3 *Coalition politics and economic development*

Empirics

The previous chapter provided a new theoretical framework to explain cross-national variation in national economic performance. Unlike previous explanations that emphasize the importance of getting policies "right," having leaders with strong "political will," or protecting leaders from special interests, my argument takes an almost opposite tack. Rather than powerful governments capable of imposing the right policies, what the developing world needs, I argue, are institutions that promote policy moderation by dispersing policymaking authority to multiple actors, each accountable to different constituencies. In this sense, what I am calling for is "more democracy," even "too much democracy" (if such a thing can be even said to exist). The more voices that have to be heard before policy can be made, the harder it is for governments to make unilateral and arbitrary policy shifts, which in turn bolsters investors' confidence about the content and stability of future policy. This counter-intuitive argument comes with one condition: it is important that the actors to whom policy is dispersed have an incentive to cooperate with each other when absolutely necessary (e.g., in response to crisis). Therefore, I argued above that coalition or minority parliamentary democracies should fare best, but that the oppositional framework of divided presidential systems might lead to "gridlock" rather than "moderation" and therefore to less desirable outcomes.

This chapter is the first of four empirical chapters in which I shall try to convince the reader of the empirical utility of this explanatory framework. Specifically, here I will analyze the main outcome-of-interest – national economic performance – by disaggregating the concept into its components: the volatility of growth and the long-term mean around which it fluctuates. I will use a variety of statistical techniques to analyze a data set of all countries for which data are available between 1960 and 2004. The results are extremely supportive of the principal arguments made in this book: states whose leaders are credibly

constrained from policy change experience higher and more stable economic growth over this time period.

The chapter is organized in five main sections. Given the dominant focus in the existing literature on average growth, the first section makes a case for focusing instead on growth-rate volatility as an important dependent variable in its own right, including for the simple reason that volatility itself is bad for long-term economic growth in the developing world. Then the bulk of the chapter is spent describing and explaining cross-national variation in growth-rate volatility. I then apply my argument to explaining average growth rates, both cross-nationally and within Africa. The final empirical section tackles the question of endogeneity, and utilizes matching techniques to bolster confidence in the core findings reported in this chapter. The conclusion considers the implications of my results for our understanding of economic development, and sets the stage for the second cross-national empirical chapter.

Why growth-rate volatility matters

> The economic history of the world is replete with recessions and depressions. From the bursting of the British South Sea Bubble and the French Mississippi Bubble in 1720. . .to the industrial depressions of the 1870s and the 1930s, to the Latin American middle income debt crisis, African low income debt crisis, ex-Communist output collapse, and East Asian financial crisis, crises have been a constant of market capitalism. Add to that the collapses that have accompanied non-economic shocks like wars, hurricanes, earthquakes, volcanoes, fires, pests, droughts, and floods, and it is a wonder that anyone in the world has economic security. (Easterly *et al.* 2001:1)

The history of economic development around the world is not a steady march towards ever increasing economic efficiency and production. The quote from Easterly *et al.* above makes that abundantly clear. Indeed, if it were, things undoubtedly would be better for the majority of the world's population. Instead, a more accurate characterization of long-term growth patterns cross-nationally presents them as volatile and unstable. For reasons as yet poorly understood, most countries do not maintain their economic growth rates over an extended period of time. Periods of steady, even outstanding, growth are followed by stagnation and disasters from one decade to the next. While most countries fortunately never experience crises on the scale experienced by

the East Asian states in 1997 or by Argentina in 2002, most countries go through recessions and slight crises at some points in their history. Growth-rate volatility, of which crises are the most dramatic instances, is under-studied in political science, but as the cases of Argentina and East Asia demonstrate, the human effects of extreme volatility make understanding the political determinants of growth-rate volatility a central question for comparative and international political economy.

The decision to focus on the volatility of growth rates rather than their mean does not alter the overarching question of what impact politics has on economic development. Traditional research strategies with their focus on long-term mean growth rates have yielded little partly because, as I argued in the introduction, averages of growth rates often mask as much as they reveal. World Bank economist Lant Pritchett underscores this point emphatically:

> [the] fixation on differences in long-run (even possibly steady-state) differences in growth in the theoretical and empirical research explaining growth has led to an underestimation of the importance of the *instability* and *volatility* in growth rates. . .Is explaining Brazil's "growth" explaining the 4.2 percent growth from 1965 to 1980 or explaining the stagnation from 1980 to 1992 (actually a fall of −0.2 percent) or explaining Brazil's average growth percent from 1960 – 1992 of 3.14. (Pritchett 2000:3)

It is a fair question; most countries oscillate between good and bad periods of growth, but some do so much more frequently and more intensely (i.e., have bigger swings). Understanding the determinants of such volatility is important in its own right, since growth-rate volatility extracts large human costs as Argentina's 2002 experience demonstrates, but increasing evidence suggests that the volatility of growth is intimately linked to its long-term average too. In other words, it is not as simple as suggesting that high volatility is simply the price of high rewards (i.e., high average growth), but rather, in the developing world, that high volatility drags countries into a low *and* unstable growth equilibrium from which it is difficult to escape.

This idea – that volatility is something to be avoided even if one is solely interested in maximizing long-term average growth – is central to my thinking about national economic performance, and shortly I will provide some evidence to buttress this view. But before that it is worth considering the alternative view that volatility, while painful in the short term, might be beneficial in the long term. Growth-rate

volatility could enhance future growth prospects in two distinct ways: (1) "positive" volatility could encourage technological progress while (2) "negative" volatility "cleanses" the economy of inefficient firms through a process of "creative destruction."[1]

Traditionally the business-cycle literature maintained that aggregate demand shocks had no permanent effects on technology or growth and that monetary shocks had no long-term impact on technological progress. By the late 1980s, however, endogenous growth theorists had begun to embed temporary shocks into the economy's long-run growth trajectory (King *et al.* 1988; Stadler 1990). In particular, temporary increases in the quantity of money in the economy were found to stimulate higher levels of economic activity in the short run, which in turn generated an otherwise unlikely increase in technological growth, either because firms experienced more rapid "learning-by-doing" or because they increased their research and development to maximize returns from increased profit opportunities. And, importantly, these effects prove not transitory, but stick around with real income ending up at "'a permanently higher level" (Aghion and Howitt 1998:236).

A second tradition in business-cycle theory is based in the Schumpeterian "creative destruction" view that recessions (negative growth shocks) serve a cleansing or purging function for the economy, and reduce inefficiencies in firm organization and resource allocation (e.g., labor input utilization). Consider four separate mechanisms through which negative volatility might aid future productivity: (1) the "cleaning-up" effect, (2) the "opportunity cost" effect, (3) the "disciplinary" effect, and (4) the "externality" effect.[2]

First, the "clean-up" (also known as the "lame duck") effect states that recessions serve the purpose of eliminating inefficient and less productive firms, thereby increasing average productivity. The extent to which this effect might hold is limited by the severity of the recession since recessions also lower the rate of entry of new firms that push out the older inefficient firms. Second, the "opportunity cost" (or the "cross-temporal substitution" argument) explanation suggests

[1] The discussion below borrows heavily from Chapter 8, section 8.3 of Aghion and Howitt (1998).

[2] Interested readers can find a more detailed and technical exposition in (Aghion and Howitt 1998: 240–2). See also the Clarendon Lectures in Economics by Aghion and Banerjee (2005).

that firms might be more inclined to invest in reorganization or retraining programs when business is slow since the opportunity costs of doing so then are lower than when business is booming. This argument is most relevant for investments that are profitable in the long-term such as training, machine replacement, and labor reorganization since such expenditures are likely to be counter-cyclical.[3] Third, recessions might aid growth by imposing a "disciplinary" effect, whereby firms that do not make the necessary commitment to improving productive efficiency through investments in reorganization are more likely to face bankruptcy. The fourth and final argument builds on the logic of the previous three. The "externality effect" argument speaks to the selection of which firms are more likely to be phased out of business during recession. It posits that if the difference in performance between good and bad firms (or labor) widens more than proportionally with the degree of adversity, then good firms are more likely to survive than "bad" firms, and this "Darwinian" selection process benefits the economy as a whole.

Economic theory thus suggests that growth-rate volatility should have positive effects for average levels of productivity and therefore for long-run economic growth because (1) temporary increases in money supply can generate rapid technological growth and (2) even recessionary periods can improve average productivity by weeding out inefficient actors in the market. More volatile states should enjoy more of both mechanisms and therefore have higher long-term growth.

The accumulated evidence, however, is much less sanguine, especially in the developing world, where volatility is more extreme. Indeed, after summarizing the reasons that volatility might be good for the economy, Aghion and Howitt offer this caveat: "these [positive] effects should not be overemphasized. . .[rather] *the idea that excessive macroeconomic volatility is an obstacle to growth appears to be largely supported by recent empirical evidence*" (1998: 243, emphasis in original). This evidence is mounting rapidly, staring with an influential piece by Ramey and Ramey in the *American Economic Review* in

[3] While the "opportunity cost" argument suggests that recessions might generate investment in physical infrastructure and reorganization activities, the same does not necessarily hold for investment in technological R&D since those expenditures are more likely to be procyclical because of cash constraints (Stiglitz 1993).

1995, which used a panel of ninety-two developing states to show that "countries with higher volatility have lower mean growth, even after controlling for other country-specific growth correlates" (Ramey and Ramey 1995: 1139). Dennis Quinn and John Woolley replicate this finding in their 2001 paper which used data from 109 countries from 1974 to 1989 (Acemoglu *et al.* 2003b; Imbs 2007; Mobarak 2005).

What explains this conflict between theory and evidence? We do not know for sure yet, but the most promising explanation suggests that macroeconomic instability in the developing world discourages investment. High growth-rate volatility creates greater uncertainty that future growth will be stable, thereby making investment riskier (Aizenman and Marion 1993; Lucas and Prescott 1971; Ramey and Ramey 1991). Agenor and Montiel write that "'uncertainty has been an important factor in the macroeconomics of development, in many instances triggering currency substitution, capital flight, exchange-rate crises, and the collapse of private investment" (1999: 37). This negative effect on investment is particularly pronounced if irreversibilities in investment exist since investors who cannot withdraw their investment without losing significant sunk costs, such as the costs of physical plant construction, tend to be more risk-averse. Aghion and Howitt (1998) summarize this argument well: "'Large swings in output increase uncertainty, which may be a major factor in delaying or stopping investment and R&D." Similarly, Ramey and Ramey show "'if firms must commit to their technology in advance, then volatility can lead to lower mean output because firms find themselves producing at suboptimal levels *ex post*" (1991). As plausible as this sounds, one might wonder nevertheless, if higher volatility implies higher equilibrium average return, why might volatility not encourage investment, even if of a riskier sort. Possibly. To compensate for the higher risk, though, a project must have a higher expected (average) return to attract investment, which means that higher volatility probably deters investments in projects that might have occurred at lower levels of volatility, something developing countries cannot afford.

