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Revisiting Economic Shocks and Coups

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Abstract

This article revisits the oft-cited relationship between economic shocks and coups. According to conventional wisdom, economic recessions trigger coups. However, existing empirical studies have not consistently produced supporting evidence for that relationship. This article claims that this is partly because existing studies have not differentiated transitory from permanent shocks to the economy. Two different economic shocks could have different effects on coups. Moreover, existing studies have not sufficiently addressed measurement error in gross domestic product (GDP) data. To overcome these problems, I use exogenous rainfall and temperature variation to instrument for economic growth. Instrumental estimates demonstrate, consistently across four different GDP per capita growth measures, that a decrease in GDP per capita growth rates, induced by short-run weather shocks, significantly increases the probability of a coup attempt. Conversely, noninstrumental variable estimates vary according to different GDP measures, and are close to zero, consistent with previous findings.

Keywords

coups, transitory economic shocks, permanent economic shocks

This article revisits the oft-cited relationship between economic shocks and coup attempts. Most scholars agree that economic downturns make a country more prone to coups (e.g., Johnson et al. 1984; Fossum 1967; Galetovic and Sanhueza 2000;

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Needler 1966; Nordlinger 1977; O'Kane 1981). But this conventional view has not been consistently supported by cross-national empirical findings.[1] In particular, several recent studies (Goemans and Marinov 2013; Powell 2012; Svolik 2013), based on a global sample, have not found a statistically significant relationship between economic growth and coups.

In this article, I argue that there are both theoretical and empirical reasons for this disparity between this conventional wisdom and empirical findings. First, the existing theory regarding the relationship between economic growth and coups has been underspecified. Permanent shocks as well as transitory shocks drive fluctuations in income. Unlike transitory shocks, negative permanent shocks to the economy can not only increase the probability of a successful coup but also decrease the prize of the coup. If negative shocks are permanent enough to diminish the expected value of expropriable rents, the probability and the rewards of seizing power can move in opposite directions. The total effect of economic growth on coups depends on which effect dominates. This means that when one simply estimates the effect of gross domestic product (GDP) growth on coups, one may capture the average effect of both transitory and permanent shocks which is smaller in absolute magnitude than that of transitory shocks.

Second, I contend that measurement error in GDP data plays a substantial role in previous findings. It is well known that GDP measurements are often inaccurate in developing countries where most coups occur (e.g., Deaton 2010). Measurement error generally tends to bias coefficient estimates on poorly measured variables and their statistical significance toward zero. As shown subsequently, real GDP per capita growth rates, published in different data sources, are not highly correlated with one another. Thus, the estimated effects of GDP growth on coups can differ according to which GDP measures are used. These theoretical and empirical reasons explain the mixed and weak findings of the existing studies on the effect of economic growth on coups.

I address these two issues using an instrumental variable (IV) approach. I exploit year-to-year fluctuations of rainfall and temperature as sources of exogenous variation in income that help to estimate the short-term causal effect of economic growth on coup attempts. These weather instruments not only generate exogenous variation but also isolate transitory shocks in GDP per capita, since weather shocks are strongly mean-reverting.[2] With this strategy, I can tease out the short-run effect of economic shocks, precluding the wealth effect of permanent shocks. For comparison, I utilize oil price shocks, slowly mean-reverting, as an additional instrument to capture persistent shocks to the economy. This IV approach helps me to address measurement error because IV estimates are known to be more robust to measurement error than non-IV estimates. Looking at 142 to 148 countries during the period of 1960 to 2005, I investigate within-country variation of economic growth.

This article provides several interesting results. First, the IV estimates yield that economic shocks strongly influence coup attempts, while the non-IV estimates are close to zero. A one percentage point decrease in per capita GDP growth, induced by weather shocks, increases the short-run likelihood of a coup attempt by 1.3 to 1.5 percentage points. Given that the unconditional probability of a coup attempt is approximately 5 percent, this effect is immense. This finding stands up to four different measures of real GDP per capita growth rates. In contrast, GDP per capita growth, instrumented by oil price shocks to capture permanent economic shocks, is not significantly associated with the likelihood of coup attempts. Third, I explore possible channels through which economic growth affects coups. The effect of economic growth on coups operates partly through social unrest. However, I find little evidence for economic growth influencing coups via military expenditure.

The finding presented in this article accords well with existing research. The seminal article of Miguel, Satyanath, and Sergenti (2004) shows that negative economic shocks, induced by rainfall shocks, increase the risk of civil conflict onset. Other studies find similar results when using climate shocks to examine civil conflict onset (Burke et al. 2009) and when investigating the effect of rainfall shocks on ethnic riots (Bohlken and Sergenti 2010) and various kinds of social conflict (Hendrix and Salehyan 2012). Similarly, Burke and Leigh (2010) and Brückner and Ciccone (2011) demonstrate that transitory negative shocks to the economy, captured by weather shocks, open a window of opportunity for democratic regime change. This article, along with these previous studies, demonstrates that transitory negative economic shocks increase the risk of political instability, including political violence and regime transitions. Moreover, the finding reported here may explain the mechanism underlying the relationship between economic shocks and democratization. Combined with the fact that violent leader turnovers can facilitate democratization (Miller 2012), it suggests that violent turnovers may be the channel through which weather shocks affect democratization.

This article is organized as follows: the next section reviews previous literature and discusses the problems that arise in estimating the causal effect of economic growth on coups. The third section presents my empirical strategy and data to identify the causal effect of economic shocks on coup attempts. The following section reports the main results and checks their robustness. The last section provides concluding remarks.

From Economic Shocks to Coups

Possible Links

Scholars have generally hypothesized that poor economic performance triggers coups (Johnson et al. 1984; Fossum 1967; Galetovic and Sanhueza 2000; Needler 1966; Nordlinger 1977; O'Kane 1981). For instance, O'Kane (1981, 308) states that "perhaps coups are just the drastic response to an unstable and hopeless economic situation against which little can be done." Nordlinger (1977, 89) contends that "intervention against governments during periods of economic decline, stagnation, or inflation is more common than at times of economic good health."

One possible channel through which economic growth could affect coups is via social unrest (Finer 2002). During a period of stagnant growth, citizens tend to have fewer job opportunities and lower household income. Economic decline casts doubt on the competence of the incumbent government, since its economic record can be the yardstick by which the government's performance is judged (Nordlinger 1977, 88). When the government fails to deliver what citizens expect of it, it loses legitimacy in their eyes (Lipset 1959). When disenchantment with the incumbent government is widespread, citizens have greater incentives to engage in antigovernment activities and to vote against the incumbent. Citizens then are more likely to remain passive or acquiesce to a coup (Galetovic and Sanhueza 2000). In addition, widespread protests can provide a clear indication of a regime's weakness, particularly in autocracies where accurate information about popular support for the incumbent government is hard to obtain (Casper and Tyson 2012). Coup plotters utilize observable economic and social factors to assess the likelihood that a coup will be successful and to update their beliefs about the regime's strength and popularity.

Moreover, a stagnant economy may give the military, the main but not the only perpetrator of coups, a motive to intervene (Fossum 1967). When popular protests are prevalent, the military often steps in politics to "restore public order." Social and political unrest caused by an underperforming economy may ignite class antagonism and increase populist pressure. The military may decide to stage a coup if pressures grow enough that politicians begin to consider policies that threaten to nationalize and expropriate private property. As is often the case in Latin America, the military may take the vanguard role of protecting "the existing middle class order" (Huntington 1968; O'Donnell 1973; Tusalem 2010).

