0.05	-0.05	-0.12 **
	6.55	0.75	0.76	1.34	0.12	0.04	0.04	0.05
Control Variables								
Economic Growth	0.00 ***	0.00 *	0.00	-0.05 **	0.00	0.00	0.00 *	-0.08 ***
	0.00	0.00	0.00	0.02	0.00	0.00	0.00	0.02
Presidential Regime	-0.35	0.49 **	0.76 ***	-0.08	-0.39	0.45 *	0.74 ***	-0.26
	0.34	0.19	0.15	0.33	0.33	0.24	0.16	0.32
Effective Parties	0.03	-0.07 *	-0.10 ***	-0.06	-0.03	-0.09 *	-0.12 ***	-0.08
	0.06	0.04	0.04	0.06	0.05	0.05	0.04	0.06
Religious Frac.	-0.14	-0.01	0.00	0.30	0.07	-0.41	-0.40	0.63
	0.70	0.44	0.33	1.01	0.97	0.50	0.35	1.00
Ethnic Frac.	-0.84	-0.02	-0.26	1.24 *	-0.21	0.20	-0.07	1.67 **
	0.68	0.36	0.31	0.68	0.84	0.43	0.30	0.79
Previous Dem.	-0.22	---	---	---	0.04	---	---	---
	0.17	---	---	---	0.21	---	---	---
Former British Colony	0.28	---	---	---	-0.08	---	---	---
	0.36	---	---	---	0.45	---	---	---
Micro State	-0.09	---	---	---	9.48 ***	---	---	---
	0.50	---	---	---	1.29	---	---	---
Ln(Population)	---	0.25 ***	0.34 ***	0.44 ***	---	0.30 ***	0.41 ***	0.38 ***
	---	0.07	0.05	0.11	---	0.10	0.05	0.11
Ln(Elevation)	---	0.26 *	0.35 ***	0.26	---	0.22	0.31 ***	0.33
	---	0.16	0.11	0.26	---	0.19	0.12	0.29
Time Since Last Event	---	-0.66 ***	---	-0.12	---	-0.65 ***	---	-0.18
	---	0.07	---	0.19	---	0.08	---	0.20
Constant	5.00 *	-6.05 **	-8.24 ***	-16.27 ***	6.88 ***	1.15	-8.10 ***	-12.39 ***
	2.86	2.77	2.42	5.17	1.90	2.40	1.09	2.38
ln(p)	0.32 **	---	---	---	0.32 ***	---	---	---
	0.13	---	---	---	0.10	---	---	---
Log pseudo-likelihood	-90.85	-850.58	---	-177.99	-42.20	-763.34	---	-74.12
Wald χ^2 (df)	205.04(14)	297.57(17)	239.8(13)	271.3(17)	160.64(14)	263.45(17)	296.29(13)	
N	2252	2197	1940	2056	1913	1860	1826	2142

Note: Robust standard errors clustered by country (survival models) or democratic episode (all other models)

$^{*}p<.10$, $^{**}p<.05$, $^{***}p<.01$, two-tailed tests

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Table 1. Additive Models of Legal Institutions' Effect on Political Disorder (BNR data)

