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Do Output Contractions Trigger Democratic Change?

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ABSTRACT

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Does faster economic growth increase pressure for democratic change, or reduce it? Using data for 154 countries for the period 1963-2007, we examine the short-run relationship between economic growth and moves toward and away from greater democracy. To address the potential endogeneity of economic growth, we use variation in precipitation, temperatures, and commodity prices as instruments for a country's rate of economic growth. Our results indicate that more rapid economic growth reduces the short-run likelihood of institutional change toward democracy. Output contractions due to adverse weather shocks appear to have a particularly important impact on the timing of democratic change.

NON-TECHNICAL SUMMARY

For outsiders wishing to encourage moves to democracy, it is important to know whether growth shocks affect the chance of democratization. We find that they do. Countries are more likely to democratize after a recession than after a boom. Just as voters in rich countries are more likely to vote out a government in an economic downturn, so too citizens in an autocracy are more likely to demand democratic concessions when economic times are bad.

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"(Maya) kings got into trouble with their peasants if a drought came, because that was tantamount to the breaking of a royal promise" – Jared M. Diamond (2005, p. 168)

Do output contractions increase or decrease the short-run likelihood that a country will experience democratic change? While a sizeable literature in economics and political science has explored the relationship between a country's income level and its regime type, there is less evidence on whether the rate of economic growth positively or negatively affects the short-term likelihood of democratic change.

From a theoretical standpoint, the impact of economic growth on the short-run likelihood of democratic change could go in either direction. Periods of sluggish growth may increase the threat of popular protests against an incumbent regime and force ruling elites to allow increased democratic freedoms. Conversely, rapid growth could favor democratization by spurring expectations of political change among the citizenry. The pace of economic growth may also affect the probability of backsliding from democracy to autocracy.

A standard approach to estimating the short-run impact of growth on democratic change would be to model moves toward greater democracy as a function of lagged growth. But doing so may produce biased estimates if growth is partly a function of future democratic change. As the probability of regime change increases, growth could falter because autocrats appropriate as much of the country's economic resources as possible. Investors may delay investment projects because of concerns about political instability. Alternatively, rulers might attempt to stave off revolutions by engineering temporary economic booms. In any of these cases, a relationship

between growth in year *t*-1 and democratic change in year *t* will not necessarily tell us about the causal impact of growth on the likelihood of democratic change.

We estimate the effect of growth on the commencement of moves toward and away from greater democracy for a large sample of countries over the period 1963-2007. To address the potential endogeneity of economic growth, we use variation in precipitation, temperatures, and commodity export prices as instrumental variables (IVs) for economic growth. These instruments – which to our knowledge have not been used as instruments for economic growth in prior global studies – provide exogenous sources of variation that affect growth in different ways.[1] Changes in precipitation and temperatures principally affect the agricultural sector, a sector that accounted for more than half of the global labor force on average over the period, and particularly large shares of the labor forces of many autocratic countries.[2] Our commodity export price index is a country-specific weighted index of prices for a bundle of 50 export commodities, the most important of which are non-agricultural commodities such as oil. Rents from the export of these commodities typically accrue to a more concentrated share of the population and/or the government. The multiple-instrument approach allows us to compare the impact on regime changes of exogenous growth shocks that come through the weather and exogenous growth shocks that come through commodity revenues.

Our primary finding is that, once the potential endogeneity of economic growth is addressed, faster economic growth reduces the short-run likelihood of institutional change toward

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[1] Precipitation and commodity prices have been used as instruments for economic growth in studies of sub-Saharan Africa, such as those of Markus Brückner and Antonio Ciccone (2007, 2008).

[2] In 1961, autocratic countries on average had 69 percent of their labor forces engaged in agriculture (compared to 38 percent in democracies). In 2004, autocratic countries on average had 44 percent of their populations engaged in agriculture (compared to 28 percent in democracies). Source: World Resources Institute, http://earthtrends.wri.org (accessed 1 September 2009).

democracy, but has no discernible impact on the likelihood of institutional change toward autocracy. There is, however, evidence of heterogeneity in the impact of economic shocks on the likelihood of democratic change. Output contractions due to adverse weather shocks appear to be important triggers of democratic change, whereas commodity price shocks have insignificant implications for the likelihood of democratic change.

The short-run impact of the economic growth rate on the likelihood of democratic change is of substantial policy importance. While recent decades have seen the process of democratization accelerate globally, 48 countries (of more than 500,000 people) remained classified as autocratic in 2007 by the Polity IV dataset.[3] These countries account for more than one-third of the global population. Our results may be of use in predicting the timing of future moves toward democracy. The results are also relevant to understanding how policies that help or hinder growth are likely to affect the short-term likelihood of democratic change.

The remainder of this paper is organized as follows. In section I, we discuss theory on the impact of economic growth on the likelihood of democratic change. We outline our empirical approach in section II, and discuss data in section III. Estimation results are presented in section IV. In section V, we compare our results to prior empirical studies and country case studies. The final section concludes.

I. Theory on the short-run impact of growth on democratic change

A substantial literature looks at the effects of income on regime type. The mainstay hypothesis is Seymour M. Lipset's (1959) modernization hypothesis: that democracy emerges with economic

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[3] http://www.systemicpeace.org/polity/polity4.htm (accessed 15 July 2009).

development. The modernization hypothesis implies that higher incomes (the result of long-run growth) increase the likelihood of democratization. But the difficulty with long-run theories such as modernization theory is that democratization has tended to occur in sudden episodes, the timing of which has not been well explained.[4] Reversions to autocracy have also often been sudden events, commonly characterized by coups.

Peter Gourevitch (1986, p. 9-10) argues that economic crises create "points of critical choice" during which "critical realignments" may occur (p. 32-33). There are several reasons why realignments spurred by output contractions may favor democratization. The first of these is that if output contractions reduce the opportunity cost of joining a pro-democracy protest, they may facilitate the conversion of built-up discontent with an incumbent regime into active resistance. Negative growth shocks would thus result in temporary increases in the political power of ordinary citizens relative to ruling elites, increasing pressures for democratic change. Such a situation is depicted in a simple game theoretic model presented in Appendix II. The model's result is similar to those elsewhere, such as Daron Acemoglu and James A. Robinson's (2001, 2006) theory of political transitions, and Paul J. Zac and Yi Feng's (2003) dynamic general equilibrium model, in which democratic transitions are triggered by economic crises during which citizens have "nothing to lose" in demonstrating for a better government (p. 15). Strong growth, on the other hand, may increase the legitimacy of autocratic regimes and reduce the likelihood of protest.

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[4] Keith Jaggers and Ted R. Gurr (1995, p. 479) observe that "most shifts among regime types… have occurred quite abruptly". Michael Bratton and Nicolas van de Walle (1997, p. 4) report that democratization in African countries "happened rapidly". Edward Aspinall (2005, p. 240) notes that Indonesia's democratic transition "involved rapid escalation of protest, considerable violence, and the abrupt collapse of the government". The importance of short-run triggers of democratic change is highlighted by Benjamin F. Jones and Benjamin A. Olken (2009), who find that assassinations of autocrats on average leads to democratic change.

That output contractions heighten pressures for democratic change may also be due to a signal extraction problem citizens face if they cannot separate out the effects of exogenous shocks (luck) and good economic management (competence) on the rate of growth in their incomes (Alberto Alesina, Nouriel Roubini, and Gerald D. Cohen 1997). This signal extraction problem would mean that overall support for incumbent administrations is a positive function of the growth rate. There is evidence of a signal extraction problem in democracies: national leaders are more likely to win reelection when the world economy is strong (Andrew Leigh 2009). But citizens in democracies who are dissatisfied with the nation's economic performance have a clear safety valve: they can change governments at the ballot box. By definition, democratic institutions allow political change without regime change. Minxin Pei and Ariel D. Adesnik (2000, p. 147) argue that democracies are not particularly vulnerable to economic crises because, compared with a regime collapse, "a scheduled election is a less costly mechanism for punishing politicians for an economic crisis." In contrast, options to influence political change in autocratic countries are limited, and citizen protest during growth slowdowns may aim at changing the "rules of the game" in addition to changing the ruler. If true, growth slowdowns would increase the pressure for autocracies to democratize, but would not induce democracies to become autocracies.

Another avenue via which growth slowdowns may increase the likelihood of democratic change is by constraining government expenditure possibilities. The clientelistic nature of many autocratic regimes may make their continuation particularly dependent on the state of the economy. Stephan Haggard and Robert R. Kaufman (1997, p. 267) contend that "poor economic performance reduces the bargaining power of authoritarian incumbents" and "disrupts the political bargains rulers typically forge." Barbara Geddes (1999, p. 138) similarly argues that

shocks that "prevent passable economic performance" impede the ability of autocratic regimes to assuage and coerce important stakeholders.

Counter-arguments also exist. Samuel P. Huntington (1991) argues that rapid economic growth increases inequality, strains the social fabric, and leads to heightened demands for political participation (although Huntington recognized that recessions also undermine authoritarian regimes). Strong growth may also heighten expectations of political change. Conversely, poor economic performance in democracies may make it easier for coups to succeed, or may lead to calls for the establishment of bureaucratic-authoritarian regimes that focus on strong economic management (Guillermo A. O'Donnell 1973).

It is also possible that the effect of economic shocks on the likelihood of democratic change depends on the nature of the shock. Benjamin Smith (2006) conjectures that negative shocks to the oil sector may be less important in explaining sudden episodes of democratic change than those to other sectors: "even as regimes in oil-rich states have experienced some of the harshest economic shocks in the developing world in the last 30 years, few of them have collapsed, and even fewer have undergone transitions to democracy" (p. 55-56). Saudi Arabia, for example, went through almost a decade of negative growth in the 1980s, a time of falling oil prices, but faced little pressure to democratize. That many oil-rich countries have exhibited a stable form of autocracy has spawned a literature on the relationship between natural resources and democracy (see Michael L. Ross 2001, Jay Ulfelder 2007). Our approach of instrumenting economic growth with commodity prices provides a test of whether shocks to commodity revenues affect the timing of instances of democratic change.

II. Empirical approach

We use the Polity IV dataset to identify the commencement of substantial movements toward democracy and toward autocracy. Polity IV provides annual classifications of regime type for all nations with populations exceeding 500,000 people in 2007. Countries receive a POLITY score along a 21-point continuum from -10 (most autocratic) to +10 (most democratic). The POLITY score is an institution-based measure of regime type that reflects the competitiveness and regulation of political participation, the openness and competitiveness of executive recruitment, and constraints on the chief executive.

The primary advantage of Polity IV for our purposes is that it is the only measure of regime type that codes the precise date of the commencement of significant changes in regime classification (either toward or away from greater democracy). Polity IV flags these changes with a regime transition variable (REGTRANS). Polity IV's focus on identifying the commencement of regime transitions contrasts with other measures of democracy, such as the dichotomous regime indicator of Adam Przeworski et al. (2000) and the Freedom House Political Rights Index, which identify when transitions to or from democracy conclude (in the case of the Przeworski et al. measure) or when political rights of individuals are realized (in the case of the Freedom House measure). Because regime transitions are inherently messy, these processes often take several years (e.g. there may be an agreement to hold full democratic elections in one year, but the country does not go to the polls until a subsequent year).[5] In considering the effect of economic growth on the timing of instances of regime change, what matters most is identifying when transitions start, not

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[5] As an illustration of this, consider the case of Portugal. Polity IV codes a "major democratic transition" commencing in Portugal in 1974, and that the transition concluded in 1976. Przeworski et al. do not record the establishment of a new democratic regime until 1976. Freedom House also does not record an improvement in individuals' political rights until 1976. We think that it is appropriate to code 1974, the year of the Carnation Revolution, as the year of the commencement of democratization in Portugal, despite the fact that it did not immediately herald fully-functional democracy. See Appendix IV for additional examples.

when they finish. Accurately identifying the commencement of instances of regime change is particularly important in this study because we adopt an IV approach that uses mean-reverting weather instruments. An investigation of the short-run relationship between the rate of economic growth and the likelihood of regime change that uses a slow-responding measure of regime change may produce misleading results.[6]

Our primary specifications use dichotomous dependent variables, derived from the POLITY score, which flag the commencement of discrete and significant instances of democratic and autocratic change. We define a "democratic change event" as a 3 or more point increase in the POLITY score over 3 years or less that commences in year t, indicated by a positive REGTRANS score. These events cover the commencement of "major democratic transitions", "minor democratic transitions", and instances of "positive regime change" as defined by Polity IV. We define an "autocratic change event" as a 3 or more point decrease in POLITY score over 3 years or less that commences in year t, indicated by a negative REGTRANS score. This definition covers the commencement of "negative regime change" and "adverse regime transitions".[7] The commencement of democratic and autocratic change events is coded as 1, and other country-years are coded as 0. The mean democratic change event in our sample involves a 9-point increase in POLITY score, from -5 to +4. The mean autocratic change event involves a 10-point decrease in POLITY score, from +4 to -6.

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[6] The Przeworski et al. (2000) alternation rule, which requires an electorally-induced change in government before a country may be considered democratic, makes the use of that measure particularly problematic in a study of the triggers of democratic change. The Freedom House Political Rights Index (http://www.freedomhouse.org/template.cfm?page=439, accessed 10 July 2009) is an outcome-based measure of individuals' freedoms rather than a measure of regime type. We obtain similar linear probability model results using either measure, but insignificant IV results. See Appendix III for details.

[7] The commencement of interregnal periods is not considered to be an autocratic change event here, since such periods are not moves toward autocracy.

The primary reason for constructing these dichotomous measures of institutional change stems from the difficulty of treating regime changes that occur over more than one calendar year. A POLITY score is generally not available for years for which a regime transition has not come to a conclusion by December 31. Many events in our dataset are affected by this missing POLITY score issue. A POLITY score is also not provided for years in which countries are classified as being in interregnum or under foreign interruption at year end. The POLITY2 variable, designed for use in time-series contexts, uses coding rules that provide a misleading picture of the level of democracy during such periods (Thomas Plümper and Eric Neumayer 2009). Specifically, POLITY2 uses a linear interpolation of the POLITY score across the years of a multi-year transition. It also uses a "neutral" score of 0 for years of interregnum, which could be misinterpreted as a movement toward greater democracy in many instances. Our dependent variables best allow us to model the triggers of episodes of institutional change that may not necessarily occur within a single calendar year, and avoid conflating the effect of growth on the initiation of regime transitions with the effect of growth on mid-transition dynamics. We also investigate the robustness of our results to alternative specifications of the dependent variables.

For our democratic change event estimates, the primary model to be estimated is of the form:

$$(1) \qquad D(0,1)_{c,t} = \alpha DY_{c,t-1} + \mathbf{x}'_{c,t-j}\boldsymbol{\beta} + I_c + I_t + \varepsilon_{c,t}$$

The dependent variable equals 1 if a country (c) commences a democratic change event in year t and 0 if not, $DY_{c,t-1}$ is the real per capita economic growth rate in year t-1, $\mathbf{x}'_{c,t-j}$ is a vector of time-varying control variables for year t-1 or t-2 (i.e. j = 1 or 2), I_c is a vector of country fixed effects, I_t is a vector of year fixed effects, and $\varepsilon_{c,t}$ is an error term, with $E(\varepsilon_{c,t}) = 0$. We adopt a

similar approach for modeling autocratic change events, and in exploring links between anti-government protests, growth, and institutional change.

Equation (1) is estimated using both a fixed effects linear probability model (LPM) and a fixed effects logit model. A probit model is not estimated because it is not suited to a fixed effects treatment (William H. Greene 2000). The inclusion of country fixed effects takes account of the possibility that time invariant factors – such as colonial history, the date of independence, initial institutions, geography, religion, and culture – affect both economic growth and the likelihood of institutional change.

We present results for specifications that control for only country and year fixed effects, and specifications that also control for time-varying measures of development level (a sample-specific categorical variable, discussed below), education (the secondary school enrollment rate), demographic structure (the share of people aged 65 years and over), POLITY score, regime tenure in years, and the share of countries in the region that are democratic. The first three of these controls are measured at t-2 so that they are not affected by year t-1 growth, while the other three are measured at t-1. We employ a country-specific development level measure that equals 0 when a country's t-2 per capita gross domestic product (GDP) is within 30 log points of its sample average, +1 (-1) when 30-60 log points above (below) its sample average, and +2 (-2) when 60 or more log points above (below) its sample average. This variable allows us to control for long-run economic development (or decline) in estimates of the likelihood of year-t institutional change.[8] We also explore whether the short-run impact of economic growth on

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[8] Results are similar in specifications that use alternative definitions of this long-run development variable, or that control for either t-2 GDP per capita or log GDP per capita.

democratic or autocratic change differs according to a country's long-run development experience.

Despite the lagging of economic growth, it is possible that it is endogenous to the system. This endogeneity may operate through a number of mechanisms. Alesina et al. (1996), Przeworski et al. (2000), and others have shown that political instability is associated with slow growth. The mobilization of protestors in the build-up to regime transitions may hurt the economy. Such times may be characterized by uncertainty and low investment. Alternatively, leaders may increase public spending in the face of looming regime change and induce artificial booms. They may call on their mints to print additional money for this purpose. Further, regime competence may affect both growth and the likelihood of regime change. As we are unable to control for anticipation of regime change or factors such as regime competence, an IV approach is required to obtain a consistent estimate of the impact of economic growth on the likelihood of institutional change in the subsequent year. The IV approach also allows us to address potential attenuation bias arising from measurement error in national income data, for which there is significant evidence (Alan Heston 1994).

We use three IV strategies. The first follows Edward Miguel, Shanker Satyanath, and Ernest Sergenti (2004), who use rainfall variation as an instrument for economic growth in a study on the causes of conflict in Africa. There is evidence that precipitation is highly correlated with agricultural output (Charlotte Benson and Edward Clay 1998). Precipitation, exogenous to the system under consideration, is also of direct importance for other water-intensive economic activities, such as hydroelectricity generation. Similarly to Miguel, Satyanath, and Sergenti (2004), we instrument with the percentage change in precipitation, and the lagged percentage

change in precipitation. (The use of lagged precipitation variation reflects that high precipitation in one year may affect planting decisions and quantities of produce available for sale in the next.) To increase the strength of the first-stage identification, and because precipitation is more highly correlated with output in countries that are more dependent on rain-fed agriculture, we interact precipitation variation with the share of the labor force in the agricultural sector and the share of cropland that does not have irrigation (both measured in 1961). To be precise, we instrument economic growth in year t-1 with the percentage change in precipitation in years t-1 and t-2, each multiplied by the percentage of the labor force in agriculture in 1961 and the percentage of cropland without irrigation in 1961.

