A Global Model for Forecasting Political Instability

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Examining onsets of political instability in countries worldwide from 1955 to 2003, we develop a model that distinguishes countries that experienced instability from those that remained stable with a two-year lead time and over 80% accuracy. Intriguingly, the model uses few variables and a simple specification. The model is accurate in forecasting the onsets of both violent civil wars and nonviolent democratic reversals, suggesting common factors in both types of change. Whereas regime type is typically measured using linear or binary indicators of democracy/autocracy derived from the 21-point Polity scale, the model uses a nonlinear five-category measure of regime type based on the Polity components. This new measure of regime type emerges as the most powerful predictor of instability onsets, leading us to conclude that political institutions, properly specified, and not economic conditions, demography, or geography, are the most important predictors of the onset of political instability.

Background

Paul Collier and Anke Hoeffler's (2002, 2004) seminal essays on "greed and grievance" sparked a rich debate, and a surge of quantitative studies, on the causes of civil wars. Their view that economic opportunities were the main factors fueling insurgencies was met by James Fearon and David Laitin's (2003) claim that political factors giving rebels advantages vis-à-vis states were the key to the outbreak of civil wars. Further important contributions to this debate have been made by Gates and Hegre and their coauthors (Collier et al. 2003; Gates et al. 2006; Hegre 2004; Hegre et al. 2001), Sambanis (2001; Elbadawi and Sambanis 2000, 2002; Hegre and Sambanis 2006), and others (Regan, 2000; Regan and Norton 2005; Walter 2004).

Strikingly, these approaches focus their attention on the economic resources available to states and insurgents. Collier and Hoeffler stress insurgents' ability to finance themselves from "lootable" natural resources; Fearon and

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Laitin stress the capacity of states to finance armies versus insurgents' ability to take advantage of large populations, rough terrain, and political instability. Other authors focus on states' control of natural resources (Dunning 2005; Franke 2007; Ross 2004; Snyder and Bhavani 2005). Though far more sophisticated in their modeling and data analysis, these analyses echo the "resource mobilization" theory of conflict advocated by the sociologist Charles Tilly (1978) several decades ago.

Recent trends in the study of revolutions (one of the most common events producing civil wars) have moved in a different direction, however, adopting a *state-centered* approach that focuses on political structures and elite relationships, rather than simply on state resources or insurgent capabilities, as the critical factors determining where and when revolutions occur (Bates 2008; Goldstone 1991, 2001; Goodwin 2001; Skocpol 1979). In this view, most states have potential insurgents with grievances and resources, but almost always possess far greater military power than do insurgents. A united and administratively competent regime can defeat any insurgency; it is where regimes are paralyzed or undermined by elite divisions and state-elite conflicts that revolutionary wars can be sustained and states lose out to insurgencies.

We draw on this insight to develop a model that forecasts with a two-year lead time the outbreak of civil wars and other forms of political instability. Notable features of this model are as follows:

1. Its simplicity: it is based upon a small number of variables (four) that are categorical or continuous and does not depend on complex interactions or nonlinear effects.
2. Its accuracy: with equal levels of type 1 and type 2 errors, the model achieved over 80% accuracy in distinguishing country-years followed by instability from ones where stability continued.
3. Its generality: it accurately forecasts different kinds of instability, both violent events such as revolutionary and ethnic wars and the nonviolent failure of democracies.
4. And its novelty: our search for a predictive tool led to the evaluation and exclusion of many of the variables employed in the earlier literature. Our analysis therefore suggests a new approach focusing on state structures and elite relationships that provides greater predictive power than approaches focused on economic resources—even when we restrict our dependent variable to the onset of civil war.

Data and Definitions

We compiled our data from open sources to construct a cross-national time-series data set covering the period 1955 through 2003 for all countries with a population over 500,000. We identified "instability episodes" in part by identifying conflicts from existing databases (such as the Correlates of War) and in part by consulting with area experts.

Types of Instability

We began with a conventional identification of civil wars (including both ethnic and revolutionary wars) similar to that used by Fearon and Laitin (2003). These are events that resulted in at least 1,000 total deaths from conflicts involving state forces, sustained at a rate of at least 100 deaths per year. The onset of each event is dated from the first year in which the conflict produced at least 100 directly related deaths. A civil war is deemed to have ended when three consecutive years have passed with fewer than 100 deaths per year; the end date is then the first year in which deaths involving conflicts with state forces fell below 100 per year.

In addition to the onset of large-scale violent conflicts, we wanted to examine the onset of other types of undesirable political instability, including democratic reversals, genocides, and state collapse. These have been of great interest to policy makers and scholars in recent years. Given the recent sharp drop in civil wars and the simultaneous increase in democratic reversals (Freedom House 1972–2006; Human Security Centre 2005; Marshall and Gurr 2001/2003/2005), such events may be of even greater importance in the future. We thus compiled data on two additional types of instability events:

Adverse Regime Changes are major, adverse shifts in political institutions that involve the sudden loss of authority of central state institutions and/or their replacement by a more radical or nondemocratic regime. Most of our adverse regime changes are substantial shifts away from democratic toward authoritarian rule. Signaling the latter is a downward shift, within three years, of six or more points on the 21-point Polity IV autocracy-democracy scale. Also counted as an adverse regime change is the collapse of central state authority, as occurred in Somalia and the Democratic Republic of the Congo in the early 1990s; the overthrow of a government by a radical revolutionary regime, as in Cuba in 1959 and Iran in 1979; and the contested dissolution of federated states or the secession of a substantial area of a state by extrajudicial means, as occurred in the USSR and Yugoslavia in 1991, or in Pakistan in 1972.

Although adverse regime changes sometimes coincide with or result from civil wars, adverse regime changes are analytically distinct events, as they are identified by a substantial change in the institutional structure of political authority, not by a change in the level of violence. Many civil wars do *not* involve adverse regime changes, as the ruling regime remains in authority throughout; and many adverse regime changes (e.g., bloodless military coups against democratic governments) occur with little violence, and thus do not count as civil wars.

Peaceful transitions to democracy (involving fewer than 1,000 total deaths), as occurred after the death of Salazar in Portugal or Franco in Spain and in several of the post–Cold War transitions from communist regimes in Eastern Europe, are not counted as adverse regime changes. Neither is the peacefully negotiated dissolution of a federal union, as occurred in Czechoslovakia in 1993.

Genocides and Politicides are sustained and purposive efforts by states or their agents to visit extreme violence and/or death upon a particular communal or political group (see Harff 2003). In genocides, the victimized groups are defined primarily by their communal character. Rwanda provides an apposite example. In politicides, the targeted groups are defined by their political opposition. Examples include the targeting of opposition groups in South and Central America: Chile in 1973–76, Argentina in 1976–80, and El Salvador in 1980–89. Genocides and politicides often result in fewer victims than civil wars; the extreme nature of the violence—the attempt to destroy and eradicate a particular group—nonetheless warrants their inclusion in a study of instability.

In practice, revolutionary and ethnic wars, regime crises, and mass atrocities often occur concurrently. Adverse regime changes, for example, sometimes precipitate and sometimes follow from civil wars. Genocides and politicides virtually always occur in the train of other types of instability. Moreover, the different kinds of instability often take place in rapid succession, marking them as interdependent events. We therefore treated overlapping and closely sequenced instability events within a single country as forming a single complex episode of political instability. In cases where multiple events overlapped, the episode was considered to have begun at the start of the first event and to have ended at the conclusion of the last. Sequential events in the same country were treated as a single case if fewer than five years elapsed between the end of one event and the start of another.

Properties of the Data

Using these definitions, we identified 141 separate instability episodes over the period 1955–2003 (see Appendix A). Roughly half (71) of these were complex episodes involving a combination of different types of instability that overlapped or followed upon each other in close sequence. The remaining episodes consisted of adverse regime changes (44), roughly equal numbers of revolutionary wars (12) and ethnic wars (13), and a single isolated politicide.