Figure 3.1 uses data from 1960 to 2004 to shed some light on the question of whether volatility is good or bad for economic growth. Since previous research has found that volatility appears to have different effects depending on the level of development in the economy, I separate the developed OECD states (those in Europe and North America) from the developing world (defined here simply as

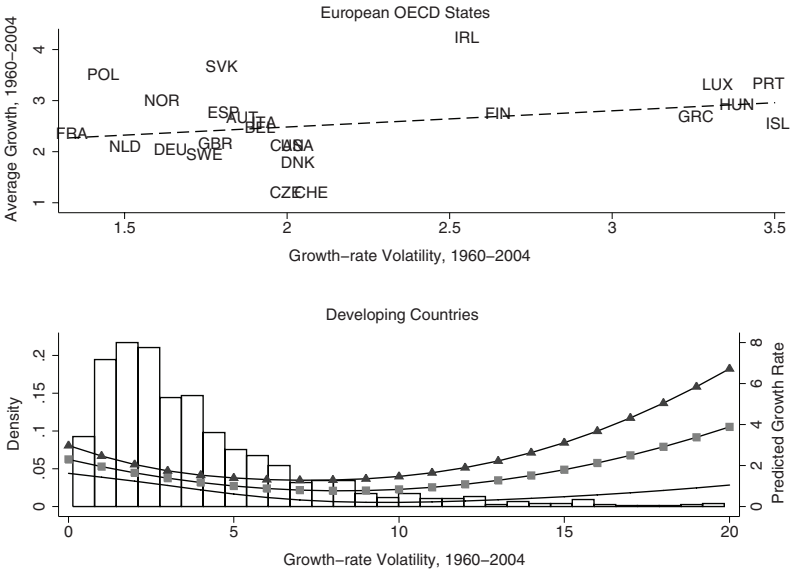

Figure 3.1 Volatility and growth (source: author's calculations from World Bank 2006).

non-members of the OECD group). Efficient high-growth economies might in fact benefit from Schumpeterian "creative destruction," while less efficient economies might be hurt if the uncertainty introduced by economic volatility deters business activity, thereby lowering future growth (Quinn and Woolley 2001). The top graph in Figure 3.1 is for the OECD states only, and indicates a positive relationship between volatility and average growth, which is in line with economic theory. But the relationship is the opposite in the developing world, where volatility hurts growth except at very high levels. The developing world graph plots the predicted growth rate for different levels of growth-rate volatility, along with a 95 percent confidence interval, *ceteris paribus*.[4] Nor is this negative relationship for over 90 percent of the developing world sample (note the distribution of growth-rate volatility in the

[4] There's no clear theoretical justification for the quadratic relationship between volatility and growth but the data demand one. A closer look at the data suggest that this is because there are many fuel and mineral exporting countries with higher levels of growth even though they are quite volatile too. It remains an interesting question for future research.

background of the lower panel of Figure 3.1) spurious; in fact, the plot reflects the predicted growth rate from a model of 123 developing countries that included controls for previous levels of economic growth, regime type, political instability, population size, level of economic development, trade openness, the share of agriculture in the economy, the share of government consumption in the economy, and levels of foreign direct investment (see Appendix B for full results). The effect of volatility on growth is also quite sizable: for the bulk of the distribution with growth-rate volatility under ten, a one-point increase in volatility leads to one-third of a percent lower growth!

A model of growth-rate volatility

The results above indicate the existence of a vicious trap where states are stuck with low-and-volatile economic performance, which should move identifying the political determinants of growth-rate volatility squarely and firmly onto the central policy agendas of developing countries. In this section, I develop an empirical model of growth-rate volatility designed to test the theoretical framework of national economic performance offered in the last chapter. Since this forms the main empirical assessment of my argument, I proceed carefully, and organize the section as follows. I will first discuss how I measure growth-rate volatility, and describe some basic empirical trends in the data so that it is clear to the reader just what we are seeking to understand. I then discuss three alternative political explanations for growth-rate volatility offered by scholars in recent years, and contrast my explanation with theirs, pointing out similarities and points of divergence. A discussion of the specific indicators used to assess my argument versus the existing explanations follows, as does a consideration of the economic control variables to be included in the model. After presenting the results and discussing a robustness check using an alternative dependent variable, I turn to the growth model.

The dependent variable: measuring and describing growth-rate volatility

Economic data on economic growth come primarily from the World Bank (World Bank 2006). Data were collected for all developing countries for the years 1960 to 2004, but missing data in the included

variables mean that the regression results reported below are based on fewer than that many cases at times (the fewest number of developing countries represented in any of the models reported below is 105).[5]

The dependent variable of interest in this section is *growth-rate volatility*, where the growth rate is measured by the annual percentage growth rate of real GDP per capita. Conceptually volatility contains two closely related but importantly distinct aspects: stability and predictability. Applied to economic growth, stability refers to a country's ability to return to its normal performance levels following a shock to the system. Predictability, on the other hand, has less of a long-run equilibrium feeling to it, but rather speaks to how well one can forecast future performance based on the past. Therefore, predictable states have future growth rates that are accurately foretold by their current growth rate and macroeconomic conditions.

The most commonly-used strategy for measuring growth-rate volatility is to calculate the standard deviation of the growth rate of a country for the period of interest.[6] Using the standard deviation as a measure of volatility captures the concept of stability perfectly. Here the unconditional mean growth rate serves as a measure of the long-run (or steady-state) equilibrium and deviations from this mean are treated as shocks to the system. The larger the deviation, the larger is the shock. Thus, if the growth rate for a particular state is stable, the standard deviation should be low, since the stable growth trajectory will be well-represented by the trend.

While the standard deviation accurately measures a country's long-term growth-rate stability, it ignores the dynamics of the growth trajectory that aggregate to form the long-term stability. The core concern is that the standard deviation would be unable to distinguish

[5] Summary statistics and a list of countries included in the analysis are reported in Appendix B. By comparison, the largest sample size used for any of the regressions reported by Quinn and Woolley in their 2001 *American Journal of Political Science* article is 105. Rodrik's sample, as reported in his 2000 *American Economic Review* article, covers ninety-six countries. Therefore, while I certainly wish I had more data for more countries, the results reported in this chapter are based on approximately the same number of cases as in previously published studies.

[6] Ramey and Ramey (1995); Wu and Rapallo (1997); Easterly and Kraay (2000); Easterly *et al.* (2001); Rodrik (2000); Quinn and Woolley (2001) all use this measure.

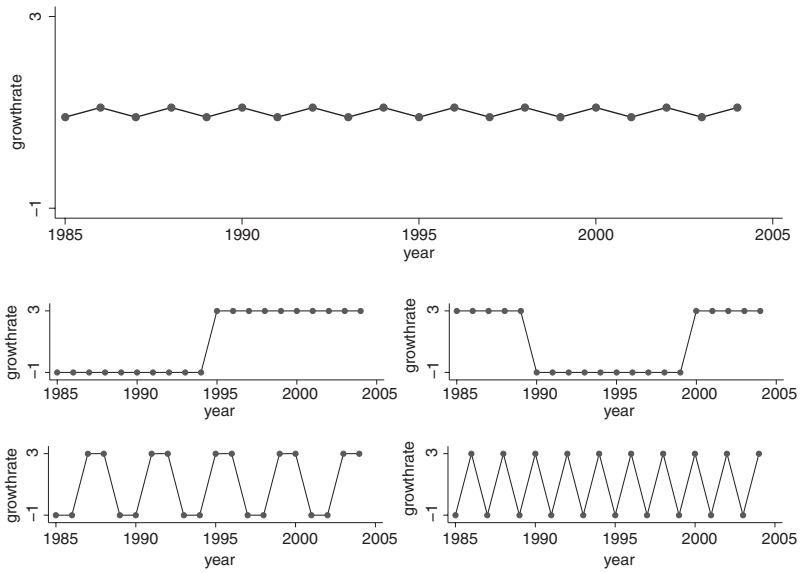

Figure 3.2 Measuring growth-rate volatility.

between two countries, one of which had volatile growth about a stable trend and another that had stable growth but around two very different trends (for instance, if there's a structural break in the time series). But distinguishing between such cases is very important if we are to understand the determinants of volatility. Therefore, I use an alternative to the standard deviation of growth rates: the standard deviation of the residuals from the autoregressive process for growth in GDP per capita, estimated for each country in the sample. The mechanics are simple (i indexes countries of which there are n in total; t indexes time which runs from year 1 to year T; and Trend is a year variable):

$$y_{i,t} = \beta_0 + \beta_1 y_{i,t-1} + \beta_2 \text{Trend}_t + \varepsilon_{i,t}$$
$$\varepsilon_{i,t} = \widehat{y_{i,t}} - y_{i,t}$$
$$\text{Volatility}_i = \sqrt{\frac{\sum_{t=1}^{T}(\varepsilon_{i,t} - \bar{\varepsilon}_i)^2}{n}} \tag{3.1}$$

Figure 3.2 illustrates the advantages of this alternative approach over the conventional technique quite vividly. In it, I plot the growth rates of five hypothetical countries. Just looking at the figures, it should be fairly

clear that the top graph is the most stable growth trajectory, and that the the bottom-right graph is the least stable or most volatile. The second row shows growth trajectories of countries that are fairly stable but around different means in different sub-periods of their history, while the bottom-left graph shows a slightly less volatile version of the bottom-right. The two ways of measuring volatility provide the same answer in the first most-stable case, but they diverge with the more interesting cases illustrated in the lower four graphs. There the standard deviation of growth is exactly the same in all four cases (2.05), which means that the conventional method for measuring volatility is unable to distinguish between these very different cases. The alternative method, fortunately, does distinguish between them, and in a way that accords with our intuition: the top-left graph is least volatile of the four (1.72) and the bottom-right graph is most volatile (2.43), with the top-right and bottom-left graphs coming in between (1.77 and 2.07 respectively).

Having decided on a measure of growth-rate volatility, the second decision to be made is the appropriate length of time over which to measure volatility. How long a period is long enough? There's no clear theoretically-prior answer to this question, and different choices come with different tradeoffs. On the one hand, one could calculate the volatility over the entire time period for which data are available, thereby producing one observation per country under study, but this reduces the number of cases available for analysis. An alternative would be to create shorter panels from the larger time series, collapsing five- or ten-year time periods at a time, a tactic common in studies of long-run correlates of economic growth (see Barro 1997). The problem here is that it is not any clearer that five or ten years is the right length of time.[7] Therefore, I err on the side of caution and estimate the regression models using three different dynamic specifications: a pure cross-section in which volatility is measured over the entire period available; ten-year panels; and five-year panels. To the extent that the results are robust across all three choices, we should have more confidence that they are not being driven by research design choices.