The second IV strategy is to instrument growth with temperature variation. Temperature, like precipitation, provides a source of exogenous variation with direct implications for economic activity. We instrument with the change in temperature in years t-1 and t-2. The sign of the coefficient on the temperature instrument in the first-stage regressions is expected to differ for cold countries (where crops grow better in warmer years) and warm countries (where crops grow better in cooler years). To allow for this, we multiply temperature variation by -1 for countries with an average temperature of less than 12ºC for the period 1960-1970.[9] The countries on either side of this cut-off are Azerbaijan (11.95ºC) and Spain (13.21ºC). We also interact temperature variation with the 1961 share of the labor force in agriculture to allow the effect of temperature variation on economic growth to be larger in countries that are more dependent on agriculture. As far as we are aware, this IV strategy has not been employed previously.

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[9] This operation affects 27 percent of country-years. The 12ºC cut-off maximizes the F statistic on the excluded instruments in our preferred specification (column 2, panel B, Table 4). Results are similar if cut-off temperatures of 11.5ºC or higher are used. We were unable to find a scientific basis upon which to draw a line between countries in which warmer-than-average temperatures aid plant growth and those in which colder-than-average temperatures aid plant growth. George H. Hargreaves and Gary P. Merkley (1998) present estimates of optimum temperature ranges that vary from 15-20ºC for crops such as Arabica coffee and wheat to 30-35ºC for crops such as sugar cane and rice.

The third IV strategy is to instrument economic growth using changes in commodity export prices. There is evidence that countries grow faster when the prices of their exports increase, and since countries are typically price-takers for their commodity exports, world price variations are exogenous in most instances (Angus S. Deaton and Ronald I. Miller 1995). We construct a country-specific commodity export price index for a bundle of 50 agricultural and non-agricultural commodity exports using a similar methodology to that of Deaton and Miller (1995), Jan Dehn (2000), Brückner and Ciccone (2007), and Paul Collier and Benedikt Goderis (2007). For each country, we calculate commodity weights based on the share of each of the 50 commodities in the country's export bundle in 1995. These weights are then held fixed over time and applied to deflated world price indices to produce a country-specific arithmetically weighted index of commodity export prices. Maintaining constant weights means that changes in economic structure, a potential source of endogeneity, are not captured by the instrument. To allow the effect of commodity export price fluctuations on growth to be larger for countries that are more dependent on commodity exports, we weight the differenced log of the index by the value of exports of the 50 commodities as a share of GDP in 1995. A full description of the instrument is provided in Appendix V. An additional lag of the instrument is statistically insignificant in the first-stage regressions, so to minimize weak instrument problems, we do not include it.

The use of three IV strategies is appealing because the instruments are correlated with economic growth in different ways, allowing us to estimate IV regressions based on different local average treatment effects. The correlation between the precipitation and temperature instruments is -0.19, that between the temperature and commodity price instruments is 0.01, and that between the precipitation and commodity price instruments is -0.02. Precipitation and temperature variations

are supply-side factors likely to be most strongly correlated with growth in agricultural output. As discussed, the agricultural sector accounts for a large share of the labor force in most countries, and as such economic shocks to this sector are of general relevance. Further, there is evidence that agricultural growth has a causal impact on the growth rate of other sectors of the economy (Richard Tiffin and Xavier Irz 2006), that precipitation in hinterlands has a flow-on impact on urban incomes (J. Vernon Henderson, Adam Storeygard, and David N. Weil 2009), and that weather shocks have an important impact on industrial output (Melissa Dell, Jones, and Olken 2008). Weather shocks thus provide a source of exogenous variation that is likely to be correlated with the incomes of a fairly broad representation of the population of many countries. The commodity price index, on the other hand, is dominated in value terms by non-agricultural commodities such as oil. Variation in this instrument may affect a narrower base of incomes.

The identification assumption is that the instruments are not correlated with the error term in (1); in other words, that shocks from the weather and commodity export prices only influence the likelihood of democratic and autocratic change through their impact on growth. Admittedly, each of our instruments has its limitations, and one can imagine ways in which this assumption might be violated. For example, the weather and commodity prices could have direct impacts on street protests.[10] Or commodity price swings may have a disproportionate impact on government revenues, and this might directly affect regime stability. Although the data suggest that the

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[10] There is evidence from the US that the weather affects street protests. J. Merrill Carlsmith and Craig A. Anderson (1979), for instance, find that higher temperatures led to more riots during the period 1967-1971. (Recall however that our instrument multiplies temperature changes by -1 for countries with a 1960-1970 mean temperature below 12°C.) William J. Collins and Robert A. Margo (2004) show that cities that received more rainfall in April 1968 (the month Martin Luther King was assassinated) had less severe race riots. There is also evidence that commodity price movements have sparked anti-regime protests, such as those in Myanmar in 2007 (Kyaw Yin Hlaing 2008). Export prices may be less relevant for domestic protests, however, as they are not always directly transmitted to domestic markets (even if they do directly affect national income).

growth channel is quantitatively important, they do not allow us to dismiss such explanations.[11]

To the extent that our instruments do not satisfy the exclusion restriction, our results nevertheless demonstrate a strong relationship between weather variation and the timing of democratic change which merits attention in future research.

An issue of concern is potentially weak instruments. We present estimates using the Fuller 1 estimator (Wayne A. Fuller 1977), which is a bias-corrected limited information maximum likelihood estimator, and provides the most unbiased estimates for inference purposes when instruments are potentially weak (James H. Stock, Jonathan H. Wright, and Motohiro Yogo 2002, Jinyong Hahn and Jerry Hausman 2003, James H. Stock and Motohiro Yogo 2005). Two-stage least squares estimates are similar. For all regressions, we show the partial R squared and F statistic for the excluded instruments. This F statistic is the Stock-Yogo weak instruments test statistic. 5 percent significance level critical values for tests of both 30 percent and 5 percent maximal Fuller relative bias are also presented. Any global trends in the IVs (e.g. due to climate change) are accounted for by the inclusion of year fixed effects.

III. Data

As discussed, our source of data on regime type is Polity IV. Countries are included in our dataset for years in which they received a normal POLITY score in the prior year. This means that countries experiencing foreign interruption, interregnum, or transition at the start of year t (end of year t-1) are excluded from the sample. Countries experiencing foreign interruption at the

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[11] One way to investigate the direct impact of our instruments on protests is to estimate the reduced-form relationships between the instruments and the likelihood of anti-government protests of at least 100 people, as measured by Databanks International (http://databanksinternational.com; accessed 13 August 2008). In specifications that include country and year fixed effects, we find no significant impact of precipitation, temperature, or commodity export prices on the number of anti-government protests for our sample, with or without controlling for per capita GDP growth (results not reported).

end of year *t* are also excluded from the sample as they do not receive a POLITY2 score for this year (this affects only 2 observations). Years in which regime change coincided with the natural death of the national leader are identified using the Archigos dataset (Henk E. Goemans, Kristian S. Gleditsch, and Giacomo Chiozza 2009) and excluded from the dataset (this affects three democratic change events: Spain 1975, Nigeria 1998, Croatia 1999). GDP data are sourced from the World Bank.[12] Precipitation and temperature data are sourced from the TYN CY 1.1 dataset of Timothy D. Mitchell et al. (2004), which was constructed by geographically locating meteorological stations according to grids of 0.5º latitude and longitude, allocating grid boxes to countries, and calculating the mean of grid boxes for each country. A list of data sources and variable definitions is provided in Appendix VI.

The full dataset consists of 5,207 observations for 45 years (1963-2007) and 154 countries. However, countries are excluded from estimations for years in which a positive value of the dependent variable is technically infeasible: in years in which the lagged POLITY score was 8 or higher in the case of democratic change regressions, and in years in which the lagged POLITY score was -8 or lower in the case of autocratic change regressions.[13] The weather data are available to 2000, limiting the IV estimation period to 1963-2001. (Commodity price IV results for 1963-2007 are similar to those for 1963-2001.) The full sample includes 132 democratic change events and 70 autocratic change events. These events are listed in Appendix I. Summary statistics are presented in Table 1. Autocratic countries that did not experience a within-sample democratic change event, such as China, tended to grow faster than other autocratic countries.

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[12] http://go.worldbank.org/6HAYAHG8H0, accessed 25 August 2009.
[13] Results are similar if samples are restricted so that democratic change regressions include only countries that were autocratic (POLITY<1) in year *t*-1 and autocratic change regressions include only countries that were democratic (POLITY>0) in year *t*-1.

Table 1-Summary statistics

	Full sample		Sub-sample 1: Autocracies		Sub-sample 2: Autocracies with no democratic change	
	Full estimation sample for period 1963-2007		Countries classified as autocratic in *t*-1		Countries classified as autocratic in *t*-1 that did not record a democratic change event	
	Mean	*(St. dv.)*	*Mean*	*(St. dv.)*	*Mean*	*(St. dv.)*
GDP per capita growth$_{t-1}$	2.0	(5.9)	1.7	(7.2)	2.7	(8.9)
Country-specific development level$_{t-2}$	0.0	(0.6)	-0.1	(0.6)	0.0	(0.9)
Secondary school enrollment rate$_{t-2}$ (percent gross)	50.0	(33.7)	33.5	(27.0)	44.3	(29.7)
Population aged 65 years and above$_{t-2}$ (percent)	6.0	(4.2)	3.8	(1.9)	3.6	(2.2)
POLITY score$_{t-1}$	1.1	(7.6)	-6.4	(2.5)	-7.2	(2.4)
Tenure of regime$_{t-1}$ (years since the most recent 3-point change in POLITY score over a period of 3 years or less, or transition period)	22.8	(29.4)	16.3	(17.4)	21.3	(16.8)
Democracy in region$_{t-1}$ (percent of countries)	48.5	(30.6)	28.4	(20.1)	28.4	(23.4)
Labor force in agriculture in 1961 (percent) [Φ]	57.1	(27.2)	69.5	(22.7)	61.3	(29.1)
Non-irrigated cropland (percent) in 1961 [Ψ]	88.7	(18.7)	86.1	(23.6)	77.3	(27.6)
Commodity export bundle in GDP in 1995 (percent) [Ω]	8.8	(11.2)	12.2	(13.5)	16.7	(16.9)
Precipitation variation$_{t-1}$ (without interacting with Φ or Ψ) (percent)	2.1	(23.3)	2.4	(26.5)	5.1	(38.5)
Temperature variation$_{t-1}$ (without interacting with Φ) (°C)	0.0	(0.5)	0.0	(0.5)	0.0	(0.5)
Commodity export price variation$_{t-1}$ (without interacting with Ω) (percent)	-0.2	(14.8)	-0.5	(16.9)	0.7	(19.3)
Anti-government protests$_{t-1}$	0.6	(1.9)	0.4	(1.4)	0.3	(1.2)
	Sum		*Sum*		*Sum*	
Democratic change events	132		119		0	
Autocratic change events	70		14		3	
Countries	154		106		30	
Observations	5,207		2,456		713	

Notes: Precipitation and temperature data are available to 2000. Countries are classified as autocratic if their POLITY score is 0 or less.

The share of countries classified as democratic (positive POLITY score) by Polity IV is presented in Figure 1. The figure also shows the democratic and autocratic change events that are the focus

of the estimations. Since the mid-1970s, instances of democratic change have exceeded instances of autocratic change, and the share of countries classified as democratic has increased. Huntington (1991) refers to this modern period of democratic progress as the "third wave of democratization".

Figure 1. Global democratic change, 1963-2007
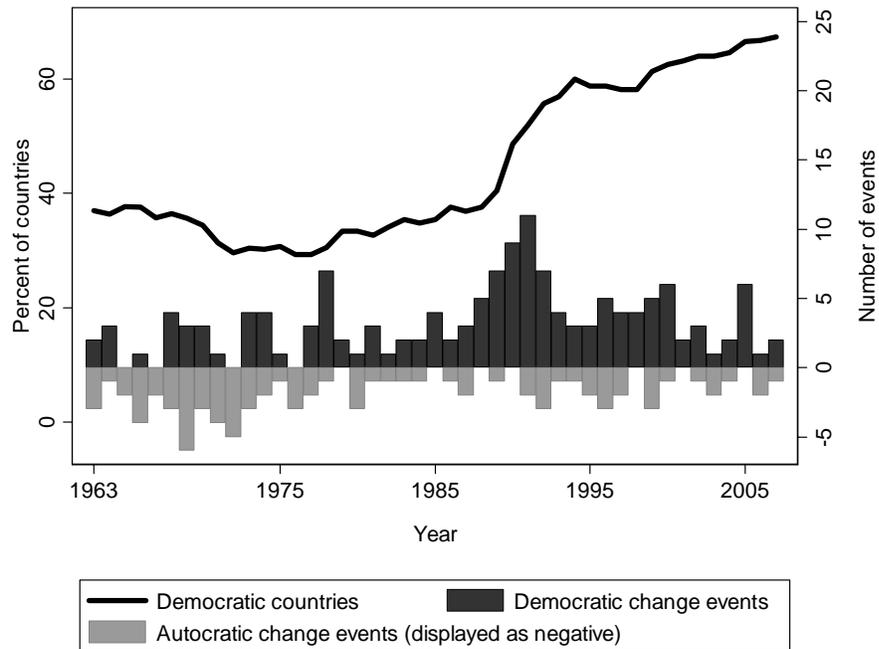
Notes: Series calculated from Polity IV. Democratic/autocratic change events flag the commencement of moves to/from democracy of 3 POLITY points or more. Events include those not in the estimation sample. Countries are classified as democratic if their POLITY2 score is positive.

Figure 2 displays the average GDP per capita growth rate and the average number of anti-government protests in the lead-up to instances of democratic and autocratic change. The protests data are sourced from Databanks International, based upon mentions in the *New York Times*.[14] Economic growth tends to fall in the lead-up to regime transitions (panel A of Figure 2). For

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[14] We use this dataset since it covers substantially more countries and years than other data on political protests. However, it is possible that the *New York Times'* coverage of protests may be incomplete, or may even have shifted in ways that are correlated with economic growth and/or democratic change. For a discussion of the issues that arise in coding political protests, see Taehyun Nam (2006).

instance, the average per capita GDP growth rate in the year immediately preceding the 132 democratic change events is 0.5 percent (median of 1.3 percent), substantially less than the average growth rate in all other years (2.0 percent; median 2.3 percent). The number of protests tends to increase in the lead-up to regime transitions, particularly instances of democratic change (panel B of Figure 2).

Figure 2. Average GDP per capita growth and number of anti-government protests in the lead-up to democratic and autocratic change events
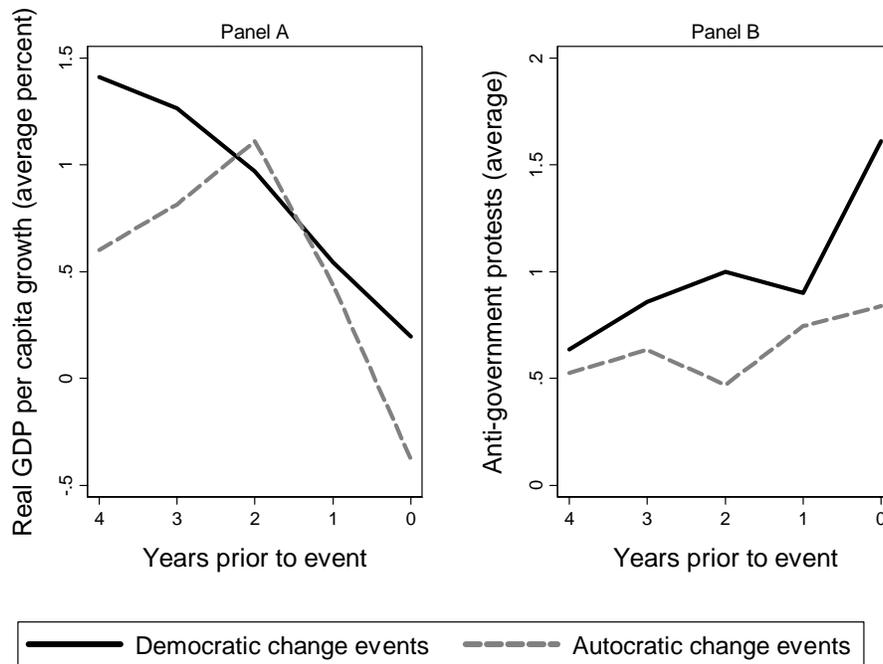
Notes: Calculations using World Development Indicators and Databanks International. Polity IV used for identifying democratic/autocratic change events. 4 years of lagged data are not available for all events.

IV. Results

A. *Fixed effects LPM and logit results*

Fixed effects LPM results for the determinants of democratic and autocratic change events are presented in Table 2. Standard errors are robust to heteroscedasticity and are clustered at the country level to allow for possible serial correlation. The results in column 1 (with country and

year fixed effects) and column 2 (with additional controls) indicate that a one percentage point increase in the GDP per capita growth rate on average reduces the likelihood of a democratic change event in the next year, among countries for which such an event is possible, by around 0.1 percentage points. This amounts to a 2-3 percent reduction in the likelihood of democratic change, an effect that is significant at the 5 percent level.

Table 2-Fixed effects LPM results

Dependent variable	Democratic change events		Autocratic change events	
	(1)	(2)	(3)	(4)
GDP per capita growth$_{t-1}$	-0.0008**	-0.0011**	-0.0009**	-0.0007*
	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Country-specific development level$_{t-2}$		-0.0112*		-0.0015
		(0.0068)		(0.0044)
POLITY score$_{t-1}$		-0.0099***		0.0075***
		(0.0015)		(0.0011)
Tenure of regime$_{t-1}$ (years)		0.0003		0.0015***
		(0.0006)		(0.0004)
Democracy in region$_{t-1}$ (percent of countries)		0.0001		-0.0007***
		(0.0005)		(0.0002)
Country and year fixed effects	Yes	Yes	Yes	Yes
R^2	0.028	0.060	0.016	0.053
Chow test: Null hypothesis is that the growth coefficient is the same as in the corresponding democratic change regression (p-value)	-	-	0.97	0.53
Observations	3,398	3,398	4,438	4,438
Number of events	132	132	70	70
Countries	125	125	147	147
Years: 1963-2007				

Notes: A democratic change event is the year-*t* commencement of a 3 or more point increase in POLITY score that occurs within 3 years, flagged by a positive REGTRANS score; an autocratic change event is the year-*t* commencement of a 3 or more point decrease in POLITY score that occurs within 3 years, flagged by a negative REGTRANS score. Robust standard errors clustered by country are in parentheses. GDP per capita growth is scaled so that one percentage point of additional growth is 1, not 0.01. The country-specific development level variable equals 0 when a country's *t*-2 per capita GDP is within 30 log points of its sample average *t*-2 per capita GDP, +1 (-1) when 30-60 log points above (below) its sample average, and +2 (-2) when 60 or more log points above (below) its sample average. Columns 2 and 4 control for the secondary school enrollment rate (percent gross) and the percent of the population aged 65 years and above in year *t*-2. The sample in columns 1 and 2 excludes country-years in which the *t*-1 POLITY score exceeds 7. The sample in columns 3 and 4 excludes country-years in which the *t*-1 POLITY score is less than -7. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

The results in columns 3 and 4 of Table 2 indicate that faster growth also reduces the short-term likelihood that a country will experience democratic reversals. An additional percentage point of per capita GDP growth on average reduces the likelihood of an autocratic change event by almost 0.1 percentage points, equal to a 5-6 percent reduction in this likelihood. The LPM results thus indicate that the rate of economic growth has similar implications for the likelihood of regime changes in the directions of both greater democracy and greater autocracy. Indeed, Chow tests fail to reject the null hypothesis that the impacts of growth on the likelihoods of democratic change and of autocratic change are equal.