Spiking in the early 1960s and 1990s, the number of new episodes per decade has remained fairly constant over time, averaging roughly three onsets per annum over the entire period, though it has declined markedly in recent years. The number of instability onsets by decade was: before 1964 (35), 1965–74 (35), 1975–84 (26), 1985–1994 (28), and 1995–2003 (17).[1] The relative frequency of the different types of instability remained consistent across much of the period of observation; the chief exception came in the 1980s, when there were more revolutionary and ethnic wars but fewer adverse regime changes.

Sub-Saharan Africa generated the most instability episodes during our period of observation with 49, or 34.8% of the global total. This was followed by the North Africa/Middle East/Central Asia region with 32 episodes (22.7%), Europe and the former Soviet Union with 23 episodes (16.3%), Latin America and the Caribbean with 19 (13.5%), and East Asia and the Pacific with 18 (12.8%).[2]

The approximately 7,500 country-years in our period of observation gave rise to 141 episodes of political instability, with an average of two to three onsets of instability per year. The phenomenon thus occurred in only 1.9% of our observations. Because our forecasts are based on data from two years prior to the onset of instability, the number of usable observations declined further, leaving us with 117 onsets for our research.[3] Even though of

[1] Recent updates of the problem set through 2004 raised the number of conflict onsets in 1995–2004 to 21. However, as the number of independent states has also risen since 1990, the rate of new instability onsets per state in the global system has declined even more sharply. For discussions of the late 1990s decline in conflict, see Gurr (2000), Marshall and Gurr (2001/2003/2005), and Human Security Centre (2005).

[2] The higher number of instability onsets in Africa is not simply due to the larger number of countries in sub-Saharan Africa versus other regions. The universe of country-years in which instability onsets can occur excludes countries that are not independent and those that are experiencing an ongoing conflict—both situations common in Africa. Thus from 1955 to 2003 the North Africa/Middle East/Central Asia region and sub-Saharan Africa had roughly the same number of stable country-years eligible for instability onsets.

[3] We had to set aside episodes in newly independent countries and those ongoing or starting in 1955 or 1956, as in these cases we could not obtain reliable data from the same unit two years prior to event onset. Using available postindependence data to characterize

major significance, outbreaks of political instability are infrequent.

Building the Model

Given the rarity of the events of concern, we adopted the case-control method, which is widely employed to study uncommon afflictions, such as heart attacks or the onset of cancers.[4] When applying this method, individuals who experienced the affliction are compared to a sample of similar individuals drawn from the larger unaffected population, and factors are sought that distinguish the afflicted (the problem cases) from the nonafflicted (the controls).

Having identified our instability onset cases (the problems), we compared data from those cases, observed two years prior to the instability onset, with data from three randomly chosen control sets of country-years drawn from countries that remained stable (our control cases). The universe of control cases consisted of all country-years from 1955 to 2003 in which countries with at least 500,000 inhabitants had been stable for at least seven years: a minimum of two years prior to and four years following the country-year used in our analysis. These intervals were chosen to ensure that no country-years used as controls were drawn from countries that had just emerged from, or were about to descend into, an episode of instability.

The controls were matched to the onset cases by geographic region and year, thus correcting for possible bias arising from the different propensities for instability in different regions and time periods. Country-years from sub-Saharan Africa form a large part of the problem set, for example; but were we to compare these cases to a random set of stable country-years drawn from all parts of the world, country-years from North America and Europe would be overrepresented among the controls. In addition, many instability events occurred following the Cold War; we wished to compare countries that experienced instability in those years with countries that remained stable in that same period. We therefore designed our control sets of country-years to have the same regional and temporal distribution as our problem set and drew time and region-matched sets of controls that were three times as large as the number of problem cases.[5]

To illustrate: for every country in sub-Saharan Africa that had an episode of instability onset in the year 1969, we randomly selected three controls from among all those countries in sub-Saharan Africa that were free of instability *at least* from 1965 to 1971. Data for all four countries were then collected from 1967 for our analysis. The resultant selection of controls allowed us to compare conditions in 1967 in the country that went on to experience instability two years hence with conditions at the same time in countries in the same region that remained stable.[6]

Repeating this procedure for all instability onsets for which we had reliable data from two years prior to onset provided a sample of 351 stable controls against which to compare our 117 instability cases. To allow more robust inferences, we repeated this process three times, drawing three separate sets of matched random controls, and tested any proposed model on all three of the resulting samples. Each model was thus tested against $117 + (3 \times 351) = 1{,}170$ observations.

To assess the accuracy of candidate models in identifying conditions that led to instability onsets, we examined how well they distinguished between cases and controls, using conditional logistic regression. This method estimates the independent variables' effects conditional on membership in clusters consisting of the matched samples. This technique is commonly used when background factors (such as age or gender in disease treatment) are suspected of confounding the effects of the independent variables; the clustering removes the confounding effects

newly independent countries, we undertook a sensitivity analysis that compared results with and without these cases in our samples and found no significant differences in the substantive findings. We nonetheless chose to take a cautious approach and to exclude them entirely. The specific episodes excluded are noted in Appendix A.

[4]King and Zeng (2001a) explicitly recommend this method to study rare conflict events.

[5]One could use dummy variables for region and year for this purpose, but at the cost of a proliferation of right-hand-side variables. Not only would the additional dummies draw variance away from the economic, political, demographic, and other substantive variables whose effects we wish to examine, but also they would further reduce the small number of degrees of freedom afforded by the modest number of cases of instability onsets. Following King and Zeng (2001a), we sought to ensure that the number of randomly drawn control cases should be large enough to produce a data set with several hundred data points, large enough to provide a sound statistical basis for analysis, but small enough that the cases of interest (the problem cases) not fall below 25% of the cases examined.

[6]Because in our case-control analysis the problem and control cases are observed only in a specific year (the year two years prior to onset of the failure event), our data *do not* contain strings of observations from consecutive years in the same country. It is, of course, still possible that prior instability events affect the risk of new events; but when we explored this possibility, we found no evidence for it. Our procedure eliminates the possibility of serial autocorrelation, which arises when strings of consecutive years are included in the data. For studies of the difficulties with serial autocorrelation in pooled time-series, see Beck, Katz, and Tucker (1998); Beck (2001); Beck and Katz (2001); Green, Kim, and Yoon (2001); and King and Zeng (2001a, 2001b).

(Rothman 2002, 44–66). This method is analogous to a fixed-effects design in conventional regression, only in this instance the fixed effects are associated with specific combinations of region and year rather than with individual countries, as is usually the case in analysis of time-series cross-sectional data.

The conditional logistic models identify the impact of each independent variable, namely the relative odds that a country will experience an instability onset in year $t + 2$, given the value of that independent variable in that country in year t, compared to a similarly situated country with the reference level of that independent variable. The coefficients thus obtained, plus the value of the independent variables for each country-year, allow us to rank all the country-years in each sample set (117 problem cases and 351 controls) by their model scores.[7] To make binary predictions—that an instability onset will or will not occur—we choose a cut point in the ranking and consider those country-years above the cut point as cases that will experience an instability onset, and those below the cut point as cases that will remain stable. These predictions are then compared with the actual observed incidence of instability in the cases above and below the cut point in each of the three case-control samples. To ensure that the cut point is not chosen arbitrarily, we adjust it to equalize the percentage of type 1 and type 2 errors; in other words each model is forced to balance the percent of false positives (the fraction of countries above the cut point that remain stable) and the percent of false negatives (the fraction of countries below the cut point that do experience an instability onset).

In choosing among different models, we noted that a wide variety of variables have been shown to be statistically significantly associated with civil conflict (Hegre and Sambanis 2006). We therefore did not wish to make the statistical significance of coefficients our main criteria for evaluating competing models (Ziliak and McCloskey 2008). Rather, we wished to identify the combination of factors that could best predict the onset of violent conflict from prior conditions. We therefore posed our research problem as follows: Given a set of states that have been stable (e.g., without major conflicts) for at least three years, which variables and relationships will best enable us to distinguish those states where a major conflict will soon arise from those that will remain stable?

We kept or discarded models and independent variables based on their overall accuracy and robustness across our samples of controls. We sought models that would be stable across the different sample sets, and with the least error in discriminating between countries that would and would not actually experience instability onsets.

