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# 3 The Seizure of Executive Power and Economic Growth: Some Additional Evidence

John Londregan and
Keith Poole

Despite the euphoric wave of democratization that swept the world in 1989, the prevalence of nonconstitutional and coercive rule remains a basic feature of world politics. Although nonconstitutional rulers sometimes gain power legally and then subvert the system that brought them in, as did Hitler, it is more common for them to directly seize the reigns of power by the use or threat of force in a coup d'etat. Because coups are a primary means by which countries become afflicted by coercive rule, a systematic analysis of the determinants and consequences of coups is of more than intrinsic interest.

In earlier work (Londregan and Poole 1990) we assembled a large cross-national data set matching annual data on the incidence of coups and other political events with annual economic time series. In this analysis we review the methods used in that earlier study and compare them with fresh results obtained using a new, and much richer, set of data on leadership change (Bienen and Van De Walle 1990). These results confirm and strengthen our earlier findings. However, using leader-specific data now enables us to learn more about the effects of postcoup rule, which we find inhibits economic growth.

There are substantial discrepancies between the coup counts used in our earlier work, from Jodice and Taylor's (1983) *World Handbook of Political and Social Indicators III* (hereafter referred to as the *World Handbook*), and the numbers we derive from the Bienen and Van De Walle codings. Bienen and Van De Walle employ very conservative criteria when coding for nonconstitutional rule, so that their "nonconstitutional" rulers are a relatively homogeneous group. In contrast, the *World Handbook* coded a number of questionable regime transfers that nevertheless contained elements of legitimacy, such as de Gaulle's 1958 accession to power in France, as "irregular transfers of executive power"; that is, as coups. The Bienen and Van De Walle data have the advantage of reporting leader-specific variables, as well as "event counts" for the country as a whole, enabling us, for example, to observe a leader's constitutional status directly. An additional point in favor of the Bienen and Van De Walle data set is the considerable care taken in its assembly. The Bienen and Van De Walle data set drew on a wider set of sources than the *World Handbook* and was directly coded by the authors, whereas much of the *World Handbook's* coding was delegated to research assistants.[1]

Differences between the two data sets notwithstanding, the substantive results uncovered using the Handbook data are robust to the use of the Bienen and Van De Walle data. Our findings on the fresh data set corroborate the contention (Luttwak 1969 and Finer 1962) that coups are a feature of poverty: they almost never occur in developed countries, but they are commonplace among the poorest nations. We also confirm the finding of a "coup trap": the political culture of a country suffers serious erosion in the wake of a coup d'etat; once the ice is broken more coups follow (Finer 1962). Opposition groups apparently respond to the forceful seizure of executive power with a more ruthless willingness to resort to the same means (Blondel 1980), and the involvement of the military in politics creates a praetorian political climate that fosters further coups (Huntington 1968).

Using the Bienen and Van De Walle leader-specific data, we are now able to address some questions about the nature of the "coup trap." Is it simply the case that nonconstitutional rulers are at a heightened risk of a coup, so that he who lives by the coup dies by the coup, or do the aftereffects of a coup continue to taint a country's politics even after the coup leader has himself lost power? Either of these hypotheses could have led to our earlier findings that a past history of coups affects a country's current probability of a coup. Leader-specific data enable us to directly test whether countries that have suffered past experience with coups are more coup prone even after controlling for the current leader's constitutional status. We find that they are.

Knowing leaders' constitutional status also enables us to separate the economic effects of nonconstitutional rule from the potential economic disruption caused directly by coups. We confirm our earlier result that a country's coup history, as summarized by the number of coups occurring during the most recent six years, and the number of coups occuring in the more distant past, does not affect economic growth. However, nonconstitutional rule *does* slow the pace of economic growth. Our estimates indicate that nonconstitutional rule (as coded by Bienen and Van De Walle) costs about half a percentage point of growth per year. While coups themselves



are not damaging to growth, our estimates indicate that postcoup despotism is.

Our analysis also affords an opportunity for a methodological comparison of the robust bootstrap calculation of standard errors in our earlier work with standard errors calculated according to the more conventional delta ($\delta$) method. The bootstrap method is a resampling procedure that provides robust estimates of standard errors (Efron 1979). It involves constructing multiple pseudosamples by drawing observations from the actual sample with replacement. The distribution of parameter estimates among pseudosamples is then used as an estimate of the probability distribution of the actual parameter estimates. In contrast, the delta method uses the analytical asymptotic variance-covariance matrix of the parameter estimates, with estimated parameters used in place of the true, but unknown, parameter values. Although the $\delta$ method is less robust to specification errors, it can be calculated directly from the parameter estimates without need for resampling. In the context of the coup data, both methods yield very similar standard error estimates; the main difference is the substantially greater time required to calculate the bootstrap estimates.

The outline of the chapter is as follows: section 3.1 compares the Bienen and Van De Walle data set with the coup data available from the *World Handbook of Political and Social Indicators*. In section 3.2, we estimate a simultaneous equations model of coups and economic growth, analyze the robustness of our results across the two data sets, and check their sensitivity to the use of bootstrapped vs. conventional standard errors. Section 3.3 incorporates leader-specific information into the analysis—extending our results on the effect of political variables on the economy and on the nature of the "coup trap." We conclude in section 3.4.

## 3.1 The Data

We use Summers and Heston's annual economic data, which cover 130 countries during the interval 1950–1985 (Summers and Heston 1988). To measure income we use real GDP per capita in constant 1980 U.S. dollars. The issues raised by comparing incomes between countries and across time are not trivial (Lucas 1988). However, the data we use were compiled with painstaking sensitivity to differences in consumption patterns, both among sectors of a given country's economy and among different countries.

In our earlier work, we used political data from *The World Handbook of Political and Social Indicators*. The *World Handbook* provides data on political activity at an annual level for 148 countries during the period 1948–1982.

These data include counts of riots, elections, political executions, deaths from domestic political violence, successful irregular transfers of executive power (that is, successful coups), and unsuccessful irregular transfers of executive power (failed coups).

Our analysis draws on newly available leadership data from Bienen and Van De Walle (1990), who catalogue individual characteristics for 2258 modern leaders. These data include some straightforward variables, such as year of entry, age at entry, and number of years in power, and also some qualitative variables that reflect the judgments of the compilers. This second set of variables includes a dichotomous classification of leaders' means of gaining executive power as either nonconstitutional, if they gained power outside the framework of established and regular procedures, or constitutional (Bienen and Van De Walle, pp. 21, 28).

The nonconstitutional rule variable dichotomizes what is in principle a continuous variable. On one end of this continuum we might put Eyadema of Togo, who is said to have murdered the fleeing President Olympio as he tried to reach a foreign embassy during a coup, or Uganda's Idi Amin Dada, who seized power while his predecessor Milton Obote was traveling abroad. At the other extreme we could place the likes of George Bush, who after a long "probationary period" of public service was nominated by a major political party and came to power during a regularly scheduled competitive election.

But many "intermediate" cases have elements of both coercion and constitutionality. Argentina's Frondizi came to power in 1958 with the grudging acquiesence of the junta headed by Aramburu, but after having spent a long career as a civilian politician. Frondizi was hardly a textbook example of a constitutional ruler, yet he clearly had more institutional backing than Eyadema or Amin. An ambiguous case is that of de Gaulle's 1958 rise to power, which was widely popular, and yet took place against a background of military pressure that forced de Gaulle's predecessor, Pfimlin, from office. Moving most, but not all, of the way toward the constitutional end of the spectrum, consider the 1974 electoral defeat of Heath by Wilson, an early election that Heath is generally acknowledged to have called under the pressure of a miners' strike (Blondel 1980). Although the accession of Wilson to the prime ministership took place within the framework of electoral politics, the coercive pressure of the miners' strike did influence the timing of the election so that the succession was not entirely free from extraconstitutional pressure.

The coding of nonconstitutional rule was conservative, accession to power within constitutional frameworks of questionable legitimacy, such as



the ascendence of Generals Roberto Viola and subsequently Leopoldo Galtieri of Argentina, who assumed power under the rubric of a constitution imposed by a military junta, are nevertheless coded as constitutional. Because of this conservatism, the nonconstitutional rulers in this sample are a relatively homogeneous sample of illegal entrants, and the constitutional rulers are more eclectic, ranging from rulers who were brought in with the merest trappings of a showcase constitution to leaders of competitive, multiparty parliamentary governments.