	Legal Instititions (CIM measure)				Legal Instititions (XCONST measure)			
	Survival	**Banks Logit**	**Banks Count**	**Prio Logit**	**Survival**	**Banks Logit**	**Banks Count**	**Prio Logit**
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8
Main Variables								
Legal Institutions	1.82 **	-0.63	-0.81	-0.83	0.01 *	-0.19 ***	-0.22 ***	-0.04 *
	0.92	1.00	0.63	1.77	0.01	0.08	0.05	0.02
Ln(GDP per capita)	0.61 ***	-0.02	-0.12 **	-0.25	0.68 ***	0.03	-0.06	-0.24
	0.09	0.09	0.06	0.23	0.11	0.10	0.06	0.25
Diamond Production	0.05	0.01	-0.01	0.07 *	0.05	0.00	-0.01	0.08 **
	0.06	0.03	0.02	0.03	0.06	0.03	0.02	0.03
Oil Production	0.17	-0.04	-0.10 **	0.13	0.13	-0.05	-0.11 **	0.14
	0.13	0.06	0.05	0.12	0.27	0.06	0.05	0.11
Control Variables								
Economic Growth	0.08 ***	-0.07 ***	-0.04 ***	-0.06 ***	0.08 ***	-0.06 ***	-0.03 ***	-0.07 ***
	0.02	0.02	0.01	0.03	0.02	0.02	0.01	0.03
Presidential Regime	-0.23	0.31	0.61 ***	-0.44	-0.34	0.27	0.59 ***	-0.78 *
	0.26	0.22	0.15	0.41	0.30	0.23	0.17	0.42
Effective Parties	0.07	-0.09 **	-0.09 **	-0.08	0.10	-0.10 *	-0.09 **	-0.07
	0.05	0.04	0.04	0.08	0.09	0.05	0.04	0.08
Religious Frac.	-1.61 **	-0.65	-0.88 ***	-0.48	-1.63 *	-0.90 *	-1.06 ***	-0.65
	0.72	0.43	0.31	1.00	0.93	0.51	0.35	1.12
Ethnic Frac.	0.04	0.19	-0.10	2.03 ***	-0.14	0.42	0.07	2.58 ***
	0.72	0.40	0.30	0.72	0.61	0.43	0.32	0.78
Previous Dem.	0.07	---	---	---	0.06	---	---	---
	0.23	---	---	---	0.30	---	---	---
Former British Colony	0.55 *	---	---	---	0.63 *	---	---	---
	0.32	---	---	---	0.34	---	---	---
Micro State	-0.06	---	---	---	0.24	---	---	---
	0.52	---	---	---	0.84	---	---	---
Ln(Population)	---	0.33 ***	0.43 ***	0.44 ***	---	0.38 ***	0.48 ***	0.44 ***
	---	0.07	0.05	0.12	---	0.10	0.06	0.12
Ln(Elevation)	---	0.19	0.34 ***	-0.03	---	0.11	0.34 ***	-0.06
	---	0.13	0.09	0.17	---	0.14	0.10	0.19
Time Since Last Event	---	-1.28 ***	---	-0.27	---	-1.34 ***	---	-0.30
	---	0.17	---	0.18	---	0.18	---	0.19
Constant	-1.94	-5.75 ***	-8.35 ***	-8.55 ***	-1.07	-5.72 ***	-8.96 ***	-9.05 ***
	1.31	1.12	0.94	1.91	1.11	1.41	1.09	1.99
ln(*p*)	0.34 ***	---	---	---	0.41 **	---	---	---
	0.13	---	---	---	0.18	---	---	---
Log pseudo-likelihood	-74.96	-730.92	---	-159.54	-58.42	-642.37	---	-137.77
Wald χ2 (df)	193.77(12)	324.25(15)	290.09(11)	137.86(15)	267.2(12)	290.01(15)	252.94(11)	174.77(15)
N	2142	2088	2082	1960	1752	1706	1687	1723

Note: Robust standard errors clustered by country (survival models) or democratic episode (all other models)

$^{*}p<.10$, $^{**}p<.05$, $^{***}p<.01$, two-tailed tests

Table 2. Multiplicative Models of Legal Institutions' Effect by GDP on Political Disorder (BNR data)