We also sought models that were parsimonious. This criterion led us to drop from the analysis variables that bore a statistically insignificant relationship with subsequent instability, as well as those that proved statistically significant but whose inclusion made no appreciable impact upon the model's ability to distinguish stable from unstable cases.

We tested a large number of independent variables, drawn from the theoretical literature and suggested to us by experts, in diverse specifications, to seek models that best differentiated stable from unstable countries. A short list of some of the variables tested is given in Appendix B.

The Model

Applying this strategy, we developed the model summarized in Table 1. The model incorporates just four independent variables: a categorical measure of *Regime Type*, as indicated by patterns in the process of executive recruitment and the competitiveness of political participation; *Infant Mortality*, logged and normalized to the global average in the year of observation; a *Conflict-Ridden Neighborhood* indicator, flagging cases that have four or more bordering states with major armed civil or ethnic conflict, according to the Major Episodes of Political Violence data set; and a binary measure of *State-Led Discrimination*, as indicated by a coding of 4 on either of the indices of political or economic discrimination for any group tracked by the Minorities at Risk Project (CIDCM 2006; Gurr 1993).[8]

Classifying Regimes

While the latter three variables—infant mortality, conflict in neighboring states, and political/economic discrimination—are straightforward and have been commonly discussed in studies of conflict, our measure of regime type is new. Most studies of democracy have used a binary categorization of democracy/autocracy (e.g., Przeworski et al. 2000) or a three-category measure (autocracy/anocracy/democracy) based on the Polity IV 21-point autocracy/democracy scale. Yet as many scholars

[7]These country-year model scores are *not* actual estimates of the risk of instability, which require unconditional regression and a correction for the random sampling procedure (see King and Zeng 2001a, 2001b).

[8]See www.systemicpeace.org/inscr/inscr.htm for the Major Episodes of Political Violence data and www.cidcm.umd.edu/mar/data.asp for the Minorities at Risk Project data.

TABLE 1 Results of Global Analysis of Onsets of Instability

Independent Variables	Full Problem Set Coefficient (S.E.)	Full Problem Set Odds Ratio (95% CI)	Civil War Onsets Coefficient (S.E.)	Civil War Onsets Odds Ratio (95% CI)	Adverse Regime Change Onsets Coefficient (S.E.)	Adverse Regime Change Onsets Odds Ratio (95% CI)
Regime Type (Full Autocracy as Reference)						
Partial Autocracy	1.85***	6.37	1.94***	6.98	2.85***	17.32
	(0.47)	(2.53, 16.02)	(0.62)	(2.05, 23.8)	(0.86)	(3.19, 94.0)
Partial Democracy with Factionalism	3.61***	36.91	3.35***	28.5	5.06***	157.0
	(0.51)	(13.5, 101)	(0.73)	(6.86, 118)	(1.02)	(21.1, 1164)
Partial Democracy without Factionalism	1.83***	6.22	.981	2.67	2.58***	13.23
	(0.54)	(2.17, 17.8)	(0.79)	(0.57, 12.4)	(0.91)	(2.20, 79.5)
Full Democracy	0.981	2.67	.545	1.73	1.26	3.51
	(0.68)	(0.70, 10.2)	(0.92)	(0.29, 10.4)	(1.09)	(0.42, 29.5)
Infant Mortality†	1.59***	6.59	1.64***	4.19	1.38*	4.56
	(0.35)	(2.91, 14.9)	(0.48)	(1.82, 9.60)	(0.58)	(1.30, 16.0)
Armed Conflict in 4+ Bordering States	3.09***	22.0	2.81***	16.7	.091	1.10
	(0.95)	(3.42, 142)	(0.82)	(3.36, 83.0)	(1.49)	(0.06, 20.4)
State-Led Discrimination	0.657*	1.93	1.17***	3.23	−.502	0.61
	(0.30)	(1.08, 3.45)	(0.36)	(1.59, 6.55)	(0.62)	(0.18, 2.04)
N = Total (Problems, Controls)	468 (117, 351)		260 (65, 195)		196 (49, 147)	
Onsets Correctly Classified	80.3%		80.0%		87.8%	
Controls Correctly Classified	81.8%		81.0%		87.8%	

*** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$. †Odds ratios for continuous variables compare cases at the 75th and 25th percentiles.

have noted, these operationalizations of regime type are highly problematic. A binary categorization is too crude to capture the most common movements into and out of democracy (Epstein et al. 2006). At the same time, the "anocracy" category in the three-category scale is ambiguous. Because the Polity scale is made up of varied components, quite diverse combinations of characteristics can place countries in the middle-range or "anocracy" category (Elkins 2000; Gates et al. 2006; Munck and Verkuilen 2002; Trier and Jackman 2008; Vreeland 2008). As these scholars have suggested, we found it more useful to abandon the linear Polity scale and instead work directly with the Polity components to develop a categorical measure of regime type, based on various combinations of those components and their values.

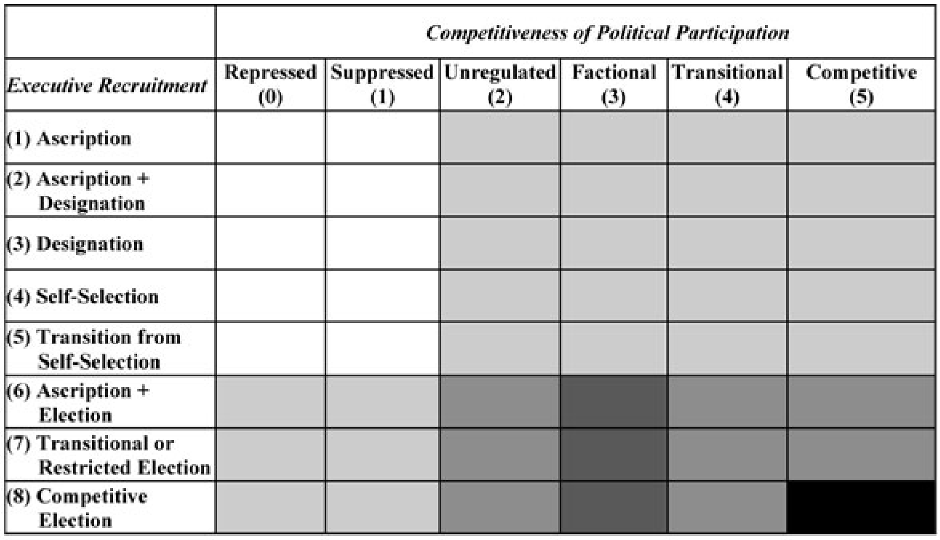
Our measure is derived from two variables in the Polity data set (Marshall and Jaggers, 2003) that roughly correspond to the two dimensions Dahl (1971) uses to characterize modern forms of government. We use Polity's scale for the openness of executive recruitment (EXREC) as a measure of contestation and Polity's scale of the competitiveness of political participation (PARCOMP) to capture variation in the degree and forms of inclusiveness. Figure 1 shows the two-dimensional space produced by the intersection of these two variables and illustrates how we divide that space into categories for purposes of regime identification.[9]

The white cells in Figure 1 represent regimes we call *full autocracies*—systems that combine an absence of effective contestation for chief executive with repressed or suppressed political participation. This category includes repressive one-party states, absolutist monarchies, and authoritarian dictatorships (e.g., North Korea, China, Saudi Arabia, and Sudan). In the opposite corner, in black, are regimes we call *full democracies*—systems that combine free and fair elections with open and well-institutionalized political participation (e.g., all OECD countries, and some developing countries such as Costa Rica, Uruguay, and Mongolia).

Three intermediate categories occupy the conceptual space between those extremes. The light grey cells represent regimes we call *partial autocracies*, which hold competitive elections for national office but repress or tightly

[9]Coppedge, Alvarez, and Maldonado (2008) have also identified these two dimensions as critical to most conceptions of democracy.

FIGURE 1 Characterizing Regime Types for Analysis of Instability



Based on POLITY IV scales for Executive Recruitment (EXREC) and Competitiveness of Political Participation (PARCOMP).