Finally, the military may intervene during economic downturns to protect its corporate interests. Several scholars emphasize the role of the military's corporate interest in motivating coups (Nordlinger 1977; Thompson 1973). A powerful military, as a repressive agent of last resort, is necessary to thwart popular uprisings (Svolik 2012). However, the military can act self-interestedly against the regime (Acemoglu, Ticchi, and Vindigni 2010; Svolik 2012).[3] A powerful military can exploit its position to demand greater concessions to the military or threaten to intervene in the politics. Rulers thus buy off key military officers with pecuniary rewards to keep them in their barracks (Decalo 1989). In particular, military expenditures are critically related to organizational interests of the military. Greater military expenditures, including higher salaries or a larger budget, can be considered a concession from the ruler to the military or a signal that military interests are being taken into account by the ruler (Powell 2012). However, economic slowdowns may undermine the fiscal capability of governments to distribute benefits to sustain ruling support coalitions or clientelist networks (Haggard and Kaufman 1995). Stagnant growth could lead to cuts in military budgets and lower living standards for officers. To protect its organizational interest, the military will be more likely to intervene in politics when the economy is performing poorly. Therefore, economic performance influences both opportunity and motive for coups.

Gap between Theory and Empirics

As Belkin and Schofer (2003) conclude, however, existing empirical studies produce mixed findings concerning the effect of economic growth on coups. While many studies find a negative relationship between GDP per capita growth rate and coups, their results are often statistically insignificant (see Table 1). Several studies, including the most recent studies based on a global sample (e.g., Goemans and Marinov 2013; Svolik 2013), report a *positive* but statistically insignificant effect of GDP growth on coups.

One of the potential reasons that previous studies have found only weak or mixed supporting evidence is their failure to distinguish transitory from persistent economic shocks. Most studies simply estimate the effect of GDP per capita growth rate on the likelihood of coups. However, a change in GDP growth rate is due to some combination of transitory shocks, such as monetary policy or rainfall shocks, and permanent shocks, such as technology or commodity price shocks (Hamilton 1994).

Permanent shocks influence not only the opportunity for a coup but also the discounted present value of staging a coup.[4] Suppose that commodity prices have been rising. This increases resource rents in commodity exporting countries. Given that capturing the state is the ultimate prize of seizing power (Bazzi and Blattman 2012; Besley and Persson 2008; Fearon 2007), coups then become more attractive to potential coup plotters. In addition, coups, unlike civil wars, do not deal a huge blow to the economy. As discussed previously, however, growing incomes make citizens happier with the incumbent government, and rising government revenues help the incumbent leader's financial capability to buy off the military, strengthening his control on power. This implies that positive permanent shocks may have an ambiguous effect on the risk of a coup, having opposite effects on the expected value of a coup and the probability that a coup attempt succeeds. Contrarily, transitory shocks affect only the probability of successful coups, without affecting the discounted present value of seizing the state's authority. Transitory negative shocks to the aggregate economy increase grievances and reduce citizens' opportunity cost of engaging in popular protest, leading to a short-term increase in their de facto political power (Acemoglu and Robinson 2006). They thus open up a window of opportunity for coups but change few of the rents appropriable by the state.

This suggests that existing studies likely capture the combined effects of both transitory and permanent shocks, since they simply estimate the effect of GDP growth on coups without isolating transitory shocks. Given the difference between persistent and transitory economic shocks, the estimated effect of GDP growth on coups, capturing both shocks, should be smaller in magnitude than the estimated effect of transitory economic shocks.

Second, the mixed findings of existing empirical studies may be related to measurement error in GDP data. The accuracy of GDP measurement is often questionable in developing countries (e.g., Deaton 2010). Economic activities conducted

Table 1. Previous Cross-national Studies on Economic Growth and Coups.

Author	Sample	GDP data	Method	Sign	Significance
Johnson et al. (1984)	35 African countries 1960–1982	Bornschier and Heintz (1979)	OLS	−	√
Londregan and Poole (1990)	121 countries 1950–1982	Penn World Table	Simultaneous equations	−	
Alesina et al. (1996)	113 countries 1950–1982	Penn World Table (Mark 5)	Simultaneous equations	−	√
Galetovic and Sanhueza (2000)	89 non-communist 1950–1982	Penn World Table (Mark 5)[a]	Logit	−	√
Collier and Hoeffler (2007)	Global sample and 48 African 1960–2001	World Development Indicators (2003)	Two-stage least squares probit	−	√
Arriola (2009)[b]	40 African countries 1971–2001	World Development Indicators (2007)	Proportional hazards model	−	√
Thyne (2010)	19 Latin America countries 1960–1999	Gleditsch (2002)	Logit	−	
Goemans and Marinov (2013)	Global sample 1960–2004	Penn World Table 6	Probit	+	
Powell (2012)	143 countries 1961–2000	Gleditsch (2002)	Heckman selection probit	−	
Svolik (2013)	139 countries 1946–2002	Maddison (2010)	Random effects logit	+	

Note: I do not include studies that do not include GDP per capita growth rate. Significance indicates whether a study finds a statistically significant relationship between GDP per capita growth and coups.
[a]They create a recession indicator that equals one when GDP per capita growth is negative.
[b]He examines extra constitutional changes including both civil wars and coups.

within informal sectors play a substantial role in the economies of many developing countries where government statistical infrastructures are weak. Measurement error in GDP data will bias coefficient estimates on GDP growth toward zero. Even worse, the attenuation bias due to measurement error is magnified by other issues in analyses that aim to explore the short-run effects of economic shocks. First, annual panels with higher frequency, better suited to estimating the short-run effect of economic shocks than lower frequency data such as ten-year panels, worsen measurement error (Johnson et al. 2013). Second, a fixed effects estimator, which addresses omitted variable bias, tends to exacerbate measurement error when the right-hand-side variables are more time persistent than the errors in measurement (Hauk and Wacziarg 2009). Fixed effects estimators remove a significant portion of variation in the right-hand-side variables by demeaning them.

To better understand measurement error in GDP data, the top panel of Figure 1 depicts correlations among measures of annual real GDP per capita growth rates drawn from Gleditsch (2002), Maddison (2010), the Penn World Table (PWT) 7.0, and the World Development Indicators (WDI; World Bank 2011). Among all countries' GDP growth rates, correlations range from .6 to .8. Next, I categorize each country based on the International Monetary Fund's (IMF) indices of the frequency and timeliness of its dissemination of macroeconomic data. Countries that subscribe to the IMF's Special Data Dissemination Standard (SDDS) and meet a set of specifications for data provided to the IMF. The other group consists of countries that do not conform to the SDDS. Most high-income countries belong to the SDDS group, and 75 percent of coup attempts occur in the nonSDDS group. Correlations among GDP per capita growth rates obtained from the four data sets are substantially lower (ranging from .485 to .778) in the non-SDDS group than in the SDDS group (ranging from .721 to .953). The correlation between Gleditsch (2002) and Maddison (2010) is merely .485 in the non-SDDS group. This exercise indicates that the choice of a different GDP data set would affect estimation results.

The two bottom panels of Figure 1 show the sensitivity of estimation results to the choice of a GDP data set. Using the previously mentioned GDP data, I estimate the effect of economic growth with a linear probability model and a logit model.[5] All coefficient estimates are negative, but their sizes and statistical significance vary according to GDP data sources. When Maddison's data are used, for instance, the estimated coefficients of GDP per capita growth are consistently significant and larger than coefficients when using other GDP measures. In contrast, the use of Gleditsch and WDI's data results in weak and insignificant coefficient estimates. The estimate on GDP growth using Maddison's GDP data is almost an order of magnitude greater than the estimate using Gleditsch's data. Even when I use negative growth indicators instead of annual real GDP per capita growth, estimates of negative growth vary according to GDP measures (see the online appendix). Moreover, the estimated effects of GDP growth, including those estimated with Maddison's measures, are quite modest. A standard deviation drop in GDP per capita growth rate