The results on the control variables in Table 2 indicate that countries are less likely to undergo democratic change after experiencing significant advances in their development level (contrary to the modernization hypothesis). While the long-run impact of development on democratic change is not the focus of this paper, and we have not instrumented for the development level, this result is similar to those of others, such as Renske Doorenspleet (2004). Countries with higher POLITY scores are less likely to experience democratic change and more likely to experience autocratic change, all else equal. Regime duration and the share of countries in the region that are democracies have insignificant impacts on the likelihood of democratic change. There is evidence, however, that regime duration increases the likelihood of autocratic change and that countries in more democratic regions are less likely to experience reversions toward autocracy.

Fixed effects logit results are presented in Table 3. Note that fixed effects logit estimations exclude countries that do not experience within-sample variation in the dependent variable

(Greene 2000). We present our fixed effects logit results as odds ratios.[15] An odds ratio of 1 indicates that a conditional increase in the independent variable is not associated with any change in the dependent variable, while an odds ratio above (below) 1 indicates that an increase in the independent variable raises (lowers) the dependent variable.

Table 3-Fixed effects logit results

Dependent variable	Democratic change events		Autocratic change events	
	(1)	(2)	(3)	(4)
GDP per capita growth$_{t-1}$	0.9661**	0.9556**	0.9588**	0.9527*
	(0.0151)	(0.0169)	(0.0168)	(0.0269)
Country-specific development level$_{t-2}$		0.7958		0.8944
		(0.3001)		(0.3102)
POLITY score$_{t-1}$		0.6459***		1.5247***
		(0.0484)		(0.1340)
Tenure of regime$_{t-1}$ (years)		1.0176		1.1812***
		(0.0243)		(0.0687)
Democracy in region$_{t-1}$ (percent of countries)		1.0120		0.9423***
		(0.0124)		(0.0120)
Country and year fixed effects	Yes	Yes	Yes	Yes
Pseudo-R^2	0.117	0.289	0.156	0.374
OLS coefficient for same estimation	-0.0014**	-0.0017**	-0.0023**	-0.0022**
Observations	2,474	2,474	1,628	1,628
Number of events	132	132	70	70
Countries	82	82	49	49
Years: 1963-2007				

Notes: Coefficients are reported as odds ratios. A democratic change event is the year-*t* commencement of a 3 or more point increase in POLITY score that occurs within 3 years, flagged by a positive REGTRANS score; an autocratic change event is the year-*t* commencement of a 3 or more point decrease in POLITY score that occurs within 3 years, flagged by a negative REGTRANS score. Robust standard errors clustered by country are in parentheses. GDP per capita growth is scaled so that one percentage point of additional growth is 1, not 0.01. The country-specific development level variable equals 0 when a country's *t*-2 per capita GDP is within 30 log points of its sample average *t*-2 per capita GDP, +1 (-1) when 30-60 log points above (below) its sample average, and +2 (-2) when 60 or more log points above (below) its sample average. Columns 2 and 4 control for the secondary school enrollment rate (percent gross) and the percent of the population aged 65 years and above in year *t*-2. The sample in columns 1 and 2 excludes country-years in which the *t*-1 POLITY score exceeds 7. The sample in columns 3 and 4 excludes country-years in which the *t*-1 POLITY score is less than -7. Logit estimation samples are restricted to countries that experienced within-sample variation in the dependent variable. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

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[15] Estimated marginal effects from fixed effects logit models are biased in the presence of substantial heterogeneity across the fixed effect unit, and so are not reported.

The odds ratio on GDP per capita growth is approximately 0.96 in the two democratic change specifications (columns 1 and 2), indicating that a one percentage point increase in growth lowers the probability of a democratic change event in the next year by 4 percent. For the logit sample, the average likelihood of these events is 5.3 percent, so at the mean, a 4 percent drop in the probability is equivalent to a 0.2 percentage point reduction (i.e. from 5.3 percent to 5.1 percent). When we estimate the linear fixed effects models restricting the sample to that used in columns 1-2 of Table 3, the coefficient on growth is around -0.0015, indicating that a one percentage point increase in growth lowers the probability of democratic change by 0.15 percentage points. This suggests that the result on the short-run impact of economic growth on democratic change is marginally stronger in the logit specifications. However, because non-linear IV models can be unreliable (Joshua D. Angrist and Alan B. Krueger 2001), we present only linear IV model results.

The logit results on autocratic change events (columns 3-4 of Table 3), like the LPM results of Table 2, indicate that the rate of output growth also has a statistically significant negative impact on the short-run likelihood of democratic backsliding. The estimated coefficients in the autocratic change regressions imply that a one percentage point increase in the growth rate lowers the probability of an autocratic change event by 0.2 percentage points (from 4.3 percent to 4.1 percent), which is a similar effect to that obtained for democratic change events. The LPM and logit results thus indicate that, all else equal, both democratic change and autocratic change are more likely to occur after output contractions.

B. Instrumental variable results

The foregoing results may not represent the causal impact of growth on the likelihoods of democratic change and autocratic change, and may suffer bias due to the possible endogeneity of economic growth. In Table 4, we reestimate the main equation for democratic change events using IV estimation (for a 1963-2001 sample that includes countries that have no democratic change events). We present estimates using each of the instruments separately (columns 3-5), and considering the instruments jointly (column 2). The results indicate that stronger economic growth in year *t*-1 significantly reduces the likelihood of democratic change in year *t*. Considering the instruments jointly, an increase in economic growth of one percentage point is estimated to reduce the likelihood of a democratic change event in the subsequent year by 1.3-1.4 percentage points (significant at the 5 percent level). This estimated effect is an order of magnitude larger than the LPM estimates. It implies that, on average, an additional percentage point of GDP per capita growth reduces the likelihood of significant democratic change in the next year by more than 30 percent.

The effect of (instrumented) economic growth on the timing of democratic change emerges via the impact of shocks in temperature and precipitation. (The estimate instrumenting with precipitation variation is not statistically significant for the full sample, though is statistically significant for sub-samples of agricultural countries and sub-Saharan African countries; see Appendix III.) The similarity of the results instrumenting with either precipitation variation or temperature variation provides some reassurance that the estimated impact of output growth on the likelihood of democratic change in the next year is reasonably robust. However, the estimates provide no evidence that shocks from changes in commodity export prices influence the timing of

democratic change, indicating that the local average treatment effects of weather and commodity

export price shocks differ.

Table 4-Instrumental variable results for democratic change events

	(1)	(2)	(3)	(4)	(5)
Estimation	LPM	IV (F1)	IV (F1)	IV (F1)	IV (F1)
Excluded instruments	None	All	Precipitation instrument t-1, t-2	Temperature instrument t-1, t-2	Commodity price instrument$_{t-1}$
Panel A: No time-varying country controls					
GDP per capita growth$_{t-1}$	-0.0007	-0.0126**	-0.0096	-0.0154**	0.0007
	(0.0004)	(0.0061)	(0.0077)	(0.0075)	(0.0063)
F statistic on excluded instruments	-	7.83	4.60	16.53	2.72
Partial R^2 on excluded instruments	-	0.012	0.005	0.008	0.001
Panel B: With time-varying country controls					
GDP per capita growth$_{t-1}$	-0.0009*	-0.0140**	-0.0112	-0.0173**	0.0001
	(0.0005)	(0.0063)	(0.0085)	(0.0078)	(0.0060)
Country-specific development level$_{t-2}$	-0.0156**	-0.0370***	-0.0324**	-0.0424***	-0.0140
	(0.0075)	(0.0132)	(0.0155)	(0.0157)	(0.0120)
POLITY score$_{t-1}$	-0.0098***	-0.0099***	-0.0099***	-0.0100***	-0.0098***
	(0.0015)	(0.0016)	(0.0016)	(0.0017)	(0.0015)
Tenure of regime$_{t-1}$ (years)	0.0010*	0.0006	0.0007	0.0006	0.0010*
	(0.0006)	(0.0011)	(0.0010)	(0.0013)	(0.0006)
Democracy in region$_{t-1}$ (percent of countries)	0.0005	-0.0003	-0.0001	-0.0004	0.0006
	(0.0006)	(0.0008)	(0.0008)	(0.0008)	(0.0007)
F statistic on excluded instruments	-	6.66	3.50	14.65	3.70
Partial R^2 on excluded instruments	-	0.011	0.004	0.007	0.001
Country and year fixed effects	Yes	Yes	Yes	Yes	Yes
Stock-Yogo critical values	-	4.03/6.42	7.49/13.46	7.49/13.46	12.71/24.09
Observations	2,897	2,897	2,897	2,897	2,897
Number of democratic change events	118	118	118	118	118
Countries	121	121	121	121	121
Years: 1963-2001					

Notes: The dependent variable indicates the commencement of democratic change events, which involve a 3 or more point increase in POLITY score that occurs within 3 years, flagged by a positive REGTRANS score. Robust standard errors clustered by country are in parentheses. GDP per capita growth is scaled so that one percentage point of additional growth is 1, not 0.01. The country-specific development level variable equals 0 when a country's *t*-2 per capita GDP is within 30 log points of its sample average *t*-2 per capita GDP, +1 (-1) when 30-60 log points above (below) its sample average, and +2 (-2) when 60 or more log points above (below) its sample average. Panel B estimates control for the secondary school enrollment rate (percent gross) and the percent of the population aged 65 years and above in year *t*-2. The sample excludes country-years in which the *t*-1 POLITY score exceeds 7. Reported Stock-Yogo critical values are the 5 percent significance level critical values for weak instruments tests based on, respectively, 30 percent and 5 percent maximal Fuller relative bias. The null of weak instruments is rejected in the case that the *F* statistic on the excluded instruments exceeds the Stock-Yogo critical value/s. F1 is Fuller 1. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

The coefficients on the instruments in the first-stage regressions are of the expected signs. The instruments together explain more than 1 percent of the within-country variation in GDP per capita growth, and pass the Stock-Yogo test for weak instruments for 5 percent maximal Fuller relative bias at the 5 percent significance level. The strongest of the instruments for economic growth are the temperature instruments. The commodity price instrument is a somewhat weak instrument for output growth in the presence of year fixed effects. It is a much stronger instrument for growth in gross domestic income (GDI) (which involves an adjustment to GDP growth to reflect the effect of terms of trade changes on the purchasing power of domestic production). However, we also find that commodity price shocks to GDI growth have statistically insignificant implications for the likelihood of democratic change (results presented in Appendix III).

Why are the IV estimates for the short-run impact of growth on the likelihood of episodes of democratic change so much larger than the LPM estimates? One possibility is that democratic change is foreseen, and this anticipation effect has a positive impact on output. Another possibility is that the LPM estimates suffer from attenuation bias due to measurement error in the GDP data. A further possibility is that governments in crisis put pressure on their statistical offices to inflate the growth figures (i.e. that measurement error in economic data is correlated with looming regime changes). Each of these theories would mean that LPM estimates are underestimates of the true impact of output contractions on triggering democratic change.

Table 5 presents IV estimates of the impact of economic growth on the short-run likelihood of autocratic change. The results provide no statistically significant evidence that the rate of economic growth in year t-1 affects the likelihood of an autocratic change event in year t. Indeed,

IV results using the weather instruments indicate a positive but statistically insignificant impact of GDP growth on the likelihood of autocratic change. Chow tests for the equality of the corresponding growth coefficients in Tables 4 and 5 allow us to reject the null for the specifications in columns 2-4. We therefore conclude that weather-induced changes in economic growth do not have the same impact on the likelihood of reversions toward autocracy that they have on the likelihood of achieving gains in the direction of democracy.

Unfortunately, commodity export price variation is an extremely weak instrument for GDP growth for the sample used in Table 5 (which excludes the most autocratic countries, many of which are highly dependent on primary commodities; see Ross 2001), and so the results using this instrument in column 5 of Table 5 provide no meaningful information on the relationship between commodity price shocks to GDP and the likelihood of democratic backsliding. Again, our commodity price variable is a much stronger instrument for growth in GDI. Results using GDI instead of GDP provide no evidence that price shocks to commodity exporters significantly affect the likelihood of autocratic change (see Appendix III).

Together, the IV results indicate that the rate of GDP growth significantly affects the likelihood of discrete steps toward democracy, but provide no evidence that growth affects the likelihood of reversions toward autocracy. The insignificant IV findings for the short-run impact of economic growth on the likelihood of autocratic change indicate that the negative estimated coefficient on t-1 GDP growth in the LPM and logit estimations may have been due to reverse causation. For instance, growth may be depressed in the lead-up to military coups due to increasing concern over investment risks.

Table 5-Instrumental variable results for autocratic change events

	(1)	(2)	(3)	(4)	(5)
Estimation	LPM	IV (F1)	IV (F1)	IV (F1)	IV (F1)
Excluded instruments	None	All	Precipitation instrument t-1, t-2	Temperature instrument t-1, t-2	Commodity price instrument$_{t-1}$
Panel A: No time-varying country controls					
GDP per capita growth$_{t-1}$	-0.0006	0.0082	0.0180	0.0052	-0.0009***
	(0.0004)	(0.0077)	(0.0132)	(0.0072)	(0.0002)
F statistic on excluded instruments	-	5.03	2.20	10.93	0.00
Partial R^2 on excluded instruments	-	0.009	0.002	0.008	0.000
Chow test: Null hypothesis is that the growth coefficient is the same as in the corresponding democratic change regression (*p*-value)	0.96	0.03	0.04	0.05	0.17
Panel B: With time-varying country controls					
GDP per capita growth$_{t-1}$	-0.0004	0.0079	0.0189	0.0045	0.0003
	(0.0005)	(0.0079)	(0.0147)	(0.0070)	(0.0006)
Country-specific development level t-2	-0.0039	0.0077	0.0229	0.0029	-0.0029
	(0.0050)	(0.0132)	(0.0244)	(0.0115)	(0.0051)
POLITY score$_{t-1}$	0.0087***	0.0091***	0.0096***	0.0089***	0.0087***
	(0.0012)	(0.0013)	(0.0016)	(0.0013)	(0.0012)
Tenure of regime$_{t-1}$ (years)	0.0019***	0.0021***	0.0024**	0.0020***	0.0019***
	(0.0005)	(0.0005)	(0.0010)	(0.0005)	(0.0005)
Democracy in region$_{t-1}$ (percent of countries)	-0.0008***	-0.0006*	-0.0003	-0.0007**	-0.0008***
	(0.0003)	(0.0003)	(0.0006)	(0.0003)	(0.0003)
F statistic on excluded instruments	-	4.94	1.91	11.15	0.00
Partial R^2 on excluded instruments	-	0.008	0.002	0.008	0.000
Chow test: As in Panel A	0.42	0.02	0.03	0.03	0.21
Country and year fixed effects	Yes	Yes	Yes	Yes	Yes
Stock-Yogo critical values	-	4.03/6.42	7.49/13.46	7.49/13.46	12.71/24.09
Observations	3,598	3,598	3,598	3,598	3,598
Number of autocratic change events	63	63	63	63	63
Countries	141	141	141	141	141
Years: 1963-2001					

Notes: The dependent variable indicates the commencement of autocratic change events, which involve a 3 or more point decrease in POLITY score that occurs within 3 years, flagged by a negative REGTRANS score. Robust standard errors clustered by country are in parentheses. GDP per capita growth is scaled so that one percentage point of additional growth is 1, not 0.01. The country-specific development level variable equals 0 when a country's t-2 per capita GDP is within 30 log points of its sample average t-2 per capita GDP, +1 (-1) when 30-60 log points above (below) its sample average, and +2 (-2) when 60 or more log points above (below) its sample average. Panel B estimates control for the secondary school enrollment rate (percent gross) and the percent of the population aged 65 years and above in year t-2. The sample excludes country-years in which the t-1 POLITY score is less than -7. Reported Stock-Yogo critical values are the 5 percent significance level critical values for weak instruments tests based on, respectively, 30 percent and 5 percent maximal Fuller relative bias. The null of weak instruments is rejected in the case that the *F* statistic on the excluded instruments exceeds the Stock-Yogo critical value/s. Chow test *p* values are based on two-stage least squares estimates. F1 is Fuller 1. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

C. Robustness analysis and additional specifications

How robust are our results? In this section, we consider robustness checks based on alternative samples, the inclusion of additional control variables, and the use of different dependent variable specifications. We first consider whether the IV result on the short-run impact of growth on the likelihood of episodes of democratic change is driven by weather shocks in a specific set of countries. Table 6 presents estimates for similarly-sized sub-samples of "more agricultural" and "less agricultural" countries, "low mean GDP per capita" and "high mean GDP per capita" countries, and "sub-Saharan Africa" and the "rest of the world". As might be expected, in the first stage regression, the weather instruments are stronger for poorer, agricultural countries. In the second stage, the impact of shocks to the economy from the weather on the likelihood of democratic change is also stronger (though not significantly so) for these sub-samples.

Nevertheless, we obtain similar coefficient estimates of the impact of growth on democratic change events using the full set of instruments (panel A) or the weather instruments (panel B) for each of the sub-samples in Table 6. Chow tests fail to reject the hypothesis that the impact of growth on the likelihood of democratic change for each of these sub-samples is equal to that for the full sample. We thus conclude that the impact of weather shocks to the economy on the likelihood of democratic change is relatively robust, and is not solely driven by the impact of these shocks in any particular type of country or any single region. We find no evidence that shocks to GDP from commodity prices have a significant impact on the likelihood of democratic change in any of the sub-samples (panel C).