White = Full Autocracy Light grey = Partial Autocracy Dark grey = Partial Democracy

Very dark grey = Partial Democracy w/factionalism Black = Full Democracy

control participation (e.g., Singapore or apartheid-era South Africa) or allow substantial political participation but fail to subject the office of chief executive to truly competitive elections (e.g., Cambodia or Jordan). The dark grey cells represent regimes we call *partial democracies:* systems in which the chief executive is chosen through competitive elections and political competition is not effectively repressed, but either elections are not fully free and fair, or political participation is not fully open and well institutionalized (e.g., Albania or Venezuela).[10]

Among partial democracies, we further distinguish between those that exhibit factionalism (shown in darker grey), as coded on the PARCOMP variable, and those that do not. Polity describes factionalism as a pattern of sharply polarized and uncompromising competition between blocs pursuing parochial interests at the national level. This winner-take-all approach to politics is often accompanied by confrontational mass mobilization, as occurred in Venezuela in the early 2000s and Thailand prior to the 2006 military coup, and by the intimidation or manipulation of electoral competition.

[10]For countries in years that Polity designates regimes as "in transition" (−88), EXREC and PARCOMP are not defined. The few country-years in this category that appeared in our data set are treated as a separate regime category in our analysis, but they did not produce statistically significant results in any data run, so these coefficients are not displayed in our tables.

Performance

Table 1 presents the coefficients and odds ratios for the model when applied to all onsets of instability and all control cases, and then separately to those cases where political instability began with ethnic or revolutionary wars and their matched controls, and then to cases that began with adverse regime changes and their matched controls. The percentages of cases classified correctly are the results obtained when we choose a cut point to equalize as closely as possible the model's accuracy in identifying both instability onsets and cases that remained stable.

Although we randomly selected three different control sets for each analysis and obtained estimates from each resulting sample, the coefficients and percentage of cases correctly identified were virtually the same across the different control sets. Thus to simplify the presentation, only the results from the control set that had median accuracy for each type of instability are shown in Table 1.

For all kinds of political instability, the model correctly classified instability onsets in 81.7% of the cases in each of the three different control sets. For stable cases, the model's postdictive accuracy across the three control sets ranged from 81.6% to 82.4%. When we restricted the analysis to civil war onsets and their matched controls, the model correctly classified civil war onsets 80.0% to 81.5% of the time, while accuracy in identifying stable cases ranged from 79.5% to 81%. Looking only at adverse regime changes and their associated controls, adverse regime changes were identified with accuracy ranging from 87.7% to 89.2%, while stable states were identified with accuracy from 86.2% to 88.2%.[11]

As Table 1 shows, our different regime types were significantly related to the future onset of instability. The exception is full democracy, which was no more associated with higher instability than full autocracy. Partial autocracies, and partial democracies without factionalism, both had markedly higher relative odds of future instability than full democracies or full autocracies, yet there was a difference. Partial autocracies faced higher relative odds of both civil war and adverse regime change, while partial democracies without factionalism only had significantly higher relative odds of adverse regime change; they were not significantly more prone to civil war than full democracies or full autocracies.

The most striking result in the model, however, is the identification of partial democracies with factionalism as an exceptionally unstable type of regime. The relative odds of instability for such regimes were over 30 times greater than for full autocracies, other things being equal. This high level of relative risk was similar for the onset of civil wars, and even greater for adverse regime changes. Thus not all "anocracies" have similar properties; the relative risks of instability vary depending on specific combinations of regime characteristics.

In addition to regime type, several other variables emerged as significant risk factors. As shown in Table 1, the odds of future instability in countries at the 75th percentile in global infant mortality levels were nearly seven times higher than in countries at the 25th percentile. The impact of infant mortality remained significant, and at roughly the same level of relative odds, for onsets of both civil wars and adverse regime change.

Being situated in a "bad neighborhood" also was a major risk factor, with countries with four or more neighbors experiencing armed conflict being far more likely to have future onsets of instability. Although we examined other ways to measure the "bad neighborhood" factor—including both linear and quadratic measures based on the specific number of neighboring countries in conflict, and other thresholds—the simple two-category approach shown here provided the greatest accuracy in the overall model. While it may seem that setting the threshold at four or more neighboring countries having violent conflict would make for a very extreme and rare condition, that was not the case. Of the 160 countries with populations greater than 500,000 in 2003, nearly half (77) had four or more bordering countries, and 11 of our problem onsets (nearly one-tenth of all problem cases) occurred in countries with conflicts in four or more neighbors. So while this condition may seem extreme, it was well represented in our data.[12]

Lastly, countries with high levels of state-led discrimination against at least one minority group, according to the Minorities at Risk political or economic discrimination indicators, faced roughly triple the relative odds of future civil war onsets than those without such discrimination. But discrimination was not significantly associated with the onset of adverse regime changes.

The Role of Factionalism

One of our most striking results is the extraordinarily high relative risk of instability onsets in partial democracies with factionalism. Because this effect is so powerful, it deserves closer scrutiny. James Vreeland (2008) has recently argued that because Polity IV's identification of factionalism includes low-level violence (as during elections to intimidate voters) as a factor in its coding, it should not be used as an independent variable in any model to forecast or explain conflict. Given the manner in which factionalism is coded, he argues, its inclusion as an independent variable in models of civil conflict would introduce "violence" on both sides of the equation. It is important that we address his objection and the implications that flow from it.

To test Vreeland's contention, we reestimated our model with explicit measures of civil violence and civil protest (derived from several variables in the Banks Cross-Polity Survey) added to our four right-hand variables. If our measures of regime type were merely proxies for the presence of political conflict or violence, then including these explicit political unrest variables in the model should greatly diminish the estimated effects of regime type.

[11] Results for all three runs for each type of instability, the underlying data for replication, and the list of country-years correctly and incorrectly classified by the model are available at the PITF web site: http://globalpolicy.gmu.edu/pitf/. We do not analyze genocides/politicides separately in this article, as all but one of the genocides/politicides occurred as part of complex events that began with a civil war or adverse regime change. For an analysis of our genocide/politicide cases, see Harff (2003).

[12] Moreover, the "bad neighborhood" variable was not significantly related to the onset of adverse regime changes in the median accuracy run shown in Table 1; that is the one result that varied across the three control sets. In runs with the other two control sets, countries with four or more neighbors experiencing violent conflicts were also more likely to experience adverse regime changes, and this result was significant at the .05 level.

However, models with explicit indicators of political conflict or violence added showed no significant change in the odds ratios or significance levels for the regime-type variables, or in overall accuracy. The impact of our regime-type variables is thus not due to their acting as proxies for the presence of political unrest. Their effect is quite distinct from that of explicit political conflict variables and is fully retained even when such variables are added to the model. We thus are confident that it is not violence per se, but a certain kind of relationship among political elites—a polarized politics of exclusive identities or ideologies, in conjunction with partially democratic institutions—that our categories capture, and that most powerfully presages instability.[13]

Tests of the Model

We developed our model by comparing cases of instability onset to a matched sample of control cases, and by testing the ability of variables to distinguish, in binary fashion, between country-years in which instability onsets were imminent from those that would be followed by stability. If, however, the model is actually tapping political risk, then it should be able to do more. We thus subject it to a series of additional tests.

First, we examine how the observed incidence of political instability covaried with relative risk as estimated by the model. Did the country-years with the very highest relative risk scores, as projected by the model, show a proportionately higher incidence of instability onsets?

Second, we asked if the model captures dynamic changes in risk. How long after countries have moved to a higher risk level, according to the model, did they experience onsets of instability?

Finally, and most important, we wish to know how well the model performs outside of our constructed samples. Thus our third test is an out of sample test, using the model to analyze data from *all* countries and *all* years for an extended period.

To conduct these tests, it is necessary to rank various country-years according to their actual estimated risk of instability onset. This cannot be done directly with the coefficients in Table 1, which depend on the sample and are matched to suppress the effect of time and region. We thus used the same independent variables, but reestimated the model with regional dummies using unconditional logit.[14] We then adjusted the resulting coefficients to reflect the distribution of instability onsets and stable years across all country-years from 1955 to 2003, rather than the 3:1 ratio produced by the matched sampling (King and Zeng 2001a, 2001b). These adjusted coefficients were used with the independent variables of this model to rank the country-years in our problem and control sets according to estimated risks of instability onset.