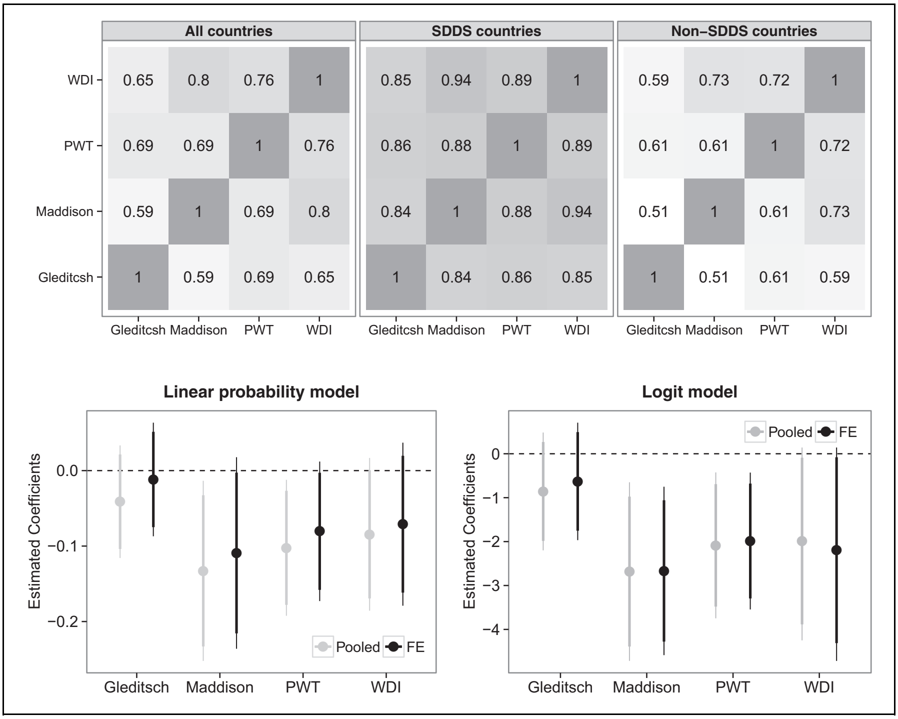

Figure 1. Brief look at difference of GDP data sets.

Note: The top graphs present pairwise correlations among annual real GDP per capita growth rates taken from four different GDP data sets. The Special Data Dissemination Standard (SDDS) countries refer to countries that subscribes to the IMF's Special Data Dissemination Standard. The bottom graphs display the non-IV estimates of GDP per capita growth. Dots represent the point estimates, and the associated horizontal thin (thick) line segments show the 95 percent (90 percent) confidence intervals for the estimates. The left panel shows the linear probability model (LPM) estimates, while the right panel shows the logit estimates. "Pooled" refers to pooled estimates and "FE" denotes country fixed effects estimates.

increases the probability of a coup attempt by merely .08 to .6 percentage points (when estimating linear probability models).

The sensitivity of non-IV estimates to one's choice of GDP per capita measure is consistent with the finding of Johnson et al. (2013) that different versions of the PWT data produce different estimation results of the same empirical model and that studies using annual GDP growth data are sensitive to a version of the PWT data. Johnson et al. (2013) emphasize that research using annual GDP data should demonstrate robustness to alternative data series.[6] In sum, measurement error in GDP data and estimates' sensitivity to different GDP measures, neglected in the previous studies, need to be addressed to accurately investigate the relationship between economic growth and coups.

Empirical Strategy

I employ an IV strategy to isolate exogenous variation in income. As discussed earlier, it is important to differentiate two different types of economic shocks. To this end, I focus on different sources of economic shocks. First, I utilize year-to-year fluctuations in rainfall and temperature to estimate the short-run effect of economic growth on coup attempts. These weather shocks can be considered transitory shocks to aggregate income, since weather instruments are strongly mean-reverting (Miguel, Satyanath, and Sergenti 2004; Brückner and Ciccone 2011; Ciccone 2011). Second, I use oil price shocks to explore the effect of persistent shocks to income on the likelihood of coup attempts. Oil price shocks exhibit slow mean-reversion, and the response of aggregate income to these shocks is very persistent (Brückner, Ciccone, and Tesei 2012).[7] Exogenous growth in income, driven by rainfall and temperature shocks, is expected to decrease coup risk, while oil price–driven income shocks should have a much smaller impact on coups.

Insofar as noise in income estimates is uncorrelated with noise in weather or oil price measurements, the error in measuring GDP would merely affect non-IV estimates, but not IV estimates. Dell, Jones, and Olken (2012) examine the relationship between the temperature and precipitation data quality and economic conditions or political instability, but find no evidence for it. In addition, Johnson et al. (2013) show that when they replicate the results of Miguel, Satyanath, and Sergenti (2004) using two different measures of GDP per capita from PWT 6.1 and 6.2, OLS estimates vary substantially across different versions of PWT, but IV estimates do not.

Following the pioneering work on civil conflict in sub-Saharan Africa by Miguel, Satyanath, and Sergenti (2004), several studies use rainfall shocks as an instrument for economic growth to study democratization (Burke and Leigh 2010; Brückner and Ciccone 2011), and ethnic riots (Bohlken and Sergenti 2010). Water is an important input in agricultural production, and rainfed agriculture contributes substantially to economies in countries where agriculture represents a large share of GDP. Moreover, rainfall can significantly affect the hydroenergy supply. For instance, many countries in sub-Saharan Africa rely heavily on water as both a direct and an indirect input (Harrison and Whittington 2001). One concern in using rainfall variation to instrument for economic growth is to ensure that rainfall variation is closely associated with economic growth in other regions of the world besides sub-Saharan Africa, where most economies are agrarian and do not have adequate irrigation systems. Miguel, Satyanath, and Sergenti (2004) argue that the identification strategy is not well suited to other regions of the world. Similar to Burke and Leigh (2010), I thus interact rainfall variation with the median share of agriculture in a country's GDP to increase the correlation between rainfall and economic growth.[8] This rainfall instrument is strongly correlated with GDP per capita growth in other regions of the world.

The second weather instrument is year-to-year variation in temperature.[9] Dell, Jones, and Olken (2012) find that a 1 °C rise in temperature reduced economic growth in that year by about 1.1 percentage points in poor countries, defined as having below-median PPP-adjusted GDP per capita in the first year the country enters the data set. Furthermore, higher temperatures are found to reduce industrial output and aggregate investment, in addition to agricultural output (Dell, Jones, and Olken 2012). Therefore, I do not interact temperature shocks with the share of the labor force in the agricultural sector. Instead, I adopt Burke and Leigh's (2010) strategy and multiply temperature shocks by -1 for countries with an average temperature below a certain cutoff. I choose 12 °C for the period 1960 to 1970, a cutoff that maximizes the F-statistic for a first stage estimation.

The measure of a weather shock is a standardized deviation $(x_{it} - \bar{x}_i)/sd_i$ where x_{it} is precipitation or temperature for country i in year t, and $\bar{x}_i$ and sd_i are the mean and standard deviation of x_{it}, respectively. Standardizing weather shock measures, along with controlling fixed effects, eliminates much of the covariance between annual fluctuations in rainfall and temperature and relatively time-invariant characteristics of countries. I consider alternative measures such as levels, growths, or deviations from the mean, but they are not as strongly associated with real GDP per capita growth as is the standardized deviation.

For the oil shocks instrument, I multiply the log change in annual international oil prices with the ratio of the net oil exports to GDP. This is the same method used by Brückner, Ciccone, and Tesei (2012). The effect of oil price change may vary according to the volume of net exports and degree of oil-dependency. An increase in oil price will increase the GDP growth rate in oil exporting countries and decrease the GDP growth rate in oil importing countries. One potential concern with this measure is that not all countries are price-takers in the oil market. The anticipation of political instability in important producers or consumers may influence oil prices. To lessen this concern, I omit countries that produce or consume more than 1 percent of the world's oil production following Brückner, Ciccone, and Tesei (2012).