Figure 3.3 compares the growth-rate volatility of the OECD states to that of the developing world, and also over time. Regardless of the

[7] For instance, Rose and Spiegel (2009) justify using eleven years as the length of the panel in their study since they have fifty-five years of data and this allows them to create five panels of equal length.

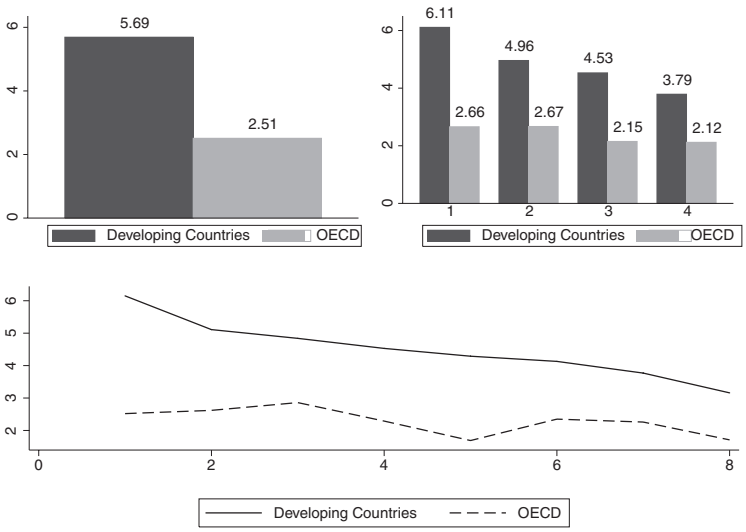

Figure 3.3 Volatility is higher in developing countries, but has declined over time (source: author's calculations from World Bank 2006).

length of time used to calculate volatility, the OECD states have much lower volatility than the developing world, less than half in fact (Prasad *et al.* 2006).[8] However, while the gap between the developed and developing world in terms of volatility persists, the good news is that volatility appears to be declining in recent years (note the downward trend in volatility in both the ten-year and five-year panels (top-right and bottom graphs respectively)).

Political determinants of growth-rate volatility

Credible constraints

What explains the variation in growth-rate volatility cross-nationally?[9] In the previous chapter, I developed a theoretical framework that emphasized the diffusion of policymaking authority to different actors,

[8] Between 1960 and 2004, OECD states had an average growth rate of 2.60 percent while the developing world averaged 1.62 percent. Given that we know from previous research as well as Table B.2 above that volatility hurts growth, it is quite plausible that much of the difference in growth between developing and OECD countries may be due to volatility.

[9] Appendix A of Klomp and de Haan (2009) provides an extremely useful summary of previous research on the determinants of economic volatility.

which checks arbitrary policy change, and therefore bolsters confidence of economic actors. Specifically, the discussion of the sources of government credibility suggests the following set of empirical indicators for different types of political institutions:

1. minority or coalition governments in parliamentary democracies
2. divided government in presidential democracies
3. independent judiciaries
4. federalism
5. central bank independence
6. binding legislatures in non-democracies.

I construct binary indicators for the existence of these institutions from various cross-national data sources. Data for the first four indicators come from the World Bank's Database of Political Institutions (Beck *et al.* 2001) and the Quality of Government Dataset (Teorell *et al.* 2009). Central bank independence data come from Cukierman *et al.* (1992) and Stürm and de Haan (2001a). Finally, Wright (2008) provides the data on the existence of binding legislatures in non-democracies. Details on the construction of each indicator can be found in Appendix B.

Because volatility is measured over time (either all years, ten-year or five-year panels), the independent variables are measured similarly. Given the binary nature of these indicators, taking their average over time yields a pleasing measure of the percentage of years during which the particular institution was present. That is, if a country never had minority or coalition government, then its average will be 0, whereas if these were present throughout the period, the average would be 1. If minority/coalition government was in effect for only half the period, the average will be 0.5, and so on. The expectation quite obviously is that the longer these credible constraints were in place for a given time period, the lower should be the growth-rate volatility in that time period. Table 3.1 provides preliminary evidence to support this claim. The pairwise correlations between the prevalence of the credible constraint indicators and the level of growth-rate volatility in a given period indicate that coalition/minority governments in parliamentary democracies are correlated with lower volatility (recall that the sample for that correlation is developing country parliamentary democracies only). Similarly, among presidential democracies in the

Table 3.1 ***Pairwise correlations between growth-rate volatility and credible constraint indicators***

	Length of panel		
	All years	**Ten years**	**Five years**
Minority and coalition governments	−0.31	**−0.21**	**−0.18**
Parliamentary democracies only			
Divided government	**−0.12**	**−0.13**	**−0.14**
Presidential democracies only			
Independent judiciaries	−0.19	−0.04	−0.07
Democracies only			
Federalism	−0.07	−0.03	0.03
Democracies only			
Central bank independence	−0.25	−0.09	−0.04
Democracies only			
Binding legislatures	**−0.29**	−0.13	−0.08
Non-democracies only			

Note: Cell-entries are pair-wise correlation coefficients. Correlations that are statistically significant at the 0.05 level of better are reported in **bold**.

developing world, greater frequency of divided government is also correlated with lower volatility, though this correlation is lower than in the parliamentary case. None of the other institutions has a robustly statistically-significant correlation with growth-rate volatility, which suggests that partisan checks exert greater influence than more formal institutional structures that might have less credibility in the eyes of investors.

Of course, countries do not necessarily have coalition or minority governments for the entire period (whether all years, ten years, or five years) that constitutes the unit of observation in Table 3.1. This raises the very real concern that the good years within a period might in fact be those in which the credible constraint institutions are absent! The correlations above suggest that this is not the case, but additional confirmation would be useful. Therefore, I utilize annual data to compare years in which, for instance, a minority or coalition government was in charge in a parliamentary democracy to those in which a unified government held power. Unified governments in this sample have a

growth-rate volatility score of 5.69; their coalition and minority government counterparts have a score of 3.76, almost two full points lower. This is very encouraging and suggests that the pairwise correlations discussed above are not spurious to the construction of the institutional indicators.

Do these initial results hold up to controlling for alternative political and economic explanations? The main political alternative is that democracy itself is what matters, rather than the particular configuration the government takes. In fact, arguments that focus on democracy have become increasingly common in the literature.

Democracy

Prior to 2000 there were no generalizable explanations of growth-rate volatility in the political economy literature. Since then, however, economists and political scientists have recognized the importance of growth-rate volatility and a few competing explanations have been tendered. Three in particular merit consideration, each of which argues democracies should have lower growth-rate volatility than non-democracies, albeit for different reasons. Harvard economist Dani Rodrik argues that democracies deal with social conflict through compromise rather than through volatility-inducing divisive policymaking. Dennis Quinn and John Woolley argue that democracies have lower growth-rate volatility because leaders are constrained by risk-averse citizens through elections in democracies but face no such constraints in non-democracies. Finally, Siddharth Chandra and Nita Rudra attribute lower growth-rate volatility in democracies to the diversity of institutions found in democracies relative to non-democracies.

In a series of papers, several of which focus exclusively on the Latin American context, Dani Rodrik discusses the fact that democracies have lower growth-rate volatility than non-democracies. Rodrik argues that democracies experience lower volatility because they "induce greater willingness to cooperate and compromise in the political sphere, generating greater stability as a result" (Rodrik 2000: 3). He offers three causal mechanisms through which democracy might facilitate such social cooperation. First, the process of deliberation inherent to democracies can alter original preferences of diverse actors and make them "'less selfish and more public spirited" (Rodrik 2000: 3). Second, democracies have more restrictions on the sorts of redistribution possible through policy due to "constitutional rules [that] curtail the power

of the majority to expropriate the minority" (Rodrik 2003: 3), which makes contending groups more likely to compromise with each other *ex ante*. Third, democracies induce cooperation by providing a forum for repeated interaction among political groups. So long as political groups in power know that they will not be in power forever, but are likely to return to power at some point in the future, they have an incentive to cooperate with their competitors for fear of retribution when their turn to be out of power arrives.

This cooperation-inducing effect of democratic governance is the reason democracies are better at dealing with negative external shocks than non-democracies (Rodrik 1998a). In the face of negative economic shocks, competing political actors must decide how to behave and, more specifically, how much of the economic pie to demand. The worst-case scenario for a country would be if these political groups were not able to put aside their narrow preferences to attempt to formulate the best policies possible for the nation as a whole. Good institutions, by which Rodrik means institutions that facilitate conflict management and social cooperation, make distributional outcomes less vulnerable to any group's opportunistic behavior aimed at obtaining a disproportionate share of the available resources (Rodrik 1998a: 10). Additionally, adjustment to shocks is worse in states with deep latent social conflicts, which presumably make cooperation that much more difficult by raising the stakes of any agreement (Rodrik 1997a: 8).

Unlike Rodrik's cooperation-based argument, Quinn and Woolley offer a preference-based explanation rooted in the notion of electoral accountability. The key to the Quinn–Woolley story is an assumption that national leaders are systematically different in their propensity to take risks from the populations they govern. Leaders are assumed to be more risk-acceptant than mass publics regardless of the country in which one resides. A second assumption is that "'volatility is the result, among other things, of unpredictable, arbitrary, or poor government policies" (Quinn and Woolley 2001: 642). A final assumption is that democracies and elections are better able to hold leaders to avoid risk than non-democracies and threat of overthrow.

If risky or bad policy is the source of growth-rate volatility and leaders are risk-acceptant then countries will have different levels of growth-rate volatility depending on how effectively their leaders are

restrained from taking risks. Societies in which leaders face no consequences for poor policymaking should, by this logic, have higher levels of growth-rate volatility than those that punish their leaders for poor performance. Quinn and Woolley argue that the presence of elections as an institutional means of selecting a nation's leaders is critical to constraining leaders and preventing them from making risky choices. Since leaders know that their publics are risk-averse and that they will be punished for volatile national economic performance, in countries where leaders are accountable to their publics for re-election (i.e., democracies) leaders should be less likely to make poor choices. Quinn and Woolley replicate three published studies of economic voting, including in these replications a measure for growth-rate volatility. They find that increased growth-rate volatility hurts the vote share of the incumbent, which they take as evidence that (1) voters punish volatility and (2) that, therefore, leaders in democracies should avoid volatility-inducing policy.

Siddharth Chandra and Nita Rudra draw on finance theory to model a country's set of formal national institutions as a portfolio, where a formal national institution "is an institution that has effective authority over aspects of policy making and implementation" (Chandra 1998: 5). The more institutions a country has, the more diversified its institutional portfolio. And, institutional diversity should lead both to less growth-rate volatility cross-sectionally (meaning that more institutionally diverse countries should have lower volatility as a group than less institutionally diverse countries) as well as lower volatility over time.