	Legal Institutions (CIM measure)				Legal Institutions (XCONST measure)			
	Survival	Banks Logit	Banks Count	Prio Logit	Survival	Banks Logit	Banks Count	Prio Logit
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8
Main Variables								
Legal Institutions	-9.06**	8.65**	12.20***	17.62**	-1.39***	-0.31	-0.28	0.90***
	4.05	4.03	2.93	7.36	0.24	0.40	0.27	0.32
Ln(GDP per capita)	-0.70	1.02**	1.31***	1.97**	-0.86**	-0.07	-0.12	1.00***
	0.48	0.44	0.33	0.87	0.34	0.34	0.25	0.35
Diamond Production	0.03	0.00	-0.01	0.05	0.23**	0.01	-0.01	0.03
	0.06	0.03	0.02	0.03	0.11	0.03	0.02	0.04
Oil Production	0.16	-0.02	-0.08	0.15	-0.08	-0.05	-0.11**	0.10
	0.13	0.05	0.05	0.11	0.14	0.06	0.05	0.10
Legal Inst. X Ln(GDP)	1.71***	-1.27**	-1.75***	-2.80**	0.28***	0.02	0.01	-0.19***
	0.61	0.56	0.39	1.23	0.05	0.05	0.04	0.06
Control Variables								
Economic Growth	0.08***	-0.07***	-0.04***	-0.07***	0.08***	-0.06***	-0.03***	-0.07***
	0.02	0.02	0.01	0.03	0.03	0.02	0.01	0.02
Presidential Regime	-0.15	0.21	0.44***	-0.53	0.03	0.28	0.59***	-0.95**
	0.24	0.21	0.15	0.42	0.38	0.23	0.17	0.38
Effective Parties	0.08	-0.09**	-0.10***	-0.11	0.10	-0.10*	-0.09**	-0.09
	0.06	0.04	0.04	0.08	0.07	0.05	0.04	0.08
Religious Frac.	-1.87**	-0.50	-0.69**	0.06	-1.92**	-0.92*	-1.07***	-0.25
	0.80	0.43	0.32	1.32	0.80	0.53	0.35	1.37
Ethnic Frac.	-0.23	0.32	0.10	2.19***	-0.19	0.42	0.07	3.09***
	0.71	0.40	0.30	0.70	0.78	0.43	0.32	0.90
Previous Dem.	0.14	---	---	---	0.05	---	---	---
	0.28	---	---	---	0.21	---	---	---
Former British Colony	0.50*	---	---	---	0.17	---	---	---
	0.30	---	---	---	0.44	---	---	---
Micro State	-0.07	---	---	---	0.17	---	---	---
	0.46	---	---	---	0.92	---	---	---
Ln(Population)	---	0.35***	0.45***	0.48***	---	0.38***	0.49***	0.47***
	---	0.08	0.05	0.12	---	0.10	0.06	0.15
Ln(Elevation)	---	0.18	0.31***	-0.03	---	0.12	0.34***	-0.15
	---	0.13	0.09	0.20	---	0.15	0.11	0.20
Time Since Last Event	---	-1.27***	---	-0.25	---	-1.34***	---	-0.29
	---	0.18	---	0.17	---	0.18	---	0.19
Constant	6.52*	-13.55***	-18.83***	-23.83***	6.93***	-5.05**	-8.65***	-15.68***
	3.62	3.51	2.60	6.09	2.44	2.46	1.75	2.97
ln(*p*)	0.319***	---	---	---	0.269**	---	---	---
	0.124	---	---	---	0.135	---	---	---
Log pseudo-likelihood	-71.82	-726.48	---	-155.67	-35.10	-642.31	---	-132.23
Wald χ^2 (df)	131.04(13)	322.14(16)	300.49(12)	224.88(16)	354.39(13)	291.59(16)	252.91(12)	205.05(16)
N	2142	2088	2082	1960	1752	1706	1687	1723

Note: Robust standard errors clustered by country (survival models) or democratic episode (all other models)

*p <.10, **p<.05, ***p <.01, two-tailed tests

Table 3. Multiplicative Models of Legal Institutions' Effect by Diamonds on Political Disorder (BNR data)