The nonconstitutional entry variable thus identifies a relatively homogeneous group of leaders at the coercive end of the constitutional spectrum—the likes of Eyadema and Amin—and leaves all of the others, from Frondizi to Bush, in a residual class labeled "constitutional." Further coding could profitably identify parliamentary regimes, a relatively homogeneous group at the constitutional end of the spectrum.

Other qualitative variables in the Bienen and Van De Walle data set include a characterization of leaders' exits. The coding distinguishes leaders who lost power constitutionally; those who left office nonconstitutionally, either through a politically motivated assassination (John Kennedy is included in this group) or a coup (e.g., Chile's Allende); and leaders who died in office from "nonpolitical" causes (e.g., Franklin Roosevelt). We count as a coup d'etat a case in which a leader lost power by nonconstitutional means, *and* his apparent and immediate successor arrived in power by nonconstitutional means. This is not a variable that Bienen and Van De Walle code for directly, but rather one that we construct from the entry and exit mode codings.

Although the economic data and the data from the *World Handbook* are both available on an annual basis, the leadership data reports the sequence of leaders, including a number who remain in power for less than a year. A further complication is that the Bienen and Van De Walle codings are not a continuous record of the exercise of executive power—interim leaders, and interregnum periods are excluded, as are periods of "shared rule" as in Uruguay between 1951 and 1958, or Yugoslavia after 1978.

There are several years in our sample for which there are multiple rulers, for example, during 1979 Bolivia had a very heterogeneous sequence of five leaders.[2] To integrate leader-specific traits with measures of country-level variables, we must develop some systematic rules for dealing with country/years with multiple leaders, such as Bolivia in 1979. We adopt the rule of matching each annual observation with the traits of the first leader to hold power during that year. Selecting any subsequent leader could, under some circumstances, lead to no exit being coded for that year, whereas our method guarantees that an exit is always coded for years with multiple leaders. Other alternatives included averaging leader characteristics (raising questions about the interpretation of average values of qualitative variables), and creating multiple records for years with multiple leaders (creating a sample that overrepresents years with leadership turnover). Our choice of assigning the traits of the first leader of the year comes at the cost of allowing some short-term leaders to fall between the cracks: a leader who ruled from January to December of the same year would not be counted.

Because the leadership data do not code for caretaker governments, and because leaders' durations in power are coded as integer values, it is possible that our reconstructed series will erroneously attribute interregnum periods to the preceding leader. A leader who comes to power in 1967 and has a length of time in power of two years may have left office in either 1969 or 1970. If the subsequent leader came to power in 1970, we cannot tell from the leader codings whether that next leader succeeded directly, or after one or more caretakers, with the initial leader leaving in 1969.

Rather than retrace the leader codings to search for caretaker governments, we treat any order of succession that *could* have occurred without an interim caretaker as though it *did*. Thus, a leader who came to power in 1967 and remained in power two years, who is followed by another leader who arrives in 1970, is treated as the head of state at the beginning of 1970 because his term *could* have lasted until his successor's time in power began. If instead his time in power had been one year, then there is clearly an interregnum period between the 1967 leader and the 1970 leader. In this case, we treat the leader as though he held power until 1968.[3] Our analysis of the sample identified several cases that must have involved gaps between leadership spells.[4] Our sample omits these missing years and also drops two intervals of contested multiple leadership.[5]

Although the leadership traits of only the country/year's first leader are included, we count all coups occurring during the year, whether they were staged against the year's first leader or not. This enables us to construct the coup history for these countries as well as to conduct a more careful robustness analysis of our earlier work that was based on annual coup counts.[6]

In our previous work, we matched data from the *World Handbook* with Summers and Heston's economic data. Because of the ambiguity and unreliability of economic data from the centrally planned economies, we omitted these states from the matched data set. This left us with 3,035 observations on 121 countries during the span 1950 through 1982. In recognition of the serial dependence of GDP growth, we then calculated the growth rate and



the lagged growth rate, taking first differences in the log of the level of real GDP per capita (measured in 1980 U.S. dollars) leaving us with 2797 observations on 121 countries over the interval 1952 through 1982 (the first two years of the sample being lost in the calculation of current and lagged GDP growth rates). Not all countries contributed the same number of observations to the data set. At one extreme, there is only one post-independence observation for Zimbabwe, whereas other countries, such as the United Kingdom, contributed thirty-one observations, spanning the entire interval from 1952 through 1982.

The matched data set permits us to readily compare the *World Handbook*'s coup counts with those derived from Bienen and Van De Walle's leader codings. The two sets of data are by no means in close accord. The *World Handbook* counts 144 country/years with at least one coup, while the leadership data implies 123 country/years with at least one coup. The reasons for these discrepancies are various. In some cases the *World Handbook* counted transfers of power via what were probably sham constitutions as coups, but the more conservative coding rule of Bienen and Van De Walle did not. Although a case can be made for counting the leaders who gained power by such means as nonconstitutional (imposing a less strict threshold for nonconstitutional rule), it would not be appropriate to count the transfer of power, typically acknowledged as proper by the exiting leader, as nonconstitutional.[7] Such transfers, though unpalatable to proponents of democratic institutions, are not coups. In other cases, it appears that the *World Handbook* and Bienen and Van De Walle disagree about the year in which a coup occurred.[8]

Some discrepancies were more complex and reflect not only the conservatism of the Bienen and Van De Walle codings but also the difference in their emphasis, which is the duration of leadership, rather than coups directly. For example, on October 28, 1963, Col. Soglo seized power from Benin's President Maga. However, he then proceeded to set up an interim government whose cabinet consisted of the country's three leading civilian politicians, including Maga. Soglo announced that the government was provisional. Elections were held in January, 1964, and convincingly won by Sourou-Migan Apithy, who assumed the office of president. Soglo stepped down to resume his duties as army chief of staff. Bienen and Van De Walle code this as nonconstitutional exit by Maga, followed by constitutional entry for Apithy, with Soglo's three-month sojourn in power counted as a period of interim rule (unlike his postcoup rule beginning in 1965). The *World Handbook* codes this as a successful irregular transfer of executive power (i.e., coup). In this case, both sets of codings appear to be right.

The conservative coup-counting rule we have adopted—counting as coups only cases of nonconstitutional exit followed by nonconstitutional entry by the successor—misses several coups, as in the case of the 1963 coup d'etat in Benin; however, it is very resistant to falsely counting a transfer as a coup. A further argument in favor of this approach is the extra care taken by Bienen and Van De Walle in coding entry and exit dates. They coded leadership spells directly from country-level histories using country and regional biographical indexes, news summary sources, and interviews with area experts as supplements. The primary sources for the *World Handbook* were news summaries, such as the *New York Times Index* and *Keesings*, and much of the *World Handbook*'s coding was delegated to research assistants. In no case have we found the Bienen and Van De Walle identification of a transition as nonconstitutional exit followed by nonconstitutional entry to be incorrect, but we have identified several factual errors in the *World Handbook*'s coup codings.

## 3.2 A Parametric Model of Coups and Income Growth

In earlier work (Londregan and Poole 1990) we estimated a simultaneous model of income growth and coups using matched data from the *World Handbook* and Summers and Heston, described in the previous section. In the reduced form of this model, income growth potentially depends on lagged income, lagged income growth (as in Barro 1989), region-specific effects, and the countries' past experience with coups d'etat. Let $y_{it}$ denote the natural log of per capita GDP during year $t$ in country $i$, while we define $\Delta y_{it}$ as

$$\Delta y_{it} \equiv y_{it} - y_{it-1},$$

which is approximately the real GDP growth rate during year $t$ in country $i$. We let $c_{it}$ denote the number of coups d'etat occurring in country $i$ during year $t$. Finally, our previous work used Summers and Heston's regions—Africa, Asia, Europe and North America, Central America and the Carribean, South America, and Oceania (they also treat the centrally planned economies as a separate class, but we did not incorporate any of these countries into our analysis). We used region-specific indicator variables for country locations of the form $r_{ij}$, where $r_{ij} = 1$ if country $i$ is located in region $j$, and 0 otherwise. We subsequently discarded several regions and combined the United States and Canada with the European countries. This left us with Africa, South America, and a region we labeled North America and Europe, although it did not include Mexico.[9]



The income growth model we estimated is of the form:

$$\Delta y_{it} = \pi_{10} + \pi_{11}\left(\sum_{s=1}^{6} c_{it-s}\right) + \pi_{12}\left(\sum_{s=7}^{\infty} c_{it-s}\right) + \pi_{13} y_{it-1} + \pi_{14}\Delta y_{it-1} + \sum_{j=5}^{7} \pi_{1j} r_{ij} + \varepsilon_{it}. \tag{1}$$

The random error term from this reduced-form growth equation, $\varepsilon_{it}$, is potentially correlated with the occurrence of a coup d'etat.