Figure 2 addresses the first question by showing how the observed incidence of instability onsets varied with the model's estimates of country-year risks. This figure shows how similar our results were to the three different, randomly selected control sets. More importantly, it shows that the model powerfully discriminated among countries with different degrees of instability. Across all three control sets, for country-years in the lowest two deciles of our model's risk estimates we observed zero instability onsets two years later. For country-years with model risk estimates in the eighth decile, only 3.7% had observed instability onsets two years later. Country-years with model risk estimates in the fifth, six, and seventh deciles had similar observed incidence of instability onsets, ranging from 7% to 11%. Country-years in the fourth decile had a 26% incidence of instability onsets two years later, those in the third decile 44%, those in the second decile 61%, and fully 87% of those country-years whose model scores placed them in the highest decile were observed to have an instability onset two years hence.

Figure 3 addresses the second question by displaying survival curves for countries. The estimates draw upon the differences in the sample sets between cases and controls—now observed over many years rather than just at t and $t + 2$ where the latter year corresponds to the observed onset of instability—to estimate how soon a country would experience an instability onset after it first enters a high decile level of estimated risk according to the model. These estimates thus use much more data than was utilized in the samples used to estimate the model.

Again, the results are striking. Almost half of the countries that enter the highest decile of estimated risk are estimated to have an instability onset within five years; within 10 years nearly 70% exhibit political instability. These results vary by region: in Africa and East Asia, almost 70% of countries entering the highest risk decile were observed to have an instability onset within five

[13] Ulfelder and Lustik (2007), in an event-history analysis of transitions to and from democracy, also find a strong association between factionalism and instability in partially democratic regimes. For a more thorough examination of the factionalism coding in Polity and Vreeland's and other critiques, see Marshall and Cole (2008).

[14] As noted earlier, instability onsets are fairly evenly distributed across time; thus we found no significant time dummies for the unconditional logit model, not even for the most recent lower conflict period.

FIGURE 2 Observed Incidence of Instability Onsets in Year $t + 2$, by Decile of Estimated Risk (1 = Highest) for Each Control Set (N = 468 for Each Set)

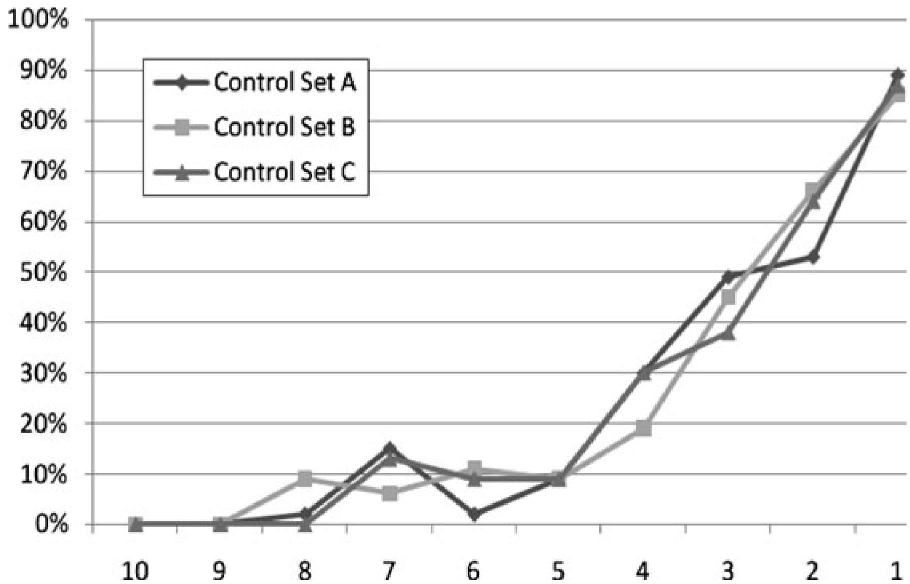
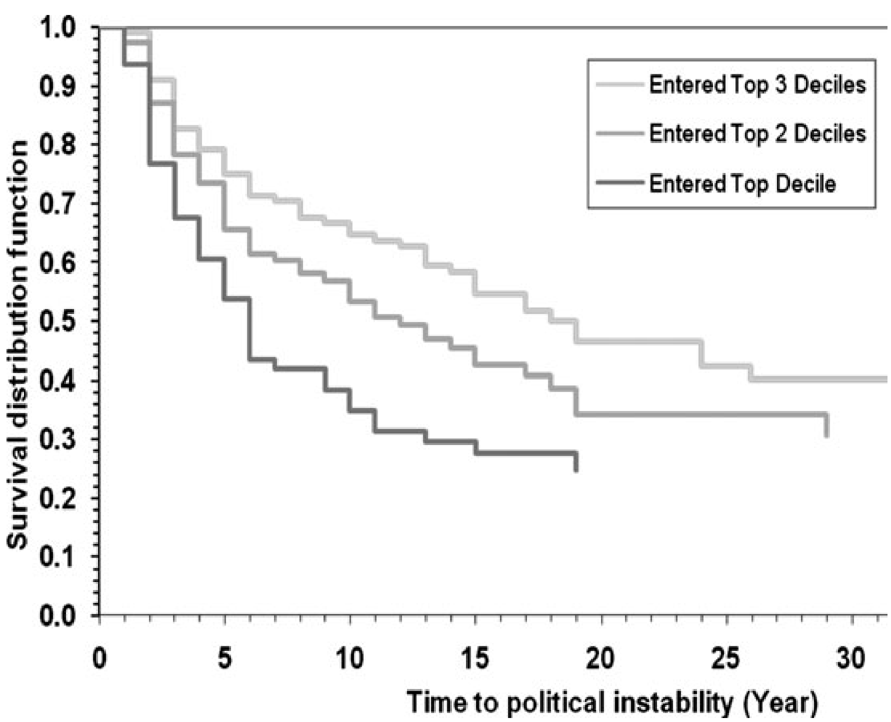


FIGURE 3 Time to Instability Onset for Top Risk Deciles



years. Everywhere, for countries entering any of the top three deciles, about one-quarter are estimated to have an instability onset in five years, but then the incidence of instability declines rapidly outside the highest risk decile. Thus the highest decile of risk estimated by the model is a powerful marker of impending instability.

Of greatest interest, however, is the ability of the model to predict out of sample, i.e., to predict onsets of political instability in sets of data other than those that were employed to create the model. To assess the capacity of the model to do so, we asked: If an analyst had had this model in hand in 1994, and then used the model to rank all countries in the world in each year by their estimated risk two years ahead, how well would she have been able to forecast instability events in the following decade, from 1995 to 2004?

To carry out this test, we first censored the data in our sample sets from 1995 to 2003, then estimated the model coefficients (using unconditional logistic regression and regional dummies) using the censored sample data, using only our sample data up through 1994. We then asked how the model would perform going forward. But we did not ask how the model would predict simply to the censored data—we asked how the model would perform when estimating instability risks for *all countries and all years from 1995 to 2004* (excepting countries less than two years old or with ongoing instability episodes). That is, we tested the model's forecasts for 1,246 country-years from 1995 to 2004, while only 200 country-years from this period had been used in estimating the model in Table 1.

Using the coefficients derived from the data up through 1994, we ranked *all countries in the world* (with population over 500,000) for their risk of instability onset in 1995, based on their 1993 values of the independent variables (as always, excepting states less than two years old or with ongoing conflicts). We then noted which countries had in fact experienced instability onsets in 1995, and their decile in that world ranking for 1993.

We then allowed our putative analyst to make use of the data for 1995 to reestimate the model coefficients, and use those coefficients to rank *all countries* (with the usual exceptions) according to their estimated risk of instability onset in 1996, based on 1994 data on the independent variables. We then observed which countries were actually observed to have instability onsets in that year and their decile in the forecast rankings. Continuing in this fashion, we allowed our analyst to keep estimating risks for *all countries in the world* (except those with ongoing conflicts) in each year from 1995 to 2004, estimating model coefficients using only data from prior to the year being forecast, and forecasting for each year from their values on the independent variables two years before.