In the Chandra–Rudra model, each institution (i) takes a policy action (j), the realization of which is a random variable (R_j). Of course, not all formal national institutions are created equal and Chandra's model allows for different institutions to have different powers (η_i) relative to each other as well as to make different contributions (ϕ_{ij}) to the implementation of any action j. Finally, institutional independence (defined as the correlation $\rho(\phi_{ij}, \phi i\prime j)$ such that two institutions are independent if the correlation is 0 and completely dependent if it's 1. The relationship between the institutions matters too and policy variability is reduced if institutions are independent of each other since their initial outputs are therefore not dependent on each other. The overall output (O) of the economy is the expectation of a weighted

sum of these policy outputs from each institution:

$$E\left[O_j\right] = \sum_{i=1}^{n} E\left[\phi_{ij}\eta_i R_j\right] \tag{3.2}$$

Given this set-up, "'if the distribution of ϕ_{ij} is identical for all countries, it follows directly that the greater is the diversification of institutional authority among institutions, the smaller is the variance of outcomes in a sample of countries with that level of diversification" (Chandra 1998: 10). In other words,

> In systems in which there is a relative absence of checks and balances, formal national institutions have the power to maintain an economic milieu which can have extreme implications for economic outcomes. In comparison, in economies that have strong systems of checks and balances, particular institutions are more constrained in the extent to which they can mold the economic environment, with the result that there is less room for formal national institutions to steer the economy toward any extreme. (Chandra 1998: 17)

We thus have three alternative political explanations for cross-national variations in growth-rate volatility, which I summarize in Figure 3.4. Each theory makes an important contribution to our understanding of the political sources of growth-rate volatility, specifically offering insights on some of the different roles democracy may play in mitigating volatility. However, each lacks precision and concreteness regarding the specific democratic institutions, economic policies, and mechanisms through which democracy and policy in the abstract reduce volatility. Empirically, all three studies use the same index of political and civil liberties to measure democracy, preventing one from distinguishing among these three possible mechanisms by which regime type could relate to volatility and from exploring what specific aspects of regime type produce that relationship.

For example, Chandra and Rudra's argument hinges on the diversification of formal national institutions (i.e., the institutional milieu), but they never specify which institutions should matter for growth-rate volatility and how. Formal national institutions, in their classification, include the press, judiciary, trade unions, religious organizations, businesses and business organizations (Chandra and Rudra 2008: 17). The only caveat they add is that each of these institutions should

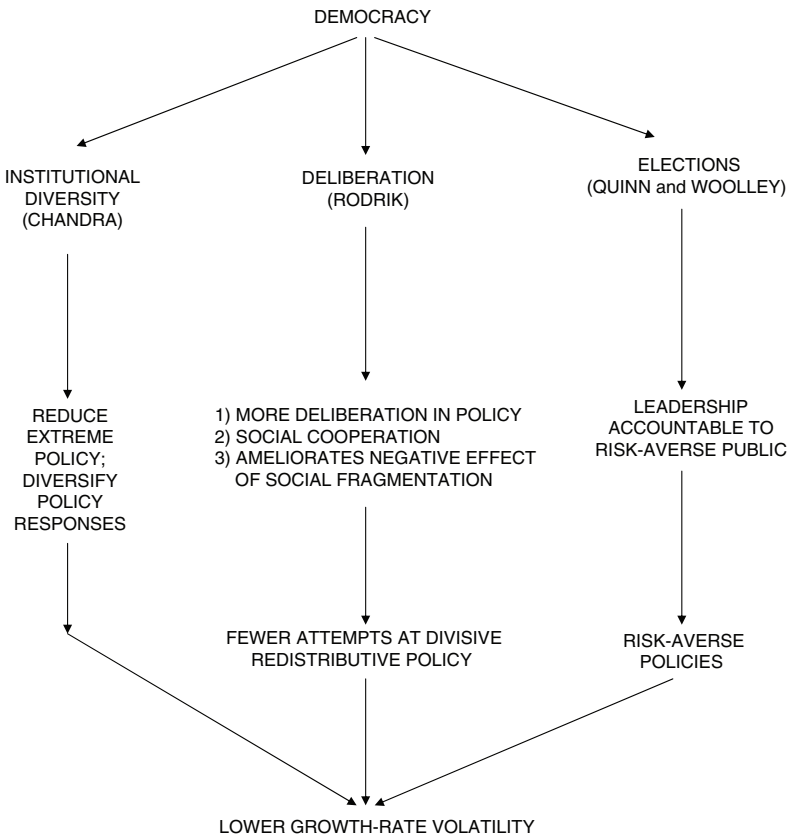

Figure 3.4 Existing political explanations of growth-rate volatility.

be "free," presumably so that each can have an independent effect on the policy outputs of the government, but there is no operational definition of "free," making it hard to know how to apply this framework to the real world. Furthermore, at this level of abstractness, it is impossible to know which institutions within a democracy are more important than others or to identify the precise causal mechanisms by which these institutions affect growth-rate volatility. (How, for example, do religious organizations or the press affect growth rates or their volatility?) Similarly, Rodrik's argument emphasizes the compromise-inducing effects of democracy but without identifying what aspects or types of democracy best produce this effect. Is compromise simply the

result of democratic leaders being elected or is it a product of the legislative process? As such, should we expect parliamentary and presidential democracies to behave the same or differently?

A second area for extension is the role played by non-governmental actors. The arguments tendered by Rodrik, Quinn and Woolley, and Chandra focus on the role played by governments such that the only causal mechanism explored is that between government policy and subsequent growth-rate volatility. Missing from such formulations is any account of what private (non-governmental) economic actors are doing, how they are responding to (expected) government policy, or how these actions and decisions might generate or reduce growth-rate volatility.

The argument I presented in the previous chapter addresses these shortcomings of the existing literature. I identify a specific configuration of state institutions – diffusion of policymaking authority to multiple actors with accountability to different constituencies – that lowers the average level of growth-rate volatility a country experiences. Specifically, the diffusion of policymaking authority across different policymakers with different accountabilities provides a set of *credible constraints* on an executive's ability to change policy autonomously (and potentially arbitrarily). Such constraints from potentially arbitrary policy and from policy variability bolsters confidence amongst private economic actors. The investor confidence engendered in countries with such institutions, in turn, both fosters longer-term, more stable investment and helps those countries withstand temporary shocks, preventing them from creating the full-blown crises that are more likely to emerge in countries lacking such institutions as panicked investors flee the likely unstable and/or poor policy-environment in the aftermath of shocks. Thus, the inability of governments lacking the credible constraints of diffuse policymaking authority and accountability to commit credibly to present and future policies induces savings and investment volatility and, thereby, growth-rate volatility. Thus, my argument can incorporate the institutional diversification argument of Chandra as well as the social compromise thesis put forth by Rodrik and, since separation of powers is more likely in democracies than non-democracies, my framework accounts for Quinn and Woolley's explanation as well. However, unlike the extant theories, which focus exclusively on "risky" government policy as the source of volatility, I

also focus on the role private investors play and introduce the importance of savings and investment volatility for growth-rate volatility.

An additional advantage of the framework I present is that it can be used to explain cross-temporal variation in growth-rate volatility within countries, as well as cross-sectional variation across states at a given point in time (or over some period of time). Consider the case of India, for instance. As I noted earlier, and as Kapur (2005) documents as well, India's economic growth trajectory displays significant variation in growth-rate volatility over time, with a definite trend towards lower volatility over time, and with clear breakpoints between periods of higher and lower volatility (I will return to the Indian case in Chapter 5). What explains such cross-temporal variation in growth-rate volatility? An explanation that simply emphasizes democracy is less useful for answering such a question since India has been a reasonably robust democracy since its independence in 1947. Yet, this difficult question of explaining dynamic variation in countries that do not experience regime transitions is answered fruitfully by using my framework of credible constraints, which directs our theoretical attention beyond questions of regime to more sophisticated considerations of the particular networks of institutions that comprise the "state."

This critique aside, I control for democracy in the models to get at other aspects of democracy not captured by my credible constraints indicators (for instance, accountability-by-elections, civil society, free press). I use a binary indicator from José Cheibub and Jennifer Gandhi for the existence of democracy (Cheibub and Gandhi 2004).

Political instability

The final "political" factors included in the model concern political instability. Including these variables allows me to distinguish the effects of "political" instability from "policy" instability, which is what the credible constraint institutions help prevent. I use two different indicators of political instability. The first is the widely-used political instability index compiled by Arthur Banks, which includes the following forms of domestic conflict: assassinations, general strikes, guerilla warfare, government crises, purges, riots, revolutions, and anti-government demonstrations. The second focuses on the incidence of violent political conflict that exceeds a minimum threshold of people killed in the conflict; this variable comes from the Uppsala Conflict Data Project. Both indicators of political instability are expected to

scare economic actors, hurt growth, and generate economic instability (Alesina *et al.* 1992).

Economic factors

While my interest in growth-rate volatility lies in its capacity to illuminate how politics influences economic outcomes, the fact is that much of what causes some countries to be more volatile than others lies in the realm of economics rather than politics. Economists' explanations for growth-rate volatility can be divided into two categories: those arguing output fluctuations are due to exogenous shocks to the economic system and those suggesting the causes of such fluctuations are endogenous to the economic system. Exogenous factors are typically terms of trade shocks caused by natural disasters in either the home state or a trading partner. The endogenous factors identified are generally the strength of the financial sector and investment patterns, as well as the stability of government policies thought to affect growth.

Trade

Openness to trade has long been considered an important aspect of a country's growth strategy. The arguments on behalf of trade-led growth are widely documented and I will not repeat them here.[10] The effect of trade openness on growth-rate volatility is less definitely known. Two relationships suggest themselves. First, countries with large trade sectors are more vulnerable to instabilities in foreign markets, which might flow across borders through the interdependent trade relationships that have been created, affecting sources of imports or markets for exports. Second, terms of trade shocks[11] can cause growth-rate volatility because sharp changes in the relative prices of exports to imports have dramatic effects on the trading portfolio of the states in question. This is especially true if the state is largely dependent on

[10] See Bhagwati (2004) for a recent discussion, but see (Rodrik 1997b, 1998a) or Rodriguez and Rodrik (1999) for a more skeptical view.

[11] A terms of trade shock is defined as "the growth in the local currency price of exports times the share of exports in GDP less the growth in the local currency price of imports times the share of imports in GDP, which captures both the magnitude of price fluctuations (changes in export and import prices) and their importance for the domestic economy (weighted by the shares of exports and imports in GDP)" (Easterly and Kraay 2000: 2021). Terms of trade shocks are typically treated as exogenous to the economy (Barro 1997: 30).

a few exports or imports, such as minerals or natural resources (Auty 2001; Sachs and Warner 1995).

The empirical support for these hypotheses has been mixed. Wu and Rapallo (1997) test the first of the above hypotheses and find no relationship. This is not really surprising since the simplistic hypothesis that states where trade comprises a larger share of gross domestic product (GDP) are more volatile is probably incorrect. The relationship between trade and growth-rate volatility is far more complex, involving the interaction of various factors. For instance, whether trade exposure dampens or heightens volatility should depend on whether domestic or foreign shock variance dominates and the diversification of the domestic economy, especially the state's export portfolio.