The second element of our model is an equation explaining the occurrence of coups. We let $z_{it}$ denote the latent "propensity for a coup," and let $\delta_{it}$ code dichotomously for the occurrences of coups: $\delta_{it} = 1$ if there is at least one coup in country $i$ during year $t$, and it equals zero otherwise. We assume that coup occurrence and coup propensity are linked by the crossing of a threshold: if $z_{it} < 0$, then $\delta_{it} = 1$, while for $z_{it} \geqslant 0$, $\delta_{it} = 0$. This is a standard probit model applied to coups, except that we allow shocks to the latent coup equation, $\eta_{it}$, to be correlated with shocks to economic growth. In our earlier work, we estimated a model in which the coup propensity depends on only predetermined variables[10]

$$z_{it}^{*} = \pi_{20} + \pi_{21}\left(\sum_{s=1}^{6} c_{it-s}\right) + \pi_{22}\left(\sum_{s=7}^{\infty} c_{it-s}\right) + \pi_{23} y_{it-1} + \pi_{24}\Delta y_{it-1} + \sum_{j=5}^{7} \pi_{2j} r_{ij} - \eta_{it}. \tag{2}$$

As with other probitlike models, the variance of $\eta_{it}$ and the coefficients of the coup equation are only identified up to a scale factor. To pin these estimates down, we adopt the arbitrary, but standard, normalizing restriction that the variance of $\eta_{it}$ in equation (2) equals 1. This leaves us with two parameters of the variance-covariance matrix to estimate: the variance of $\varepsilon_{it}$, which we denote $\sigma^2$, and the correlation between $\eta_{it}$ and $\varepsilon_{it}$, which we denote $\rho$.

Notice that the coup equation resembles empirical models of economic voting in U.S. presidential elections (see Fair 1978, Erikson 1989). However, these models typically use election year growth as their economic performance variable. It has been claimed that growth during the two quarters immediately preceding the election is a sufficient statistic for economic performance (Fair 1978; see also Rosenstone 1983). While quarterly data permits the use of lagged (and thus, in the context of our model, predetermined) growth information from as few as five weeks before the presidential election, our use of the previous year's growth rate leaves us with information that potentially predates a coup by as much as twenty-three months, and by an average of just under eighteen months.

An alternative specification of the coup equation is to include contemporaneous growth and use lagged growth as an instrument. This results in a model that is just identified, and so we cannot test the restriction. It seems reasonable that, to the extent that growth rates affect the propensity for a coup, current growth would matter more than lagged growth. However, although our exclusion restriction seems sensible, we must interpret our results with the caveat that if both current and lagged growth exert independent influences on the coup propensity (as they do not appear to do for U.S. presidential voting (Fair 1978)), our model will be misspecified.

The specification of the coup equation with current growth is of the form

$$z_{it}^* = \gamma_2 \Delta y_{it} + \alpha_{20} + \alpha_{21}\left(\sum_{s=1}^{6} c_{it-s}\right) + \alpha_{22}\left(\sum_{s=7}^{\infty} c_{it-s}\right) + \alpha_{23} y_{it-1} + \sum_{j=5}^{8} \alpha_{2j} r_{ij} - \eta_{it} \tag{2'}$$

The parameter estimates we obtain from this procedure provide us with a picture of the relative effects of past coups, economic growth, and the level of income as "risk factors" for a coup.

We can rewrite this model more compactly as:

$$y_{it} = \underline{x}_{it}' \underline{\pi}_1 + \varepsilon_{it} \tag{1a}$$

$$z_{it}^* = \underline{x}_{it}' \underline{\pi}_2 - \eta_{it} \tag{2b}$$

where $\underline{x}_{it}$ denotes the column-vector of explanatory variables, which is the same for both equations

$$\underline{x}_{it} = \left(1,\ \sum_{s=1}^{6} c_{it-s},\ \sum_{s=7}^{\infty} c_{it-s},\ y_{it-1},\ \Delta y_{it-1},\ \{r_{ij}\}_{j=5}^{8}\right)'$$

The likelihood function for our model is

$$\begin{aligned} l(\underline{\pi}_1, \underline{\pi}_2, \rho, \sigma) &= \sum_{\text{Coups}} ln(\Phi((\underline{x}_{it}'[\underline{\pi}_2 + (\rho/\sigma)\underline{\pi}_1] - (\rho/\sigma)y_{it}) \cdot (1 - \rho^2)^{-1/2})) \\ &+ \sum_{\text{Noncoups}} ln(\Phi((-\underline{x}_{it}'[\underline{\pi}_2 + (\rho/\sigma)\underline{\pi}_1] + (\rho/\sigma)y_{it}) \cdot (1 - \rho^2)^{-1/2})) \\ &- \left(\frac{1}{2\sigma^2}\right)\sum (y_{it} - \underline{x}_{it}'\underline{\pi}_1)^2 - \frac{N}{2} ln(2\pi) - n ln\sigma \end{aligned}$$



We first replicate the full information maximum likelihood (FIML) estimates of the reduced-form model of equations (1) and (2) from our earlier work. The estimation algorithm we use exploits the special structure of our model, converges very quickly (in about forty seconds using Gauss386 computer software on an IBM PS70 machine[11]), and may be of practical interest to applied econometricians and data analysts.[12]

In our earlier work we calculated standard errors using Efron's bootstrap technique, a resampling procedure, with 1,024 replications. With a fresh draw of 1,024 resamples the estimated standard errors will change slightly, but the parameter estimates themselves, and the value of the likelihood function, remain exactly as reported in our earlier work. These estimates are reported in column 1 of table 3.1, and the newly calculated bootstrap standard errors are reported in column 2.

We also calculate the variance-covariance matrix of our parameter estimates by the $\delta$ method, using the inverse Hessian of the likelihood function. These standard errors are reported in column 3 of table 3.1. For reference, we report the bootstrap estimates of the standard errors reported in our earlier work in column 4. Comparison of the competing estimates of the standard errors reveals that they are nearly identical;[13] the differences between the estimates obtained by the $\delta$-method and the bootstrap estimates are of the same order of magnitude as the differences between the two sets of bootstrap estimates. This suggests that, in the context of this model and these data, there is little reason to expend the extra effort of calculating the bootstrap estimates. Using our computer system, the $\delta$ method estimates standard errors along with the other parameters of our model in forty seconds, the bootstrap (with 1,024 replications) requires over ten hours, and even with only 64 replications, almost three-quarters of an hour would be required.