Applying this procedure produced 1,246 country-year observations from these 10 years of data, compared to the 200 country-year observations from these years (20 onsets and 180 controls from 1995 to 2003) that had been used in our original sample data set for model estimation. These data also included four new problem cases—three from 2004 and one from 2000—that we identified in updating our instability data set in 2006, and which were *not* in the 1955–2003 data we used to identify the model.[15]

If our sampling procedure had produced a biased sample, then the coefficients estimated from that sample would not likely prove accurate in identifying instability onsets among *all* the country-years from 1995 to 2004, four-fifths of which were not included in the data in our original estimation samples.

Table 2 identifies all of the onsets of instability that occurred worldwide from 1995 to 2004 and reports the estimated risk by decile and quintile for the associated country-years, based on our global forecasting model. We found these results highly encouraging. Over the 10-year period covered in this exercise, 21 previously stable countries suffered onsets of instability. Of those 21 onsets, 18 (86%) occurred in countries that the model placed in the top quintile of estimated risk two years before onset. Of the three onsets not ranked in the top quintile, one occurred in a country in the second quintile, and two in the third quintile; none of the instability onsets occurred in countries ranked in the lowest two quintiles. The model also placed three of the four instability onsets that had not been in our original data for estimating the model in the highest quintile of risk. Moreover, fully 14, or two-thirds, of the instability onsets occurred in countries placed in the highest decile. Stable cases were identified with similar accuracy, with 81% of the country-years that did not have an instability onset appropriately ranked in the second through fifth quintiles.

In sum, when tested against an out-of-sample data set including all countries and all years for a 10-year period, the model performed just as well as when applied to our matched samples of cases and controls.

Comparisons

Prediction is not the same as explanation or hypothesis testing. Nonetheless, our confidence in an explanation is clearly influenced by the degree to which future events conform to the expectations that it engenders. We therefore found it unsettling that so many of the variables often linked to political instability failed to provide grounds for discriminating between political systems that would or would not suffer future onsets of political instability.

In seeking variables that might represent significant risk factors of political instability, we began with a wide

[15]These were Guinea in 2000 (revolutionary war), and Iran (adverse regime change), Thailand (ethnic war), and Yemen (revolutionary war) in 2004.

TABLE 2 Out-of-Sample Prediction Exercise for Observed Onsets of Instability, 1995–2004

A. Countries That Had Instability Onsets, 1995–2004. Quintile/decile in model score rankings based on 2-yr. prior data

Year	Top Decile	Second Decile	Second Quintile	Third Quintile
1995	Armenia, Comoros	Belarus		
1996	Albania, Niger, Zambia		Nepal	
1997	Cambodia, Congo-Brazz.			
1998	Guinea-Bissau, Lesotho			Serbia/Montenegro
1999	Ethiopia, Haiti			
2000		Solomon Ils., Guinea*		
2002	Cote d'Ivoire			
2003	Central African Republic			
2004	Iran*	Yemen*		Thailand*

B. Tabulation of All Country-years, 1995–2004. Model estimates based on censored data, using only sample data from prior to year of forecast (countries w/population over 500,000, no ongoing conflict, at least two years old)

	Countries with Instability in $t + 2$	Countries Remaining Stable
Predicted for Instability (Top Quintile)	18	233
Predicted for Stability (Not Top Quintile)	3	992
N = 1,246 Percent Classed Correctly	85.7%	81.0%

Number of instability onsets, 1995–2004: 21. Number of instability onsets in top quintile of model scores: 18 (86%).
*Cases added to the problem set in 2005 update.

survey of the existing literature on conflict. Modernization theorists had stressed the impact of rising levels of literacy, urbanization, income, and media exposure (Deutsch 1961; Huntington 1968). Sociologists and some economists focused on the role of inequality (Acemoglu, Johnson, and Robinson 2006; Alesina and Perotti 1996; Muller and Seligson 1988), while political economists focused on the impact of macroeconomic factors—rampant inflation, recession, or public debt (Bates 2008)—or on demography (Cincotta, Engelman, and Anastasion 2003). Some scholars (Collier and Hoeffler 2002, 2004; Sambanis 2001) emphasized the role of ethnic diversity. Others (Dunning 2005; Ross 2004) pointed to oil exports or other forms of resource dependency. None of these variables was significant when added to our model.

While we find that nations with governments that discriminate against minorities at risk are themselves at higher risk of political instability, no measure of ethnic composition significantly entered the forecasting model. We examined a wide variety of social, economic, and political variables that had been suggested in various theories of political conflict (we offer a partial list in Appendix B). More often than not, they failed to enhance the quality of our forecasts. Models that drew variables from the most recent work on civil war onsets (Collier and Hoeffler 2004; Fearon and Laitin 2003; Hegre et al. 2001)—such as population size, income level, and anocracy—produced statistically significant coefficients, but were substantially less accurate in identifying countries that would experience instability. Moreover, these variables became insignificant when added to our model.

To lend greater specificity to this discussion, we compare our model to that of Fearon and Laitin (2003). To support this comparison, we employ the same data to test both models (Hegre and Sambanis 2006), using all the cases and controls in our data set for which we could obtain data on the key Fearon/Laitin variables. Using those variables to discriminate between countries that will or will not experience an onset of political instability two years from the year of observation generates the data presented in Table 3.

Since the Fearon/Laitin model was developed to identify the onsets of civil wars, rather than all kinds of instability events, we enter the tests for civil war onsets and adverse regime changes in separate equations.[16] Given the object of their research, civil wars, we had expected their model might do best at identifying civil war onsets. But both their model and our own predicted the onset of composite events and adverse regime changes with a higher degree of accuracy than they predicted the onset of civil wars. More relevantly, however, note that most of the variables in the Fearon/Laitin model turn out to be

[16]We again performed each test with three different control sets, but only one set of results is shown here, as the results were essentially the same on all three sets.

TABLE 3 Fearon and Laitin Model Comparisons

	All Problems				Civil War Onsets				Adverse Regime Change Onsets			
	Fearon/Laitin Model		Global Forecasting Model (with the Same Data Set)		Fearon/Laitin Model		Global Forecasting Model (with the Same Data Set)		Fearon/Laitin Model		Global Forecasting Model (with the Same Data Set)	
Model Variables	**Coefficient**	**Odds Ratio**	**Coefficient**	**Odds Ratio**	**Coefficient**	**Odds Ratio**	**Coefficient**	**Odds Ratio**	**Coefficient**	**Odds Ratio**	**Coefficient**	**Odds Ratio**
Per capita income, logged†	−1.20***	7.15	.	.	−1.03***	5.85	.	.	−2.17***	32.16	.	.
Population, logged†	0.302**	1.79	.	.	0.384*	2.20	.	.	0.116	1.21	.	.
% Mountainous, logged†	−0.020	1.05	.	.	0.101	1.31	.	.	−0.285	2.17	.	.
Noncontiguous state (yes vs. no)	−0.370	0.69	.	.	−0.516	0.60	.	.	−0.388	0.68	.	.
Oil export revenues from fossil fuels of total export (>=1/3 vs. <1/3)	0.052	1.05	.	.	0.330	1.39	.	.	−1.44	0.24	.	.
Instability dummy (three-step change in Polity IV scale, yes vs. no)	0.415	1.51	.	.	0.022	1.02	.	.	0.708	2.03	.	.
Ethnic fractionalization†	0.376	1.22	.	.	0.259	1.15	.	.	0.339	1.21	.	.
Religious fractionalization†	0.156	1.07	.	.	0.248	1.10	.	.	0.0729	1.04	.	.
Anocracy dummy (−5< = POLX< = 5 vs. POLX<−5)	1.67***	5.32	.	.	0.782	2.19	.	.	3.91***	49.7	.	.
Democracy dummy (POLX>5 vs. POLX<−5)	1.61***	4.99	.	.	1.18*	3.25	.	.	3.09***	21.9	.	.