These interactive effects are supported by Easterly and Kraay (2000) who, in their study of why small states, such as island nations in the Caribbean or Pacific, have more volatile growth rates than their larger counterparts, argue small states are more sensitive to terms of trade shocks because international trade comprises a relatively higher proportion of their national economy and because smaller states have more specialized export portfolios "both in terms of products exported and in terms of export markets" (Easterly and Kraay 2000: 2022). Adverse trade shocks therefore reverberate through the small economy quickly and often devastatingly since the domestic economy is too small to balance the foreign shock. Extending this argument beyond small island states, Easterly *et al.* (2000: 10) find that terms of trade volatility and openness to trade are associated with increased growth-rate volatility across their sample.

To account for these effects of trade, I include in the model four independent variables, each designed to capture a different part of the overall relationship trade has with volatility. First, I measure total "trade openness," which is simply the proportion of a country's gross domestic product comprised by exports and imports. Just which way this relationship works is unclear, and depends on just what is being traded and with how many countries, i.e., how diversified the country's trading portfolio is.[12] What we are particular interested in is the degree to which a country's trading patterns make it vulnerable to

[12] Malik and Temple (2009) find that geographically remote countries have less diversified export portfolios and greater volatility in output growth. Rose and Spiegel (2009), discussed below, report a similar finding for financial remoteness.

external shocks. Therefore, I control for whether a state is a primary commodity exporter. Oxford University's Paul Collier summarizes the concern with respect to African countries, which comprise the bulk of the world's primary commodity exporters:

> The world prices of primary commodities are highly volatile, producing both booms and crashes. Most African countries are dependent upon a very narrow range of commodities and this exposes them to severe macroeconomic shocks. For other developing regions such shocks are largely a thing of the past due to export diversification. Globally these large shocks are problematic for exporting countries. Typically, booms do not translate into sustained increases in income – they are missed opportunities – whereas crashes produce devastating and long-lasting declines. (Collier 2002:2–3)

Put another way, high concentration of exports on a few primary commodities is a major source of terms-of-trade and growth-rate volatility. I therefore create an indicator for whether over 75 percent of a country's exports are comprised by primary commodities, excluding fuels.[13] Third, I control for the size of the agricultural sector in the economy. Finally, I calculate whether a country experienced a terms-of-trade shock using World Bank data the on the country's export and import values.

Financial sector and capital flows

In a report to the Annual Bank Conference on Development Economics, (Easterly *et al.* 2001) point their collective finger at the financial sector as the principal culprit for growth-rate volatility. Neoclassical economic theory, in their opinion, fails to anticipate wealth and cash flow constraints arising in imperfect markets. As a result, previous macroeconomic analyses have underestimated how the behavior of firms facing cash and wealth flow constraints result in output fluctuations. This relationship is mediated by financial market institutions (such as banks and security markets), which affect how firms handle shocks to their cash flows (Easterly *et al.* 2000: 1). The most important role of such financial institutions is to deter the occurrence of

[13] Data come from (UNCTAD 2002). By this definition, primary commodity exporters are Belize, Botswana, Burundi, Chad, Chile, Côte d'Ivoire, Cuba, Ethiopia, Guinea-Bissau, Jamaica, Kiribati, Malawi, Mali, Mauritius, Mozambique, Myanmar, Nicaragua, Niger, Paraguay, Solomon Islands, Sudan, Suriname, Tajikistan, Tanzania, Tonga, Uganda, Zaire, and Zimbabwe.

bankruptcy chains, which could be triggered by the bankruptcy of a single firm in conjunction with the complex credit interrelationships among firms characterizing modern economies.

Initial empirical investigations by (Easterly *et al.* 2000: 3) on a sample of "60 to 74 countries in a panel created by aggregating over the periods 1960 – 78 and 1979 – 97" support the hypothesis that financial depth, as measured by the ratio of private credit to GDP, dampens growth-rate volatility. Therefore, I control for the financial liquidity of the country by including a measure of the level of credit available in the economy, operationalized as the level of money and quasi-money (M2) as a percentage of GDP.

An alternative claim is that capital account openness might give states access to international capital, thereby easing credit constraints, or that capital account openness actually induces volatility. Since the behavior of capital-holders is a consequence of the political institutions that are the focus of the analysis, I prefer not to control for capital account openness in the model. However, when I use the Chinn and Ito (2006) measure of capital account openness, that variable is not statistically significant in the models reported below.[14] Another alternative financial factor has been identified recently by economists Andrew Rose and Mark Spiegel (2009), who find that international financial remoteness – a country's proximity to major international financial centers – is positively correlated with macroeconomic volatility. Yet, as the authors themselves note, the effect of financial remoteness "does not matter as consistently or robustly as political institutions" and their results are sensitive to model specification details. Nevertheless, their analysis makes clear that future research must pay greater attention to the role of financial integration in affecting output volatility.

Macroeconomic conditions and policy

The final set of economic factors concern the macroeconomic conditions present and policy choices of governments. Quinn and Woolley state that "volatility is the result, among other things, of unpredictable, arbitrary, or poor governmental policies" (Quinn and Woolley 2001: 642). In this view, macroeconomic crises are the result of unsustained or unsustainable government policies, such as large government sectors, budget deficits, and high inflation. These particular policies are

[14] Results available from author.

also associated with poor economic performance in general and have been roundly condemned by international development agencies. For instance, the World Bank states that "high inflation increases uncertainty, discourages investment and technological change, [and] distorts relative prices" (World Bank 1987: 14), while Ricardo Caballero, in his work for the Inter-American Development Bank, argues that inflation and inefficient government budgets are the primary cause of Latin American economic crises (Caballero 2000b, 2000c). Acemoglu *et al.* (2003b) investigate the relationship between macroeconomic policies and volatility empirically using cross-national data from 1970 to 1997. They find that "average size of government (measured by government consumption to GDP ratio) [and] (log) average rate of inflation" have a robust positive effect on growth-rate volatility and the likelihood of severe crisis. Therefore, I include a measure of government consumption as a share of GDP and the level of inflation in the model.

I also control for basic macroeconomic conditions that form the backdrop for the country. I control for overall growth in the economy, population growth, and the overall level of economic development in the economy. Joined with the political variables and other economic variables discussed above, this gives me a theoretically-motivated model of growth-rate volatility, suitable for empirical testing. While I have tested variants of this model to ensure the robustness of the results reported below, I focus in the text on this base model to ease presentation.

Results

I combine the set of economic and political factors suggested by theory in a single model of growth-rate volatility. Because the credible constraint variables are only coded as present if the country is a democracy, the baseline political category is non-democracy. Some might protest the lack of differentiation in my treatment of non-democratic governments, but to the extent that non-democratic governments can form credible commitments and reduce the volatility they experience, this should make it harder to find any effect in the credible constraint indicators, and therefore this constitutes a more rigorous test of the argument. As a robustness check, I have checked to see if Wright's conception of binding legislatures in authoritarian regimes gives us any purchase

on growth-rate volatility, but it does not. Since that variable is more limited than the rest of the data set (it only exists from 1991–2002), including it here is very costly in terms of missing data. However, given the fascinating scholarly work being done on dictatorial institutions by scholars such as Jennifer Gandhi and Joseph Wright, among others, future scholars should definitely add to the models described below once we have more comprehensive indicators for these regimes.

Table 3.2 presents the results of four regression models explaining growth-rate volatility.

The results broadly confirm the expectation that states in which credible constraints against policy change exist experience lower levels of growth-rate volatility, but with important exceptions. Minority and coalition governments in parliamentary democracies emerge as the most consistent dampener of volatility, with independent central banks a close second. Neither federalism nor judicial independence appear very important for volatility. Interestingly, however, while the divided presidential government indicator is largely insignificant, when one considers the shortest-length panel of five years, that indicator is correlated with *higher* levels of growth-rate volatility ($p = 0.06$), *ceteris paribus*. To some extent, this could be seen as confirmation of the intuitions of scholars who have bemoaned the gridlock it has created in Latin America (Ames 2001, Mainwaring 1993). The dynamics of divided government differ fundamentally from those of minority/coalition governments: while the latter encourage policy moderation, the former generates policy gridlock, making it near impossible for states to make the policies they require in order to attract savings and investment or stimulate economic growth.

Interestingly these results are robust to controlling for democracy more generally, which also proves a robust determinant of lower growth-rate volatility. Clearly the channels through which democracy dampens volatility are multifarious, and may include any or all of those suggested by scholars like Rodrik, Quinn and Woolley, or Chandra and Rudra.[15] Given that democracy co-exists with the credible constraint institutions, finding an effect for the latter while controlling for the former provides strong evidence that the effects of credible constraints are not just those of democracy, but represent a distinct channel: increased

[15] See also Mobarak (2005), Klomp and de Haan (2009).

Table 3.2 ***Regression results: credible constraints and growth-rate volatility***

DV: growth-rate volatility				
Sample:	*All years, all LDCs*	*All years, democracies*	*Decade, all LDCs*	*Five-year, all LDCs*
Minority/coalition govts	−2.34 (1.61)$^{0.15}$	−4.03 (1.09)$^{0.00}$	−1.59 (0.96)$^{0.10}$	−1.35 (0.71)$^{0.06}$
Divided presidential govts	3.64 (3.05)$^{0.24}$	1.51 (1.29)$^{0.27}$	2.11 (1.33)$^{0.12}$	2.03 (1.07)$^{0.06}$
Independent judiciaries	−0.93 (1.14)$^{0.42}$	0.54 (1.03)$^{0.61}$	−0.52 (0.60)$^{0.39}$	−0.45 (0.53)$^{0.40}$
Federalism	−0.08 (1.14)$^{0.95}$	2.77 (0.97)$^{0.02}$	0.35 (1.04)$^{0.74}$	0.23 (1.01)$^{0.82}$
Central bank indep.	−1.73 (0.75)$^{0.02}$	−0.71 (0.92)$^{0.46}$	−1.28 (0.56)$^{0.02}$	−0.81 (0.49)$^{0.11}$
Democracy	−2.89 (0.92)$^{0.00}$		−1.70 (0.66)$^{0.01}$	−1.28 (0.66)$^{0.05}$
Political instability	0.25 (0.14)$^{0.09}$	0.04 (0.24)$^{0.87}$	0.07 (0.07)$^{0.32}$	−0.03 (0.07)$^{0.72}$
Civil wars	0.09 (0.61)$^{0.88}$	−1.29 (0.73)$^{0.10}$	1.09 (0.64)$^{0.09}$	0.62 (0.66)$^{0.35}$
GDP growth	−0.08 (0.20)$^{0.69}$	−0.39 (0.18)$^{0.06}$	0.06 (0.09)$^{0.51}$	−0.05 (0.11)$^{0.65}$
Population growth	−0.49 (0.24)$^{0.04}$	−0.72 (0.27)$^{0.02}$	−0.19 (0.18)$^{0.29}$	−0.24 (0.19)$^{0.23}$
GDP per capita (Log)	1.79 (0.45)$^{0.00}$	0.29 (0.74)$^{0.69}$	1.25 (0.42)$^{0.00}$	1.05 (0.34)$^{0.00}$
Inflation (Log)	0.26 (0.24)$^{0.28}$	−0.18 (0.29)$^{0.54}$	0.39 (0.25)$^{0.12}$	0.33 (0.29)$^{0.25}$
Trade openness (% GDP)	0.01 (0.01)$^{0.36}$	0.04 (0.01)$^{0.01}$	0.02 (0.01)$^{0.05}$	0.01 (0.01)$^{0.39}$
Agriculture (% GDP)	0.16 (0.04)$^{0.00}$	0.13 (0.13)$^{0.12}$	0.13 (0.04)$^{0.00}$	0.13 (0.03)$^{0.00}$
Primary commodity exporter	0.12 (0.66)$^{0.86}$	1.46 (0.81)$^{0.10}$	−0.06 (0.63)$^{0.92}$	−0.16 (0.64)$^{0.81}$
Terms of trade shock	0.02 (0.04)$^{0.63}$	0.20 (0.10)$^{0.07}$	−0.004 (0.01)$^{0.77}$	0.01 (0.01)$^{0.41}$
Government consumption	0.17 (0.06)$^{0.01}$	−0.04 (0.07)$^{0.58}$	0.07 (0.04)$^{0.07}$	0.05 (0.04)$^{0.13}$
Financial liquidity (M2)	0.02 (0.02)$^{0.36}$	−0.09 (0.03)$^{0.01}$	0.01 (0.01)$^{0.63}$	0.03 (0.01)$^{0.07}$