While using 1,024 bootstrap replications is a cumbersome procedure, smaller numbers may suffice for the purpose of preliminary data analysis. A sensible exploratory analysis of a parametric model such as ours on a new data set might include an initial bootstrap estimation with only 32 or 64 replications. The bootstrap standard errors could then be compared with those generated by the method. If no notable discrepancies were detected, then the further application of the bootstrap could be abandoned in lieu of the less time consuming method. Otherwise, further iteration of the bootstrap would be in order given its more reliable convergence to the underlying distribution of the parameter estimates (see, for example, Efron, 1979). A contingent use of the bootstrap only when the $\delta$ method yields notably different results than a small scale application of the bootstrap makes

**Table 3.1**
Joint maximum likelihood estimation of the reduced form (using coup counts from *World Handbook*)

| | 1 | 2 | 3 | 4 |
|---|---|---|---|---|
| *Growth equation* | | | | |
| Constant | 0.0758 | 0.0134 | 0.0112 | 0.013 |
| Coups occurring during the previous six years | 0.0007 | 0.0015 | 0.0014 | 0.0016 |
| Coups occurring more than six years earlier | −0.0032 | 0.0015 | 0.0009 | 0.0016 |
| Log of the previous year's per capita GDP | −0.0072 | 0.0018 | 0.0014 | 0.002 |
| The previous year's per capita GDP growth rate | 0.1596 | 0.0311 | 0.0176 | 0.032 |
| Africa | −0.0174 | 0.0034 | 0.0031 | 0.003 |
| Europe and North America | 0.0131 | 0.0032 | 0.0032 | 0.003 |
| South America | −0.0027 | 0.0039 | 0.0038 | 0.004 |
| *Coup equation* | | | | |
| Constant | 0.8671 | 0.4529 | 0.4953 | 0.427 |
| Coups occurring during the previous six years | 0.1835 | 0.0431 | 0.0435 | 0.043 |
| Coups occurring more than six years earlier | 0.0408 | 0.0321 | 0.0321 | 0.032 |
| Log of the previous year's per capita GDP | −0.3675 | 0.0628 | 0.0698 | 0.061 |
| The previous year's per capita GDP growth rate | −1.1014 | 0.7744 | 0.6812 | 0.743 |
| Africa | −0.1839 | 0.1161 | 0.1143 | 0.111 |
| Europe and North America | −0.0337 | 0.1897 | 0.1751 | 0.001 |
| South America | 0.5392 | 0.1273 | 0.1273 | 0.131 |
| $\rho$ | −0.1322 | 0.0437 | 0.0371 | 0.045 |
| $\delta$ | 0.0571 | | | |
| Log of the likelihood function: | 3,533.0828 | | | |
| Number of observations: | 2,797 | | | |
| Number of bootstrap replications: | 1,024 | | | |

Column 1: Parameter estimate.
Column 2: Bootstrap standard errors, calculated from a fresh pseudosample.
Column 3: Standard errors calculated by the $\delta$ method.
Column 4: Bootstrap standard errors from Londregan and Poole (1990).

particular sense in case of more elaborate likelihood functions with less tractable convergence properties.

As in our earlier work, we proceed to test the direction of feedback between coups and income growth. This amounts to imposing various exclusion restrictions on our model. Can we omit the coup variables from the growth equation? Can we eliminate the growth and income variables from the coup equation? These are essentially tests of Granger Causality.

We first turn to the question of whether we can exclude the past history of coups from the growth equation, that is, do coups "Granger cause" growth? More formally, we test (with respect to equation (1))

$$H_0\colon \pi_{11} = \pi_{12} = 0.$$



Rather than reestimate the entire model with these coefficients excluded, we conduct an asymptotically equivalent test that calculates the optimal minimum distance (OMD) estimate of the constrained model from the unconstrained coefficient estimates. Let $\hat{\underline{\alpha}}$ denote the constrained coefficient estimates. In the context of the hypothesis that lagged coups do not affect the current rate of growth, $\pi_{11} = \pi_{12} = 0$, the constrained model becomes

$$\Delta y_{it} = \alpha_{10} + \alpha_{13} y_{it-1} + \alpha_{14} \Delta y_{it-1} + \sum_{j=5}^{7} \alpha_{1j} r_{ij} + \varepsilon_{it} \qquad (1'')$$

$$z_{it}^{*} = \alpha_{20} + \alpha_{21}\left(\sum_{s=1}^{6} c_{it-s}\right) + \alpha_{22}\left(\sum_{s=7}^{\infty} c_{it-s}\right) + \alpha_{23} y_{it-1} + \alpha_{24} \Delta y_{it-1} + \sum_{j=5}^{7} \alpha_{2j} r_{ij} - \eta_{it} \qquad (2'')$$

We adopt the general notation $\underline{\pi}(\underline{\alpha})$ for the set of reduced form coefficients that correspond to the vector $\underline{\alpha}$ of structural parameters. We estimate $\hat{\underline{\alpha}}$ using the OMD technique (Rothemberg 1973), which chooses $\hat{\underline{\alpha}}$ as the solution to the following minimization:

$$\operatorname*{Min}_{\underline{\alpha}}\, (\hat{\underline{\pi}} - \underline{\pi}(\underline{\alpha}))' \Delta_\pi^{-1} (\hat{\underline{\pi}} - \underline{\pi}(\underline{\alpha}))$$

where $\Delta_\pi^{-1}$ denotes the variance-covariance matrix of $\hat{\underline{\pi}}$. The solution to this minimization is asymptotically equivalent to the maximum likelihood estimate of the constrained parameter vector. The asymptotic variance-covariance matrix of $\hat{\underline{\alpha}}$ is given by

$$V(\hat{\underline{\alpha}}) = \nabla_\alpha \underline{\pi}(\underline{\alpha})' \Delta_\pi^{-1} \nabla_\alpha \underline{\pi}(\underline{\alpha})$$

where we denote the Jacobian of a vector valued function $f$ with respect to its arguments, $\underline{x}$, by $\nabla_x f(\underline{x})$. Under the null hypothesis, the minimized value of the objective function for the OMD,

$$\chi^2(\hat{\underline{\alpha}}) = (\hat{\underline{\pi}} - \underline{\pi}(\hat{\underline{\alpha}}))' \Delta_\pi^{-1} (\hat{\underline{\pi}} - \underline{\pi}(\hat{\underline{\alpha}}),$$

is asymptotically distributed according to an $\chi^2$ distribution with $k$ degrees of freedom, where $k$ is the number of linear restrictions imposed by $\underline{\alpha}$ on the vector of reduced-form coefficients, $\hat{\underline{\pi}}$, which in this case is 2.

These estimates are reported in columns 1 and 2 of table 3.2.[14] The value of the criterion function is 4.534, corresponding to a p-value of 0.103, indicating acceptance at the $\alpha = 0.05$ significance level. Both the parameter estimates and the value of the test statistic echo our earlier finding that a country's coup history does not affect the growth rate. However, using the

**Table 3.2**
Simultaneous estimation (using coup counts from *World Handbook*)

| | 1 | 2 | 3 | 4 |
|---|---|---|---|---|
| *Growth equation* | | | | |
| Constant | 0.0672 | 0.0089 | 0.0731 | 0.0078 |
| Coups occurring during the previous six years | * | * | * | * |
| Coups occurring more than six years earlier | * | * | * | * |
| Log of the previous year's per capita GDP | −0.0061 | 0.0012 | −0.0071 | 0.0010 |
| The previous year's per capita GDP growth rate | 0.1478 | 0.0216 | 0.1623 | 0.0124 |
| Africa | −0.0172 | 0.0024 | −0.0170 | 0.0022 |
| Europe and North America | 0.0124 | 0.0022 | 0.0144 | 0.0022 |
| South America | −0.0062 | 0.0025 | −0.0043 | 0.0026 |
| *Coup equation* | | | | |
| Constant | 0.7597 | 0.3182 | 0.8703 | 0.3503 |
| Coups occurring during the previous six years | 0.1907 | 0.0303 | 0.1843 | 0.0307 |
| Coups occurring more than six years earlier | 0.0350 | 0.0227 | 0.0370 | 0.0227 |
| Log of the previous year's per capita GDP | −0.3518 | 0.0441 | −0.3676 | 0.0493 |
| The previous year's per capita GDP growth rate | −1.2057 | 0.5458 | −1.1046 | 0.4819 |
| Africa | −0.1817 | 0.0821 | −0.1842 | 0.0808 |
| Europe and North America | −0.0673 | 0.1336 | −0.0352 | 0.1239 |
| South America | 0.5397 | 0.0900 | 0.5411 | 0.0900 |

Column 1: OMD parameter estimates using the bootstrap covariance matrix.
Column 2: Standard errors based on the bootstrap OMD estimate.
Column 3: OMD parameter estimates via the $\delta$ method covariance matrix.
Column 4: Standard errors based on the $\delta$ method covariance matrix.