continued

TABLE 3 Continued

	All Problems				Civil War Onsets				Adverse Regime Change Onsets			
	Fearon/Laitin Model		Global Forecasting Model (with the Same Data Set)		Fearon/Laitin Model		Global Forecasting Model (with the Same Data Set)		Fearon/Laitin Model		Global Forecasting Model (with the Same Data Set)	
Model Variables	**Coefficient**	**Odds Ratio**	**Coefficient**	**Odds Ratio**	**Coefficient**	**Odds Ratio**	**Coefficient**	**Odds Ratio**	**Coefficient**	**Odds Ratio**	**Coefficient**	**Odds Ratio**
Regime type (full autocracy as reference)												
Partial autocracy	.	.	2.03***	7.58	.	.	1.46*	4.32	.	.	2.93***	18.7
Partial democracy with factionalism	.	.	3.62***	37.5	.	.	2.84***	17.1	.	.	4.89***	32.5
Partial democracy without factionalism	.	.	2.09***	8.10	.	.	2.04**	7.71	.	.	2.56**	12.9
Full democracy	.	.	1.16	3.20	.	.	0.637	1.89	.	.	1.40	4.04
Infant mortality†	.	.	1.68***	7.14	.	.	1.64***	6.98	.	.	1.50*	5.15
Bordering states with major civil or ethnic conflict (4+ vs. 0–3)	.	.	2.95***	19.1	.	.	3.56**	35.2	.	.	1.24	1.09
State-led discrimination (yes vs. no)	.	.	0.634*	1.88	.	.	1.03**	2.80	.	.	−0.118	0.68
N = total (problems/controls)	450 (115, 335)		450 (115, 335)		248 (64, 184)		248 (64, 184)		190 (48, 142)		190 (48, 142)	
% Problems predicted correctly	71.3		81.7		68.8		81.3		79.2		87.5	
% Nonproblems predicted correctly	71.0		81.8		69.0		79.3		79.6		88.0	

*** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$. †Odds ratios for continuous variables compare cases at the 75th and 25th percentiles.

statistically insignificant when used to identify the onset of conflict with a two-year lead time, with only regime type (anocracy, democracy), income per capita, and population retaining significance.[17]

Our model substantially outperforms the variables in the Fearon and Laitin model. While the difference in percentage accuracy may seem modest, the difference in cases accurately identified is not small. In forecasting adverse regime changes, across the three different case-control data sets the Fearon/Laitin model missed on average 10 problem cases and 31 control cases, while our model missed on average only six problems and 18 controls. In forecasting civil wars, the Fearon/Laitin model missed on average 19 problem cases and 56 control cases, while our model missed on average 12 problems and 37 controls. For all instability events, the Fearon/Laitin model misclassified on average 32 problem cases and 95 controls, while our model on average misclassified 21 problem cases and 60 controls. In sum, our model performed about a third better on forecasting civil wars and for the combined problem set, and missed 40% fewer cases among the adverse regime changes.

In closing this discussion, we would like to make a few further points. We originally expected that no simple model could capture the processes associated with varied kinds of instability onsets. Rather, we expected that we would need different models for different kinds of political instability. Moreover, given the large number of variables that had previously appeared as significant in the literature, we assumed that useful models would have to be complex, incorporating not only many variables, but also their rates of change and various interactions between them. To our surprise, these expectations proved wrong. Despite testing many independent variables in many combinations and specifications, we have not found greater predictive power than in the parsimonious model shown in Table 1.

In search of greater complexity, we tried using neural network analysis, in which we trained numerical function algorithms over multiple iterations to find the best functional form for predicting instability onsets. In contrast to results reported by Beck, King, and Zeng (2000) on international war onsets, the neural networks failed to yield substantially better predictions of instability onsets than did simpler regression models. The use of continuous variables in lieu of our binary and categorical variables also failed to improve accuracy.[18]

Moreover, the model is robust across different regions. As a further check, we ran the model on a data set using problems and random controls drawn *only* from sub-Saharan Africa, to see how well it identified at-risk states drawn entirely from within an unusually high-risk region. The model's accuracy was even higher (averaging 85%) in distinguishing instability onsets and stable cases in this regional test, and its outperformance of the variables in the Fearon/Laitin model is slightly greater.[19]

Finally, we wished to know how much our results were dependent on the specific two-year lag we used between observations of the variables and event onset. We thus reran the entire analysis observing the independent variables in the year prior to onset, and four years prior to onset. The model was slightly more accurate using prior year data, and only slightly less accurate (75%) using four-year advance data. Coefficients for the independent variables hardly changed. The accuracy of the model was thus fairly stable with respect to time lags.

Conclusion

The onset of political instability is rare but important and our goal has been to develop a model capable of forecasting its occurrence accurately and with a two-year lead time. The results we report suggest that we have substantially achieved that objective.

Most of the variables and specifications suggested by previous resource-based models of political instability offer substantially less predictive power in regard to instability onsets than the regime type variables developed in this research. While infant mortality, discrimination, and bad neighborhood effects are significant, our categorical measure of political institutions was by far the most powerful factor for distinguishing stable country-years from those that soon experienced instability onsets. Indeed, once regime characteristics are taken into account, most other economic, political, social, or cultural features of the countries in our sample had no significant impact

[17]Note that conditions such as mountainous terrain and large population do not change over time, and thus may be of little use in forecasting near-term instability. States with mountainous terrain may experience higher levels of instability for many reasons—mountains may give rebels places to hide, or it may be that states with many mountains are more likely to be divided into distinct regions with disparate interests, or it may be that mountain dwellers are more likely to be nomadic warriors and capable of rebellion. However, these conditions are not significant in forecasting coming instability in currently stable states.

[18]Ward and Bakke (2005, 16) also found that replacing binary and categorical measures with their continuous counterparts did not improve model accuracy. Reflecting on this, they speculate that the categorical measures may be working like the kinds of step functions neural network analysis is often used to derive from large data sets.

[19]The full analysis and comparisons for sub-Saharan Africa are also posted on the PITF web site.

on the relative incidence of near-term instability. In our view, this finding should encourage scholars in this field to redirect their attention from the economic to the institutional foundations of political instability (Snyder and Mahoney 1999).

From a policy perspective, we find this result hopeful. Many of the factors that others have found to be important prior correlates of civil war onset—income per capita, mountainous terrain, population size, age structure, and resource endowments—are beyond the reach of short-term policies to change. The most influential factor in our model, however—the institutional character of the national political regime—may sometimes be more amenable to policy reforms.

At the same time, the model also warns us that the process of institutional reform can be and often is destabilizing. Previous research has shown that transitions to democracy frequently involve movement through intermediate regimes (Epstein et al. 2006). We have shown that a specific kind of intermediate regime—partial democracies with factionalism—has exceptionally high risks of instability. This more complex view of the characteristics of intermediate regimes suggests that it is essential to seek policies and institutions that blunt or discourage factionalism when opening up political participation and competition, if reforms are to produce stable regimes rather than greatly increase the risks of instability.

We also wish to underline the model's high accuracy in forecasting both violent and relatively nonviolent forms of instability. Ironically, this suggests that we view the model not as one of instability but rather as one of resilience (cf. Goldstone 2001). If the factors that appear associated with stability in the model are in place—high income, low discrimination, few conflicts in the neighborhood, and most important, a noncontested or unified political regime—the model suggests that the polity will remain stable. This result suggests that we may need to think more about the factors that underlie regime survival (Gates et al. 2006) and provide resilience in a troubled world, rather than about the diverse and often idiosyncratic causes of varied types of conflicts.