The main challenge for the IVs strategy is to ensure that instruments are relevant and valid (Murray 2006). First, the relevance condition implies that the weather instruments must be strongly correlated with economic growth. When instruments are weak, the two-stage least squares (2SLS) estimation, the main method here, is biased in the same direction as the OLS estimates (Murrary 2006, 123). Moreover, estimated standard errors in the 2SLS become far too small, resulting in incorrect sizes in tests of significance. Therefore, the 2SLS estimates can be misleading in the presence of weak instruments. To test for the weakness of instruments, I report the Kleibergen-Paap Wald F statistics (an adjusted version of the Cragg-Donald statistic when errors are robustly estimated). Together with the critical values presented by Stock and Yogo (2005), these statistics allow me to assess, at different levels of significance, whether the IV estimates have bias that is larger than a certain percentage bias in the OLS results.

Second, a valid instrument must be uncorrelated with the error term of the second-stage equation. That is to say, rainfall and temperature must have no effect on coup attempts through any channel other than income growth, conditional on other control variables. Otherwise, the identification assumption is not valid, which can result in far worse bias than that of the OLS estimation. If weather shocks directly affect the costs of executing a coup, for instance, the exclusion restriction is violated. However, it is not clear how annual average levels of rainfall and temperature directly affect coup costs. One possible channel is that weather disasters, such as droughts and floods, would lead to botched relief efforts by the government and thus the loss of legitimacy. To test for the possibility that weather disasters such as droughts and floods directly influence the risk of coups, I estimate several reduced-form regressions of coups on indicators for droughts and floods, with and without the baseline control variables. I do not find any indication that weather disasters are positively associated with an increased likelihood of coups (reported in the online appendix).[10]

The following specification is the baseline empirical model:

$$\begin{aligned}\text{Coup}_{it} = {} & \alpha\text{Coup}_{it-1} + \beta\text{Growth}_{it-1} + \gamma\ln(\text{GDPp.c.})_{it-2} \\ & + X_{it-1}\delta + \theta_i + \tau_t + \varepsilon_{it},\end{aligned} \tag{1}$$

$$\begin{aligned}\text{Growth}_{it-1} = {} & \phi\text{Rainfall}_{it-1} + \varphi\text{Temperature}_{it-1} + \gamma\ln(\text{GDPp.c.})_{it-2} \\ & + X_{it-1}\delta + \theta_i + \tau_t + \varepsilon_{it},\end{aligned} \tag{2}$$

$$\text{Growth}_{it-1} = \text{Oil Price}_{it-1} + \gamma\ln(\text{GDPp.c.})_{it-2} + X_{it-1}\delta + \theta_i + \tau_t + \varepsilon_{it}, \tag{3}$$

where θ_i are country fixed effects, τ_t are year fixed effects, and ε_{it} is an error term. To control for the joint determination problem by unobserved or omitted country-level factors, country fixed effects are included. Income growth and coup attempts may change over time in tandem due to factors independent of any causal effect of income growth on coup attempts. Year fixed effects are thus included to absorb changes in the average risk of coups across time.[11] They control for international political or economic shocks common to all countries in a given year (e.g., international business cycle or the end of the Cold War) that may simultaneously affect economic conditions and coups. X_{it-1} is a vector of time-varying political and economic controls for each country. I will discuss them subsequently.

Growth_{it-1} is the annual percentage change in real GDP per capita for year $t-1$; β is the parameter estimate of main interest that captures the extent to which a change in economic growth induces a change in the probability of coups; and $\hat{\beta} < 0$ is expected.[12] I estimate these two equations using the two-stage least squares (2SLS).

Data and Measurement

I now provide an overview of the data and their sources. I construct a time-series cross-sectional data set using several public sources. The data set is composed of 142 to 148 countries from 1960 to 2005. The dependent variable is a binary indicator of whether a coup is attempted in any given country year. I use a data set developed by Powell and Thyne (2011) that covers coup attempts (457 cases) from 1950 to 2010. They define coup attempts as "illegal and overt attempts by the military or other elites within the state apparatus to unseat the sitting executive" (p. 252). This definition does not limit coup perpetrators to military actors. It carefully differentiates coup attempts from other types of anti-regime activities, such as riots, protests, or civil wars, which have previously been lumped in with coup attempts. Second, it includes not only successful, but also failed coup attempts.

The independent variable of main interest is economic growth. I measure economic growth as an annual percentage change of real GDP per capita from year $t-2$ to year $t-1$. As discussed in the third section, I utilize the four different widely used data sets—Gleditsch (2002), Maddison (2010), PWT 7.0, and WDI—to obtain GDP measures.

I use Dell, Jones, and Olken (2012) to obtain annual weather data. Using historical weather data taken from the Terrestrial Air Temperature and Precipitation: 1900 to 2006 Gridded Monthly Time Series, version 1.01 (Matsuura and Willmott 2007), they aggregate data to the country-year level and average them based on constant 1990 population weights. Temperature is measured in degrees Celsius (°C) and precipitation is in units of 100 mm per year. Data on oil exports and imports and oil prices are obtained from Brückner, Ciccone, and Tesei (2012). They use US dollar-denominated prices from international markets.

Existing research suggests that several factors other than economic shocks may be associated with the risk of coup attempts. The level of economic development is known to be one of the most important determinants of coups (Belkin and Schofer 2003; Londregan and Poole 1990). To control for that, I include a natural log of GDP per capita in year $t-2$. Political regime type can influence the incidence of coup attempts. For example, democracies may be less likely to experience coups because of their legitimacy in the eyes of citizens (Lindberg and Clark 2008). On the other hand, Geddes (1999) shows that military dictatorships tend to be internally weak relative to other regime types, and leaders in military dictatorships are more likely to be ousted by coups. To account for these possibilities, I include two indicators for democracies and military dictatorships. These data come from Cheibub, Gandhi, and Vreeland (2010). Countries suffering from ongoing interstate or civil conflict may be more susceptible to coups since they are facing legitimacy crises, providing more opportunities for successful coups. In addition, the literature on civil war finds that rainfall and temperature fluctuations significantly influence the likelihood of civil war (e.g., Burke et al. 2009; Miguel, Satyanath, and Sergenti 2004). Therefore, I include two indicators, coded 1 for every year in which the state is experiencing a civil conflict or interstate war with at least twenty-five annual battle deaths. The measure of war prevalence comes from the Uppsala Conflict Data Program/International Peace Research Institute (UCDP/PRIO) Armed Conflict Data set, version 4 (Gleditsch et al. 2002). Finally, the probability of a coup attempt conflict in a given year is likely to be dependent on the coup history of that country. To control for duration dependence, I follow the method of Carter and Signorino (2010) by including a cubic time polynomial of years that have elapsed since a previous coup attempt. More detailed information on these variables is documented in Section 2 of the online appendix.

Results

Main Findings

Table 2 presents the baseline 2SLS estimates. Columns 1 through 4 report the coefficient estimates of GDP per capita growth using weather shocks, while columns 5 through 8 report those using oil price shocks. At the bottom of Table 2, I present the first-stage estimations.[13] As expected, higher rainfall and lower temperature induce higher GDP per capita growth. All the estimates on weather instruments are consistent when using different measures of GDP per capita. Similarly, oil shocks are positively and significantly associated with GDP growth rates except when I use Gleditsch's GDP data. Of particular concern in the first stage is the possibility of weak instruments. With the exception of column 5, however, the Kleibergen-Paap *F* statistics exceed Stock and Yogo's critical values for 15 percent (10 percent) maximal bias of the IV estimator relative to the OLS estimator. This implies that if one is willing to accept a maximal weak instrument bias of 15 percent (10 percent), the hypothesis of weak instrument bias is rejected.[14]

Now I turn to the effect of economic growth on coups. Several points stand out from the IV estimates based on weather instruments. First, the IV estimates yield that GDP per capita growth rates are negatively associated with the likelihood of coup attempts in all models. I interpret this finding as evidence that negative rainfall and positive temperature shocks translate into a transitory negative GDP shock, opening a window of opportunity for a coup attempt. No matter what GDP measures are used, the coefficient estimates of GDP growth are negative and statistically significant at the 1 percent level. This consistency of IV estimates across different GDP measures markedly contrasts with the non-IV estimates, reported in Figure 1, indicating that IV estimates are robust to measurement error.

