Income, Democracy, and the Cunning of Reason

Daniel Treisman*

March 2012

Abstract

A long-standing debate pits those who think economic development leads to democratization against those who argue that both result from distant historical causes. Using the most comprehensive estimates of national income available, I show that development is associated with more democratic government—but in the medium run (10 to 20 years). The reason is that higher income usually only prompts a breakthrough to more democratic politics after the incumbent leader leaves office. And in the short run, faster economic growth increases the leader's odds of survival. I present evidence that leader turnover matters because conservatism grows with tenure and newer leaders are readier to risk political reform. This logic helps explain why democracy advances in waves followed by periods of stasis and why dictators, concerned only to entrench themselves in power, end up preparing their countries to leap to a higher level of democracy when they are eventually overthrown.

Keywords: democracy, economic development, modernization, leadership
JEL classifications: D78, I25, N10, O10

*Professor, Department of Political Science, University of California, Los Angeles, 4289 Bunche Hall, Los Angeles CA 90095, email: treisman@polisci.ucla.edu and NBER.

I thank Bruce Bueno de Mesquita, Carles Boix, Jim DeNardo, Jack Goldstone, Ryan Kennedy, Michael Miller, Jim Robinson, Andrei Shleifer, and Jim Vreeland for valuable comments and suggestions, and the UCLA College of Letters and Sciences for support.

1 Introduction

Does economic development render countries more democratic—and, if so, why do dictators ever promote it? Following the Arab uprisings of 2011, these questions are particularly topical. To many observers, the protests that erupted from Libya to Bahrain seemed direct results of modernization, which created glaring inequalities, spread literacy and information, and provided networking tools such as Twitter and Facebook to mobilize discontent into the streets (Giglio 2011). Yet if development undermines authoritarian rule, why do authoritarian rulers nevertheless encourage it?

One answer might be that they do not. Concerned precisely to forestall the organization of opposition, some dictators deliberately *de*-modernize their countries. President Mobutu allowed Zaire's infrastructure to decay, shrinking the network of paved roads along which regime opponents might travel (Robinson 2001, p.28). Yet, although some dictators fit the Mobutu mold, others have overseen—and often actively supported—economic development. Under South Korea's General Park Chung-hee and Singapore's Prime Minister Lee Kuan Yew, per capita GDP grew by more than six percent a year. Such cases are not unique to Asia. When Zine al-Abidine Ben Ali became president of Tunisia in 1987, GDP per capita was $2,512. By the time he fled in 2011, it was more than $8,000. On Ben Ali's watch, adult literacy rose from 48 to 78 percent; enrollment in higher education from 5 to 34 percent; women in parliament from 4 to 28 percent; internet users from zero to 34 percent; and mobile phone subscriptions from zero to 93 per 100 people.[1] Dictators like Ben Ali may not grow their economies as fast as democratic leaders do on average (Persson and Tabellini 2009). The puzzle is why they grow them at all if doing so prepares the ground for a democratic revolution.

A second possibility is that Lipset and other modernization theorists were wrong: dictators

[1] Statistics in this paragraph are from World Bank (2011), using data from closest available years; GDP per capita in current international dollars, at purchasing power parity.

need not fear economic development because development does not erode the bases of their regime. Examining the post-war period, Przeworski et al. (2000) concluded that although higher income helped entrench democracies it did not substantially increase the odds that a dictatorship would become democratic. Acemoglu, Johnson, Robinson, and Yared (AJRY 2008, 2009), also focusing mostly on recent decades, contend that development has no effect on either the stability of democracy or transitions to it if one controls for countries' historical legacies. However, these claims have been challenged. Boix (2011) and Benhabib, Corvalan, and Spiegel (2011) find that higher income does favor democracy if one broadens data coverage to earlier periods and adjusts for censoring at the top of the democracy scale. I begin by confirming and extending these findings. I show that the evidence linking income and democracy is much stronger in the medium run (10-20 year periods) than in the short run (annual or five-year periods).