Table 6-Instrumental variable results for democratic change events: sub-samples

Sample	(1) Full	(2) More agricultural	(3) Less agricultural	(4) Low mean GDP per capita	(5) High mean GDP per capita	(6) Sub-Saharan Africa	(7) Rest of world
Panel A: All instruments							
GDP per capita growth$_{t-1}$	-0.0140**	-0.0154**	-0.0102	-0.0144**	-0.0074	-0.0110	-0.0109
	(0.0063)	(0.0063)	(0.0091)	(0.0071)	(0.0130)	(0.0072)	(0.0104)
F statistic on exc. ins.	6.66	5.63	2.90	5.00	2.10	4.38	3.27
Partial R^2 on exc. ins.	0.011	0.014	0.011	0.010	0.010	0.011	0.011
Chow test *p*-value of equality with parameter estimate in column 1	-	0.86	0.63	0.86	0.22	0.45	0.56
Stock-Yogo critical values: 4.03/6.42							
Panel B: Weather instruments							
GDP per capita growth$_{t-1}$	-0.0158**	-0.0164**	-0.0118	-0.0139*	-0.0150	-0.0145*	-0.0112
	(0.0069)	(0.0069)	(0.0098)	(0.0073)	(0.0164)	(0.0083)	(0.0117)
F statistic on exc. ins.	7.93	6.48	3.45	5.91	2.36	3.94	3.98
Partial R^2 on exc. ins.	0.009	0.013	0.009	0.010	0.007	0.008	0.010
Chow test: As in Panel A	-	0.97	0.62	0.81	0.74	0.66	0.41
Stock-Yogo critical values: 4.63/7.63							
Panel C: Commodity price instrument							
GDP per capita growth$_{t-1}$	0.0001	-0.0015	0.0013	-0.0083	0.0102	0.0026	-0.0052
	(0.0060)	(0.0072)	(0.0092)	(0.0071)	(0.0080)	(0.0051)	(0.0093)
F statistic on exc. ins.	3.70	1.90	1.71	0.77	2.74	4.77	1.27
Partial R^2 on exc. ins.	0.001	0.001	0.001	0.000	0.003	0.002	0.001
Chow test: As in Panel A	-	0.78	0.82	0.10	0.05	0.68	0.46
Stock-Yogo critical values: 12.71/24.09							
Country, year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,897	1,464	1,433	1,452	1,445	1,224	1,673
Number of democratic change events	118	50	68	57	61	43	75
Countries	121	54	67	59	62	43	78
Estimation: IV (F1). Years: 1963-2001							

Notes: The dependent variable indicates the commencement of democratic change events, which involve a 3 or more point increase in POLITY score that occurs within 3 years, flagged by a positive REGTRANS score. The weather instruments include the precipitation instrument$_{t-1, t-2}$ and the temperature instrument$_{t-1, t-2}$. Robust standard errors clustered by country are in parentheses. GDP per capita growth is scaled so that one percentage point of additional growth is 1, not 0.01. Estimates control for the country-specific development level in year t-2, the POLITY score in year t-1, the tenure of the regime in year t-1 (years), the percent of countries in the region that are democracies in year t-1, the secondary school enrollment rate (percent gross) in year t-2, and the percent of the population aged 65 years and above in year t-2. More agricultural countries are those with a 1961 labor force share in agriculture equal to the sample median level (72 percent) or higher; non-agricultural countries are others. Low mean GDP per capita countries are those with a sample average t-2 GDP per capita equal to the sample median ($765.5, in 2000 US$) or below; high mean GDP per capita countries are others. Samples exclude country-years in which the t-1 POLITY score exceeds 7. Reported Stock-Yogo critical values are the 5 percent significance level critical values for weak instruments tests based on, respectively, 30 percent and 5 percent maximal Fuller relative bias. The null of weak instruments is rejected in the case that the F statistic on the excluded instruments exceeds the Stock-Yogo critical value/s. Chow test *p* values are based on two-stage least squares estimates. Separate results using the 1) precipitation instruments, and 2) the temperature instruments are presented in Appendix III. F1 is Fuller 1. Exc. ins. is excluded instruments. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Does the short-run impact of economic growth on the likelihood of democratic change vary by income? Conceivably, if output contractions are more important triggers of democratic change at higher income levels, they may constitute an avenue via which a modernization effect of income on the likelihood of democratic change operates. We find no evidence of this for samples of countries with low mean GDP per capita and high mean GDP per capita in Table 6. We also explore for such an effect using within-country variation in income level. In Appendix III, we present LPM and IV regressions for both democratic change events and autocratic change events that include an interaction between t-1 growth and the country-specific development level variable. The interaction term is negative but statistically insignificant in these specifications, providing no significant evidence that the average impact of GDP growth on the short-run likelihood of either democratic or autocratic change is systematically affected by a country's level of development.[16]

We also explore whether the main IV result for democratic change events (column 2, panel B, Table 4) is robust to the inclusion of additional controls (results not reported). We obtain similar results controlling for the primary school enrollment rate, the infant mortality rate, energy depletion as a share of GNI, mineral depletion as a share of GNI, exports as a share of GDP, the urbanization rate, the size of the population, the population growth rate, the percentage of the population under 15 years of age, a transition economy dummy for the 4-year period 1989-1992, the occurrence of internal armed conflicts, a military regime dummy variable, the highest

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[16] We do not instrument for the country-specific development level variable, and interaction effects are imprecisely estimated. Caution is therefore needed in the interpretation of these results. We also obtain insignificant interaction terms in specifications that interact t-1 GDP per capita growth with t-2 GDP per capita (or its natural logarithm), with or without country fixed effects.

POLITY score previously recorded by that country (a measure of experience with democracy), the incumbent leader's age, or the tenure of the incumbent leader (in years). The result is also not sensitive to the inclusion of interactions among the control variables, or the use of higher-order polynomials of the control variables or logged control variables. We included additional lags of GDP per capita growth in the fixed effects LPM estimations, but these are not statistically significant, and their inclusion does not receive support from the Akaike and Bayesian information criteria. An interaction effect between *t*-1 growth and the lagged POLITY score is insignificant (see Appendix III).

Similar results are obtained using aggregate rather than per capita GDP growth, or using GDP growth from the Penn World Table (see Appendix III). Results are similar if an additional lag of the weather instruments is not included in the first-stage regressions, if the instruments are not interacted with the country-specific variables (the 1961 share of the labor force in the agricultural sector, 1961 share of non-irrigated cropland, and 1995 share of commodity export bundle in GDP), or if the weather instruments are interacted with the share of agriculture in GDP in 1995 instead of the share of the labor force in agriculture in 1961.[17] Large commodity exporters may affect world commodity prices, which means that commodity prices may not be exogenous for these countries, but IV results using the commodity price instrument remain insignificant if we remove commodities from a country's commodity export bundle in instances in which that country contributes a non-trivial (15 percent or more) share of world exports.

An issue of interest is whether there is asymmetry in the short-run impact of economic growth on the likelihood of democratic change. To test for this, we estimate an equation in which we split *t*-

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[17] Data on the share of agriculture in GDP for the early 1960s are not available for many countries.

1 GDP per capita growth into two variables: one equal to the *t*-1 growth rate in years when this exceeds the *t*-2 growth rate, and another equal to the *t*-1 growth rate when this rate is less than or equal to the *t*-2 growth rate. A test for the equality of the coefficients on these variables is a test of whether the impact of growth on the likelihood of democratic change differs for years in which growth is accelerating and years in which growth is decelerating. The results, presented in Appendix III, provide no statistically significant evidence of asymmetry in the relationship between growth and the short-run likelihood of democratic change. We also explore whether positive economic shocks have a different impact on the likelihood of democratic change to that of negative shocks by instrumenting with only positive shocks and only negative shocks. Estimates of the short-run impact of the rate of growth on democratic change are similar for both (see the Appendix III). We thus conclude that the effect of output contractions in increasing the likelihood of democratic change is similar to the effect of output expansions in reducing this likelihood.

To what extent are our regime change results related to leadership change? Using the leader dataset of Goemans, Gleditsch, and Chiozza (2009), we find that 53 percent of the democratic change events in the IV estimation sample are associated with a change in the effective national leader in the same year. Leadership change often precedes democratic change (18 percent of the democratic change events occurred in the year after a change in national leader), or follows democratic change (42 percent of the time, democratic change events were followed by a change in national leader in the two subsequent years). Democratic change and leader change are thus often a joint process. (Good examples of democratic change being triggered by leader change are the three democratic change events associated with the natural death of the incumbent autocrat that are excluded from the estimation dataset; see also Jones and Olken (2009).) But not all

changes in national leader give rise to greater democracy. Formally, our analysis estimates the

probability of a democratic change event, $D(x)$, which can be decomposed as follows:

(2) $D(x) = \Pr(L = 1 \mid x)D(x; L = 1) + \Pr(L = 0 \mid x)D(x; L = 0)$

where $L = 1$ indicates a change in leader and x is a vector of variables that affect the probabilities.

Although we do not separately estimate each of the terms in equation (2), we carry out another

robustness check in which we ask: does the effect of growth on democratic change remain even

once we consider leader change? We find that it does: including three controls for the number of

leader changes in years t-2, t-1, and t, we obtain a similar estimate of the effect of growth in year

t-1 on the likelihood of democratic change in year t.

We also investigate whether our result is robust to different definitions of democratic change

events. Results are presented in Appendix III. We find a similar, although slightly weaker, result

if democratic change events are defined to cover only Polity IV's definition of democratic

transitions (REGTRANS = 2 or 3). This indicates that output contractions not only increase the

likelihood of democratic change (which includes non-transitioning progress toward democracy),

but also increase the likelihood of transitions to more democratic regime types (partial or full

democracies). We also find similar results in specifications excluding democratic change events

involving only a 3-point improvement in the POLITY score, excluding events reversed by a shift

toward autocracy within the subsequent four years, excluding events taking more than two years

to transpire, and excluding events not associated with a change in national leader in the same year

or the subsequent two years. Our results thus indicate that growth slowdowns lead to significant,

lasting episodes of democratic change, which in most cases also result in changes in leadership.

Finally, we explore whether the finding that output contractions increase the likelihood of democratic change holds using the POLITY2 score and dichotomous measures derived from the POLITY2 score. As discussed, we believe that the POLITY2 score is less suitable to a study on the triggers of institutional change than Polity IV's regime transition indicators. Nevertheless, the POLITY2 score remains popular in the literature, and estimates using the POLITY2 score, presented in Table 7, allow a check on the robustness of the main results. In the first three columns of Table 7, we present IV estimates using binary variables of regime type derived from the POLITY2 score.[18] The sample in column 1 is restricted to the most autocratic countries (those with a t-1 POLITY score equal to -7 or less), and the dependent variable measures a year-t transition to a POLITY2 score of -6 or above. We find a negative coefficient IV estimate for t-1 GDP growth, significant at the 10 percent level, indicating that output contractions increase the likelihood of democratic change among the most autocratic countries. The sample in column 2 is countries classified as autocracies (those that have a t-1 POLITY score between -10 and 0). The IV estimate on the impact of growth in t-1 on the likelihood of a year-t transition to a positive POLITY2 score is negative, but statistically insignificant at the normal levels. A similar result is obtained for transitions to full democracy (POLITY2>6) (column 3, Table 7).

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[18] Binary regime classification variables derived from POLITY2 can exaggerate the importance of small movements in POLITY score, and in many cases do not measure the commencement of regime transitions. To continue the Portugal example, Portugal is not coded as being a democracy by POLITY2 until the year after the commencement of its "major democratic transition". The variables also offer less variation than our main specification of democratic change events.

Table 7-Instrumental variable results using POLITY2-based dependent variables

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable (year *t*)	Binary variable: Transition away from full autocracy (POLITY2>-7)	Binary variable: Transition to democracy (POLITY2>0)	Binary variable: Transition to full democracy (POLITY2>6)	POLITY2	POLITY2 adjusted (using alternate coding for interregnum)	POLITY2 adjusted (using alternate coding for interregnum and transition periods)
Sample	Countries with *t*-1 POLITY score of -7 or less	Countries with *t*-1 POLITY score of 0 or less	Countries with *t*-1 POLITY score of 6 or less	Full	Full	Full
GDP per capita growth$_{t-1}$	-0.0088*	-0.0040	-0.0040	-0.0960	-0.1101	-0.1316*
	(0.0053)	(0.0042)	(0.0032)	(0.0674)	(0.0687)	(0.0732)
Country, year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
F statistic on exc. ins.	6.35	7.22	8.74	7.49	7.49	7.49
Partial R^2 on exc. ins.	0.024	0.015	0.012	0.010	0.010	0.010
Occasions binary dependent variable=1	89	58	34	-	-	-
Observations	1,476	2,183	2,715	4,327	4,327	4,327
Countries	86	103	117	150	150	150

Estimation: IV (F1)
Excluded instruments: All
Stock-Yogo critical values: 4.03/6.42
Years: 1963-2001

Notes: The set of all instruments includes the precipitation instrument$_{t-1, t-2}$, the temperature instrument$_{t-1, t-2}$, and the commodity price instrument$_{t-1}$. Robust standard errors clustered by country are in parentheses. GDP per capita growth is scaled so that one percentage point of additional growth is 1, not 0.01. Estimates control for the country-specific development level in year *t*-2, the POLITY score in year *t*-1, the tenure of the regime in year *t*-1 (years), the percent of countries in the region that are democracies in year *t*-1, the secondary school enrollment rate (percent gross) in year *t*-2, and the percent of the population aged 65 years and above in year *t*-2. The dependent variable in column 5 is equal to the lagged POLITY score in cases of interregnum (POLITY=-77). The dependent variable in column 6 is equal to the lagged POLITY score in cases of interregnum (POLITY=-77) and the post-transition POLITY score in cases of transition (POLITY=-88). All samples exclude country-years in which a conventional POLITY score is not available for year *t*-1. Reported Stock-Yogo critical values are the 5 percent significance level critical values for weak instruments tests based on, respectively, 30 percent and 5 percent maximal Fuller relative bias. The null of weak instruments is rejected in the case that the *F* statistic on the excluded instruments exceeds the Stock-Yogo critical value/s. F1 is Fuller 1. Exc. ins. is excluded instruments. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

The dependent variable in column 4 of Table 7 is the POLITY2 score. We obtain a negative IV estimate of the effect of lagged growth on the POLITY2 score, in line with the general result that faster growth reduces the likelihood of democratic change. However, the estimate is insignificant

at traditional levels ($p = 0.15$). As discussed, POLITY2 is based on coding rules that may be inappropriate for a study on the triggers of institutional change. First, cases of interregnum (POLITY = -77) are given a "neutral" POLITY2 score of 0. This coding rule may give a misleading picture of progress toward democracy, as autocracies with negative POLITY2 scores that fall into anarchy improve their POLITY2 score. In column 5, we present estimates using an adjusted POLITY2 set equal to the lagged POLITY score for the 15 cases of interregnum in the dataset. We obtain a slightly larger estimate (in absolute value terms) of the impact of growth on the likelihood of democratic change, with a p-value of 0.11.

Our second concern with the POLITY2 coding is that it is linearly interpolated over the years of a multi-year transition (when POLITY = -88). To better estimate the effect of growth on the commencement of changes toward or away from greater democracy, we make a second adjustment to the POLITY2 coding: instead of linearly interpolating the POLITY2 score for the duration of any multi-year regime change, we set POLITY2 for these years equal to the post-transition POLITY code. This affects 35 additional observations (less than 1 percent of the sample). Our estimate using this adjusted POLITY2 score is presented in column 6 of Table 7. The coefficient on growth in this estimate is negative and statistically significant ($p = 0.07$), indicating that an additional percentage point of growth on average reduces the POLITY2 score by about $1/10^{th}$ of a point. This can be compared with the corresponding specification in column 2, panel B, Table 4. There, we found a coefficient on growth in a democratic change event regression of -0.014. Since the average democratic change event in our sample involved a 9-point increase in the POLITY2 score, it is to be expected that the coefficient on growth in a POLITY2 regression is around an order of magnitude larger than the coefficient on growth in a democratic change event regression. While the evidence on the impact of growth on democratic change using

the POLITY2 variables is not particularly strong, it does provide a degree of support for the preferred specifications that use Polity IV's regime transition indicators.

D. Protests, economic growth, and democratic change

We also explore whether our finding of a negative impact of GDP growth on the short-run likelihood of democratic change operates via the channel of anti-government protests (see Appendix III). In models that control for country and year fixed effects, we find that each additional protest is associated with a 1.6 percentage point increase in the probability of a democratic change event occurring during the *same* year (equal to a 40 percent increase in this probability). However, we find no evidence that protests – at least those reported in the *New York Times* – are significantly related to the likelihood of democratic change the *following* year. The magnitude of the effect of an additional protest on the likelihood of democratic change in the same year is large, but may not be causal (since we only have annual data, the protest might have occurred in the months after the commencement of democratic change). Protests appear to not be important for reversions toward autocracy. This is in line with expectations, as democratic reversals are normally the result of military coups rather than popular mobilization.

While democratic change is often associated with protests, we do not find strong evidence that the impact of GDP growth on the short-run likelihood of democratic change operates via protests. In additional specifications with our standard set of controls (see Appendix III), we observe a negative association between GDP per capita growth and anti-government protests, but this result is not robust to instrumenting growth. Further, LPM and IV estimates of the impact of *t*-1 GDP growth on the likelihood of democratic change in year *t* remain similar in specifications that control for the number of anti-government protests in years *t* and *t*-1. This evidence is consistent

with two conclusions: either the relationship between growth and democratic change occurs through channels other than political protest, or our measure of protests is too imprecise for the relationship to show up in regressions with country and year fixed effects. The precise causal mechanism via which economic growth affects the short-run likelihood of democratic change thus remains to be identified.

V. Relating our results to the existing evidence

A. Empirical studies on the determinants of democratic change

How do our results fit into the existing literature? The bulk of the literature focuses on the importance of structural factors, such as income level, education, natural resources, culture, religion, or colonial origins, in explaining regime type (see e.g. Robert J. Barro 1999). Numerous studies test the hypothesis that higher incomes make countries more likely to be democratic. However, much of the literature on the modernization hypothesis is called into question by Acemoglu et al. (2008). Once country fixed effects are considered, Acemoglu et al. find that income has no causal effect on democracy. (Similar results can be seen in Marcus Alexander, Matthew Harding, and Carlos Lamarche (2008), who conclude that country-specific factors hold the key to political development.) The approach in this paper, like that of Acemoglu et al., incorporates country fixed effects to control for time-invariant omitted variables. We estimate the short-run impact of GDP growth on the likelihood of democratic change, in contrast to Acemoglu et al.'s long-run focus, but our results are quite consistent. While Acemoglu et al. find little evidence of a long-run relationship between GDP and democracy, we find a short-run relationship between the rate of GDP growth and the likelihood of democratic change.