Second, the IV estimates are much larger in magnitude than the corresponding OLS estimates presented in Figure 1.[15] A one-standard-deviation drop (6 to 8 percentage points) in real GDP per capita growth rate increases the probability of a coup attempt by approximately 9 to 11 percentage points. According to the previous OLS estimates, in contrast, the same amount of decline in GDP growth increases the likelihood of a coup attempt by merely .08 to .6 percentage points. As discussed previously, this implies that OLS coefficient estimates are more attenuated toward zero than are the IV estimates. This attenuation bias can be attributed partly to measurement error in GDP data. Another reason that the IV estimates are larger in magnitude than the OLS estimates could be that IV estimates isolate transitory income shocks, whereas the OLS estimates pick up the weighted average of the effects of transitory

Table 2. Economic Growth and Coups.

	Weather shocks				Oil price shocks			
	Gleditsch	Maddison	Penn World Table	World Development Indicators	Gleditsch	Maddison	Penn World Table	World Development Indicators
Second stage								
GDP growth$_{t-1}$	−1.534***	−1.422***	−1.327***	−1.279***	−2.019	−0.599	−0.592	−0.589
	(0.529)	(0.530)	(0.434)	(0.479)	(2.966)	(0.366)	(0.436)	(0.429)
ln(GDP per capita)$_{t-2}$	−0.109***	−0.059***	−0.078***	−0.076***	−0.153	−0.041*	−0.049	−0.056**
	(0.038)	(0.022)	(0.029)	(0.025)	(0.175)	(0.022)	(0.031)	(0.027)
Democracy$_{t-1}$	0.013	0.000	0.007	0.019	0.012	0.005	0.010	0.018
	(0.021)	(0.020)	(0.019)	(0.021)	(0.023)	(0.022)	(0.021)	(0.022)
Military dictatorship$_{t-1}$	−0.002	−0.008	0.003	0.006	0.010	0.002	0.007	0.012
	(0.024)	(0.023)	(0.022)	(0.023)	(0.026)	(0.026)	(0.024)	(0.025)
Civil war$_{t-1}$	0.027	0.018	0.024	0.026	0.050*	0.047**	0.055***	0.061***
	(0.018)	(0.015)	(0.017)	(0.017)	(0.027)	(0.021)	(0.020)	(0.020)
Interstate war$_{t-1}$	−0.064***	−0.041**	−0.056***	−0.046**	−0.071*	−0.051***	−0.057***	−0.050***
	(0.024)	(0.020)	(0.020)	(0.020)	(0.040)	(0.018)	(0.020)	(0.019)
Coup$_{t-1}$	0.077**	0.081**	0.075**	0.084**	0.075	0.090**	0.085**	0.095 **
	(0.039)	(0.032)	(0.035)	(0.034)	(0.063)	(0.040)	(0.042)	(0.040)
First stage								
Rainfall deviation$_{t-1}$ × Agriculture	0.020***	0.018***	0.024***	0.011***				
	(0.004)	(0.005)	(0.005)	(0.004)				
Temperature deviation$_{t-1}$	−0.512***	−0.606***	−0.570***	−0.691***				
	(0.120)	(0.151)	(0.118)	(0.151)				
Oil price shock$_{t-1}$					0.366	0.347**	0.293***	0.464***
					(0.317)	(0.136)	(0.105)	(0.100)
Observations	4,604	4,456	4,428	4,385	3,361	3,237	3,333	3,297
Kleibergen–Paap *F* statistic	20.882	17.308	22.443	16.060	1.058	89.040	18.661	31.090
Country fixed effects	√	√	√	√	√	√	√	√
Year fixed effects	√	√	√	√	√	√	√	√
Cubic polynomial	√	√	√	√	√	√	√	√

Note: Two-stage least squares estimates are reported. All models include country fixed effects, year fixed effects, and cubic polynomials of the number of years since the last coup attempt. The coefficients of these variables and control variables in the first stage are omitted to save space. Standard errors are clustered at the country level (in parentheses). The dependent variable is a binary indicator of whether a coup is attempted.
*p < .10, **p < .05, ***p < .01.

and persistent shocks. Using weather shocks as instruments may preclude a possible wealth effect of economic growth.

Columns 5 to 8 use oil price shocks as the instrument for GDP per capita growth rate. The estimates on GDP growth are negative but not significant at a conventional level, except in column 5 where the first-stage relationship is weak. In addition, these estimates are smaller than the estimates based on weather shocks. This result is consistent with the theoretical expectation that oil shocks capture persistent shocks to GDP growth and that the GDP growth rate driven by oil shocks should have a smaller effect on coup attempts than the GDP growth rate driven by transitory weather shocks.

Next, I explore potential causal mechanisms that connect economic growth to coups. I focus on two possible channels, political unrest or military expenditures. Columns 1 through 4 of Table 1 examine whether the negative impact of GDP growth on coup attempts operates through political unrest. I control for the level of popular antigovernment protests by using the Cross-National Time-Series (CNTS) Data Archive (Banks 2010). To construct an index on the level of popular protest, I sum and log-transform yearly counts of antigovernment demonstrations, riots, general strikes, and revolutions in a given country. I then create its standardized deviation $(x_{it} - \bar{x}_i)/sd_i$ where x is the logged sum of antipopular protests.

If economic growth affects coup attempts through mass protests, including protests would wash away the effect of economic growth on coups. Columns 1 through 4 of Table 3 show that popular protests are positively associated with coup attempts. The coefficients of GDP growth are reduced to two-thirds of those obtained without controlling for political unrest and their statistical significance also decreases. However, I should note that the relationship between economic growth and coup attempts does not "go away" even after controlling for mass protests. The coefficients of GDP growth are distinguishable from zero. To check whether economic growth affects antigovernment protests, I estimate the effect of economic growth on antigovernment protests using the same weather instruments. Consistent with previous studies (e.g., Hendrix and Salehyan 2012), the first panel of Table 4 shows that GDP growth instrumented by weather shocks negatively and strongly influences antigovernment protests. These findings show that part of the indirect effect, through which economic growth influence coup attempts, is channeled through political unrest.

On the other hand, columns 5 through 8 of Table 3 and panels 2 through 4 of Table 4 indicate that economic growth does not affect coups through military spending. I control for military expenditures as a percentage of GDP and the natural log of military size, both of which are obtained from Correlates of War capability components (V3.02; Singer, Bremer, and Stuckey 1972). The ratio of military spending to GDP is negatively associated with the likelihood of coup attempts, while military size is positively associated with it. However, the inclusion of these variables does not affect much the coefficients on GDP per capita growth. A similar pattern emerges when I control for total military expenditures or military expenditures per

Table 3. Exploring Possible Channels 1.