Appendix A

TABLE A1 Episodes of Political Instability, 1955–2003

Country	Type of Conflict	Began	Ended	Country	Type of Conflict	Began	Ended
Afghanistan	Complex	4/78	—	Cen. Afr. Rep.	REG CHG	3/03	3/03
Albania	Complex	5/96	5/97	Chad	Complex	10/65	10/94
Algeria[2]	Complex	7/62	12/62	Chile	Complex	9/73	12/76
Algeria	Complex	5/91	—	China	Complex	2/56	12/59
Angola[2]	Complex	1/75	3/02	China	Complex	5/66	3/75
Argentina	REG CHG	6/66	6/66	China	Complex	7/88	12/98
Argentina	Complex	3/76	12/80	Colombia[1]	REV WAR	4/48	12/60
Armenia	REG CHG	7/95	9/96	Colombia	REV WAR	5/84	—
Azerbaijan[2]	Complex	8/91	6/97	Comoros[2]	REG CHG	1/76	1/76
Bangladesh	Complex	12/74	6/91	Comoros	Complex	9/95	4/99
Belarus	REG CHG	4/95	11/96	Congo-Brazz.	REG CHG	12/63	12/63
Benin	REG CHG	10/63	12/65	Congo-Brazz.	Complex	6/97	12/99
Benin	REG CHG	10/72	10/72	Congo-Kinsh.[2]	Complex	6/60	11/65
Bos. & Herz.[2]	Complex	4/92	12/95	Congo-Kinsh.	Complex	3/77	12/79
Brazil	REG CHG	9/61	10/65	Congo-Kinsh.	Complex	3/92	—
Burkina Faso	REG CHG	11/80	11/80	Croatia[2]	ETH WAR	6/91	12/95
Burma	Complex	8/61	—	Cuba[1]	Complex	3/52	12/61
Burundi[2]	Complex	6/63	12/73	Cyprus	Complex	12/63	4/68
Burundi	Complex	8/88	—	Cyprus	Complex	7/74	8/74
Cambodia	Complex	3/70	5/91	Czechoslovakia	REG CHG	8/68	7/69
Cambodia	REG CHG	7/97	7/97	Dom. Republic	Complex	1/64	7/66

continued

TABLE A1 Continued

Country	Type of Conflict	Began	Ended	Country	Type of Conflict	Began	Ended
Ecuador	REG CHG	6/70	2/72	Niger	REG CHG	1/96	1/96
Egypt	REV WAR	2/92	3/99	Nigeria	Complex	12/64	1/70
El Salvador	Complex	2/77	1/92	Nigeria	Complex	12/80	4/85
Eq. Guinea[2]	Complex	2/69	8/79	Oman	REV WAR	6/70	3/76
Ethiopia	Complex	7/61	5/93	Pakistan	REG CHG	10/58	10/58
Ethiopia	ETH WAR	2/99	6/00	Pakistan	Complex	3/71	12/71
Fiji	REG CHG	12/87	12/87	Pakistan[2]	Complex	2/73	7/77
France	REG CHG	6/58	6/58	Pakistan	Complex	8/83	10/99
The Gambia	REG CHG	7/94	7/94	Panama	REG CHG	10/68	10/68
Georgia[2]	Complex	6/91	12/93	Pap. N. Guinea	ETH WAR	5/89	5/97
Ghana	REG CHG	1/72	1/72	Peru	REG CHG	10/68	10/68
Ghana	REG CHG	12/81	12/81	Peru	Complex	3/82	4/97
Greece	REG CHG	4/67	4/67	Philippines	Complex	11/69	—
Guatemala	Complex	7/66	12/96	Romania	REV WAR	12/89	12/89
Guinea-Bissau	Complex	6/98	9/03	Russia	ETH WAR	8/94	—
Guyana	REG CHG	4/78	10/80	Rwanda[2]	Complex	11/63	11/66
Haiti	REG CHG	9/91	9/91	Rwanda	Complex	10/90	7/01
Haiti	REG CHG	1/99	11/00	Senegal	REG CHG	12/62	3/63
Hungary	Complex	10/56	5/57	Senegal	ETH WAR	9/92	12/99
India	ETH WAR	5/56	10/58	Sierra Leone	Complex	3/67	4/71
India	ETH WAR	5/67	12/71	Sierra Leone	Complex	3/91	3/02
India	Complex	4/83	—	Singapore[2]	REG CHG	9/63	8/65
Indonesia[1]	Complex	12/49	—	Solomon Isles	REG CHG	6/00	—
Iran[1]	REG CHG	8/53	3/55	Somalia	REG CHG	10/69	10/69
Iran	Complex	10/77	12/92	Somalia	Complex	5/88	—
Iraq	Complex	3/59	3/75	South Africa	Complex	8/84	6/96
Iraq	Complex	9/80	—	Sri Lanka	Complex	7/83	2/02
Israel	ETH WAR	12/87	—	Sudan[2]	Complex	10/56	3/72
Ivory Coast	Complex	9/02	—	Sudan	Complex	7/83	—
Jordan	REG CHG	3/57	3/57	Swaziland	REG CHG	4/73	4/73
Jordan	REV WAR	2/70	7/71	Syria[2]	REG CHG	9/61	2/66
Kenya[2]	Complex	1/64	10/69	Syria	Genocide/politicide	4/81	2/82
Kenya	ETH WAR	10/91	9/93	Tajikistan[2]	REV WAR	4/92	12/98
Korea, S.	REG CHG	5/61	5/61	Thailand	Complex	11/65	12/83
Korea, S.	REG CHG	10/72	10/72	Turkey	REG CHG	4/71	4/71
Laos	Complex	1/60	6/79	Turkey	Complex	9/80	2/00
Lebanon	REV WAR	5/58	7/58	Uganda	Complex	4/66	—
Lebanon	Complex	4/75	7/91	UK	ETH WAR	1/71	10/82
Lesotho	REG CHG	1/70	1/70	Uruguay	REG CHG	11/71	2/73
Lesotho	Complex	5/98	1/99	USSR	REG CHG	8/91	12/91
Liberia	Complex	11/85	8/03	Vietnam, S.	Complex	1/58	4/75
Malaysia	REG CHG	5/69	5/69	Yemen, N.	REV WAR	9/62	1/70
Mali	ETH WAR	6/90	1/95	Yemen, S.	REV WAR	1/86	2/86
Moldova[2]	ETH WAR	3/92	12/92	Yemen	REV WAR	4/94	7/94
Morocco	REG CHG	6/65	6/65	Yugoslavia	Complex	6/91	1/92
Morocco	ETH WAR	10/75	11/89	Yugoslavia	Complex	2/98	6/99
Mozambique[2]	REV WAR	7/76	10/92	Zambia[2]	Complex	7/64	12/72
Nepal	REG CHG	12/60	12/60	Zambia	REG CHG	11/96	11/96
Nepal	Complex	2/96	—	Zimbabwe	Complex	12/72	12/87
Nicaragua	Complex	9/78	3/88				

[1]Dropped because event started before 1955.
[2]Dropped because event began before or within two years of existence as a separate or independent country.

Appendix B

Table B1 A selected list of variables tested that were NOT statistically significant when added to the Global Forecasting Model

Social Variables
Ethno-linguistic Fractionalization
Ratio of largest to 2nd largest ethnic groups
Total Population (relative to world mean, logged)
Percent of Population in Urban Areas
Growth rate of Urban Population
Number of Refugees
Life Expectancy at Birth
Female Life Expectancy
Total Fertility Rate
Youth Bulge (Age 15–24/Age >15)
Youth Bulge (Age 15–24/Age >25)
% postsecondary school completed by pop age >15
% secondary school completed by pop age >15
% of population w/access to safe drinking water
Population Density
Economic Variables
Real GDP/capita (normalized, logged) PPP adjusted
GDP Growth (annual rate and five-year prior moving average)
GDP/capita Growth Rate previous year
Fuel Exports as % of merchandise exports
Crude Petroleum Exports as % of all commodities
Ores and Metal Exports as % of merchandise exports
Diversity of Trading Partners (Herfindahl)
Trade Openness (Imports+Exports)/GDP
Employment in Agriculture as % of total labor force
Rate of Price Increase, prior five years (inflation)
GINI coefficient
Damage due to drought
Balance of Payments
Direct Foreign Investment per capita
Foreign Assistance per capita
Political Variables
Regime Duration
Leaders' Years in Office
Ethnic War in previous 15 years
Corruption (Political Risk Services index)
Adverse Regime Change in prior 10 years
Presidential System
% of population in the military
Mountains, % of total land area (logged)

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