A strand of the literature also shares our focus on the short-run relationship between economic growth and democratic change. Huntington (1991) contends that short-term economic crises played a large role in triggering democratization during the "third wave of democratization". Haggard and Kaufman (1997), Junhan Lee (2002), and Acemoglu and Robinson (2006) also discuss the tendency for countries to democratize subsequent to economic slowdowns. Econometric studies generally find that higher economic growth reduces the likelihood of democratic change (e.g. Przeworski et al. 2000, Robert H. Bates et al. 2003, Doorenspleet 2004, Marcus Noland 2005, David L. Epstein et al. 2006, Ulfelder and Michael Lustik 2007; cf. Mark J. Gasiorowski 1995). However, to our knowledge, the only study to have addressed the potential endogeneity of economic growth is Brückner and Ciccone (2008). Written contemporaneously with this study, Brückner and Ciccone use rainfall variation as a source of exogenous variation in growth, and find – consistent with our results – that negative growth shocks increased the likelihood of democratization in sub-Saharan African nations in the period since 1980. In reduced-form estimations, Dell, Jones, and Olken (2008) also find that higher temperatures and lower precipitation increase political instability, but do not explore whether this effect favors movements toward democracy.

A number of studies also look at the effect of economic growth on reversions to autocracy. Several conclude that democracies are less likely to backslide to autocracy if their economy is growing strongly (John B. Londregan and Keith T. Poole 1990, Przeworski and Fernando Limongi 1997, C.L. Chiou 1998, Gasiorowski and Timothy J. Power 1998, Przeworski et al. 2000). Others, however, find that regime stability is less affected by economic growth in democracies than in autocracies (Karen L. Remmer 1996, 1999) and that slow growth does not increase the short-run likelihood of autocratic reversions (Ulfelder and Lustik 2007). However,

studies on growth and autocratic change may produce biased estimates if growth is endogenous (e.g. if economies grow more rapidly when there is a low risk of coups). Our results, like those of Brückner and Ciccone (2008) for sub-Saharan Africa, indicate that exogenous growth shocks have little impact on the probability of democratic backsliding.

B. Country case study evidence

Our results fit well with evidence from country case studies. In Asia, democratization in the Philippines and Indonesia followed economic crises and mass protests. In both cases, the incumbent autocratic regime appeared to receive the acquiescence of the people while the economy was growing strongly (see Albert F. Celoza 1997 for the case of the Philippines, and Aspinall 2005 and Aspinall, Herb Feith, and Gerry van Klinken 1999 for the case of Indonesia). When such progress was reversed by recession, however, implicit social contracts between the people and the regimes appear to have been broken, and pressures to democratize escalated.[19] In these cases, protests against autocratic rule were triggered by income reversals, which accentuated growing discontent toward the incumbent regime. By contrast, other autocracies in Asia that have enjoyed consistent growth in recent times (such as China, Singapore, and Vietnam) have not democratized.

Output contractions also appear to have triggered democratic change in Latin America. Gordon Richards (1986, p. 449) contends that recessions "largely destroyed the military's claims to political legitimacy" in many Latin American countries, and that democratization had "less to do

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[19] Aspinall (2005, p. 209) concludes that Suharto's regime was "able to deliver steady improvements in living standards to much of the population" but that "the cataclysmic economic collapse from late 1997, by removing this prop of performance legitimacy, propelled a range of social and political forces into action and was the proximate cause of Suharto's political downfall".

with the internal political situation … than with the economic crises faced by the military regimes." In the Middle East and Africa, economic slowdowns also led to popular protests and democratic gains in Algeria, Jordan, Benin, Mali, Niger, Zambia, and Malawi.[20] Bratton and van de Walle report "a clear, positive relationship between popular protest and political reform in contemporary Africa" (1992, p. 432), and that democratic transitions in Africa "usually began with spontaneous popular protests against a political-economic crisis" (1997, p. 98). Christopher Clapham and John A. Wiseman (1995), Wiseman (1995), Richard Joseph (1997), van de Walle (2001), and Julius O. Ihonvbere (2003a) also conclude that democratization in Africa has been triggered by economic crises.

Since we use weather variables as instruments for growth, we also searched the country case studies for evidence that weather shocks have contributed to pressures for democratic change. This appears to be the case, particularly in Africa. Ihonvbere (2003b, p. 248) writes that drought in Malawi increased pressures for democratization as "unemployment, crime and hunger had reached unprecedented proportions" and "displaced peasants migrated to the urban centers". There is also evidence that drought increased pressures for democratization in Zambia (Ihonvbere 2003c). Outside Africa, the financial crisis faced by Indonesia during 1997-1998 was coupled with perhaps the most severe El Niño event of the 20th century (Dewi G.C. Kirono, Nigel J. Tapper, and John L. McBride 1999).[21] The combined effects made it difficult for President Suharto to curtail growing momentum for democratic change in Indonesia. Kirono, Tapper, and McBride (1999, p. 422-423) report that:

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[20] See Rex Brynen (1992) for the case of Jordan. Bratton and van de Walle (1997, p. 1) note that political liberalization was the consequence of Benin's "economic quandary" and "mass street protests".
[21] 1998 was Indonesia's hottest calendar year of the 20th century. 1997 was Indonesia's seventh-hottest and seventh-driest calendar year of the 20th century.

"During the "El Niño" year from March 1997 to February 1998, virtually the whole country had rainfall below the 10th percentile, with many stations recording their lowest rainfall on record. ... Associated with the drought were large-scale biomass fires that burned as much as 9.5 million hectares of plantation, crop land, and natural and production forest ... The plumes of smoke from these fires grounded air traffic and were a threat to public health ... (and) led to food shortages over much of the country ... the severity of the drought placed severe stress on the economic infrastructure of the country and possibly played a role in the subsequent end of the 30-year old Suharto government."

While there are numerous examples of countries that, consistent with our results, have democratized during economic slowdowns, counter-examples exist. Taiwan and Chile democratized during periods of strong growth. Other countries, such as Swaziland, have faced periods of economic contraction and severe drought in recent decades, but have not seen democratic reforms. Growth slowdowns do not invariably lead to democratic change, and democratic change does not require slow growth. Nevertheless, our results indicate that sluggish growth on average increases the short-run likelihood of democratic change.

VI. Conclusion

This paper explores whether output contractions affect the short-run likelihood of democratic and autocratic change. Our primary identification strategy uses exogenous shocks to economic growth from the weather and international commodity prices to estimate the causal impact of economic growth on the likelihood of regime transitions involving a change in POLITY score of 3 points or more. We find evidence of a negative causal impact of the rate of economic growth on

the short-run likelihood of democratic change. Specifically, output contractions due to adverse weather shocks appear to significantly increase the likelihood of democratic change in the next year. We find no evidence, however, that exogenous shocks to the economy from international commodity prices affect the timing of democratic change.

The episodes of democratic change in our main specification entail reform in the direction of democracy, but do not always result in the establishment of fully functional democracies. However, we obtain similar (although slightly weaker) results in specifications that consider only democratic transitions. Our findings are similar across different samples and robust to the inclusion of additional covariates, but are weaker in specifications using regime indicators based on the POLITY2 score or more slow-moving measures of democratic change such as those of Przeworski et al. (2000) or Freedom House. As discussed, we also cannot rule out that weather shocks break our exclusion restriction – i.e. that they impact the likelihood of democratic change via channels other than economic growth. Such matters mean that caution is required in interpreting the results. They also open the door to further research into the precise mechanisms via which the weather affects prospects for political change.

The results identify output contractions as a contemporary trigger of within-country democratic change, and complement existing evidence on the importance of the historical antecedents of modern institutions (Acemoglu et al. 2008). The findings give support to the idea that citizens are more likely to acquiesce to an autocratic regime if they feel they are achieving material gains from one year to the next, but that pressures to democratize can increase during economic reversals. Adverse weather shocks have particularly important implications for rural incomes, and significantly increase pressures for democratic change. This is consistent with the idea that

progress toward democracy occurs as a result of concessions from incumbent regimes in times of increasing resistance from non-elites, in line with the results of models of the determinants of democratic change, such as those of Acemoglu and Robinson (2001, 2006). The finding that shocks to commodity revenues are not important triggers of moves toward democracy is consistent with a line of research on the "stubborn authoritarianism" of oil-rich countries (Smith 2006, p. 56).

Once we address the potential endogeneity of economic growth, we find no evidence that growth affects the short-term likelihood of backsliding toward autocracy. However, this finding should be carefully interpreted. It is certainly the case that many instances of autocratic change (often, coups) occur during times of slow growth. But we find no statistical evidence that these events are more likely to occur because of slow growth.

How important are growth fluctuations for democratic change? In a linear probability model in which growth enters linearly (i.e. without higher order polynomials in growth) the predicted number of democratic and autocratic change events implied by a smooth growth path is precisely the same as the predicted number of events under the observed growth path. This is because smooth development paths symmetrically reduce the size of output contractions (which increase the probability of democratic change) and output expansions (which reduce this likelihood). This is not merely an artefact of the linear model. Even if we use an ordered logit model to simultaneously model the likelihood of episodes of democratic and autocratic change, the resulting predictions suggest that smooth development paths have virtually no impact on the number of democratic and autocratic change events over the long run.

Nevertheless, the results highlight an important impact of economic growth on the likelihood of democratic change in the short run. Our preferred IV estimate (column 2, Panel B, Table 4) suggests that a one-year recession in an autocratic nation that reduced GDP per capita growth by 6 percentage points would increase that country's probability of undergoing significant democratic reform in the next year by 8 percentage points. This is equivalent to a 200 percent increase in this probability. This demonstrates that output shocks affect the timing of democratic change, even if there is little evidence of a causal impact of income on democracy in the long run (Acemoglu et al. 2008).

The result that stronger economic growth reduces the short-run likelihood of democratic change needs to be considered alongside the finding of Miguel, Satyanath, and Sergenti (2004) that stronger growth reduces the short-run likelihood of civil conflict in Africa. The combined evidence indicates that output contractions trigger anti-government resistance activities. Further research into the conditions which determine whether output contractions lead to peaceful or to violent opposition to incumbent regimes may be of considerable value.

Lastly, our results support the argument of Bruce Bueno de Mesquita et al. (2003, p. 481) that autocratic leaders are "amenable to change" during economic slowdowns, and that such times are an opportunity for external parties to pressure for reforms to governance systems. Making economic assistance conditional on democratic reform during adverse economic times may be an effective way of supporting domestic momentum for democratic change.

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A-I. Democratic and autocratic change events included in estimation sample

Democratic change events included in the estimation sample (132): Peru 1963; South Korea 1963; El Salvador 1964; Sudan 1964; Guatemala 1966; Ecuador 1968; Sierra Leone 1968; Thailand 1968; Venezuela 1968; Burkina Faso 1969; France 1969; Ghana 1969; Benin 1970; Malaysia 1971; Argentina 1973; Thailand 1973; Turkey 1973; Brazil 1974; Greece 1974; Portugal 1974; Burkina Faso 1977; Mexico 1977; Thailand 1977; Bangladesh 1978; Bolivia 1978; Dominican Republic 1978; Ghana 1978; Nigeria 1978; Peru 1978; Senegal 1978; Ecuador 1979; El Salvador 1979; Honduras 1980; Nepal 1981; Philippines 1981; South Korea 1981; Bolivia 1982; Argentina 1983; Turkey 1983; Guatemala 1984; Nicaragua 1984; Brazil 1985; Pakistan 1985; Sudan 1985; Uruguay 1985; Haiti 1986; Philippines 1986; South Korea 1987; Tunisia 1987; Chile 1988; Hungary 1988; Mexico 1988; Pakistan 1988; Algeria 1989; Jordan 1989; Panama 1989; Paraguay 1989; Romania 1989; Albania 1990; Benin 1990; Bulgaria 1990; Comoros 1990; Fiji 1990; Gabon 1990; Mongolia 1990; Nepal 1990; Nicaragua 1990; Bangladesh 1991; Central African Republic 1991; Chad 1991; Ghana 1991; Guinea-Bissau 1991; Madagascar 1991; Mali 1991; Niger 1991; Republic of the Congo 1991; Togo 1991; Zambia 1991; Azerbaijan 1992; Cameroon 1992; Guyana 1992; Paraguay 1992; South Africa 1992; Thailand 1992; Lesotho 1993; Malawi 1993; Peru 1993; Uganda 1993; Haiti 1994; Mexico 1994; Mozambique 1994; Algeria 1995; Guinea 1995; Tanzania 1995; Dominican Republic 1996; Ghana 1996; Guatemala 1996; Romania 1996; Sierra Leone 1996; Albania 1997; Iran 1997; Kenya 1997; Tajikistan 1997; Armenia 1998; Cambodia 1998; Indonesia 1998; Côte d'Ivoire 1999; Djibouti 1999; Niger 1999; Zimbabwe 1999; Burkina Faso 2000; Comoros 2000; Peru 2000; Russia 2000; Senegal 2000; Yugoslavia 2000; Ghana 2001; Zambia 2001; Kenya 2002; Macedonia 2002; Sudan 2002; Liberia 2003; Algeria 2004; Bhutan 2005; Egypt 2005; Guinea-Bissau 2005; Kyrgyzstan 2005; Mauritania 2005; Uganda 2005; Nepal 2006; Pakistan 2007; Thailand 2007.

Autocratic change events included in the estimation sample (70): Benin 1963; Burundi 1963; Republic of the Congo 1963; Nigeria 1964; Morocco 1965; Argentina 1966; Greece 1967; Sierra Leone 1967; Panama 1968; Peru 1968; Kenya 1969; Malaysia 1969; Philippines 1969; Sudan 1969; Bolivia 1970; Ecuador 1970; Lesotho 1970; Sierra Leone 1971; Thailand 1971; Turkey 1971; Uruguay 1971; Benin 1972; Ghana 1972; Madagascar 1972; Zambia 1972; South Korea 1972; Chile 1973; Swaziland 1973; Bangladesh 1974; Guatemala 1974; Argentina 1976; Thailand 1976; El Salvador 1977; Pakistan 1977; Guyana 1978; Bolivia 1980; Burkina Faso 1980; Turkey 1980; Ghana 1981; Bangladesh 1982; Zimbabwe 1983; Nigeria 1984; Fiji 1987; Zimbabwe 1987; Sudan 1989; Haiti 1991; Thailand 1991; Algeria 1992; Peru 1992; Tajikistan 1992; Azerbaijan 1993; Gambia 1994; Armenia 1995; Belarus 1995; Albania 1996; Niger 1996; Zambia 1996; Cambodia 1997; Republic of the Congo 1997; Comoros 1999; Haiti 1999; Pakistan 1999; Ecuador 2000; Nepal 2002; Central African Republic 2003; Guinea-Bissau 2003; Iran 2004; Fiji 2006; Thailand 2006; Bangladesh 2007.

A-II. A model of democratic change after negative income shocks

We present a simple game-theoretic demonstration of one mechanism via which democratic change may be more likely following output contractions.

A. The game

Consider a game of two players: an "autocrat" (who represents an autocratic government) and a "citizen" (a representative agent of the people, or a critical grouping of the people). The autocrat's choice set includes two potential choices: 1) maintaining the status quo of autocratic governance, or 2) providing increased political rights to the citizen (some degree of democratic change). The citizen's choice set consists of two possibilities: 1) acquiescing to autocratic rule, or 2) protesting autocratic rule.[22] The game is played twice: in year 1, and again in year 2. Neither player has perfect information on the other player's payoffs.

Figure A1 displays a normal-form representation of the game, with utility payoffs for the citizen and autocrat, respectively, in parentheses.

Figure A1. Regime choice game

		Autocrat	
		Status quo	*Confer political rights*
Citizen	*Acquiesce*	*(s, t)*	*(u, v)*
	Protest	*(w, x)*	*(y, z)*

Let the citizen's utility function in year t be:

---

[22] In a real-world game, the citizen may be able to decide on the degree to which he or she acquiesces to or protests autocratic rule along a continuum. For parsimonious representation here, we assume citizens are faced with only two distinct choices.

(1) $U_t = U_{t-1} + f(\Delta Y_t, \Delta D_t, C_t)$

where ΔY_t is the change in per capita national income in year t, ΔD_t is the change in democratic

freedoms in year t,[23] C_t is the cost of protesting in year t, and:

(2) $\dfrac{\partial U_t}{\partial \Delta Y_t} > 0, \ \dfrac{\partial U_t}{\partial \Delta D_t} > 0, \ \dfrac{\partial U_t}{\partial C_t} < 0$

(3) $C_t = 0$ if the citizen chooses to acquiesce or the autocrat chooses to confer political rights

(4) $C_t > 0$ if the citizen chooses to protest and the autocrat chooses to maintain the status quo

(5) $\Delta D_t = 0$ if the autocrat chooses to maintain the status quo

(6) $\Delta D_t > 0$ if the autocrat chooses to confer political rights.

A1: The cost of protesting is a positive function of the economic growth rate, so that $C_t'(\Delta Y_t) > 0$.

This assumption appears justified, given that one of the largest costs of protesting is the

opportunity cost of time, and a stronger economy in year t increases the opportunity cost of time

in year t.[24] If the citizen becomes unemployed or switches to being a student as a result of a fall in

the economic growth rate, the opportunity cost of protesting may drop particularly significantly.

This assumption is equivalent to assuming that protesting is, in the short run, an inferior good.

---

[23] Benefits to the citizen from increased democracy may include an increase in 1) the citizen's ability to select the national leader into the future, and 2) the share of GDP flowing to the citizen. However, the empirical literature provides mixed evidence on the expected impact of democratization on economic growth and inequality.
[24] Michael Bratton and Nicolas van de Walle (1992, p. 424) argue that political protests in sub-Saharan Africa have been "spurred by deepening economic hardship"; Dipak K. Gupta, M.C. Madhavan, and Andrew Blee (1998, p. 600) that faster per capita GDP growth "dampens the spirits of protest"; Daron Acemoglu and James A. Robinson (2001) that the cost of revolutionary activity is lower during recessions; Jenny A. Minier (2001, p. 1000) that "as wages increase, the opportunity cost of demonstrating also increases"; Paul J. Zac and Yi Feng (2003, p. 9) that "as wages rise, the opportunity cost of demonstrating increases"; and Sam H.K. Tang and Linda C.W. Yung (2008, p. 245) that economic growth "increases the opportunity cost of participating in democratic movements".

A2: Protesting increases the citizen's expectation of the likelihood of democratic change during year t (p_t).

A3: p_t and ΔD_t are not direct functions of ΔY_t. This allows for a partial equilibrium analysis.

For exposition, we adopt the utility function:

(7) $U_t = U_{t-1} + \alpha \Delta Y_t + \beta \Delta D_t - C_t(\Delta Y_t)$

Adopting an additive- rather than multiplicative-form utility function appears appropriate, given that ΔD_t and/or $C_t(\Delta Y_t)$ may be equal to zero. α and β are assumed to be fixed.