$\delta$ method estimate of $\Delta_\pi$ yields very similar parameter estimates, reported in columns 3 and 4 of table 3.2, but a considerably larger value of the test statistic. Using the $\delta$ method, we instead obtain a test statistic of 11.653, with a corresponding p-value of 0.003, indicating rejection at all conventional levels. Our answer to whether coups (as coded by the *World Handbook*) inhibit growth depends on the fairly esoteric question of which variance-covariance matrix estimator to use: the bootstrap or the $\delta$-method.

We also test for the impact of the economy on coups. More formally, we test whether the coefficients of lagged income and lagged growth are simultaneously equal to zero in the coup equation. Using the same methodology as employed in the test of the hypothesis that past coups do not affect the economy, we obtain a test statistic of 34.308 using the bootstrap method, and 31.765 via the $\delta$-method. Under the null hypothesis, each test statistic has an $\chi^2$ distribution with two degrees of freedom. Both test statistics indicate rejection of the null at all standard levels of significance.



The notable discrepancies between the coup counts derived from the Bienen and Van De Walle data and the counts reported by the *World Handbook* raise a serious question about the dependence of our results on the *World Handbook* data. To assess the robustness of our conclusions, we reestimate our model using the Bienen and Van De Walle coup counts. We also adopt their regional definitions, rather than the modified Summers and Heston regions we used in the coup paper. Countries within these regions have been argued to be relatively homogeneous with respect to national unity and political culture (Blondel 1980, pp. 29, 30). The primary change here is the creation of the Middle East as a region distinct from Africa and Asia, and the inclusion of South and Central America under the common heading of Latin America.

With these region definitions, the bootstrap coefficient estimates become somewhat problematic: there are only two coups d'etat, as derived from the Bienen and Van De Walle codings, in the region labeled North America-Europe-Australasia.[15] This implies that in the course of resampling, approximately 13 percent of the pseudosamples drawn by the bootstrap procedure will contain *no* coups for this region, leading to nonconvergence of the probit estimates. For these samples, location in North America-Europe-Australasia will be treated by the probit as making coups "impossible," that is, the algorithm will attempt to assign the coefficient of the indicator variable for this region a value of "$-\infty$."

To cope with nonconvergent pseudosamples, the bootstrap algorithm was modified to omit pseudosamples in which North America-Europe-Australasia was "coup-free," and then the standard errors were calibrated for the remaining subset of the 1,024 bootstrap replications. Thus the bootstrap variance-covariance matrix in this setting is conditional on the occurrence of at least one coup in North America-Europe-Australasia. However, as with the *World Handbook* coup codings, the results for this region are very similar to those generated by the $\delta$ method.

Using the Bienen and Van De Walle coup codings, we reestimate the model, with the region definitions suggested by Blondel, but otherwise preserving the list of explanatory variables.[16] Column 1 of table 3.3 reports parameter estimates for this model. Standard errors calculated according to the $\delta$ method appear in column 2, while the bootstrap standard errors are listed in column 3. The parameter estimates of column 1 are very similar to the estimates based on the *World Handbook* data. The small but statistically significant effect of coups on growth found in the *World Handbook* data is not present in the new estimates. This may in part be due to the differences in region definitions; coups occurring in the distant past may have proxied

**Table 3.3**
Joint maximum likelihood estimation of the reduced form (using coup counts derived from Bienen and Van de Walle)

| | 1 | 2 | 3 |
|---|---|---|---|
| *Growth equation* | | | |
| Constant | 0.0794 | 0.0112 | 0.0135 |
| Coups occurring during the previous six years | −0.0007 | 0.0015 | 0.0016 |
| Coups occurring more than six years earlier | −0.0008 | 0.0007 | 0.0009 |
| Log of the previous year's per capita GDP | −0.0074 | 0.0015 | 0.0018 |
| The previous year's per capita GDP growth rate | 0.1590 | 0.0176 | 0.0330 |
| Africa | −0.0232 | 0.0037 | 0.0038 |
| Middle East | −0.0031 | 0.0044 | 0.0043 |
| Latin America | −0.0063 | 0.0042 | 0.0040 |
| North America-Europe-Australasia | 0.0112 | 0.0045 | 0.0041 |
| *Coup equation* | | | |
| Constant | 0.0947 | 0.5261 | 0.4630 |
| Coups occurring during the previous six years | 0.1720 | 0.0452 | 0.0449 |
| Coups occurring more than six years earlier | 0.0558 | 0.0253 | 0.0256 |
| Log of the previous year's per capita GDP | −0.2853 | 0.0778 | 0.0663 |
| The previous year's per capita GDP growth rate | −1.5451 | 0.7148 | 0.7792 |
| Africa | −0.0222 | 0.1520 | 0.1555 |
| Middle East | 0.1817 | 0.1855 | 0.1914 |
| Latin America | 0.4537 | 0.1674 | 0.1667 |
| North America-Europe-Australasia | −0.3998 | 0.2984 | 0.2561 |
| $\hat{\rho}$ | −0.1209 | 0.0391 | 0.0442 |
| $\hat{\sigma}$ | 0.0571 | | |
| Log of the likelihood function: | 3,595.0269 | | |
| Number of observations: | 2,797 | | |
| Number of bootstrap replications: | 1,024 | | |

Column 1: Parameter estimate.
Column 2: Standard errors calculated by the $\delta$ method.
Column 3: Bootstrap standard errors, calculated from a fresh pseudosample.

for subsaharan Africa in the estimates reported in table 3.1, which aggregate the Maghreb with sub-Saharan Africa, despite the very different experiences of these regions with colonialism.

The effect of lagged growth reaches the threshold of statistical significance using the Bienen and Van De Walle data, while the coup-inhibiting effect of location in North America-Europe-Australasia is significantly negative, unlike the similarly defined Europe and North America variable in the earlier estimates. Aside from these small differences, the coefficient estimates obtained using the new data set are remarkably similar to those appearing in table 3.1. Even the estimates of $\rho$ are much the same: $-0.121$ using the new leadership data, $-0.132$ using the *World Handbook* data. Because of the slightly smaller event probability in the new sample, estimates based on the new data set need to be slightly larger to correspond to the same impact on the event probability as those in table 3.2.



The sign patterns of the two sets of coefficient estimates, with the exceptions noted above, are the same. It is also noteworthy that the bootstrap standard errors once again closely resemble those obtained by the $\delta$ method, with the one exception of the standard error of the lag coefficient in the growth equation, just as we found using the *World Handbook's* coup codings and region definitions.

The strong resemblance between the parameter estimates derived from the two data sets is a reassuring check on the robustness of the model. Why are the results so robust to what is apparently a substantial dose of measurement error? Probably because the "falsely positive" miscodings in the *World Handbook* tend to occur in countries and during years where the coup propensity was very high anyway—for example, the erroneous dating of the New Year's Eve 1965 coup by Bokassa as occurring in 1966, another year during which the Central African Republic and its leader were prone to a coup. In some of the ambiguous cases, such as the installation of Argentina's Frondizi in 1958, there were likewise many conditions favoring a coup. The propensity to code false positives was shaped by the same empirical regularities that are reflected in our coefficient estimates. In any event, the coup counts derived from Bienen and Van De Walle are, in our view, more reliable.

An important source of difference between the two data sets emerges when we repeat our Granger causality tests using this fresh data. The test of the joint restriction that real income growth is unaffected by the past history of coups is accepted at all standard significance levels using *either* the bootstrap *or* the $\delta$ method to calculate the variance-covariance matrix of the vector of reduced-form coefficients. Using the bootstrap, the test statistic, which is asymptotically distributed according to an $\chi^2$ distribution with two degrees of freedom, takes on a value of 0.988; using the $\delta$ method the corresponding statistic is 1.361. Both methods lead to the same conclusion: we accept the hypothesis that a country's growth rate is unaffected by its past experience with coups.

As discussed earlier, the coup equation bears a strong resemblance to U.S. presidential voting equations that include economic growth among their explanatory variables. However, to avoid simultaneity bias in our coefficient estimates, we must instrument contemporaneous growth on the right-hand side of our coup equation. Fair (1978) avoids simultaneity bias at low cost by exploiting quarterly data on U.S. growth, using quarters 2 and 3 of the U.S. presidential election year. Because U.S. presidential elections are always held in the fifth or sixth week of quarter 4, using lagged income growth leaves almost no slippage between the realization of the lagged (and thus predetermined) growth variable and the presidential election. However, using the preceding year's growth leaves a larger space between lagged growth and the current coup propensity.