Table 3.2 (*cont.*)

DV: growth-rate volatility				
Sample:	*All years, all LDCs*	*All years, democracies*	*Decade, all LDCs*	*Five-year, all LDCs*
Time trend			−0.57 (0.25)$^{0.03}$	−0.21 (0.16)$^{0.19}$
Constant	−14.21 (4.55)$^{0.00}$	3.04 (8.50)$^{0.73}$	−8.34 (4.29)$^{0.05}$	−6.22 (3.81)$^{0.11}$
No. of countries	108	30	105	105
No. of observations	108	30	260	349
Root mean square error	2.44	1.09	2.94	3.49

Note: Cell entries are OLS coefficients with standard errors corrected for clustering by country reported in parentheses and two-sided p-values superscripted.

confidence that policy will remain intact or change only incrementally. I will provide more direct evidence of this channel in the next chapter, but more discussion of these results is called for before I get to that.

What other factors influence volatility? As expected, political instability (as distinguished from expected policy instability due to political institutions) increases volatility, though this result is not very robust to using different lengths of time over which to measure volatility. The more consistent predictors of growth-rate volatility tend to be variables such as level of economic development, which in the developing world has the opposite relationship to volatility than we might have expected. Richer countries have higher levels of volatility in this analysis, which makes sense when we recall that the volatility measure is sensitive to changes in the mean trend in the data. That is, as these high-performing states transition from low levels of growth early on to higher levels of growth later, the volatility measure picks this up. This might raise a concern that the volatility-dampening effects of coalition governments reported above are coming at the expense of overall growth, but I will provide some additional data later in this chapter to show that the opposite is in fact true.

Another consistent factor is the size of the agricultural sector in the economy. Agriculture is a volatile sector of the economy, subject to the vagaries of the weather and to price shocks. As expected, my analysis confirms that states that are more dependent on agriculture in

the economy experience higher levels of volatility. The same is not true for primary commodity exporters or terms of trade shocks, neither of which is revealed to have any effect on growth-rate volatility (though both variables are statistically significant in the democracy subsample model reported in column 2 of Table 3.2). Likewise, financial liquidity only appears to matter in democratic settings (column 2) but not overall. One explanation might be that democratic states have a considerable financial advantage over non-democratic states; the mean value for M2 as a share of GDP for democracies is 44 percent, while it is just 32 percent for non-democracies, a difference that is highly statistically significant. Controlling for democracy masks the effect of financial liquidity, which becomes more apparent when we only focus on democracies. The same is likely true for why terms-of-trade – a factor that has consistently emerged in the economic literature – is not significant in the pooled sample. Non-democratic states experience much larger terms-of-trade shocks on average (2.84 percent in these data) than democracies (−0.61 percent), and this difference is also statistically significant.

The final economic factor that affects growth-rate volatility is the size of government consumption as a share of the economy. Economists have long bemoaned the dangers of large states for the economy, and this concern is borne out by these results. High levels of government consumption are not sustainable and make states prone to fiscal crises.

The preceding analysis focused on the developing world's experience, and some readers might wonder if the argument fits the OECD experience in the least. The literature on the increased macroeconomic stability in the OECD focuses almost exclusively on economic variables and the improvement of demand management strategies by governments and central banks. While not disputing any of these claims, I would argue that the OECD experience is nevertheless consistent with its central argument. For most of the 1945–present period, the United States has had divided presidential government, and this coincides with the most stable period of US growth in its history, a point documented by Douglas Hibbs in his seminal work on the American political economy (Hibbs 1987). Further, many of the OECD states in Europe have the types of centrist coalition parliamentary governments that I argue should be most likely to enjoy high credibility.

This point aside, decreases in macroeconomic volatility in the OECD have much to do with increased technical expertise in smoothing the

business cycle, and the delegation of authority over monetary policy to independent central banks. This is a point anticipated by my argument which makes clear that delegation to independent central banks is one method of achieving credibility, but it does highlight the puzzle of why such institutions have been less effective in the developing world. That is a question beyond the scope of this book, though I will return to it tangentially in later chapters, and deserves more attention by future researchers.

Robustness check: extreme volatility

The results reported in the previous section support the argument that credible constraints that promote policy moderation reduce the volatility of growth rates experience by developing countries. And this result is robust to controlling for democracy and a host of economic and other political factors. But how confident can we be in the results? To assess their robustness, I use another dependent variable. I create an indicator for "extreme volatility," which I define as volatility greater than the world average volatility in a given year. That is, a state is coded as having experienced extreme volatility if in a given year its deviation from average growth is greater than the world average deviation from average growth in that year. All states experience year-to-year fluctuations in growth of course; what the extreme volatility indicators seeks to highlight are those states whose deviations from the normal are exceptional relative to their counterparts. This comparison also takes into account common international shocks that affect all states in the international system in a given year (for instance, the oil crisis of 1973). I calculate the percentage of years over the entire period that a state experienced extreme volatility. Table 3.3 reports the pairwise correlations for the frequency of years of extreme volatility with the credible constraint indicators.

As in the main analysis, only the *partisan* constraints emerge as significant correlates of volatility. Both coalition/minority parliamentary governments and divided presidential governments are correlated with fewer years of extreme volatility. The first of these findings is consistent with the prior analysis, but the divided government finding appears to contradict the earlier result that this institutional configuration increases growth-rate volatility. This is not true though.

Table 3.3 ***Pairwise correlations between extreme volatility and credible constraint indicators***

	Length of panel		
	All years	**Ten years**	**Five years**
Minority and coalition governments	−0.12	**−0.21**	**−0.15**
Parliamentary democracies only			
Divided government	**−0.14**	**−0.12**	**−0.13**
Presidential democracies only			
Independent judiciaries	0.11	0.03	0.05
Democracies only			
Federalism	−0.14	−0.02	−0.03
Democracies only			
Central bank independence	−0.25	−0.11	−0.07
Democracies only			
Binding legislatures	−0.11	−0.09	0.08
Non-democracies only			

Note: Cell-entries are pair-wise correlation coefficients. Correlations that are statistically significant at the 0.05 level of better are reported in **bold**.

Presidential democracies in which the executive and legislature are controlled by different parties experience fewer years of *extreme* volatility but it remains true that they do experience higher levels of growth-rate volatility than their unified counterparts. Since democracies overall are far less likely than non-democracies to suffer extreme volatility, it is not a surprise that divided presidential democracies are also less likely to have extreme fluctuations in growth relative to the rest of the world. But once one accounts for democracy, as I did in the previous analysis, the gridlock inherent in divided presidentialism is seen to lead to less stable growth rates.

Using the same set of variables as in the regression analysis of growth-rate volatility above, I estimate a pair of models using this "extreme volatility" variable as a dependent variable. Since extreme volatility, by definition, is an unusual event, I utilize the entire period as the unit of observation. The results are therefore based on a cross-section of countries for which data are available, and the dependent variable is thus the proportion of years under study during which a country had extreme volatility. Table 3.4 reports the results of this analysis.

Table 3.4 ***Regression results: credible constraints and extreme volatility***

	DV: extreme volatility	
Sample:	*All states*	*LDCs only*
Minority/coalition govts	−0.21 (0.11)$^{0.05}$	−0.18 (0.13)$^{0.15}$
Divided presidential govts	−0.05 (0.15)$^{0.77}$	0.004 (0.18)$^{0.98}$
Independent judiciaries	−0.04 (0.05)$^{0.45}$	−0.01 (0.05)$^{0.89}$
Federalism	−0.03 (0.05)$^{0.59}$	−0.01 (0.08)$^{0.90}$
Central bank indep.	−0.09 (0.04)$^{0.02}$	−0.06 (0.05)$^{0.18}$
Democracy	−0.15 (0.06)$^{0.02}$	−0.18 (0.07)$^{0.01}$
Political instability	0.01 (0.01)$^{0.25}$	0.01 (0.01)$^{0.16}$
Civil wars	0.03 (0.04)$^{0.48}$	0.02 (0.04)$^{0.55}$
GDP growth	−0.01 (0.01)$^{0.17}$	−0.01 (0.01)$^{0.14}$
Population growth	−0.03 (0.02)$^{0.05}$	−0.05 (0.02)$^{0.02}$
GDP per capita (Log)	0.08 (0.02)$^{0.00}$	0.10 (0.03)$^{0.00}$
Inflation (Log)	0.03 (0.02)$^{0.05}$	0.02 (0.02)$^{0.23}$
Trade openness (% GDP)	0.001 (0.001)$^{0.03}$	0.001 (0.001)$^{0.21}$
Agriculture (% GDP)	0.01 (0.002)$^{0.01}$	0.01 (0.002)$^{0.00}$
Primary commodity exporter	−0.002 (0.03)$^{0.95}$	0.003 (0.93)$^{0.91}$
Terms of trade shock	0.002 (0.003)$^{0.54}$	0.002 (0.002)$^{0.49}$
Government consumption	0.01 (0.003)$^{0.04}$	0.01 (0.003)$^{0.01}$

Table 3.4 (*cont.*)

	DV: extreme volatility	
Sample:	*All states*	*LDCs only*
Financial liquidity (M2)	0.004 $(0.001)^{0.49}$	−0.001 $(0.001)^{0.3}$
Constant	−0.64 $(0.27)^{0.02}$	−0.76 $0.27)^{0.01}$
No. of countries	123	108
Root mean square error	0.14	0.14

Note: Cell entries are OLS coefficients with robust standard errors in parentheses and two-sided p-values superscripted.