	Legal Instititions (CIM measure)				Legal Institutions (XCONST measure)			
	Survival	Banks Logit	Banks Count	Prio Logit	Survival	Banks Logit	Banks Count	Prio Logit
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8
Main Variables								
Legal Institutions	-9.72 **	8.64 **	11.78 ***	17.01 **	-1.43 ***	-0.27	-0.26	0.83 **
	4.24	3.99	2.95	7.38	0.27	0.39	0.27	0.33
Ln(GDP per capita)	-0.74	1.01 **	1.26 ***	1.91 **	-0.86 **	-0.07	-0.09	0.94 ***
	0.49	0.43	0.33	0.88	0.39	0.32	0.25	0.35
Diamond Production	-1.50	1.21 ***	1.29 ***	0.69	-47.95 ***	0.34 ***	0.28 **	0.82
	0.94	0.24	0.47	0.59	10.81	0.08	0.12	0.65
Oil Production	0.10	-0.07	-0.14 ***	0.07	-0.25	-0.10 *	-0.18 ***	0.02
	0.15	0.04	0.05	0.15	0.16	0.06	0.06	0.12
Legal Inst. X Ln(GDP)	1.76 ***	-1.24 **	-1.65 ***	-2.68 **	0.29 ***	0.02	0.01	-0.17 ***
	0.61	0.55	0.39	1.24	0.05	0.05	0.04	0.06
Legal Inst. X Diamond	2.04 *	-1.39 ***	-1.54 ***	-0.74	47.67 ***	-0.07 ***	-0.07 **	-0.25
	1.27	0.29	0.58	0.70	10.87	0.02	0.03	0.21
Control Variables								
Economic Growth	0.08 ***	-0.07 ***	-0.04 ***	-0.07 **	0.07 **	-0.07 ***	-0.03 ***	-0.06 ***
	0.02	0.02	0.01	0.03	0.03	0.02	0.01	0.02
Presidential Regime	-0.16	0.30	0.55 ***	-0.42	-0.07	0.42 *	0.72 ***	-0.80 **
	0.24	0.21	0.15	0.42	0.38	0.25	0.17	0.38
Effective Parties	0.07	-0.10 **	-0.12 ***	-0.13	0.08	-0.11 *	-0.11 ***	-0.10
	0.05	0.05	0.04	0.10	0.06	0.06	0.04	0.09
Religious Frac.	-1.82 **	-0.45	-0.64 **	0.09	-1.66 *	-0.78	-0.94 ***	-0.16
	0.79	0.44	0.32	1.30	0.94	0.54	0.36	1.32
Ethnic Frac.	-0.27	0.39	0.18	2.24 ***	-0.06	0.47	0.12	3.05 ***
	0.69	0.40	0.30	0.72	0.83	0.44	0.32	0.91
Previous Dem.	0.26	---	---	---	0.20	---	---	---
	0.28	---	---	---	0.25	---	---	---
Former British Colony	0.61 *	---	---	---	0.26	---	---	---
	0.33	---	---	---	0.49	---	---	---
Micro State	-0.16	---	---	---	0.05	---	---	---
	0.48	---	---	---	0.98	---	---	---
Ln(Population)	---	0.36 ***	0.45 ***	0.50 ***	---	0.40 ***	0.50 ***	0.49 ***
	---	0.08	0.05	0.13	---	0.11	0.06	0.15
Ln(Elevation)	---	0.17	0.30 ***	-0.04	---	0.11	0.32 ***	-0.14
	---	0.13	0.09	0.20	---	0.14	0.11	0.20
Time Since Last Event	---	-1.25 ***	---	-0.25	---	-1.31 ***	---	-0.31
	---	0.18	---	0.17	---	0.18	---	0.19
Constant	6.96 *	-13.88 ***	-18.84 ***	-23.94 ***	6.74 **	-5.74 **	-9.24 ***	-15.83 ***
	3.75	3.46	2.61	6.07	2.81	2.40	1.78	2.95
ln(*p*)	0.34 ***	---	---	---	0.24	---	---	---
	0.13	---	---	---	0.14	---	---	---
Log pseudo-likelihood	-71.11	-722.85	---	-155.15	-33.14	-638.84	---	-131.33
Wald χ2 (df)	265.37(13)	484.23(17)	306.06(13)	337.31(17)	424.64(14)	407.35(17)	254.67(13)	303.59(17)
N	2142	2088	2082	1960	1752	1706	1687	2142