An alternative version of our model holds that it is contemporaneous rather than lagged income that affects coups

$$\Delta y_{it} = \alpha_{10} + \alpha_{13} y_{it-1} + \alpha_{14} \Delta y_{it-1} + \sum_{j=5}^{7} \alpha_{1j} r_{ij} + \varepsilon_{it} \tag{1''}$$

$$z_{it}^{*} = \alpha^{*} \Delta y_{it} + \alpha_{20} + \alpha_{21}\left(\sum_{s=1}^{6} c_{it-s}\right) + \alpha_{22}\left(\sum_{s=7}^{\infty} c_{it-s}\right) + \alpha_{23} y_{it-1} + \sum_{j=5}^{7} \alpha_{2j} r_{ij} - \eta_{it} \tag{2''}$$

Exclusion of lagged growth from the coup equation identifies equation (2″), which we can then estimate using the OMD estimator set forth above. Starting with reduced-form coefficients corresponding to equations (1) and (2), we then recover the parameters of the model given by (1″) and (2″) using the OMD method. We simultaneously impose the additional restriction that coups do not affect economic growth. This leaves us with two overidentifying (and hence testable) restrictions of equation (1) and one identifying (and hence not testable) restriction for the coup equation.

The impact coefficient for current growth is informative in its own right—it calibrates the sensitivity of the coup propensity to each percentage point of growth. Estimates of the model using the Bienen and Van De Walle data, with the variance-covariance matrix calculated by the $\delta$ method, appear in column 1, table 3.4; asymptotic standard errors appear in column 2. We also present estimates using the same data, but estimate the variance-covariance matrix via the bootstrap in column 3, with associated standard errors in column 4.

Both sets of estimates are very similar. Notably, the effects of current growth on the coup propensity are large and statistically significant, although not precisely estimated. In our earlier work on coups we found that *lagged* growth had a large but statistically insignificant coup inhibiting effect. Our finding does not lend support to Olson's (1963) theory of the "revolution of rising expectations." While rapid growth may destabilize societies in other ways, it makes Bienen and Van De Walle coded coups *less*, rather than more, likely.



**Table 3.4**
Simultaneous estimation (using coup counts derived from Bienen and Van de Walle)

| | 1 | 2 | 3 | 4 |
|---|---|---|---|---|
| *Growth equation* | | | | |
| Constant | 0.0778 | 0.0079 | 0.0773 | 0.0094 |
| Coups occurring during the previous six years | * | * | * | * |
| Coups occurring more than six years earlier | * | * | * | * |
| Log of the previous year's per capita GDP | −0.0073 | 0.0011 | −0.0072 | 0.0013 |
| The previous year's per capita GDP growth rate | 0.1593 | 0.0125 | 0.1589 | 0.0229 |
| Africa | −0.0230 | 0.0026 | −0.0230 | 0.0027 |
| Middle East | −0.0035 | 0.0031 | −0.0029 | 0.0030 |
| Latin America | −0.0081 | 0.0027 | −0.0093 | 0.0024 |
| North America-Europe-Australasia | 0.0113 | 0.0032 | 0.0111 | 0.0029 |
| *Coup equation* | | | | |
| Constant | 0.8516 | 0.4526 | 0.8685 | 0.4285 |
| Coups occurring during the previous six years | 0.1711 | 0.0320 | 0.1671 | 0.0309 |
| Coups occurring more than six years earlier | 0.0550 | 0.0179 | 0.0524 | 0.0180 |
| Log of the previous year's per capita GDP | −0.3564 | 0.0597 | −0.3557 | 0.0519 |
| This year's per capita GDP growth rate | −9.7037 | 3.2912 | −10.2003 | 3.6387 |
| The previous year's per capita GDP growth rate | * | * | * | * |
| Africa | −0.2460 | 0.1390 | −0.2610 | 0.1433 |
| Middle East | 0.1475 | 0.1351 | 0.1429 | 0.1380 |
| Latin America | 0.3768 | 0.1244 | 0.3698 | 0.1262 |
| North America-Europe-Australasia | −0.2900 | 0.2164 | −0.3053 | 0.1807 |

Column 1: OMD parameter estimates via the $\delta$ method covariance matrix.
Column 2: Standard errors based on the $\delta$ method covariance matrix.
Column 3: OMD parameter estimates using the bootstrap covariance matrix.
Column 4: Standard errors based on the bootstrap OMD estimate.

Notice that in both tables 3.2 and 3.4 the coefficient estimates are slightly different when the OMD estimator is calculated using the $\delta$-method instead of the bootstrap. This is because the second stage of the estimation procedure is dependent on $\Delta_\pi$: the variance-covariance matrix of the reduced-form coefficients.

We also retest the hypothesis that lagged income and income growth do not affect a country's probability of a coup. Both the bootstrap and the $\delta$ method lead to decisive rejection of this hypothesis using the new data, although the magnitude of the test statistics is somewhat smaller than we obtained using the *World Handbook* data. Under the null hypothesis of no effect, the test statistic is distributed as $\chi^2$ with two degrees of freedom.

Using the bootstrap, the actual value of the test statistic was 23.976; using the $\delta$ method it was 18.412, both corresponding to p-values below 0.001: income Granger causes coups.

Our test of the hypothesis that income does not affect *World Handbook* coded coups was sensitive to the method used to estimate the variance-covariance matrix of the parameters. However, using the more reliably measured Bienen and Van De Walle coded coups, there is no ambiguity—Granger causality tests based on either the bootstrap or the method indicate acceptance of the null hypothesis that the past history of coups does not influence income growth. The greater reliability of the Bienen and Van De Walle codings suggests that coups do not affect income growth, the ambiguities of the *World Handbook*-based estimates notwithstanding. While this conclusion seems justified on empirical grounds, it has less theoretical appeal; misgovernment can certainly disrupt the processes of economic growth, and we would expect that a past history of coups leaves a country at increased risk of bad government. It is with this question in mind that we turn to the leader-specific data of the next section.

### 3.3 The Effect of Leaders' Characteristics

Using leader-specific data, we are now in a position to more directly test our most surprising finding from the earlier research: the claim that the past history of coups has no effect on the current rate of economic growth. We are left to question whether the governments brought to power via coups have an effect on the economy. One possibility, which the country-level coup counts from the *World Handbook* does not permit us to address, is that although nonconstitutional rule impedes economic growth, most coups simply result in the replacement of one despotic ruler by another, with no independent effect on growth.

More generally, nonconstitutional governments are a heterogeneous lot, with a range of effects on economic performance. Some foster economic growth; the replacement of Turkish President Menderes by General Gursel is often spoken of as such a case. Other nonconstitutional governments reverse decades of economic progress, as in the case of the replacement of Iranian Shah Mohammed Pahlavi by Khomeini. In the aftermath of other nonconstitutional transitions, such as the replacement of Egypt's King Farouk by Nagib, or of Benin's Soglo by Alley, economic matters appear to have remained much as they were. The "average" effect on the economy of this eclectic group may be neutral.



The Bienen and Van De Walle data permit us to conduct a more direct test of whether nonconstitutional rulers are, at least on average, different from their constitutional counterparts. Thus we are able to assess the separate influences of the current leader's constitutional status (controlling for the past history of coups) and the extent of a county's recent experience with coups. The nonconstitutional rule variable also permits a more detailed analysis of the coup trap: is the history of past coups simply telling us about the current leader's nonconstitutional status, or does it exert an independent influence on the probability of further coups?

To implement this test we match the annual coup counts and economic data with data on the first leader to hold power during each year, as described in section 3.1. Two variables are of particular interest: nonconstitutional entry and the current leader's time in power.[17] Bienen and Van De Walle used hazard functions to analyze their leader-specific data and discovered "negative duration dependence": the longer a leader remains in power, the lower his probability of being removed during the current year. Further, the pattern of time dependence for nonconstitutional rulers appeared to differ from the others. The nonconstitutional rulers started with a higher risk of losing power, but over time their risk fell below that for the others.