The results of the extreme volatility analysis confirm the major results of the chapter thus far. Controlling for whether the country is a democracy, countries with parliamentary coalition or minority governments experienced lower rates of extreme volatility. Admittedly, this effect is strongest when one includes all countries in the analysis, and falls beyond conventional thresholds for statistical significance when one limits the sample to developing countries only. Yet, even in the developing country sample, the estimated effect is negative and the estimated effect is quite stable. Rather the "loss" of significance is due to a slightly larger standard error and fewer degrees of freedom. Thus, for all intents and purposes, the volatility-dampening effect of coalition governments is substantiated by this robustness analysis.

Are both stability and higher growth possible?

Unlike the majority of studies on national economic performance published in political science or economics, I have focused thus far on explaining variation in the volatility of growth rather than its mean. This focus is justified for volatility as it has important consequences for people's lives, as well as for a country's ability to attract future investment and foster longer-term economic development. To the extent that volatility does hurt future growth, as the analysis earlier in this chapter

suggests it does, political institutional configurations, such as coalition governments in parliamentary democracies or independent central banks, have at least an indirect effect on future economic growth. But critics might well argue that the focus on future growth underestimates the costs of potentially sacrificing growth in the present. That is, if institutions like coalition governments make policymaking harder and therefore prevent necessary economic reform, they might hurt economic growth. What good is more stable growth if the stability is around a low average level of growth? Would not poor countries prefer higher average growth even if it comes at the price of increased volatility? At one level, it is unclear that this tradeoff is so simple or at any rate that its distributional implications are straightforward, and suggests that future research might want to consider the consequences of such a tradeoff explicitly. At what point, for instance, do human development outcomes become impervious to increased volatility and sensitive to the level of economic growth, or vice versa? But, at another level, even without answers to such interesting and policy-relevant debates, the question of whether stability and growth are both attainable is empirical. This section seeks to answer it.

Studying economic growth has a rich heritage. Growth is not the only outcome worthy of attention, but it might certainly be one of the more important because of its importance for other things citizens desire such as increased nutrition, better health, and longer lives. The dominant strategy among growth theorists is to focus on getting economic policies correct. To this framework, I have added the necessity of convincing private economic actors that policy commitments are credible and that they can plan economic activity with the understanding that policy changes will be infrequent and incremental. When scholars have studied the impact of politics on economic growth, they have emphasized the importance of democracy in the main, even though empirical evidence for the value of democracy for promoting economic growth is sorely lacking (Przeworski *et al.* 2000). But even democratic governments vary in how they distribute policymaking authority, and different institutional configurations that produce policy stability do so for different reasons. While coalition and minority governments in parliamentary democracies generate incentives for policy moderation, divided governments in presidential systems create policy gridlock. The former, I've argued, is far more desirable for developing countries that need their governments to govern, but that seek to moderate

their behavior by requiring them to consider the diversity of views that abound in the society-at-large. The latter, on the other hand, by allowing different branches of government to exist independently of each other, reduces the likelihood of reform – even when its sorely needed – and encourages a more divisive policy stasis. This argument was borne out by my analysis of growth-rate volatility; does it apply equally well to economic growth?

To find out, I apply the empirical model developed above to explain cross-national variation in economic growth. For consistency, I use the same set of independent variables as above, switching only the dependent variable, which makes sense since average growth and the volatility of that growth are two moments of the same time-series (data-generating process). I use averages of growth and the other independent variables over two different periods – ten-year panels and five-year panels – for the sake of robustness. And, as above, I use both the full sample and a sub-sample of countries that were democratic for at least half the period of study. Table 3.5 reports the results of this analysis.

The results from the growth model support the theoretical framework developed in this book: the most robust political determinant of economic growth is the presence of coalition or minority parliamentary governments. The more years in the period that a country was governed by a coalition or minority parliamentary government, the higher the country's average economic growth. Nor is the size of this effect small. According to the estimates for Model 1 (the first column) in Table 3.5, a country that had coalition government throughout the ten-year period had 1.33 percent higher growth per year in that period than its counterparts. If we limit the sample only to countries that were mostly democratic throughout the period, that effect increases by a full percentage point to 2.36 percent higher economic growth, all else held equal. Given that the average growth rate for developing countries over the time period being studied here is 1.6 percent, the potential gains from this political-institutional structure are significant. And it's worth emphasizing that this positive effect of coalition governments is found even when controlling for democracy, which is not significant itself. This accords with the pessimistic conclusions about democracy's growth-enhancing prospects in the existing literature, though I would hasten to remind readers that democracy had a robust negative effect

Table 3.5 ***Regression results: credible constraints and economic growth***

	DV: economic growth			
	Ten-year panels		*Five-year panels*	
Sample:	*All LDCs*	*Democracies*	*All LDCs*	*Democracies*
Minority/coalition govts	1.33 (0.79)$^{0.10}$	2.36 (0.79)$^{0.01}$	0.91 (0.68)$^{0.19}$	1.73 (0.67)$^{0.01}$
Divided presidential govts	−0.24 (1.25)$^{0.85}$	−0.44 (1.24)$^{0.72}$	−1.90 (1.09)$^{0.09}$	−1.81 (1.25)$^{0.15}$
Independent judiciaries	0.76 (0.59)$^{0.21}$	0.57 (0.45)$^{0.21}$	0.47 (0.68)$^{0.50}$	−0.02 (0.67)$^{0.98}$
Federalism	0.84 (0.98)$^{0.39}$	−0.49 (1.15)$^{0.67}$	1.59 (1.07)$^{0.14}$	2.26 (1.34)$^{0.10}$
Central bank indep.	0.12 (0.55)$^{0.83}$	−0.29 (0.55)$^{0.73}$	0.15 (0.51)$^{0.77}$	−0.88 (0.57)$^{0.13}$
Democracy	0.41 (0.47)$^{0.39}$		0.75 (0.56)$^{0.19}$	
Political instability	−0.20 (0.06)$^{0.00}$	−0.19 (0.09)$^{0.03}$	−0.11 (0.06)$^{0.06}$	−0.25 (0.13)$^{0.06}$
Civil wars	0.76 (0.45)$^{0.09}$	1.32 (0.57)$^{0.02}$	0.35 (0.54)$^{0.52}$	1.92 (0.72)$^{0.01}$
GDP per capita (Log)	−0.38 (0.39)$^{0.33}$	−0.0002 (0.39)$^{1.00}$	−0.69 (0.48)$^{0.15}$	0.31 (0.53)$^{0.56}$
Population (Log)	0.35 (0.28)$^{0.20}$	0.06 (0.24)$^{0.79}$	0.20 (0.31)$^{0.52}$	−0.31 (0.32)$^{0.34}$
Inflation (Log)	−0.47 (0.22)$^{0.03}$	0.11 (0.22)$^{0.60}$	−1.19 (0.27)$^{0.00}$	−1.29 (0.27)$^{0.00}$
Trade openness (% GDP)	0.03 (0.01)$^{0.02}$	0.01 (0.01)$^{0.41}$	0.02 (0.01)$^{0.11}$	0.003 (0.01)$^{0.78}$
Agriculture (% GDP)	−0.01 (0.03)$^{0.65}$	−0.02 (0.04)$^{0.59}$	−0.05 (0.04)$^{0.22}$	−0.01 (0.05)$^{0.83}$
Primary commodity exporter	−0.17 (0.56)$^{0.76}$	0.07 (0.43)$^{0.87}$	0.26 (0.71)$^{0.71}$	0.70 (0.64)$^{0.28}$
Terms of trade shock	0.02 (0.01)$^{0.03}$	0.07 (0.04)$^{0.08}$	−0.002 (0.02)$^{0.87}$	−0.03 (0.04)$^{0.49}$
Government consumption	−0.09 (0.04)$^{0.02}$	−0.16 (0.05)$^{0.00}$	−0.12 (0.04)$^{0.00}$	−0.14 (0.06)$^{0.03}$
Financial liquidity (M2)	0.01 (0.01)$^{0.33}$	0.01 (0.02)$^{0.62}$	0.01 (0.02)$^{0.44}$	−0.02 (0.02)$^{0.52}$
Time trend	−0.01 (0.25)$^{0.97}$	0.44 (0.27)$^{0.11}$	0.14 (0.17)$^{0.42}$	0.46 (0.23)$^{0.05}$

Table 3.5 (*cont.*)

	DV: economic growth			
	Ten-year panels		*Five-year panels*	
Sample:	*All LDCs*	*Democracies*	*All LDCs*	*Democracies*
No. of countries	105	53	105	58
No. of observations	262	87	352	135
Root mean square error	2.59	1.71	3.68	2.84

Note: Cell entries are OLS coefficients with standard errors corrected for clustering by country reported in parentheses and two-sided p-values superscripted.

on volatility. Democracy per se might lead governments to "muddle through" as Chandra and Rudra put it, i.e., achieve more stable but not higher economic growth than non-democracies, but the credible constraints framework highlights that certain types of democratic structures might allow governments to achieve both higher economic growth and greater stability.

The flip side, however, is also true. While not as consistently as in the growth-rate volatility analysis, divided presidential government is again associated with worse national economic performance. Above, I found that such governments have more volatile growth; here divided presidential governments are found to have a negative effect on economic growth, though this negative effect is only statistically significant when we use five-year panels as the unit of analysis. So, at best, we would conclude that divided presidentialism leads to only higher volatility, but, at worst, this increased volatility is accompanied by lower growth overall too. The "perils of presidentialism" are real indeed!

None of the other credible constraint indicators are statistically significant correlates of economic growth, though political federalism comes closest to boosting economic performance at statistically significant levels. This is consistent with the volatility analysis, and suggests that non-partisan formal credible constraint institutions have less direct bearing on national economic performance than their partisan counterparts, though it is quite possible that these institutions matter more indirectly through their effects on investor behavior. A second possibility is that such institutions might work in specific contexts, but on

average – which after all is what a regression coefficient is estimating – do not have an effect in the developing world. I will address the first possibility in the next chapter, and consider the effects of these non-partisan institutions in the India case study in Chapter 5, but for now return to the other variables that affect economic growth.