	Controlling for Protest				Controlling for Mil. Exp. and Size				Including Both			
	Gleditsch	Maddison	Penn World Table	World Development Indicators	Gleditsch	Maddison	Penn World Table	World Development Indicators	Gleditsch	Maddison	Penn World Table	World Development Indicators
GDP growth$_{t-1}$	−1.036**	−1.024**	−0.959**	−0.863*	−1.382**	−1.586**	−1.237***	−1.414**	−0.857*	−1.014*	−0.794*	−0.958*
	(0.468)	(0.490)	(0.388)	(0.443)	(0.553)	(0.672)	(0.469)	(0.552)	(0.482)	(0.597)	(0.414)	(0.507)
ln(GDP per capita)$_{t-2}$	−0.074**	−0.036*	−0.051**	−0.051***	−0.112**	−0.067**	−0.075**	−0.083**	−0.070*	−0.035	−0.039	−0.052*
	(0.031)	(0.020)	(0.023)	(0.020)	(0.045)	(0.031)	(0.037)	(0.033)	(0.038)	(0.028)	(0.030)	(0.027)
Democracy$_{t-1}$	0.023	0.016	0.019	0.031	0.024	0.005	0.015	0.024	0.037	0.024	0.029	0.041
	(0.020)	(0.021)	(0.020)	(0.021)	(0.026)	(0.026)	(0.024)	(0.026)	(0.026)	(0.028)	(0.025)	(0.027)
Military dictatorship$_{t-1}$	0.009	0.005	0.013	0.015	−0.006	−0.019	−0.003	−0.006	0.009	0.001	0.012	0.011
	(0.023)	(0.024)	(0.022)	(0.023)	(0.025)	(0.026)	(0.024)	(0.025)	(0.025)	(0.026)	(0.024)	(0.025)
Civil war$_{t-1}$	0.014	0.006	0.010	0.014	0.025	0.014	0.026	0.025	0.016	0.007	0.014	0.016
	(0.015)	(0.013)	(0.014)	(0.015)	(0.018)	(0.016)	(0.017)	(0.017)	(0.015)	(0.014)	(0.015)	(0.015)
Interstate war$_{t-1}$	−0.050**	−0.032	−0.044**	−0.035*	−0.056***	−0.040**	−0.051***	−0.046**	−0.043**	−0.029	−0.039**	−0.036*
	(0.022)	(0.020)	(0.020)	(0.019)	(0.020)	(0.019)	(0.019)	(0.019)	(0.020)	(0.019)	(0.018)	(0.018)
Coup	0.069*	0.071**	0.066*	0.075**	0.081**	0.080**	0.084**	0.086**	0.078**	0.075**	0.080**	0.081**
	(0.035)	(0.032)	(0.035)	(0.034)	(0.040)	(0.034)	(0.037)	(0.036)	(0.036)	(0.034)	(0.036)	(0.035)
Protest$_t$	0.049***	0.050***	0.050***	0.049***					0.053***	0.053***	0.054***	0.052***
	(0.007)	(0.007)	(0.007)	(0.007)					(0.007)	(0.007)	(0.007)	(0.007)
Military spending/GDP$_t$					−0.007*	−0.001	−0.004	−0.001	−0.005	−0.002*	−0.003	−0.000
					(0.004)	(0.001)	(0.003)	(0.002)	(0.003)	(0.001)	(0.003)	(0.002)
ln(Military size)$_t$					0.032**	0.025**	0.033**	0.033***	0.025**	0.021*	0.026**	0.027**
					(0.013)	(0.013)	(0.013)	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)
Observations	4,503	4,349	4,328	4,284	3,904	3,801	3,740	3,704	3,829	3,719	3,665	3,629
Kleibergen-Paap Wald stat.	19.729	15.378	21.422	15.660	20.895	16.787	24.302	18.183	20.483	15.376	24.357	18.814
Country fixed effects	√	√	√	√	√	√	√	√	√	√	√	√
Year fixed effects	√	√	√	√	√	√	√	√	√	√	√	√
Cubic polynomial	√	√	√	√	√	√	√	√	√	√	√	√

Note: Two-stage least squares estimates using weather shocks as instruments are reported. All models include country fixed effects, year fixed effects, and cubic polynomials of the number of years since the last coup attempt. Standard errors are clustered at the country level (in parentheses). The Stock-Yogo critical values (with a 5 percent significance level) are 11.59 and 19.93 for 15 and 10 percent maximum bias in size, respectively.

*p < .10, **p < .05, ***p < .01.

Table 4. Exploring Possible Channels 2.

	Gleditsch	Maddison	Penn World Table	World Development Indicators
			DV: Popular protests	
GDP growth$_{t-1}$	–7.161***	–7.036**	–6.274***	–5.882**
	(2.402)	(2.780)	(2.314)	(2.595)
Observations	4,483	4,327	4,307	4,266
Kleibergen-Paap Wald statistic	23.362	15.359	20.933	16.201
			DV: Military expenditure/GDP	
GDP growth$_{t-1}$	–1.043	–1.074	–0.334	–4.154
	(1.489)	(1.860)	(1.327)	(4.947)
Observations	3,854	3,752	3,695	3,676
Kleibergen-Paap Wald statistic	17.549	15.346	24.052	15.304
			DV: Total military expenditure	
GDP growth$_{t-1}$	0.236	0.569	0.622	0.548
	(0.816)	(0.991)	(0.861)	(1.093)
Observations	3,876	3,774	3,714	3,677
Kleibergen-Paap Wald statistic	15.820	14.846	22.514	15.319
			DV: Military size	
GDP growth$_{t-1}$	–0.914*	–1.005*	–0.669	–0.662
	(0.479)	(0.519)	(0.426)	(0.473)
Observations	3,982	3,862	3,818	3,775
Kleibergen-Paap Wald statistic	17.154	14.098	21.193	14.115
Country fixed effects	√	√	√	√
Year fixed effects	√	√	√	√
Cubic polynomial	√	√	√	√
Lagged dependent variable	√	√	√	√

Note: Two-stage least squares estimates using weather shocks as instruments are reported. All regressions control for the full set of baseline controls. Standard errors are clustered at the country level (in parentheses).
*p < .10, **p < .05, ***p < .05.

soldier and when I include the change, not level, of these variables (see the online appendix). Moreover, Table 4 shows that economic growth instrumented by weather shocks does not significantly affect military expenditures and military size. All the results suggest that the effect of economic shocks, induced by weather shocks, on coup attempts does not operate through effects on military spending and military size. When I include both mass protests and military variables (columns 9 to 12 of Table 3), GDP growth is significant only at the 10 percent level, and its magnitude declines.

I also explore whether the effects of economic shocks on coup attempts vary across different contexts. To begin with, it is conceivable that when a country is less developed, economic recession is more likely to trigger a coup attempt, since its politics are less stable and institutionalized. Likewise, different types of political regimes may respond differently to shocks in economic growth. When there are institutionalized procedures to redress grievances, and the control of the military by a civilian authority is well established, economic slowdown may not trigger coup attempts.

To test whether there are potential conditional effects of economic growth on coup attempts, I investigate how the level of economic development and political regime influence the relationship between economic growth and coup attempts. Following Dell, Jones, and Olken (2012), I construct a dichotomous indicator for a poor country defined as a country whose GDP per capita is below the median GDP per capita in its initial year in the data set. I add an interaction term, GDP per capita growth $\times$ poor, to the baseline specification, and then instrument GDP per capita growth and the interaction term with the weather instruments, rainfall $\times$ poor, and temperature $\times$ poor. The top panel of Figure 2 displays the estimated coefficients of GDP growth and 90 and 95 percent confidence intervals by the level of development. The effects of GDP growth are estimated precisely and are consistently negative only in poor countries. Yet the coefficient for the interaction term is negative and statistically insignificant, indicating that the difference between the two groups is statistically insignificant.[16]

The second row of Figure 2 tests the modifying effect of political regimes by comparing democracies and autocracies. Only in autocracies does economic growth negatively and statistically significantly affect the likelihood of coup attempts, although the differential effect between democracies and autocracies is statistically indistinguishable from zero. The same pattern emerges across various subsamples: countries experiencing at least one year of autocracy, countries experiencing at least a coup attempt, and low-income countries.[17] My additional analyses indicate that a substantial part of this pattern seems to be driven by military dictatorships. As the third row of Figure 2 depicts, economic shocks have a greater effect on coup attempts in military dictatorships than in other political regimes, including democracies and nonmilitary dictatorships, and their estimates are not precisely estimated in nonmilitary regimes. I find similar results when I restrict the sample to dictatorships or low-income countries. On the other hand, the comparison between