The expected net benefit to the citizen of protesting is:

(8) $B_{Protest,t} = p_t \beta \Delta D_t - (1 - p_t) C_t(\Delta Y_t)$

Let us look at the best response strategies of both players.

<u>Autocrat</u>:

A4: If the citizen acquiesces to autocratic rule, the autocrat's best response is to choose the status quo over conferring political rights. In terms of payoffs, $t > v$.

A5: If the citizen (strongly) protests autocratic rule, the autocrat's position becomes untenable, and the autocrat's best response is to confer some political rights.[25] *In terms of payoffs, $z > x$.*

---

[25] Bratton and van de Walle (1992, p. 434) argue that "elites almost invariably prefer the status quo to the unknown; only when their position becomes unsustainable do they seek instead to manage an inevitable change".

Citizen:

By (3) and (6), if the autocrat chooses to confer political rights, the citizen's best response is to protest autocratic rule (equivalent to accepting the new rights), so that $y > u$.

A6 (initial condition): In year 1, $B_{Protest,t} < 0$. The citizen's best response if the autocrat chooses to maintain the status quo is to acquiesce to autocratic rule. The acquiescence of the citizen in year 1 arises due to the costs of protesting against the status quo.

From *A4-A6* and (3)-(6), the best response choices of the two players in the game are indicated by the underlined payoffs in Figure A2. There are two Nash equilibria to the game in year 1: (Acquiesce, Status quo) and (Protest, Confer political rights). Initially, given that this is an autocracy, the game is assumed to be at the (Acquiesce, Status quo) equilibrium (*A6*). This initial equilibrium prevails because the citizen, wary of the costs of protesting autocratic rule in the case that the autocrat opts not to confer greater political rights, decides to acquiesce. Intuitively, the autocratic equilibrium is maintained in the first period because the citizen would prefer not to pay the switching costs of moving to the more democratic equilibrium.

Figure A2. Best response strategies in year 1

		Autocrat	
		Status quo	*Confer political rights*
Citizen	*Acquiesce*	(*s*, *t*)	(*u*, *v*)
	Protest	(*w*, *x*)	(*y*, *z*)

B. The economic growth rate and the likelihood of democratic change

What could potentially lead to a switch from the prevailing Nash equilibrium (Acquiesce, Status quo) to the alternative Nash equilibrium (Protest, Confer political rights) in year 2? A negative shock to income may trigger such a switch.

Proposition 1: *The likelihood of democratic change in year t is a negative function of the economic growth rate.*

The first differential of (8) with respect to ΔY_t is:

$$(9) \qquad \frac{\partial B_{Protest,t}(\Delta Y_t)}{\partial \Delta Y_t} = -(1 - p_t)C_t'(\Delta Y_t) < 0 \ \ (\text{by } A1)$$

If the citizen chooses to protest, this results in the breakdown of the prevailing autocratic equilibrium, and democratic change (by *A5*). The citizen's choice of whether to protest is made on the basis of *B*. Thus, the probability of democratic change during year *t* (η_t) is a positive function of the expected net benefit of protest in year *t*:

$$(10) \qquad \eta_t = f(B_{Protest,t}(\Delta Y_t)) \qquad\qquad\qquad \text{with}$$

$$(11) \qquad \frac{\partial \eta_t}{\partial (B_{Protest,t}(\Delta Y_t))} > 0$$

The first differential of the probability of democratic change with respect to ΔY_t is:

$$(12) \qquad \frac{\partial \eta_t}{\partial \Delta Y_t} = \frac{\partial \eta_t}{\partial (B_{Protest,t}(\Delta Y_t))} \cdot \frac{\partial (B_{Protest,t}(\Delta Y_t))}{\partial \Delta Y_t} < 0 \ \ (\text{from 9 and 11})$$

Thus, the higher the rate of economic growth in year t, the less likely the citizen is to protest autocratic rule, and the less likely the country is to experience democratic change. If ΔY_2 is sufficiently low that $B_{Protest,2} > 0$, the (Acquiesce, Status quo) equilibrium will break down in year 2 (Figure A3). In this case, the only remaining Nash equilibrium is (Protest, Confer political rights). Thus, a reduction in the switching costs faced by the citizen can lead to the more democratic Nash equilibrium (Protest, Confer political rights).

Figure A3. Best response strategies in year 2

		Autocrat	
		Status quo	*Confer political rights*
Citizen	*Acquiesce*	(*s*, $\underline{t}$)	(*u*, *v*)
	Protest	($\underline{w}$, *x*)	($\underline{y}$, $\underline{z}$)

A lower economic growth rate in year t will, *ceteris paribus*, induce the citizen to substitute toward protest and reduce the viability of the autocratic equilibrium. If a large group of citizens are affected in a similar way, the collective action problem associated with anti-government protests is likely to become much easier to solve. Whether (12) holds empirically is the focus of the paper. We also explore whether protests indeed coincide with economic slowdowns and precede democratic change.

A-III. Empirics: additional estimation results

Table A1-Democratic change events regressions with interaction terms

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable	Democratic change events				Autocratic change events			
Estimation	LPM	IV (F1)	LPM	IV (F1)	LPM	IV (F1)	LPM	IV (F1)
Excluded instruments	None	All	None	All	None	All	None	All
GDP per capita growth$_{t-1}$	-0.0010**	-0.0127**	-0.0004	-0.0203*	-0.0005	0.0178	-0.0005	0.0074
	(0.0005)	(0.0064)	(0.0005)	(0.0114)	(0.0004)	(0.0161)	(0.0005)	(0.0079)
GDP per capita growth$_{t-1}$ *Country-specific development level$_{t-2}$	-0.0002	-0.0068			-0.0007	-0.0257		
	(0.0003)	(0.0139)			(0.0005)	(0.0329)		
GDP per capita growth$_{t-1}$ *POLITY score$_{t-1}$			0.0001	-0.0011			-0.0001	-0.0005
			(0.0001)	(0.0014)			(0.0001)	(0.0013)
Country-specific development level$_{t-2}$	-0.0151**	-0.0116	-0.0155**	-0.0402***	-0.0010	0.1321	-0.0041	0.0057
	(0.0076)	(0.0529)	(0.0075)	(0.0144)	(0.0057)	(0.1730)	(0.0050)	(0.0137)
POLITY score$_{t-1}$	-0.0098***	-0.0092***	-0.0099***	-0.0087***	0.0087***	0.0117***	0.0088***	0.0097***
	(0.0015)	(0.0019)	(0.0015)	(0.0023)	(0.0012)	(0.0044)	(0.0012)	(0.0019)
Tenure of regime$_{t-1}$ (years)	0.0010*	0.0017	0.0010*	0.0004	0.0019***	0.0050	0.0018***	0.0020***
	(0.0006)	(0.0022)	(0.0006)	(0.0015)	(0.0004)	(0.0052)	(0.0005)	(0.0005)
Democracy in region$_{t-1}$ (percent of countries)	0.0005	-0.0003	0.0006	-0.0009	-0.0008***	-0.0005	-0.0008***	-0.0007*
	(0.0006)	(0.0008)	(0.0006)	(0.0012)	(0.0003)	(0.0007)	(0.0003)	(0.0003)
Country and year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F statistic on excluded instruments	-	1.05	-	2.11	-	0.38	-	2.40
Stock-Yogo critical values	-	4.44/7.16	-	4.44/7.16	-	4.44/7.16	-	4.44/7.16
Partial R^2 on excluded instruments	-	0.011, 0.002	-	0.011, 0.011	-	0.008, 0.003	-	0.008, 0.005
Observations	2,897	2,897	2,897	2,897	3,598	3,598	3,598	3,598
Number of democratic change events	118	118	118	118	63	63	63	63
Countries	121	121	121	121	141	141	141	141
Years: 1963-2001								

Notes: A democratic change event is the year-*t* commencement of a 3 or more point increase in POLITY score that occurs within 3 years, flagged by a positive REGTRANS score; an autocratic change event is the year-*t* commencement of a 3 or more point decrease in POLITY score that occurs within 3 years, flagged by a negative REGTRANS score. The set of all instruments includes the precipitation instrument$_{t-1, t-2}$, the temperature instrument$_{t-1, t-2}$, and the commodity price instrument$_{t-1}$. Robust standard errors clustered by country are in parentheses. GDP per capita growth is scaled so that one percentage point of additional growth is 1, not 0.01. The country-specific development level variable equals 0 when a country's *t*-2 per capita GDP is within 30 log points of its sample average *t*-2 per capita GDP, +1 (-1) when 30-60 log points above (below) its sample average, and +2 (-2) when 60 or more log points above (below) its sample average. Estimates control for the secondary school enrollment rate (percent gross) and the percent of the population aged 65 years and above in year *t*-2. The sample excludes country-years in which the *t*-1 POLITY score exceeds 7. The full set of instruments is used to instrument for two explanatory variables (GDP per capita growth and the interaction of this variable) in columns 2, 4, 6, and 8. Reported Stock-Yogo critical values are the 5 percent significance level critical values for weak instruments tests based on, respectively, 30 percent and 5 percent maximal Fuller relative bias. The null of weak instruments is rejected in the case that the *F* statistic on the excluded instruments exceeds the Stock-Yogo critical value/s. F1 is Fuller 1. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Table A2-Using Penn World Table GDP and GDI growth data

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		Democratic change events				Autocratic change events		
Estimation	LPM	IV (F1)	IV (F1)	IV (F1)	LPM	IV (F1)	IV (F1)	IV (F1)
Excluded instruments	None	All	Weather instruments t-1, t-2	Commodity price instrument t-1	None	All	Weather instruments t-1, t-2	Commodity price instrument t-1
Panel A: GDP per capita growth, World Development Indicators								
GDP per capita growth$_{t-1}$	-0.0008*	-0.0134**	-0.0156**	-0.0004	-0.0003	0.0113	0.0112	-0.0005**
	(0.0005)	(0.0064)	(0.0073)	(0.0061)	(0.0004)	(0.0099)	(0.0096)	(0.0002)
F statistic on excluded instruments	-	6.32	7.07	4.21	-	3.61	4.46	0.00
Partial R^2 on excluded instruments	-	0.011	0.009	0.002	-	0.006	0.006	0.000
Panel B: GDP per capita growth, Penn World Table								
GDP per capita growth$_{t-1}$	-0.0002	-0.0099*	-0.0154**	-0.0003	-0.0003	0.0072	0.0087*	-0.0065
	(0.0004)	(0.0058)	(0.0077)	(0.0037)	(0.0003)	(0.0048)	(0.0052)	(0.0064)
F statistic on excluded instruments	-	3.73	3.40	1.69	-	7.71	8.91	4.19
Partial R^2 on excluded instruments	-	0.008	0.004	0.003	-	0.009	0.008	0.001
Panel C: GDI per capita growth, Penn World Table								
GDI per capita growth$_{t-1}$	0.0004	-0.0009	-0.0168**	-0.0002	-0.0002	0.0002	0.0070	-0.0012
	(0.0005)	(0.0008)	(0.0084)	(0.0007)	(0.0004)	(0.0013)	(0.0050)	(0.0013)
F statistic on excluded instruments	-	10.75	2.50	28.52	-	10.07	6.72	26.06
Partial R^2 on excluded instruments	-	0.081	0.003	0.077	-	0.047	0.008	0.038
Country and year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Stock-Yogo critical values	-	4.03/6.42	4.63/7.63	12.71/24.09	-	4.03/6.42	4.63/7.63	12.71/24.09
Observations	2,809	2,809	2,809	2,809	3,526	3,526	3,526	3,526
Number of events	115	115	115	115	59	59	59	59
Countries	120	120	120	120	140	140	140	140
Years: 1963-2001								

Notes: A democratic change event is the year-*t* commencement of a 3 or more point increase in POLITY score that occurs within 3 years, flagged by a positive REGTRANS score; an autocratic change event is the year-*t* commencement of a 3 or more point decrease in POLITY score that occurs within 3 years, flagged by a negative REGTRANS score. The weather instruments include the precipitation instrument$_{t-1, t-2}$ and the temperature instrument$_{t-1, t-2}$. Robust standard errors clustered by country are in parentheses. Per capita growth is scaled so that one percentage point of additional growth is 1, not 0.01. Estimates control for the country-specific development level in year *t*-2, the POLITY score in year *t*-1, the tenure of the regime in year *t*-1 (years), the percent of countries in the region that are democracies in year *t*-1, the secondary school enrollment rate (percent gross) in year *t*-2, and the percent of the population aged 65 years and above in year *t*-2. The country-specific development level variable equals 0 when a country's *t*-2 per capita GDP/GNI is within 30 log points of its sample average *t*-2 per capita GDP/GNI, +1 (-1) when 30-60 log points above (below) its sample average, and +2 (-2) when 60 or more log points above (below) its sample average. The sample in columns 1-4 excludes country-years in which the *t*-1 POLITY score exceeds 7. The sample in columns 5-8 excludes country-years in which the *t*-1 POLITY score is less than -7. Reported Stock-Yogo critical values are the 5 percent significance level critical values for weak instruments tests based on, respectively, 30 percent and 5 percent maximal Fuller relative bias. The null of weak instruments is rejected in the case that the *F* statistic on the excluded instruments exceeds the Stock-Yogo critical value/s. F1 is Fuller 1. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Table A3-Instrumental variable results for democratic change events: sub-samples
(Table 6 continued)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Sample	Full	More agricultural	Less agricultural	Low mean GDP per capita	High mean GDP per capita	Sub-Saharan Africa	Rest of world
Panel A: All instruments							
GDP per capita growth$_{t-1}$	-0.0140**	-0.0154**	-0.0102	-0.0144**	-0.0074	-0.0110	-0.0109
	(0.0063)	(0.0063)	(0.0091)	(0.0071)	(0.0130)	(0.0072)	(0.0104)
F statistic on excluded instruments	6.66	5.63	2.90	5.00	2.10	4.38	3.27
Partial R^2 on excluded instruments	0.011	0.014	0.011	0.010	0.010	0.011	0.011
Chow test *p*-value of equality with parameter estimate in column 1	-	0.86	0.63	0.86	0.22	0.45	0.56
Stock-Yogo critical values: 4.03/6.42							
Panel B: Precipitation instruments							
GDP per capita growth$_{t-1}$	-0.0112	-0.0143*	-0.0015	-0.0143	0.0022	-0.0132*	-0.0016
	(0.0085)	(0.0079)	(0.0164)	(0.0106)	(0.0129)	(0.0077)	(0.0212)
F statistic on excluded instruments	3.50	3.91	1.18	2.53	1.58	2.76	1.54
Partial R^2 on excluded instruments	0.004	0.007	0.002	0.003	0.003	0.005	0.003
Chow test *p*-value of equality with parameter estimate in column 1	-	0.59	0.50	0.62	0.16	0.70	0.38
Stock-Yogo critical values: 7.49/13.46							
Panel C: Temperature instruments							
GDP per capita growth$_{t-1}$	-0.0173**	-0.0180*	-0.0140	-0.0133*	-0.0210	-0.0159	-0.0134
	(0.0078)	(0.0093)	(0.0101)	(0.0077)	(0.0156)	(0.0117)	(0.0104)
F statistic on excluded instruments	14.65	9.36	4.84	8.58	4.57	4.66	7.15
Partial R^2 on excluded instruments	0.007	0.008	0.008	0.008	0.005	0.005	0.008
Chow test *p*-value of equality with parameter estimate in column 1	-	0.93	0.74	0.60	0.67	0.69	0.57
Stock-Yogo critical values: 7.49/13.46							
Country and year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,897	1,464	1,433	1,452	1,445	1,224	1,673
Number of democratic change events	118	50	68	57	61	43	75
Countries	121	54	67	59	62	43	78
Estimation: IV (F1)							
Years: 1963-2001							

Notes: Panel A is identical to Panel A of Table 6. The dependent variable indicates the commencement of democratic change events, which involve a 3 or more point increase in POLITY score that occurs within 3 years, flagged by a positive REGTRANS score. The set of all instruments includes the precipitation instrument$_{t-1, t-2}$, the temperature instrument$_{t-1, t-2}$, and the commodity price instrument$_{t-1}$. Robust standard errors clustered by country are in parentheses. GDP per capita growth is scaled so that one percentage point of additional growth is 1, not 0.01. Estimates control for the country-specific development level in year *t*-2, the POLITY score in year *t*-1, the tenure of the regime in year *t*-1 (years), the percent of countries in the region that are democracies in year *t*-1, the secondary school enrollment rate (percent gross) in year *t*-2, and the percent of the population aged 65 years and above in year *t*-2. More agricultural countries are those with a 1961 labor force share in agriculture equal to the sample median level (72 percent) or higher; non-agricultural countries are others. Low mean GDP per capita countries are those with a sample average *t*-2 GDP per capita equal to the sample median ($765.5, in 2000 US$) or below; high mean GDP per capita countries are others. All samples exclude country-years in which the *t*-1 POLITY score exceeds 7. Reported Stock-Yogo critical values are the 5 percent significance level critical values for weak instruments tests based on, respectively, 30 percent and 5 percent maximal Fuller relative bias. The null of weak instruments is rejected in the case that the *F* statistic on the excluded instruments exceeds the Stock-Yogo critical value/s. Chow test *p* values are based on two-stage least squares estimates. F1 is Fuller 1. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

	(1)	(2)	(3)	(4)
Dependent variable	Democratic change events		Autocratic change events	
Estimation	LPM	IV (F1)	LPM	IV (F1)
Excluded instruments	None	All	None	All
(Above previous year) GDP per capita growth$_{t-1}$	-0.0010*	-0.0003	-0.0003	0.0215
	(0.0005)	(0.0112)	(0.0005)	(0.0655)
(Below previous year) GDP per capita growth$_{t-1}$	-0.0008	-0.0242*	-0.0005	0.0013
	(0.0008)	(0.0125)	(0.0007)	(0.0244)
Country-specific development level$_{t-2}$	-0.0156**	-0.0321**	-0.0039	0.0116
	(0.0075)	(0.0135)	(0.0050)	(0.0290)
POLITY score$_{t-1}$	-0.0098***	-0.0094***	0.0087***	0.0096***
	(0.0015)	(0.0016)	(0.0012)	(0.0029)
Tenure of regime$_{t-1}$ (years)	0.0010*	0.0013	0.0019***	0.0026
	(0.0006)	(0.0010)	(0.0005)	(0.0026)
Democracy in region$_{t-1}$ (percent of countries)	0.0005	-0.0007	-0.0008***	-0.0007
	(0.0006)	(0.0010)	(0.0003)	(0.0005)
Country and year fixed effects	Yes	Yes	Yes	Yes
p-value on *t*-test of equality of "above" and "below" parameters	0.79	0.24	0.78	0.82
F statistic on excluded instruments	-	1.03	-	0.42
Stock-Yogo critical value	-	4.44/7.16	-	4.44/7.16
Partial R^2 on excluded instruments	-	0.004, 0.009	-	0.001, 0.009
Observations	2,897	2,897	3,598	3,598
Number of events	118	118	63	63
Countries	121	121	141	141
Years: 1963-2001				