Note: Robust standard errors clustered by country (survival models) or democratic episode (all other models)

$^*p$ <.10, **p<.05, $^{***}p$ <.01, two-tailed tests

Table 4. Multiplicative Models of Legal Institutions' Effect by Oil on Political Disorder (BNR data)

	Legal Institutions (CIM measure)				Legal Institutions (XCONST measure)			
	Survival	**Banks Logit**	**Banks Count**	**Prio Logit**	**Survival**	**Banks Logit**	**Banks Count**	**Prio Logit**
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8
Main Variables								
Legal Institutions	-8.46**	7.74*	11.23***	15.87**	-1.40***	-0.33	-0.32	0.80**
	4.01	4.10	2.98	7.60	0.24	0.39	0.27	0.34
Ln(GDP per capita)	-0.63	0.91**	1.19***	1.78**	-0.86**	-0.11	-0.15	0.91**
	0.47	0.44	0.33	0.90	0.34	0.32	0.25	0.36
Diamond Production	0.02	-0.02	-0.04	0.02	0.18	-0.05	-0.07	-0.06
	0.07	0.03	0.03	0.05	0.22	0.04	0.05	0.07
Oil Production	-5.45	1.34**	1.08*	1.44	0.16	0.59***	0.42*	0.66**
	9.40	0.58	0.65	1.43	0.71	0.20	0.25	0.33
Legal Inst. X Ln(GDP)	1.60***	-1.13**	-1.60***	-2.53**	0.29***	0.02	0.02	-0.17**
	0.59	0.57	0.40	1.26	0.05	0.05	0.04	0.07
Legal Inst. X Oil	7.40	-1.51**	-1.30*	-1.49	-0.04	-0.10***	-0.09**	-0.11*
	12.74	0.64	0.73	1.67	0.12	0.03	0.04	0.06
Control Variables								
Economic Growth	0.08***	-0.07***	-0.04***	-0.06**	0.08***	-0.06***	-0.03**	-0.06**
	0.02	0.02	0.01	0.03	0.03	0.02	0.01	0.02
Presidential Regime	-0.15	0.24	0.49***	-0.45	0.03	0.36	0.68***	-0.81**
	0.24	0.21	0.15	0.43	0.38	0.25	0.17	0.38
Effective Parties	0.08	-0.10**	-0.11***	-0.13	0.09	-0.11*	-0.11***	-0.10
	0.06	0.05	0.04	0.10	0.07	0.06	0.04	0.09
Religious Frac.	-1.82**	-0.42	-0.61*	0.09	-1.88**	-0.82	-0.97***	-0.02
	0.79	0.44	0.32	1.29	0.87	0.53	0.36	1.32
Ethnic Frac.	-0.22	0.29	0.07	2.18***	-0.18	0.44	0.10	3.12***
	0.71	0.40	0.30	0.71	0.79	0.44	0.32	0.89
Previous Dem.	0.15	---	---	---	0.07	---	---	---
	0.28	---	---	---	0.22	---	---	---
Former British Colony	0.54*	---	---	---	0.19	---	---	---
	0.31	---	---	---	0.46	---	---	---
Micro State	-0.09	---	---	---	0.16	---	---	---
	0.46	---	---	---	0.91	---	---	---
Ln(Population)	---	0.36***	0.45***	0.49***	---	0.39***	0.50***	0.50***
	---	0.08	0.05	0.12	---	0.10	0.06	0.15
Ln(Elevation)	---	0.16	0.29***	-0.05	---	0.12	0.33***	-0.16
	---	0.13	0.09	0.20	---	0.14	0.11	0.20
Time Since Last Event	---	-1.26***	---	-0.24	---	-1.32***	---	-0.27
	---	0.18	---	0.18	---	0.18	---	0.20
Constant	6.10	-12.82***	-18.09***	-22.69***	6.88***	-5.12**	-8.72***	-15.83***
	3.60	3.54	2.63	6.23	2.52	2.39	1.77	2.99
ln(p)	0.32***	---	---	---	0.27**	---	---	---
	0.12	---	---	---	0.14	---	---	---
Log pseudo-likelihood	-71.45	-725.00	---	-155.23	-35.07	-640.63	---	-131.68
Wald χ^2 (df)	170.11(14)	363.15(17)	304.29(13)	304.05(17)	352.07(14)	398.62(17)	255.81(13)	376.23(17)
N	2142	2088	2082	1960	1752	1706	1687	1723