Political instability is one of these, though the direction of the effect depends on the source of the instability. Greater domestic strife in the form of riots and anti-government demonstrations reduces economic growth, but more violent internal conflict (such as civil wars) have a positive effect on economic growth overall. The first half of this finding is consistent with existing literature and with our intuitions, but the second half is very counter-intuitive at first glance. Why would violent civil conflict be "good" for economic growth? Actually this finding is not all that at odds with existing research. In work with Thomas Edward Flores on post-civil-conflict reconstruction, I found that half of the conflicts in the developing world from 1960 to present were accompanied by economic expansion over the years of the conflict; the other half resulted in economic decline (Flores and Nooruddin 2009). The causal mechanisms that lead some countries to the growth outcome and others to the decline outcome are as yet unclear, though it remains a topic of current research for us. What we do know, especially from research by A.F.K. Organski and Jacek Kugler (1980), is that conflict often generates economic opportunities for entrepreneurs who must supply the fighters, and later to help rebuild the society. In fact, as the cases of Germany and Japan in the aftermath of World War II prominently attest, economic growth is often facilitated by the devastation of war, a phenomenon Organski dubbed the "Phoenix Effect."

The economic control variables behave as expected. High levels of inflation hurt economic growth, and countries with large government sectors grow more slowly as well. Trade appears to have a positive effect on economic growth overall, though this effect is only statistically significant over the longer ten-year period (it is slightly less precisely estimated in the shorter five-year-period analysis). Finally, I should note that I do not include a variable commonly used in growth regression models – human capital – because of a severe lack of data availability. But when I do control for the share of the labor force with a tertiary education, that variable is statistically significant and positively-signed as

expected (five-year periods: $\beta = 0.08$; *s.e.* $= 0.04$; $p = 0.04$; $n =$ seventy-two observations and forty-nine countries). Including this variable does not change the sign of the main effects reported above, though cutting the sample by 80 percent, from 352 observations to seventy-two, reduces the statistical precision of the estimates somewhat.

As a final piece of evidence for the utility of the credible constraints framework, consider the variation in national economic performance among African countries. While most countries in Africa remain among the world's poorest, and the problems of poverty rank as the most intractable there, one must be careful not to treat this very diverse continent as a monolith. African countries display considerable variation in reality in their national economic performance, and some governments are rightly hailed as examples of success (for example, Botswana; see Acemoglu *et al.* (2003a) and Chapter 6). Following a World Bank analysis of the growth patterns of thirty-five African states (World Bank 2008), I distinguish between two basic African growth types: bad and good. The average growth-rate of the "bad" growth-type countries was a mere 0.2 percent during the period studied here; the "good" growth types had average growth rates of 1.5 percent during the same time period (the difference is statistically significant at $p < 0.05$). Likewise, while both types had higher-than-world-average levels of growth-rate volatility, the good types experienced more stable growth-rates than the bad types (4.6 versus 5.6 respectively; $p = 0.11$). Can the credible constraints framework provide any explanatory leverage over why some states fall in the good category of higher and more stable growth, while others experience more volatile and lower growth? While the sample size of thirty-five is too small for a complete statistical analysis, Table 3.6 suggests that it can.

Table 3.6 presents the results from a series of difference-of-means tests for the different indicators of credible constraints across the two African growth types. "Good" performers were more democratic than the "bad" performers, though neither category was particularly democratic overall. Similarly, good performers had more experience with coalition or divided government, though these effects are both quite small. Rather, the factor that emerges as most useful potentially is judicial independence, with good performers having on average more years

Table 3.6 *African growth-types*

African growth type	Regime type		Credible constraints				Auth. binding legislature
	democracy	polity	Coalition government	Divided government	Judicial Independence	Cent. bank Independence	
Bad	0.11	−4.19	0	0.001	0.06	0.25	0.59
Good	0.24	−0.85	0.03	0.02	0.16	0.24	0.65
p−value	0.07	0.001	0.17	0.15	0.09	0.57	0.33

List of countries in each growth-type

Bad: Burundi, Central African Republic, Comoros, Eritrea, Guinea, Guinea-Bissau, Kenya, Lesotho, Madagascar, Malawi, Mauritania, Niger, Seychelles, Togo, Zaire, Zambia, Zimbabwe; *Good*: Benin, Botswana, Burkina Faso, Cameroon, Cape Verde, Ethiopia, Ghana, Gambia, Mali, Mauritius, Mozambique, Namibia, Rwanda, Sao Tome and Principe, Senegal, Sierra Leone, Tanzania, Uganda

with strong courts than bad performers.[16] These results are hardly conclusive, but they certainly point in the direction hypothesized. Given the small sample size and the difficult test that explaining growth patterns in Africa presents, I am very encouraged.

Endogeneity?

A final point on the empirical evidence concerns the question of endogeneity. Specifically, given the work of Przeworski *et al.* (2000), one might wonder whether the results documented thus far are an artifact of some unexplored factor. For instance, might it be that coalition governments are more likely to survive only in good and stable economic conditions? If so, then any correlation between the existence of coalition governments and high economic growth might well be spurious. Addressing endogeneity concerns statistically has traditionally involved the identification and use of instrumental variables, but scholars are increasingly skeptical about whether such variables can be found (Bartels 1991; Bound *et al.* 1995). It is hard to imagine what factor might predict whether a country has a coalition government in a given year that is not also correlated with its economic conditions (potential candidates, for instance, might be constitutional rules, colonial legacy, or geographic region). Therefore, another strategy is called for. Increasingly scholars have turned to "matching" techniques as a means for addressing thorny endogeneity issues.[17]

As a robustness check against endogeneity, I conduct a basic matching analysis of the economic growth models.[18] I first limit the sample only to developing country parliamentary systems, and predict the likelihood a state had a coalition government in a given year on the basis of its Freedom House score of political and civil liberties, whether it

[16] Widner (2001) anticipates this finding in a fascinating account of how the rule of law is built from the perspective of the judicial actors at the center of the action.

[17] A full discussion of the potential virtues and limitations of matching is beyond the scope of this chapter, but interested readers would do well to consult the seminal works of eminent econometricians and statisticians such as James Heckman, Guido Imbens, Paul Rosenbaum, and Donald Rubin (Heckman *et al.* 1997, 1998; Imbens 2000; Rosenbaum and Rubin 1983, 1985; Rubin 1974, 1980; 2006).

[18] Matching was conducted using the `psmatch2` routine in Stata (Leuven and Sianesi 2003).

was a democracy, its GDP per capita, urban population growth, trade openness, terms of trade, unemployment rate, population growth, and whether the country experienced an economic crisis that year. This model yields a "propensity score" which is used to create a matched sample of country-years that experienced a coalition government (the treated sample in the jargon of matching) and country-years that did not, but whose propensity score was very similar to those that did. This latter set of country-years is thus essentially identical in terms of the covariate profile used to create the match (and a balance test for the covariates verifies this proposition),[19] but different only in that it did not experience the treatment of interest. Stated more plainly, these years look very similar in all respects to the treated sample except that they did not actually have a coalition government in power. This then is the control sample, and any difference in economic growth between it and the treated sample is more validly attributed to the whether there was a coalition government (the treatment). The results of this analysis are very encouraging: the average growth rate in the treated (coalition parliamentary government) sample was 3.16 percent, while the average growth rate in the control (unified parliamentary government) sample was 1.70 percent.

I next replicate this analysis but this time the treatment factor is divided presidential government. I limit the sample to developing-country presidential systems only, and utilize the same covariate profile as described above. A check reveals that satisfactory covariate balance is achieved, allowing one to turn to observed differences in economic growth between divided and unified presidential systems that have been matched on factors that might predict the existence of a divided government. The results confirm the negative finding reported above: divided presidential governments experienced lower economic growth (0.30 percent) than their unified counterparts (1.37 percent).[20]

[19] Balance is generally quite good across the covariate profile, but it is not perfect. The control sample after matching is slightly more democratic and richer, and less politically unstable, than the treated sample. If anything these differences should bias the findings against my argument, and so I consider this a conservative test.

[20] This result does not go away if one also matches on the country's region, specifically whether it is located in Latin America, though the difference is slightly muted with the control group's growth-rate dropping to 1.17 percent as compared to 0.30 percent for the treated group.

A statistical analysis using matching techniques thus assuages concerns that the results reported in this chapter have been driven by endogeneity. In fact, if anything, these results make the benefits of parliamentarism relative to presidentialism even clearer, and provide strong evidence in favor of the credible constraints argument. Of course, as with any statistical analysis, this is hardly the last word on the question of endogeneity, and future research should strive to build a unified model of economic performance, government formation, and investor reactions.

A final point for now: statistics are but one way to tackle endogeneity. In the case-study chapters that follow the large-*n* empirical chapters, I will try consciously to utilize the country experiences to gain leverage on the question of endogeneity. The Indian case is especially useful in this regard since it is clear there that the party system fragmentation and growth of regional parties that have made coalition governments a fact of modern Indian political life had precious little to do with economic conditions, and are far more the unintended consequence of internal Congress Party politics. But more on that later.

Conclusion

The central argument of this chapter is that governments' inability to make credible commitments to future and current policies, i.e., of policy stability, to investors, foreign and domestic, induces those investors to avoid long-term commitments in and to their investment projects and makes them, because of the nature of their investments and because of their lack of confidence and/or certainty regarding future government policy, more likely to abandon the country at smaller signs of economic trouble. Thus, in addition to the direct effect of policy instability on output instability, governments' inability to commit credibly to present and future policies, induces savings and investment volatility and, thereby, growth volatility.

My theory advances our understanding of growth-rate volatility by specifying the institutions and economic policies that cause growth-rate volatility, thereby improving on the three existing political explanations of growth-rate volatility, each of which focuses on different roles democracy plays in mitigating volatility. First, Rodrik (1998a, 2000) argues that democracies exhibit higher levels of social cooperation and compromise in the face of exogenous shocks, which allows them to

navigate and ameliorate the effects of these shocks. Next, Quinn and Woolley (2001) argue that risk-averse publics are able to constrain their more risk-acceptant leaders in democracies from making risky policies, which they argue are the cause of growth volatility. Finally, Chandra and Rudra (2008) argues that institutional diversification, which he associates with democracy, leads to less volatile policy outputs and therefore lower volatility.

Empirically, the credible constraints framework is shown to be quite powerful. In particular, the statistical analysis highlights coalition or minority parliamentary governments as a potent credible constraint that reduces growth volatility while increasing average growth overall. Divided presidential government, conversely, hurts national economic performance, presumably because policy gridlock hurts governments' ability to enact necessary economic reforms. Central bank independence lowers growth-rate volatility, though it has no discernible effect on average growth. Finally, judicial independence is found to explain variation in growth patterns in Africa, a finding that is particularly exciting given the severity of the challenges to good economic performance there.

While the evidence supports my argument, the causal mechanisms by which credible constraints reduce volatility and improve growth outcomes have thus far only been posited, rather than tested. The next chapter therefore provides direct tests of the causal mechanism implied by the theory, namely that, by reducing actual and expected policy instability, credible constraint institutions strengthen investor confidence, thereby encouraging new economic activity, promoting domestic saving, and reducing capital flight.