Notes: A democratic change event is the year-*t* commencement of a 3 or more point increase in POLITY score that occurs within 3 years, flagged by a positive REGTRANS score; an autocratic change event is the year-*t* commencement of a 3 or more point decrease in POLITY score that occurs within 3 years, flagged by a negative REGTRANS score. The set of all instruments includes the precipitation instrument$_{t-1, t-2}$, the temperature instrument$_{t-1, t-2}$, and the commodity price instrument$_{t-1}$. Robust standard errors clustered by country are in parentheses. GDP per capita growth is scaled so that one percentage point of additional growth is 1, not 0.01. The country-specific development level variable equals 0 when a country's *t*-2 per capita GDP is within 30 log points of its sample average *t*-2 per capita GDP, +1 (-1) when 30-60 log points above (below) its sample average, and +2 (-2) when 60 or more log points above (below) its sample average. Estimates control for the secondary school enrollment rate (percent gross) and the percent of the population aged 65 years and above in year *t*-2. The sample in columns 1 and 2 excludes country-years in which the *t*-1 POLITY score exceeds 7. The sample in columns 3 and 4 excludes country-years in which the *t*-1 POLITY score is less than -7. (Above previous year) GDP per capita growth$_{t-1}$ equals GDP per capita growth$_{t-1}$ if the latter exceeds GDP per capita growth in year *t*-2, and 0 otherwise. (Below previous year) GDP per capita growth$_{t-1}$ equals GDP per capita growth$_{t-1}$ if the latter is less than GDP per capita growth in year *t*-2, and 0 otherwise. The IV regressions in columns 2 and 4 each instrument for two endogenous variables (the two growth variables). Reported Stock-Yogo critical values are the 5 percent significance level critical values for weak instruments tests based on, respectively, 30 percent and 5 percent maximal Fuller relative bias. The null of weak instruments is rejected in the case that the *F* statistic on the excluded instruments exceeds the Stock-Yogo critical value/s. F1 is Fuller 1. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

In Table A5, we interact the instruments with an instrument-specific variable equal to 1 for a positive shock and 0 for a negative shock (and vice-versa). A positive value for the precipitation and commodity price instruments is considered to be a positive shock, and a negative value a negative shock. A positive value for the temperature instrument is considered to be a negative shock, and a negative value a positive shock. The results indicate, firstly, that negative shocks are much more strongly correlated with economic growth than positive shocks (as evidenced by the larger F statistic and partial R-squared on the excluded instruments). But the estimated effect of growth on the likelihood of democratic change events is very similar using either positive or negative shocks. A Chow test of the hypothesis that the growth coefficients in columns 2 and 3 are equal does not reject the null hypothesis. This indicates that the relationship between economic shocks and democratic change events is not subject to asymmetry. The estimated effect of growth on the likelihood of autocratic change events is statistically insignificant when instrumenting with either positive or negative shocks to growth, although a Chow test indicates that the effects of positive and negative shocks on autocratic change events differ.

Table A5-Testing for asymmetry in effect of growth shocks

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	Democratic change events			Autocratic change events		
Excluded instruments	All	All (Positive shocks)	All (Negative shocks)	All	All (Positive shocks)	All (Negative shocks)
GDP per capita growth$_{t-1}$	-0.0140**	-0.0147*	-0.0148**	0.0079	0.0211	-0.0037
	(0.0063)	(0.0086)	(0.0066)	(0.0079)	(0.0187)	(0.0056)
Country-specific development level$_{t-2}$	-0.0370***	-0.0382**	-0.0383***	0.0077	0.0260	-0.0084
	(0.0132)	(0.0174)	(0.0129)	(0.0132)	(0.0291)	(0.0090)
POLITY score$_{t-1}$	-0.0099***	-0.0099***	-0.0099***	0.0091***	0.0097***	0.0085***
	(0.0016)	(0.0017)	(0.0017)	(0.0013)	(0.0018)	(0.0012)
Tenure of regime$_{t-1}$ (years)	0.0006	0.0006	0.0006	0.0021***	0.0024**	0.0018***
	(0.0011)	(0.0012)	(0.0012)	(0.0005)	(0.0011)	(0.0006)
Democracy in region$_{t-1}$ (percent of countries)	-0.0003	-0.0003	-0.0003	-0.0006*	-0.0003	-0.0009***
	(0.0008)	(0.0008)	(0.0008)	(0.0003)	(0.0006)	(0.0003)
Country and year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
F statistic on excluded instruments	6.66	2.64	7.96	4.94	2.30	6.55
Partial R^2 on excluded instruments	0.011	0.004	0.012	0.008	0.003	0.010
p-value from Chow test of equality of impact of GDP per capita growth$_{t-1}$ using positive and negative shocks		0.95			0.04	
Observations	2,897	2,897	2,897	3,598	3,598	3,598
Number of events	118	118	118	63	63	63
Countries	121	121	121	141	141	141
Estimation: IV (F1)						
Stock-Yogo critical values: 4.03/6.42						
Years: 1963-2001						

Notes: A democratic change event is the year-t commencement of a 3 or more point increase in POLITY score that occurs within 3 years, flagged by a positive REGTRANS score; an autocratic change event is the year-t commencement of a 3 or more point decrease in POLITY score that occurs within 3 years, flagged by a negative REGTRANS score. Robust standard errors clustered by country are in parentheses. GDP per capita growth is scaled so that one percentage point of additional growth is 1, not 0.01. The country-specific development level variable equals 0 when a country's t-2 per capita GDP is within 30 log points of its sample average t-2 per capita GDP, +1 (-1) when 30-60 log points above (below) its sample average, and +2 (-2) when 60 or more log points above (below) its sample average. Estimates control for the secondary school enrollment rate (percent gross) and the percent of the population aged 65 years and above in year t-2. Positive shocks include only positive values of the precipitation and commodity price instruments, and only negative values of the temperature instruments (0 otherwise). Negative shocks include only negative values of the precipitation and commodity price instruments, and only positive values of the temperature instruments (0 otherwise). The sample in columns 1-3 excludes country-years in which the t-1 POLITY score exceeds 7. The sample in columns 4-6 excludes country-years in which the t-1 POLITY score is less than -7. Reported Stock-Yogo critical values are the 5 percent significance level critical values for weak instruments tests based on, respectively, 30 percent and 5 percent maximal Fuller relative bias. The null of weak instruments is rejected in the case that the F statistic on the excluded instruments exceeds the Stock-Yogo critical value/s. Chow test p values are based on two-stage least squares estimates. F1 is Fuller 1. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Table A6-Specifications using alternative definitions of democratic change events

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent variable	All democratic change events	Democratic change events: Major or minor democratic transition (REGTRANS=2, 3)	Democratic change events: POLITY score increased by 4 or more points	Democratic change events: not reversed during subsequent 4 calendar years	Democratic change events: completed within 2 calendar years	Democratic change events: leader change in year t, t+1, or t+2	Democratic change events: no leader change in year t, t+1, or t+2
GDP per capita growth$_{t-1}$	-0.0140**	-0.0092*	-0.0099*	-0.0124**	-0.0130**	-0.0102*	-0.0037
	(0.0063)	(0.0055)	(0.0053)	(0.0060)	(0.0061)	(0.0054)	(0.0039)
Country-specific development level$_{t-2}$	-0.0370***	-0.0267**	-0.0325***	-0.0343***	-0.0330***	-0.0198*	-0.0172**
	(0.0132)	(0.0118)	(0.0121)	(0.0124)	(0.0125)	(0.0108)	(0.0076)
POLITY score$_{t-1}$	-0.0099***	-0.0080***	-0.0090***	-0.0090***	-0.0087***	-0.0072***	-0.0028***
	(0.0016)	(0.0017)	(0.0018)	(0.0016)	(0.0014)	(0.0013)	(0.0008)
Tenure of regime$_{t-1}$ (years)	0.0006	0.0005	0.0009	0.0005	0.0003	0.0002	0.0005
	(0.0011)	(0.0008)	(0.0010)	(0.0010)	(0.0010)	(0.0009)	(0.0004)
Democracy in region$_{t-1}$ (percent of countries)	-0.0003	0.0009	0.0006	-0.0002	-0.0001	0.0002	-0.0004
	(0.0008)	(0.0006)	(0.0007)	(0.0007)	(0.0007)	(0.0006)	(0.0004)
Country and year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F statistic on excluded instruments	6.66	8.74	8.74	6.66	6.66	6.66	6.66
Partial R^2 on excluded instruments	0.011	0.012	0.012	0.011	0.011	0.011	0.011
Observations	2,897	2,715	2,715	2,897	2,897	2,897	2,897
Share of democratic change events (percent)	100	71	82	86	86	72	28
Countries	121	117	117	121	121	121	121

Estimation: IV (F1)

Excluded instruments: All

Stock-Yogo critical values: 4.03/6.42

Years: 1963-2001

Notes: A democratic change event is the year-t commencement of a 3 or more point increase in POLITY score that occurs within 3 years, flagged by a positive REGTRANS score. The set of all instruments includes the precipitation instrument$_{t-1, t-2}$, the temperature instrument$_{t-1, t-2}$, and the commodity price instrument$_{t-1}$. Robust standard errors clustered by country are in parentheses. GDP per capita growth is scaled so that one percentage point of additional growth is 1, not 0.01. The country-specific development level variable equals 0 when a country's t-2 per capita GDP is within 30 log points of its sample average t-2 per capita GDP, +1 (-1) when 30-60 log points above (below) its sample average, and +2 (-2) when 60 or more log points above (below) its sample average. Estimates control for the secondary school enrollment rate (percent gross) and the percent of the population aged 65 years and above in year t-2. Results in column 1 are identical to those in column 2 of Table 4. The sample excludes country-years in which the t-1 POLITY score exceeds 7. The sample in columns 2 and 3 excludes country-years in which the lagged POLITY score is equal to 7. Reported Stock-Yogo critical values are the 5 percent significance level critical values for weak instruments tests based on, respectively, 30 percent and 5 percent maximal Fuller relative bias. The null of weak instruments is rejected in the case that the F statistic on the excluded instruments exceeds the Stock-Yogo critical value/s. F1 is Fuller 1. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Table A7-Democratic change events using Przeworski et al. (2000) and Freedom House dependent variables

	(1)	(2)	(3)	(4)	(5)	(6)
Data source	Polity IV		Przeworski et al.		Freedom House	
Estimation	LPM	IV (F1)	LPM	IV (F1)	LPM	IV (F1)
Excluded instruments	None	All	None	All	None	All
GDP per capita growth$_{t-1}$	-0.0009*	-0.0144**	-0.0009*	0.0045	-0.0024**	0.0007
	(0.0005)	(0.0068)	(0.0005)	(0.0042)	(0.0010)	(0.0078)
Country and year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
F statistic on excluded instruments	-	6.68	-	5.16	-	6.96
Stock-Yogo critical value	-	4.03/6.42	-	4.03/6.42	-	4.03/6.42
Partial R^2 on excluded instruments	-	0.010	-	0.010	-	0.016
Observations	2,897	2,897	2,394	2,394	2,103	2,103
Number of democratic change events	118	118	42	42	212	209
Countries	121	121	107	107	120	120
Years: 1963-2001						

Notes: The dependent variable is the commencement of a year-*t* democratic change event. A Polity IV democratic change event is the year-*t* commencement of a 3 or more point increase in POLITY score that occurs within 3 years, flagged by a positive REGTRANS score. A Przeworski et al. democratic change event is a change in regime from dictatorship to democracy in year *t*. A Freedom House democratic change event is defined as the year-*t* commencement of a reduction in the Political Rights Index of 1 point or more that occurs within 3 years. A1 point reduction in the Political Rights Index is equivalent in relative magnitude to a 3-point increase in POLITY score. Democratic change events in the year of the natural death of the national leader are excluded. The set of all instruments includes the precipitation instrument$_{t-1, t-2}$, the temperature instrument$_{t-1, t-2}$, and the commodity price instrument$_{t-1}$. Robust standard errors clustered by country are in parentheses. GDP per capita growth is scaled so that one percentage point of additional growth is 1, not 0.01. The estimates control for the country-specific development level variable, the secondary school enrollment rate (percent gross), and the percent of the population aged 65 years and above in year *t*-2. The other controls are not included because they are not consistently defined across columns. The country-specific development level variable equals 0 when a country's *t*-2 per capita GDP is within 30 log points of its sample average *t*-2 per capita GDP, +1 (-1) when 30-60 log points above (below) its sample average, and +2 (-2) when 60 or more log points above (below) its sample average. The sample in columns 1-2 excludes country-years in which the *t*-1 POLITY score exceeds 7. The sample in columns 3-4 is further restricted to only countries classified by Przeworski et al. as dictatorships in year *t*-1, while the sample in columns 5-6 is further restricted by data availability and to countries with a lagged Political Rights Index score of 2 or above. Reported Stock-Yogo critical values are the 5 percent significance level critical values for weak instruments tests based on, respectively, 30 percent and 5 percent maximal Fuller relative bias. The null of weak instruments is rejected in the case that the *F* statistic on the excluded instruments exceeds the Stock-Yogo critical value/s. F1 is Fuller 1. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Table A8-Autocratic change events using Przeworski et al. (2000) and Freedom House dependent variables

	(1)	(2)	(3)	(4)	(5)	(6)
Data source	Polity IV		Przeworski et al.		Freedom House	
Estimation	LPM	IV (F1)	LPM	IV (F1)	LPM	IV (F1)
Excluded instruments	None	All	None	All	None	All
GDP per capita growth$_{t-1}$	-0.0006	0.0087	-0.0022**	0.0093	-0.0016	-0.0047
	(0.0004)	(0.0083)	(0.0010)	(0.0197)	(0.0013)	(0.0167)
Country and year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
F statistic on excluded instruments	-	4.82	-	0.90	-	3.62
Stock-Yogo critical value	-	4.03/6.42	-	4.03/6.42	-	4.03/6.42
Partial R^2 on excluded instruments	-	0.008	-	0.008	-	0.010
Observations	3,598	3,598	1,853	1,853	2,424	2,424
Number of autocratic change events	63	63	34	34	174	174
Countries	141	141	92	92	139	139
Years: 1963-2001						

Notes: The dependent variable is the commencement of a year-*t* autocratic change event. A Polity IV autocratic change event is the year-*t* commencement of a 3 or more point decrease in POLITY score that occurs within 3 years, flagged by a negative REGTRANS score. A Przeworski et al. autocratic change event is a change in regime from democracy to dictatorship in year *t*. A Freedom House autocratic change event is defined as the year-*t* commencement of an increase in the Political Rights Index of 1 point or more that occurs within 3 years. A1 point increase in the Political Rights Index is equivalent in relative magnitude to a 3-point decrease in POLITY score. Autocratic change events in the year of the natural death of the national leader are excluded. The set of all instruments includes the precipitation instrument$_{t-1, t-2}$, the temperature instrument$_{t-1, t-2}$, and the commodity price instrument$_{t-1}$. Robust standard errors clustered by country are in parentheses. GDP per capita growth is scaled so that one percentage point of additional growth is 1, not 0.01. The estimates control for the country-specific development level variable, the secondary school enrollment rate (percent gross), and the percent of the population aged 65 years and above in year *t*-2. The other controls are not included because they are not consistently defined across columns. The country-specific development level variable equals 0 when a country's *t*-2 per capita GDP is within 30 log points of its sample average *t*-2 per capita GDP, +1 (-1) when 30-60 log points above (below) its sample average, and +2 (-2) when 60 or more log points above (below) its sample average. The sample in columns 1-2 excludes country-years in which the *t*-1 POLITY score is less then -7. The sample in columns 3-4 is further restricted to only countries classified by Przeworski et al. as democracies in year *t*-1, while the sample in columns 5-6 is further restricted by data availability and to countries with a lagged Political Rights Index score of 6 or below. Reported Stock-Yogo critical values are the 5 percent significance level critical values for weak instruments tests based on, respectively, 30 percent and 5 percent maximal Fuller relative bias. The null of weak instruments is rejected in the case that the *F* statistic on the excluded instruments exceeds the Stock-Yogo critical value/s. F1 is Fuller 1. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Table A9-Anti-government protests and democratic and autocratic change events

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	Democratic change events			Autocratic change events		
Estimation	LPM	LPM	IV	LPM	LPM	IV
GDP per capita growth$_{t-1}$		-0.0009*	-0.0134**		-0.0005	0.0064
		(0.0005)	(0.0060)		(0.0005)	(0.0072)
Anti-government protests$_t$	0.0153***	0.0154***	0.0162***	0.0014	0.0015	0.0010
	(0.0051)	(0.0051)	(0.0053)	(0.0012)	(0.0012)	(0.0013)
Anti-government protests$_{t-1}$	-0.0009	-0.0013	-0.0064	0.0003	0.0002	0.0013
	(0.0033)	(0.0033)	(0.0045)	(0.0012)	(0.0011)	(0.0020)
Country and year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
F statistic on excluded instruments	-	-	7.69	-	-	4.20
Stock-Yogo critical value	-	-	4.03/6.42	-	-	4.03/6.42
Partial R^2 on excluded instruments	-	-	0.013	-	-	0.008
Observations	2,820	2,820	2,820	3,538	3,538	3,538
Number of events	116	116	116	60	60	60
Countries	120	120	120	140	140	140

IV estimation: F1

Excluded instruments in IV regressions: All
Years: 1963-2007

Notes: A democratic change event is the year-*t* commencement of a 3 or more point increase in POLITY score that occurs within 3 years, flagged by a positive REGTRANS score; an autocratic change event is the year-*t* commencement of a 3 or more point decrease in POLITY score that occurs within 3 years, flagged by a negative REGTRANS score. The set of all instruments includes the precipitation instrument$_{t-1, t-2}$, the temperature instrument$_{t-1, t-2}$, and the commodity price instrument$_{t-1}$. Robust standard errors clustered by country are in parentheses. GDP per capita growth is scaled so that one percentage point of additional growth is 1, not 0.01. Estimates control for the country-specific development level in year *t*-2, the tenure of the regime in year *t*-1 (years), the percent of countries in the region that are democracies in year *t*-1, the secondary school enrollment rate (percent gross) in year *t*-2, and the percent of the population aged 65 years and above in year *t*-2. The sample in columns 1-3 excludes country-years in which the *t*-1 POLITY score exceeds 7. The sample in columns 4-6 excludes country-years in which the *t*-1 POLITY score is less than -7. Estimates are for smaller samples than those used for the estimations in Table 2 due to missing anti-government protests data. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Table A10-The determinants of anti-government protests