Note: Robust standard errors clustered by country (survival models) or democratic episode (all other models)

*$p<.10$, **$p<.05$, ***$p<.01$, two-tailed tests

Figure 1. Effects of Legal System Effectiveness and Socioeconomic Context on Democratic Regime Survival

Panel A. Effect of 1 std. dev. Increase in CIM on Expected Duration across ln(GDP).

(Expected Duration Measured in Years)

Panel B. Effect of 1 std. dev. Increase in ln(GDP) on Expected Duration across CIM.

(Expected Duration Measured in Years)

Note: Dashed lines represent 95% confidence intervals around the expected marginal effect.
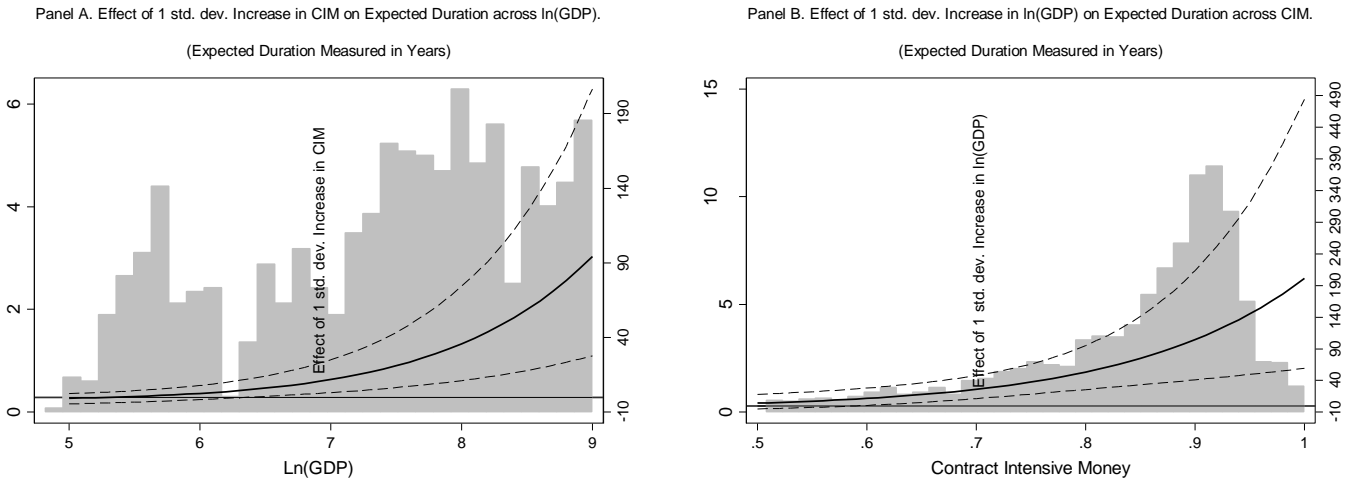
Figure 2. The Difference in Expected Duration of Democratic Regimes between Additive and Conditional Models of GDP and CIM