In light of Bienen and Van De Walle's findings on leadership duration, we include our variables for nonconstitutional rule—time in power and an interaction term that allows the effects of time in power to differ for nonconstitutional rulers. Our model also includes all of the explanatory variables in both the growth equation and the coup equation. The coefficients of time in power and time in power for nonconstitutional rulers are insignificant in both equations. When we constrain the time-in-power variable to have the same effect for both constitutional and nonconstitutional rulers, the variable remains insignificant in both equations. Parameter estimates of our model with the nonconstitutional rule variable appear in column 1 of table 3.5. Column 2 reports estimated standard errors (via the $\delta$ method).

This is surprising in light of Bienen and Van De Walle's earlier findings. Yet this is not a direct contradiction of their finding of negative duration dependence. Their dependent variable is the time elapsed until a leader loses power—whether by electoral defeat, resignation, assassination, or coup d'etat. In our analysis we focus only on coups. Our finding does suggest that the negative duration dependence found by Bienen and Van De Walle operates through some other type of risk of losing power; we find no

**Table 3.5**
Incorporating leader-specific data (using coup counts derived from Bienen and Van de Walle)

| | 1 | 2 | 3 | 4 | 5 | 6 |
|---|---|---|---|---|---|---|
| *Growth equation* | | | | | | |
| Constant | 0.0833 | 0.0117 | 0.0836 | 0.0083 | 0.0838 | 0.0118 |
| This year's coup propensity | * | * | * | * | −0.0016 | 0.0118 |
| Coups occurring during the previous six years | 0.0005 | 0.0017 | * | * | * | * |
| Coups occurring more than six years earlier | −0.0005 | 0.0008 | * | * | * | * |
| Log of the previous year's per capita GDP | −0.0078 | 0.0016 | −0.0079 | 0.0011 | −0.0083 | 0.0038 |
| The previous year's per capita GDP growth rate | 0.1583 | 0.0177 | 0.1583 | 0.0125 | 0.1557 | 0.0255 |
| Nonconstitutional leader | −0.0055 | 0.0030 | −0.0055 | 0.0019 | −0.0050 | 0.0045 |
| Leader's time in power | $3 \times 10^{-5}$ | 0.0001 | 0.0000 | 0.0001 | 0.0000 | 0.0001 |
| Africa | −0.0235 | 0.0037 | −0.0234 | 0.0026 | −0.0234 | 0.0038 |
| Middle East | −0.0036 | 0.0044 | −0.0037 | 0.0031 | −0.0033 | 0.0054 |
| Latin America | −0.0073 | 0.0042 | −0.0079 | 0.0027 | −0.0069 | 0.0085 |
| North America-Europe-Australasia | 0.0106 | 0.0045 | 0.0107 | 0.0032 | 0.0102 | 0.0060 |
| *Coup equation* | | | | | | |
| Constant | 0.0474 | 0.5401 | 0.8709 | 0.4734 | 0.8741 | 0.6714 |
| Coups occurring during the previous six years | 0.1280 | 0.0514 | 0.1286 | 0.0363 | 0.1257 | 0.0583 |
| Coups occurring more than six years earlier | 0.0471 | 0.0256 | 0.0466 | 0.0181 | 0.0466 | 0.0250 |
| Log of the previous year's per capita GDP | −0.2835 | 0.0789 | −0.3613 | 0.0613 | −0.3617 | 0.0873 |
| This year's per capita GDP growth rate | * | * | −9.8505 | 3.3235 | −9.8459 | 4.4928 |
| The previous year's per capita GDP growth rate | −1.5583 | 0.7151 | * | * | * | * |
| Nonconstitutional leader | 0.1919 | 0.1030 | 0.1371 | 0.076 | 0.1391 | 0.1095 |



**Table 3.5** (continued)

| | 1 | 2 | 3 | 4 | 5 | 6 |
|---|---|---|---|---|---|---|
| Leader's time in power | −0.0049 | 0.0078 | −0.0047 | 0.0056 | −0.0048 | 0.0080 |
| Africa | −0.0149 | 0.1528 | −0.2460 | 0.1410 | −0.2459 | 0.1969 |
| Middle East | 0.2238 | 0.1875 | 0.1867 | 0.1367 | 0.1873 | 0.1928 |
| Latin America | 0.4955 | 0.1696 | 0.4176 | 0.1259 | 0.4193 | 0.1771 |
| North America-Europe-Australasia | −0.3543 | 0.3010 | −0.2483 | 0.2181 | −0.2476 | 0.3087 |

$\hat{\rho}$: −0.1178 S.D. ($\hat{\rho}$): 0.0392 $\hat{\sigma}$: 0.0570 Log Lik: 3,598.4828 # Obs: 2797

Column 1: Parameter estimate.
Column 2: Standard errors calculated by the $\delta$ method.
Columns 3 and 5: OMD parameter estimates using the $\delta$ method covariance matrix.
Columns 4 and 6: Standard errors based on the $\delta$ method OMD estimate.

evidence that time in power reduces a leader's risk of losing power in a coup (once a country's past history of coups has been controlled for). Of course, the longer a leader who seized power in a coup continues to rule, the more distant the coup that spawned his rule becomes, and, all else held equal, the lower the count of recent coups. This effect will tend to reduce the leader's coup risk.

In contrast to our findings about time in power, our results indicate that nonconstitutional rule exerts a marginally significant influence on the probability of a coup. Rulers who commit the "original sin" of coming to power outside their country's constitutional framework are themselves at heightened risk of a coup d'etat. We further find that nonconstitutional rule, unlike the lagged coup history, does have a statistically significant impact on the rate of economic growth for the average country in our sample: a nonconstitutional ruler reduces the annual growth rate by about half a percentage point, which is the equivalent of around two months growth for the average country in our sample. While coups themselves may not directly harm the economy, nonconstitutional rule apparently does.

These estimates also shed light on the question of whether lagged coups proxy for nonconstitutional rule in our earlier estimates, or whether instead they exert an independent risk on the probability of further coups, as Finer (1962) suggests they will. After correcting for the effect of nonconstitutional rule, a country's recent coup rate continues to exert an independent influence on the probability of a coup. However, the coefficient estimates for lagged coups are somewhat smaller than in the earlier tables—part of the estimated effect was due to the role of past coups as a proxy for nonconstitutional rule.

When we retest the restriction that lagged coups do not affect growth, it now passes easily, with an $\chi^2$ statistic of 0.5304. Repeating our test of the hypothesis that lagged income variables do not matter for coups, we just as easily reject this hypothesis. Our test statistic, which is asymptotically distributed as $\chi^2$ with two degrees of freedom, takes on a value of 17.9003. Both of these tests are based on the covariance matrix calculated using the $\delta$ method.

We estimate the model with current growth in the coup equation, again adopting the identifying restriction that lagged growth does not exert an independent influence. Estimates of the restricted version of the model appear in column 3 of table 3.5, with estimated standard errors in column 4. These estimates are calculated via the $\delta$ method. These estimates confirm that current growth has a large, statistically significant, but imprecisely estimated effect on the probability of a coup. The economy affects the choice of ruler, even when this choice is not filtered through the electoral process.

Do coups affect growth? For lagged coups, the answer is consistently no. However, we can use lagged coups as instruments for the current coup propensity in the growth equation. When we do so we obtain the estimates reported in column 5 of table 3.5 (with associated standard errors appearing in column 6). The estimates are little changed from their previous values, with an $\chi^2$ statistic with one degree of freedom of 0.5067 implied by the restriction that lagged coups do not affect growth. The estimated coefficient on the current coup propensity reported in column 5 is very nearly zero and does not even approach the threshold of statistical significance. The message of this model is clear: nonconstitutional rule slows the pace of economic growth, but once this effect is controlled for, additional coups do not exert an additional growth-inhibiting influence.

Our model enables us to gauge the effects of various risk factors for a coup and also the effect of coups on the growth process, but how well does it fit the data? For the growth equation, the answer to this question is straightforward but depressing. The $R^2$ for our preferred growth model is a mere 0.0584. With 2,798 observations, this $R^2$ is highly statistically significant. However, there is a high degree of residual noise in the system; 96 percent of the variation in growth rates remains unexplained by our model. By aggregating over time, and estimating a model of, say, five- or ten-year growth rates, we could presumably gain additional accuracy.