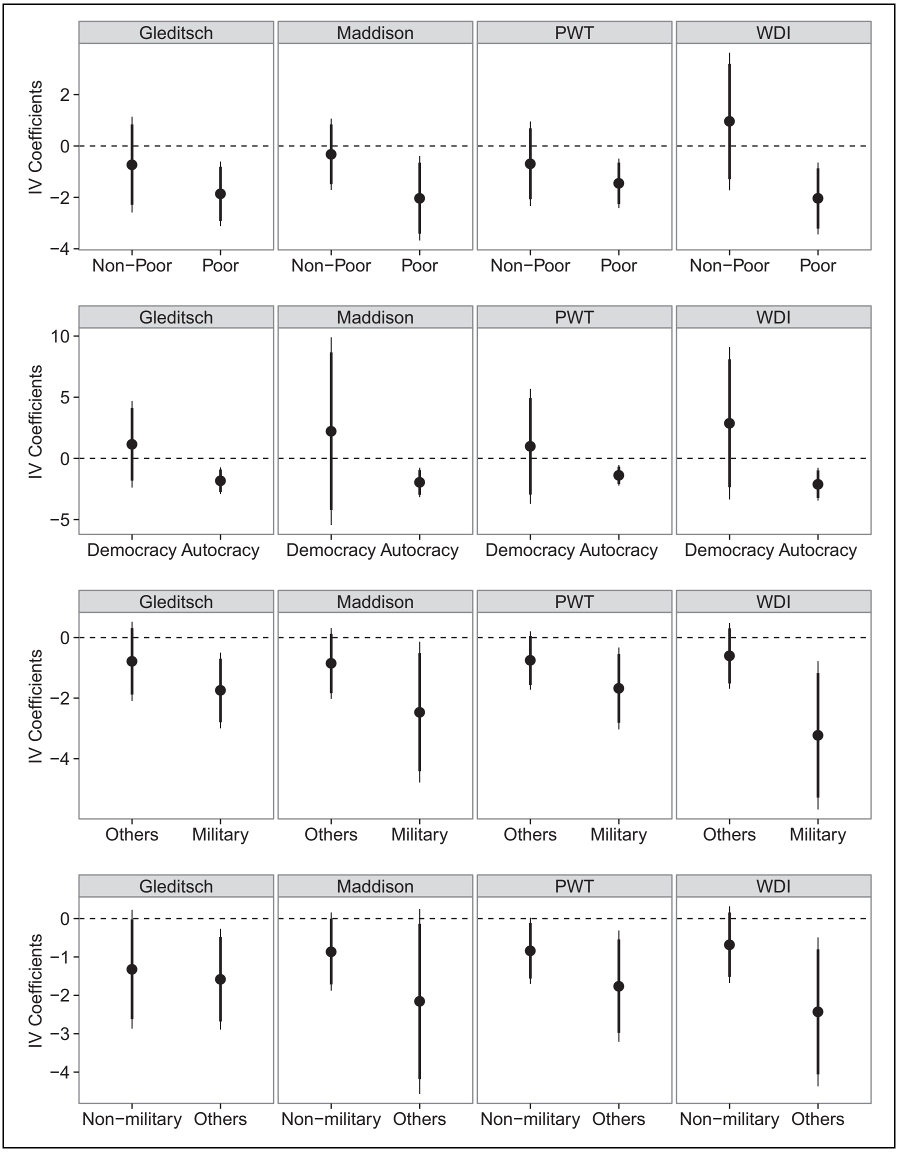

Figure 2. Conditional effects of economic growth on coup attempts.

Note: Each graph displays the IV coefficient estimates of GDP per capita growth according to different characteristics of a country. Dots represent the point estimates, and the associated horizontal thin (thick) line segments show the 95 percent (90 percent) confidence intervals for the estimate.

nonmilitary dictatorships and other political regimes, including democracies and military dictatorships, shows different results. The bottom row of Figure 2 shows that the effect of economic shocks is smaller and not distinguishable from zero at a conventional level in nonmilitary dictatorships while it is statistically significant in other regimes. These results suggest that the difference between democracies and autocracies is mainly driven by military dictatorships. This finding accords with existing accounts of military dictatorships. Geddes (1999) argues that military dictatorships are more likely to collapse in the face of economic difficulties than are nonmilitary dictatorships because they are more vulnerable to conflicts among ruling elites and economic difficulties are likely to worsen interregime conflicts. Other studies also find that military dictators' tenures tend to be shorter and are more likely to be terminated by coups (Gandhi 2008). This is because officers emphasize the survival and unity of the military as institutions over anything else (Geddes 1999) or military regimes tend to lack institutions, such as political parties, which allow them to co-opting opponents and mobilize the population (Nordlinger 1977).

It is worth reiterating that most of these differential effects are nowhere near statistical significance. The lack of statistical significance in differential effects of economic shocks may be due to the weak first-stage relationship between weather instruments and GDP per capita growth. The Kleibergen-Paap F statistics are quite low, especially in the models including the interaction term of GDP per capita growth and democracy (close to 1; see the online appendix).

Robustness Checks

I perform several robustness checks, all of which are reported in the online appendix.

Using Alternative Measures of Coups

I examine the effect of economic growth on alternative measures of coups. I use successful coups, instead of coup attempts, to explore whether GDP per capita growth rates, induced by weather shocks, have the same effect on successful coup incidence. Second, I use measures of coup attempts obtained from Center for Systemic Peace (CSP) coup events (Marshall and Marshall 2010). Similar results are found, showing that the previous findings are not driven by the specific measure of coups. As in Table 2, the estimates of economic growth are smaller in magnitude when antigovernment protest is controlled for and greater when military spending is controlled for.

Using a Negative Growth Indicator

I create an indicator for negative growth and use it instead of GDP per capita growth rate. I find a similar pattern of results as in the previous models. OLS and logit estimates are positive, but their sizes and levels of statistical significance vary by different GDP data sources. The 2SLS estimates are positive and significant at the 1 percent level regardless of GDP data. Moreover, the magnitude of the IV estimates is larger than that of non-IV estimates.

Replacing log(GDP_{t-2}) with Alternative Measures of Development Level

In the primary specifications, I include a natural log of real GDP per capita in year $t - 2$ to capture the level of economic development. This variable is not instrumented, and thus, there is a concern of endogeneity bias. I reestimate three different specifications to see how this variable affects the estimates on the growth variable. Following Burke and Leigh (2010), first, I construct and use a country-specific development-level variable instead of the natural log of real GDP per capita in year $t - 2$. They code this variable as 0 when a country's GDP per capita in year $t - 2$ is within thirty log points of its sample average in the same year, 1 (-1) when 30 to 60 log points greater (smaller) than its sample average, and 2 (-2) when 60 or more log points above (below) its sample average. Second, I just drop ln(GDP per capita) from the primary specifications. Finally, I include a log of real GDP per capita in the initial year when a country enters the data set, and estimate the specifications without country fixed effects, since the initial GDP is time-invariant. Results demonstrate that estimates on GDP growth are qualitatively identical to those reported in the primary specifications of Table 2.

Different Estimation Methods

To ensure robustness to weak instruments, I use three alternative IV estimators—Fuller 1, Fuller 4, and limited information maximum likelihood (LIML) estimators. These estimators are known to be more robust to weak instruments than 2SLS. Results are almost identical to 2SLS estimates reported in Table 2. In addition, 2SLS's 95 percent confidence intervals include all the coefficient estimates. I conclude that the main finding is not undermined by weak instruments. Second, I estimate the IV probit models, including all the baseline controls except country fixed effects. Instead of country fixed effects, I control for regional fixed effects when I estimate the IV probit models. I find that the signs of all economic variables are as expected and statistically significant at the 1 percent or 5 percent levels. Finally, I control for spatial correlation by using the multiway clustering method (Cameron, Gelbach, and Miller 2011) allowing for arbitrary residual correlation in both country and time dimensions. Estimation results are nearly identical to the baseline results.

Nickell Bias

I address the so-called Nickell bias that a model including a lagged dependent variable in the presence of fixed effects is inconsistent. To see how the Nickell bias influences the main finding, I estimate equation (1) without the lagged dependent variable and restrict the analysis to countries with more than twenty time-series observations, since this bias is known to be small when the number of periods (T) is relatively large. I obtain qualitatively similar results.