Dependent variable: Number of anti-government protests in year *t-1*

	(1)	(2)	(3)	(4)	(5)
Estimation	LPM	IV (F1)	IV (F1)	IV (F1)	IV (F1)
Excluded instruments	None	All	Precipitation instrument $_{t\text{-}1,\,t\text{-}2}$	Temperature instrument $_{t\text{-}1,\,t\text{-}2}$	Commodity price instrument $_{t\text{-}1}$
GDP per capita growth$_{t\text{-}1}$	-0.0203***	-0.0009	0.0344	-0.0258	0.0206
	(0.0061)	(0.0363)	(0.0488)	(0.0489)	(0.0424)
Country-specific development level$_{t\text{-}2}$	0.0719	0.0974	0.1436	0.0647	0.1256
	(0.0978)	(0.1107)	(0.1282)	(0.1158)	(0.1107)
POLITY score$_{t\text{-}1}$	-0.0229	-0.0217	-0.0195	-0.0232	-0.0204
	(0.0155)	(0.0154)	(0.0151)	(0.0158)	(0.0153)
Tenure of regime$_{t\text{-}1}$ (years)	-0.0103	-0.0101	-0.0096	-0.0104	-0.0098
	(0.0109)	(0.0108)	(0.0110)	(0.0107)	(0.0110)
Democracy in region$_{t\text{-}1}$ (percent of countries)	0.0148**	0.0152**	0.0158**	0.0147**	0.0155**
	(0.0062)	(0.0063)	(0.0062)	(0.0063)	(0.0064)
Country and year fixed effects	Yes	Yes	Yes	Yes	Yes
F statistic on excluded instruments	-	7.86	3.81	15.59	5.47
Stock-Yogo critical value	-	4.03/6.42	7.49/13.46	7.49/13.46	12.71/24.09
Partial R^2 on excluded instruments	-	0.012	0.004	0.008	0.002
Observations	4,240	4,240	4,240	4,240	4,240
Countries	149	149	149	149	149
Years: 1963-2001					

Notes: Robust standard errors clustered by country are in parentheses. The sample is smaller than that for the main estimations due to missing anti-government protests data. GDP per capita growth is scaled so that one percentage point of additional growth is 1, not 0.01. Estimates control for the secondary school enrollment rate (percent gross) and the percent of the population aged 65 years and above in year *t*-2. Reported Stock-Yogo critical values are the 5 percent significance level critical values for weak instruments tests based on, respectively, 30 percent and 5 percent maximal Fuller relative bias. The null of weak instruments is rejected in the case that the *F* statistic on the excluded instruments exceeds the Stock-Yogo critical value/s. F1 is Fuller 1. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

A-IV. Przeworski et al. (2000), Freedom House, and Polity IV measures compared

We use the Polity IV REGTRANS score to flag the commencement of episodes of democratic/autocratic change. The dichotomous regime measure of Przeworski et al. (2000) is less suited for this purpose, because it flags the year in which regime transitions conclude i.e. the year in which new democratic regimes emerge – essentially defined as the point at which a competitive election is first held. Democratic transitions are messy, and often take more than one calendar year to conclude. This means that the Przeworski et al. measure tends to provide a late-moving measure of democratic transitions. The Przeworski et al. measure also allows less dependent variable variation than available in our primary specification using the Polity IV regime transition variable.

The Freedom House Political Rights Index measures political freedoms of individuals rather than regime transitions. Given its focus on outcomes affecting individuals, it often records improvements in political freedoms in years subsequent to the commencement of democratic transitions. It also reflects factors such as the treatment of minorities, corruption and transparency, which are less closely related to regime transitions, and for which the timing of instances of sharp change are more difficult to identify.

Examples to demonstrate that the Polity IV measure is a more appropriate measure of the commencement of episodes of institutional change toward democracy than the Przeworski et al. (2000) and Freedom House measures are provided here (only the Portugal example is provided in the main text).

1. Benin

Polity IV codes the year of the commencement of democratization in Benin as 1990, and that Benin completed its democratic transition in 1991. Przeworski et al. record that a new democratic regime emerged in 1991, the year in which multi-party elections were held (and the incumbent lost). We think that it is appropriate to code 1990 as the year of commencement of democratization, as it was in this year that a national conference was held and a referendum to install a new democratic constitution was called. (Freedom House records improvements in political rights in Benin in 1990 and 1991.)

2. Central African Republic

Polity IV codes the year of commencement of democratization in the Central African Republic as 1991, and that the Central African Republic completed its democratic transition in 1993. Przeworski et al. code the year of the emergence of a democratic regime as 1993, and Freedom House do not record an improvement in political rights until 1993. We agree with the Polity IV identification of 1991 as the first year of the democratization process in the Central African Republic. In this year, the President announced the movement toward a parliamentary democracy and a National Commission was set up to establish a democratic constitution. Multi-party presidential elections were held in 1992 and 1993.

3. Chile

Polity IV codes the year of commencement of democratization in Chile as 1988, and that Chile completed its democratic transition in 1989. The Przeworski et al. measure does not tick over to democracy until 1990. We think that 1988 really is the year of the start of democratization in Chile: in 1988, Pinochet was defeated in a plebiscite, and the

Constitution was amended to diminish the role of the military. The scene was set for a democratic election, which was held in 1989. (Freedom House records improvements in political rights in Chile in each year during the period 1988-1990.)

4. Guatemala

Polity IV codes the year of commencement of democratization in Guatemala as 1984, and that Guatemala completed its democratic transition in 1986. Przeworski et al. code the emergence of a new democratic regime as occurring in 1986. We think that, given our focus on the commencement of democratic change, it is more appropriate to use the Polity IV coding: in 1984, an election for a Constituent Assembly to draft a democratic constitution was held. January 1986 just turned out to be when the election was finally held. (Freedom House records improvements in political rights in Guatemala in 1984, 1985, and 1986.)

5. Hungary

Polity IV codes the year of commencement of democratization in Hungary as 1988, and that Hungary completed its transition to democracy in 1990 (the year in which elections were held). Przeworski et al. code 1990 as the transition year. Freedom House record improvements in political rights in 1989 and 1990. 1988 was the year in which the Hungarian Parliament adopted a "democracy package", which included a new electoral law, a revision of the constitution, and increased democratic freedoms. These reforms set the scene for the 1990 election.

6. Indonesia

Polity IV codes the year of commencement of democratization in Indonesia as 1998, and that Indonesia completed its democratic transition in 1999. Przeworski et al. record 1999 as the year of the emergence of a new democratic regime (a year in which there was democratic elections in Indonesia). Freedom House codes an improvement in political freedoms in each of the years 1998, 1999 and 2000. We think that it is legitimate to code 1998 as the start of the democratization process rather than 1999, as this was the year in which President Suharto stepped down and constraints on executive authority began to be increased.

7. Malawi

Polity IV codes the year of commencement of democratization in Malawi as 1993, and that Malawi completed its democratic transition in 1994. Przeworski et al. code the year of regime transition as 1994. Freedom House also code 1994 as the primary year of realized improvements in political freedoms in Malawi during this period (and also code a 1-point improvement in 1992). We think it more appropriate to code democratization in Malawi as commencing in 1993 than 1994: this was the year in which a referendum was held and a vote in favor of multi-party democracy was recorded. Elections were held the following year.

8. Mongolia

Polity IV codes the year of commencement of democratization in Mongolia as 1990, which is the year widely cited as being the year of Mongolia's Democratic Revolution. 1990 saw the first free parliamentary elections in Mongolia after mass protests. Przeworski et al. do not record a democratic regime in Mongolia until 1992 – the year in

which a new constitution was introduced by the democratically elected government. (Freedom House records improvements in political rights in Mongolia in 1990 and 1991.)

9. Niger

Polity IV codes the year of democratization in Niger as 1999. This was a year in which there was a coup against the former dictator, the drafting of a new constitution, the approval of a democratic constitution by referendum, democratic elections for the legislature and president, and a transition to civilian rule. Przeworski et al. do not code the return to democracy until 2000. (Freedom House record improvements in political freedoms in both 1999 and 2000.)

10. Portugal

Polity IV codes the commencement of a "major democratic transition" in Portugal in 1974, and that the transition concluded in 1976. Przeworski et al. do not record the establishment of a new democratic regime until 1976. Freedom House also does not record an improvement in individuals' political rights until 1976. We think that it is appropriate to code 1974, the year of the Carnation Revolution, as the year of the commencement of democratization in Portugal, despite the fact that it did not immediately herald the installment of a fully-functional democracy.

11. Romania

Polity IV codes the year of commencement of democratization in Romania as 1989 (the year of the Romanian Revolution), and codes Romania as completing its democratic transition in 1990. Przeworski et al. record that the new democratic regime emerged in

occurring in 1990. Similarly, Freedom House's political rights measure does not record an improvement in political rights until 1990. 1989 appears to be the correct year to flag the commencement of democratization in Romania: the election in 1990 was a consequence of the events of 1989.

12. South Korea

Significant amendments to South Korea's constitution, which heralded the commencement of the Sixth Republic, were made in 1987, and democratic elections were held. Polity IV accordingly codes a major democratic transition commencing in 1987 in South Korea. However, Przeworski et al. do not record a democratic transition until 1988. Freedom House similarly does not record democratic change in South Korea until the year ending November 1988. (Freedom House does not provide calendar year data for most of the 1980s.)

A-V. Commodity export price index

The index was constructed for a commodity export bundle of 50 commodities – 35 agricultural commodities and 15 non-agricultural commodities. The commodities included in the bundle are listed in Table A11. This is the same export bundle of 50 commodities used in the construction of the commodity export price index in Paul Collier and Benedikt Goderis (2007).

Table A11-Commodities included in commodity export price index	
Agricultural (35)	**Non-agricultural (15)**
Bananas, Barley, Butter, Cocoa beans, Coconut oil, Coffee, Copra, Cotton, Fish, Groundnut oil, Groundnuts, Hides, Jute, Maize, Olive oil, Oranges, Palm kernel oil, Palm oil, Pepper, Plywood, Poultry, Pulp, Rice, Rubber, Sisal, Sorghum, Soybean oil, Soybeans, Sugar, Sunflower oil, Swine meat, Tea, Tobacco, Wheat, Wool	Aluminium, Coal, Copper, Gasoline, Iron ore, Lead, Natural gas, Nickel, Oil, Phosphate rock, Silver, Tin, Uranium, Urea, Zinc

Commodity export values for 1995 were obtained from the United Nations Commodity Trade Statistics Database, the United Nations Conference on Trade and Development Commodity Yearbook (2003), and the International Trade Statistics Yearbook (United Nations Statistical Office 1996, 1999).[26] 1995 was chosen as the year for which to construct country-specific commodity export weights because it allowed the inclusion of the former Soviet Union countries (Collier and Goderis (2007) used 1990). Annual world commodity price indices were obtained from the International Financial Statistics (IFS) database of the International Monetary Fund for all commodities except natural gas and gasoline.[27] For these two commodities, prices were obtained from the Annual Energy Review 2007 (Energy Information Administration 2008). Prices were deflated using the export unit value from IFS. 15 of the price series had gaps in the early periods. We filled these gaps by either linear

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[26] Commodity Trade Statistics Database: http://comtrade.un.org (accessed 15 January 2009).
[27] http://www.imfstatistics.org/imf/ (accessed 20 January 2009).

interpolation of the deflated price series or by holding the deflated price constant at the first available level.

The price index was arithmetically weighted using the 1995 commodity export shares to construct the country-specific commodity export price index.[28] The index equals 100 for all countries for the year 2000. The index was then logged and differenced, and interacted with the value of exports of commodities in the commodity bundle as a share of 1995 GDP (using GDP in current US dollars from the World Development Indicators (WDI) or, in the case of missing data, from the United Nations Statistics Division).[29] The index does not reflect that countries may receive different prices for exports of the same commodity. In value terms, non-agricultural commodities account for 60 percent of the exports in the export bundles for countries in the sample. The most important of these commodities is oil. The correlation between the commodity price instrument and the deflated and differenced log oil price index is 0.29.

A common commodity price index was constructed for two groupings of countries: 1) the five members of the Southern African Customs Union (Botswana, Lesotho, Namibia, South Africa, and Swaziland), and 2) Belgium and Luxembourg. This was necessitated by the absence of export data at a country level for these two country groups. Results are similar if these seven countries are excluded from the estimation sample.

We explore a number of alternatives in instrumenting economic growth with commodity price variation. A similar result in the main "democratic change event" specifications is obtained if

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[28] Collier and Goderis (2007) weighted their index geometrically. Given that many of the commodity shares were close to 0, we opted for an arithmetically weighted index.
[29] World Development Indicators: http://go.worldbank.org/6HAYAHG8H0 (accessed 10 July 2008). United Nations Statistics Division: http://unstats.un.org/unsd/snaama/Introduction.asp (accessed 2 February 2009).

the commodity price instrument is replaced by one that covers a commodity bundle of only the 15 non-agricultural commodities, or one that covers only the 35 agricultural commodities (although agricultural commodity export price fluctuations are an extremely weak instrument for economic growth in the global sample in the presence of year fixed effects). Results are similar if commodities for which price series are incomplete are dropped from the commodity bundle. Results are also similar if countries in the Middle East and North Africa (a region characterized by high levels of autocracy and high commodity dependence) are excluded from the sample. We also explore whether results are similar once exports of any commodity for which a country's exports make up more than 15 percent of global exports are excluded from that country's commodity bundle. (For instance, once oil is excluded from Saudi Arabia's commodity bundle.) We do this given the possible concern that commodity export prices are endogenous to political change for price setters. We obtain similar results. Finally, we explore whether the results are similar if we remove the world's largest economies from the estimation sample (as demand in these countries affects world commodity prices), and obtain similar results.

A-VI. Definitions of variables

Democratic change event

Binary variable, =1 if a 3-or-more point increase in the POLITY score over a period of 3 years or less commences in year *t*, as flagged by a positive REGTRANS score; 0 otherwise. Polity IV, version 2007. Covers instances of major democratic transition (REGTRANS==3), minor democratic transition (REGTRANS==2), and positive regime change (REGTRANS==1). Only the first year of multi-year democratic change events is considered. Years in which democratic change events coincided with the natural death of the national leader are excluded from the estimation dataset. This affected 3 years: Spain 1975, Nigeria 1998, and Croatia 1999. Natural deaths of the national leader are identified using Goemans, Gleditsch, and Chiozza (2009). Polity IV: http://www.systemicpeace.org/polity/polity4.htm (accessed 15 July 2009).

Autocratic change event

Binary variable, =1 if a 3-or-more point decrease in the POLITY score over a period of 3 years or less commences in year *t*, as flagged by a negative REGTRANS score; 0 otherwise. Polity IV, version 2007. Only the first year of multi-year autocratic change events is considered. In no years did autocratic change events coincide with the natural death of the national leader (using Goemans, Gleditsch, and Chiozza 2009). Autocratic change events cover the Polity IV categories of "negative regime change" and "adverse regime transition", but exclude the commencement of interregnal periods.

POLITY2

POLITY score adjusted for use in time series studies. Polity IV, version 2007.

GDP per capita growth

Annual percentage growth rate of GDP per capita based on constant local currency. WDI. http://go.worldbank.org/6HAYAHG8H0 (accessed 25 August 2009).

GDP per capita growth, Penn World Table

Annual percentage growth rate of GDP per capita in 2005 constant prices I\$ (chain series). Penn World Table 6.3 http://pwt.econ.upenn.edu/ (accessed 15 September 2009).

GDI per capita growth, Penn World Table

Annual percentage growth rate of gross domestic income per capita in 2005 constant prices I$ terms of trade. Penn World Table 6.3. http://pwt.econ.upenn.edu/ (accessed 15 September 2009).

Country-specific development level$_{t-2}$

Equals 0 when a country's t-2 per capita GDP is within 30 log points of its sample average t-2 per capita GDP, +1 (-1) when 30-60 log points above (below) its sample average, and +2 (-2) when 60 or more log points above (below) its sample average. WDI.

Secondary enrollment rate (percent gross)

Number of pupils enrolled in secondary education, regardless of age, expressed as a percentage of the population in the theoretical age group for secondary education. World Bank Education Statistics, Development Research Institute. Data are interpolated (linear) and extrapolated (constant). World Bank Education Statistics: http://www.worldbank.org/education/edstats (accessed 1 June 2008). Development Research Institute:

http://www.nyu.edu/fas/institute/dri/dataset/Social%20Indicators%20Fixed%20Factors_7_2005.xls (accessed 13 August 2008).

Population aged 65 years and above (percent)

Percentage of the total population aged 65 years or older. WDI.

Tenure of regime (years)

The number of years since the most recent regime change (defined by a 3-point change in the POLITY score over a period of 3 years or less), or the end of a transition period. Polity IV, version 2007.

Democracy in region (percent of countries)

Number of democracies in region divided by number of countries in the Polity IV dataset in region * 100. Countries with a positive POLITY2 score are classified as democracies. Polity IV, version 2007. 8 regions are defined following the Development Research Institute: East Asia and the Pacific, East Europe and Central Asia, Latin America and the Caribbean, Middle East and North Africa, North America, South Asia, sub-Saharan Africa, and West Europe.

Labor force in agriculture in 1961 (percent) [Φ]

Percentage of the total labor force economically active in agriculture, hunting, forestry, or fishing in the year 1961. World Resources Institute, http://earthtrends.wri.org (accessed 15 June, 2008). Countries that were not independent in 1961 and for which data are not available: data for the former jurisdiction of which they were a part (e.g. Yugoslavia) were used.

Non-irrigated cropland (percent) in 1961 [Ψ]

Share of arable land and permanent cropland that was not purposely provided with water in 1961. WDI, Food and Agriculture Organization. 7 percent of observations are for the first available year post-1961 in WDI. 5 percent of observations are for 1961 data from the Food and Agriculture Organization (http://faostat.fao.org/site/377/default.aspx#ancor, accessed 3 March 2009).

Precipitation instrument

Change in precipitation from the previous year (percent) multiplied by $Φ$ and $Ψ$. Mitchell et al. (2004, dataset available at http://www.cru.uea.ac.uk/~timm/cty/obs/TYN_CY_1_1.html, accessed 1 June 2008).

Temperature instrument

Change in temperature from previous year (ºC) (multiplied by -1 for countries where the average temperature from 1960-1970 was less than 12ºC) multiplied by $Φ$. Mitchell et al. (2004).

Commodity price instrument

Authors' construction; see A-V.

Anti-government protests

Any peaceful public gathering of at least 100 people for the primary purpose of displaying or voicing opposition to a government's policies or its authority. Databanks International. These data were constructed primarily from reports in the New York Times. Databanks International warns that they may be subject to geographic and other biases. http://databanksinternational.com (accessed 13 August 2008).