For example, using average annual growth rates over the period 1960 through 1985, Barro explains over half the variation in his growth data, using a growth model similar to ours, enriched by data on government spending and educational attainments. By accepting a longer time interval, Barro gained access to a wider set of variables, those collected with less than annual frequency, and also worked with series in which the very high frequency variation had been removed. We believe that, for our purpose of modeling the structural determinants of coups, the increased sample size and added precision of our coup coefficient estimates is worth the sacrifices entailed by working with annual data. Further work with longer time intervals, such as Barro's, that focused on the interplay of growth and coups could profitably complement our work here.



The picture for coups is more ambiguous. Because coups are a rare event, a model that predicts that coups never occur will be right about 95 percent of the time. Predicting which countries will suffer coups this year is like trying to predict which individuals in a population will suffer heart attacks this year. While we can predict that overweight male smokers over the age of forty with high blood cholesterol, high blood pressure, and a family history of heart disease are at higher risk of a heart attack, there are very few of them for whom the risk this year rises above 50 percent. However, if we aggregate an annual heart attack model and seek to predict which individuals will suffer heart attacks over, say, the next twenty years, we will predict much more accurately.

The case for our coup data is very similar. In only one country/year, Bolivia in 1980, does our model's estimate of the coup probability rise above 0.5. In other words, our model only "predicts" one coup, by the stringent standard of only predicting when the event probability goes above 0.5. When we calculate the correlation between actual coups and the probability of a coup estimated by our model, the association remains low. The mean coup rate for our data set is 0.044, which is also the predicted rate. However, the correlation between our model's predicted annual coup probability and the actual occurrence of coups is only 0.1998.

At an annual level, our model does not do a spectacular job of predicting coups. However, if we pose the somewhat less ambitious task of predicting the total number of coups for each country in our sample, the model does a more impressive job. Of course, on theoretical grounds, any probit model can always get the total number of coups for the sample exactly by simply setting all the response coefficients to zero and adjusting the constant to return the mean event probability. However, there is considerable variation among the 121 countries in our sample. The mean number of coups per country in our sample is 0.9916 (our model predicts 0.9974), with a standard deviation of 1.5744. We construct the predicted number of coups for each country by adding up the annual estimates of the coup probability generated by our model for each country. The correlation between these estimates and the actual coup counts is 0.7993. By this standard, our model does well—over time it does a good job of predicting countries' cumulative experience with coups.

### 3.4 Conclusion

The findings of our earlier work on coups (Londregan and Poole 1990) are largely confirmed when the model is reestimated using coup counts derived from Bienen and Van De Walle (1990) instead of the counts provided by the *World Handbook*. These findings include the existence of a coup trap, leaving countries that have experienced a coup at greater risk of further seizures of executive power, and the finding that coups are a poor country phenomenon—they almost never occur in high income countries. We substantially corroborate the lack of a feedback effect from coups to growth, though in the *World Handbook* data this conclusion is sensitive to the method of calculating the variance-covariance matrix. However, with the exception of testing for the effect of past coups on current growth rates, we find that our results are robust to the method of calculating standard errors, whether the bootstrap or the $\delta$-method. We find reason to prefer the coup counts based on the Bienen and Van De Walle codings because of their greater accuracy and because the more conservative definition of nonconstitutional transfers of executive power leads to the classification of a more homogeneous set of events as coups.

We extend our earlier work by adding leader-specific variables. Our results indicate that nonconstitutional leaders are themselves at greater risk of being forced from power nonconstitutionally: he who lives by the coup dies by the coup, or at least tends to lose power that way. However, this is not the entire story behind the coup trap. Even after correcting for nonconstitutional entry by the current leader, countries' past experience with coups continues to exert an independent coup-provoking influence.

Our results indicate that nonconstitutional rule is an important source of feedback *from* the political system *to* the economy. While the past history of coups does not influence the growth rate, nonconstitutional rule does, reducing annual growth by about a half percentage point per year. In addition to the coup trap there seems to be a "poverty trap" in which poor countries are more coup prone and thus more likely to be saddled with nonconstitutional rulers who will slow the rate at which economic growth eventually immunizes them from yet further coups and more nonconstitutional rule. Coups are not simply an inferior good disproportionately "consumed" by poor countries, they are a very high priced Giffen good, imposing nonconstitutional rule on countries that can least afford it.



## Notes

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1. To the *World Handbook's* credit, an extensive data appendix details the many descrepancies among the coding decisions of various research assistants. In addition, the *World Handbook* codes a wider set of political variables and does not simply focus on leadership, although for our purposes in this paper, this extra breadth of coverage is irrelevant.

2. Pereda Asbun (a 47-year-old with a military background, who nonconstitutionally seized power in 1978), Padilla Arancibia (a 54-year-old military leader, who grasped control in 1979 by nonconstitutional means), Guavara Arce (a 68-year-old civilian, who gained power "constitutionally" in 1979), Natusch Busch (a 53-year-old from a military background, who seized executive power nonconstitutionally in 1979) and Gueiler Tejeda (a 58-year-old civilian, who came to power in 1979 constitutionally).

3. The rationale being that survival for such a leader until 1969 is an uncertain proposition: a leader who acceded to power in 1967, and lasted for more than one year, but less than two, need not have remained in power until the beginning of 1969.

4. These came following Al-Shaabi of the Yemen Democratic Republic, Boumedienne of Algeria, Donitz of Germany, Gizenga of Zaire, D. Anastasio Somoza of Nicaragua, Souvanna Phouma of Laos, Tito of Yugoslavia, and Villarroel of Bolivia. There were also the cases of Karume of Zanzibar, and Minh of South Viet Nam, the last leaders of their respective countries.

5. The first four years of Zairian independence, and the Uruguayan interval of shared rule (1951–1958).

6. We compile two coup history variables, coups occurring during the previous six years (that is, during years $t-6$ through $t-1$), and coups occurring earlier (during $t-7$ or before). While the Summers and Heston economic data begin with 1950 or later, the leadership data for some countries reach back much further (the early 1800s for some of the Latin American leaders). We create annual coup counts back to 1944 or the first year of independence, whichever comes later. This means that for countries for which leadership data is available as early as 1944, the values of the recent coup history variable are not distorted by "presample" coups from 1950 onward.

7. For example, consider the case of the replacement of Thailand's Thanom by Sanya in 1973. The *World Handbook* codes this as a coup, presumably because it continued an epoch of military rule in Thailand that began with a coup led by Pibun in 1947. However, this transfer took place under the rubric of the constitution imposed by the military.

8. For example, the December 31, 1981 coup of Rawlings against Ghana's Limann is erroneously counted as occuring in 1982 by the *World Handbook* but correctly

placed in 1981 by Bienen and Van De Walle. Likewise, Bokassa's December 31, 1965 overthrow of Dacko in the Central African Republic is miscoded by the *World Handbook* as occurring in 1966 (*Facts on File* also erroneously counts this coup as occurring on January 1).

9. For notational convenience, we index these regions conformably with the following coefficient subscripts: $j = 5$ for Africa, $j = 6$ for Europe and North America, and $j = 7$ for South America.

10. In the sense of Engle, Hendry, and Richard (1983).

11. Using a Gateway 2000 machine, convergence is even faster, taking about 32 seconds.

12. This algorithm is described in detail in the appendix to Londregan and Poole 1990.

13. The one exception being the standard error for lagged income in the growth equation. The bootstrap estimate of this standard error is about twice the standard error estimated by the $\delta$ method.

14. These estimates are calculated using the method of Broyden, Fletcher, Goldfarb, and Shanno, with a final iteration of the Newton-Raphson method at the optimum employing numerical gradients and Hessians. Consistent with theory, identical parameter estimates and the same value of the criterion function are yielded by Amemiya's method of generalized least squares, (Amemiya 1978), which essentially calculates generalized least squares estimates of $\alpha$ by regressing the reduced-form parameter estimates, $\pi$, on a "selection" matrix D that depends on the set of variables excluded from the structural model.

15. This region corresponds closely to Lipset's (1959) set of "European and English-Speaking Stable Democracies."

16. The lagged coup counts used in this model are, of course, different from those based on *World Handbook* data.

17. We also examined the effect of the leader's military background and age, but neither of these was significant once we had accounted for nonconstitutional entry and time in power.

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