Examining the Direct Effect of Weather Disasters on Coups

As mentioned previously, I test for the possibility that weather disasters such as floods directly increase the risk of coups. To this end, I estimate several reduced-form regressions. First of all, I test the effect of droughts and floods on coups with and without the baseline control variables. I include an indicator for rainfall deviations above certain percentiles (1 or 5 percent) in equation (1). I do not find any indication that weather disasters are associated with an increased likelihood of coups. Contrarily, I find that the top 1 and 5 percentiles of rainfall shocks are negatively associated with the risk of coups. Similarly, the squared rainfall deviation is not significantly associated with a coup attempt.

I also test for the nonlinear relationship between rainfall and economic growth. It is conceivable that rainfall and economic growth have an inverse parabola relationship, with floods having detrimental effects on economic growth. The inclusion of the quadratic term slightly decreases the magnitude of the effects of economic growth and the F-statistics of the first stage regression, but does not change the main finding.

Conclusion

This article attempts to narrow the disparity between theory and evidence. Conventional wisdom says that poor economic performance undermines the legitimacy of the incumbent government and thus opens a window of opportunity for a coup attempt. Many scholars accept this wisdom as a stylized fact. However, that relationship has not been consistently supported by cross-national evidence. Some empirical studies end up finding an insignificant relationship between economic growth and coups. I argue that this failure of finding consistent results is caused by theoretical underspecification and little attention to measurement error in GDP per capita data. Scholars have simply estimated the effect of GDP per capita growth on the incidence of coups. Existing studies ignore the fact that persistent as well as transitory shocks drive GDP growth rates and that the two different types of shocks may have different impacts on coups. Furthermore, GDP data contain substantial measurement error, particularly in less developed countries where most coups have occurred. The estimated effects of GDP growth on coups vary across different data on GDP. This implies that existing studies could suffer substantial attenuation bias due to measurement error in GDP data.

To overcome these two problems, I exploit within-country rainfall and temperature variation as sources of exogenous and transitory variation in income. This helps me identify a short-run causal effect of economic shocks on coups. First, strongly mean-reverting weather shocks help to isolate only transitory shocks from income shocks, leaving out the wealth effect of persistent income shocks on coups. Negative transitory economic shocks are expected to raise the probability of coup attempts, which is not necessarily true of persistent shocks. Second, the IV estimation is more robust to measurement error than the noninstrumental estimation.

I find empirical results consistent with my argument. Non-IV estimates on GDP growth are close to zero, and their magnitude and statistical significance change across different GDP data. However, economic growth, driven by transitory weather shocks, has a substantially and statistically significant impact on the likelihood of coup and coup attempt incidence. This also contrasts with the finding that economic growth, instrumented by oil price shocks to capture permanent shocks to GDP growth, does not lead to a greater likelihood of coup attempts. Furthermore, IV estimates are quite robust to different GDP measures.

Empirical results presented here fit well with the previous finding that transitory negative shocks to the economy trigger democratic regime changes (Burke and Leigh 2010; Brückner and Ciccone 2011). The effect of economic shocks on coups is found to be negative and statistically significant only in autocracies. This implies that negative economic shocks could open up a window of opportunity for democratization by increasing the risk of violent leader removal and destabilizing autocratic regimes.

However, the mechanisms through which economic growth affects coup attempts should be studied in greater detail. This article finds that economic growth indirectly affects coups through social unrest, but that it does not operate through the channel of military expenditures. That said, it is likely that social unrest does not fully explain the channel from economic growth to coups, implying that the causal path should be explored further. Even after controlling for antigovernment mobilizations, GDP per capita growth affects coups. Hence, the measure of social unrest may not fully capture the legitimacy loss of the incumbent government. In addition, negative economic shocks could directly influence the military or ruler's ruling members' beliefs regarding the ruler's competence or public support for him. Therefore, the relationship between economic growth and coups deserves additional attention in the future.

Author's Note

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Notes

1. Many scholars mistakenly cite Londregan and Poole (1990) as supporting evidence for the negative effect of economic growth on coups. However, Londregan and Poole find only the level of GDP per capita, not GDP per capita growth, to be significantly associated with coups.
2. That is, a below-average (above-average) shock in one period is highly likely to be followed by above-average (below-average) shocks in subsequent periods.
3. Acemoglu, Ticchi, and Vindigni (2010) posit this assumption when they explain the emergence of military dictatorships and the effects of a powerful military in a dictatorship on the subsequent democratization and democratic consolidation. Similarly, Svolik (2012) also adopts this assumption to explain military intervention in politics.
4. I borrow this insight from the recent literature on civil conflict (Bazzi and Blattman 2012; Besley and Persson 2008; Chassang and Padró-i-Miquel 2009; Fearon 2007).
5. Both models are estimated with and without country fixed effects. I also include control variables discussed in detail subsequently.
6. They also recommend using GDP growth data measured at domestic prices, not at purchasing power parity (PPP) prices, if one is interested in economic growth. The GDP growth measure from the WDI is based on domestic prices. For a cross-country comparison in the level of GDP per capita in a particular year, they recommend using the version of the PWT, whose base year is closest to the year of one's interest.
7. I regress the change of rainfall (temperature) shocks on its lagged change, controlling for country fixed effects, year fixed effects, and country-specific trends. The coefficient on the lagged dependent variable is -0.45 (-0.38). This shows that rainfall and temperature levels are strongly mean-reverting. On the other hand, the estimated coefficient on the lagged oil price shock is only -0.05, showing that it is reverting very slowly to its average level. To illustrate difference between rainfall (or temperature) and oil price, I present yearly trends of rainfall, temperature, and oil price levels in the online appendix.
8. Instead of the median share of agriculture, I employ alternative measures such as the average share of agriculture and the share of agriculture in the first year that a country enters the data set. These different strategies produce similar results.

9. Using these two different weather shocks is advantageous, since IV regressions estimate the so-called local average treatment effect (LATE), implying that each instrument captures different sources of variation in national income. As discussed previously, Dell, Jones, and Olken (2012) show that temperature significantly affects industrial output and aggregate investment. This means that the effect of temperature shocks is not limited to agricultural output and that temperature shocks can complement rainfall shocks. In addition, rainfall and temperature instruments are not highly correlated (−0.16). The use of two different weather shocks ensures that estimates do not pick up the effect of only one type of economic shock.
10. Contrary to this conjecture, I find that the top fifth percentile of rainfall shocks is negatively associated with the risk of coups. This finding lends support to the identification assumption of my empirical strategy.
11. Differential trends across countries or regions, correlated with income growth as well as coups, could be potential confounders. For a robustness check, I control for region-specific or country-specific time trends. I obtain qualitatively identical results when I include them.
12. Another vexing problem in estimating the causal effect of economic growth is reverse causality. Previous studies show that political instability, including revolutions and coups, may detrimentally affect economic growth (Alesina et al. 1996). An irregular change in political leadership would lead to higher political instability, which is harmful for investment and growth. Economic factors may anticipate the occurrence of future political instability and decrease their investment accordingly (Miguel, Satyanath, and Sergenti 2004). However, reverse causality would result in inflated non-IV estimates of the effect of economic growth on coup attempts, which operates in the opposite direction of the bias due to the measurement error problem. This is why I do not emphasize reverse causality here.
13. Estimates on control variables are omitted to save space. Full results are reported in the online appendix.
14. The Stock-Yogo critical values (with a 5 percent significance level) are 11.59 and 19.93 for 15 and 10 percent maximal IV relative bias in the case of two instrumental variables (IVs) and 8.96 and 16.38 in the case of one IV.
15. This finding demonstrates that reverse causality, resulting in inflated OLS estimates, is not a concern here.
16. I find similar results when I compare the effects of economic growth between the IMF SDDS group and the nonSDDS group.
17. See the online appendix for these supplementary analyses.

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