So why do dictators promote the very economic changes that eventually predispose their subjects to demand political freedom? I argue that a mechanism analogous to what Hegel called "the cunning of reason" leads rulers who seek only their own survival in power to support economic growth.[2] Economic development has different effects in the short run and the long run. In the long run, it transforms societies, creating the preconditions for democracy. As a country's income rises, its population becomes more differentiated, educated, bourgeois, tolerant, interconnected by decentralized media, and eager to participate politically. However, that society is ready for democracy does not mean a transition immediately occurs. I argue that in general higher income only prompts a breakthrough to more accountable government *after the incumbent leader leaves office.* And in the short run higher economic growth increases the ruler's odds of survival. By

[2] By the "cunning of reason," Hegel (1980 [1837]) meant the way that great leaders, who sought only to dominate the existing order, were used by history to bring new political orders into being.

raising citizens' incomes, growth boosts the ruler's popularity, intimidating potential rivals; by increasing state revenues, it helps the ruler finance patronage or repression. Thus, in the short run economic progress may actually stall the expansion of political freedoms. A Ben Ali may promote growth in order to lengthen his tenure in office, and exploit the cushion of support generated by rising incomes to repress opposition, while simultaneously and quite unintentionally bringing about changes that increase the odds of democratization when he is eventually overthrown.

And no leader survives forever. International economic shocks may frustrate the dictator's efforts to promote domestic growth, or he may be deposed after losing a war or a civil war, or for other reasons. Some dictators die peaceful deaths in office. When an autocrat exits, the level of economic development then influences whether he is replaced by another dictator or a more democratic regime. At the global level, I show that this can explain the noted pattern of pro-democratic waves followed by partial reversals.

Why does turnover at the top intensify the relationship between income and democracy? It might be that in rapidly developing countries new leaders tend to be younger, more educated, and therefore more pro-democratic than their predecessors. It could also be that dictators tend to fall in the midst of public unrest, which either prompts the selection of a reformist successor or pressures the new leader to make concessions. Exploring these possibilities, I present evidence that new leaders tend to be readier to take risks than those in power for longer, which explains their greater propensity, faced with a society that has modernized, to introduce democratic reforms. Growing popular mobilization is also associated with democratization, but it is leader turnover not popular mobilization that activates the income effect.

This paper is related to several others. Kennedy (2010) demonstrates that among countries whose institutions undergo major change (a jump of at least three points on the Polity scale either

up or down), those that move up tend to be richer. Major institutional change is, in turn, prompted by low growth. Although a valuable insight, this leaves the process by which countries democratize largely unexplained. I argue that the event that typically initiates this process is the replacement of the authoritarian state's leader. I show that the impact of leader turnover dwarfs that of economic growth; to the extent that economic growth matters, it is primarily as a cause and catalyst of the effects of leader turnover. Miller (2010) also examines the relationship between income and democracy conditional on leader replacement. However, he restricts attention to autocrats who are *violently* overthrown, which he takes as a proxy for underlying weakness of the regime: high income leads to democracy in orders that are already "fragile and unstable." I contend that it is leadership change itself that—even in previously stable autocracies—intensifies the effect of economic development on political change. I show that, in fact, leader turnover has the strongest and most statistically significant effect on the income-democracy relationship when it occurs as part of a peaceful, regular transition or after the incumbent dies of natural causes. Franco and Brezhnev died quietly in their beds; yet the leader change this prompted, in societies that had modernized significantly, resulted within a decade in major expansions of political freedom.[3] Jones and Olken (2009) analyze the influence of the assassination of dictators on political institutions and military conflict, but do not examine the effect of income on democratization.

In the following sections, I report statistical evidence for each step in my argument. First, I reprise the current debate about development and democracy, replicating the findings of previous

[3] Unlike Kennedy or Miller, I also analyze *why* in richer countries leadership turnover facilitates democratization and show how turnover can explain the pattern of pro- and anti-democratic waves worldwide. Neither Kennedy nor Miller considers the divergent effects of income on democracy in the short and medium run or the dilemma for dictators, both central elements here.

papers using the most up-to-date income data, and showing that the income-democracy link is stronger in the medium than in the short run. Section 3 explores why this is the case and shows that the effect of income is conditional on leader turnover. Section 4 checks robustness. Section 5 shows evidence that low economic growth precipitates leader turnover, and, indirectly, democratization. Section 6 explores why leader replacement catalyzes the income-democracy relationship. Section 7 relates the findings to the timing of democratization waves. Section 8 concludes.

2 Income and democracy

Since Lipset (1959), many scholars have held that as countries develop economically they tend to become more democratic. This was consistent with the strong cross-national correlation between income and measures of democracy observable in any year. Moreover, a variety of plausible mechanisms—from the spread of education and mass media to growing tolerance and social differentiation—seemed likely to render citizens of richer societies more eager to participate and harder to control.[4] Confidence in this logic was shaken in the 1970s by the appearance of military dictatorships in some relatively rich Latin American countries (O'Donnell 1988). But after these returned to democracy they came to seem exceptions that proved the rule.

A stronger challenge emerged more recently. Acemoglu et al. (AJRY 2008, 2009) argue that rather than economic development causing democracy, the two evolve in parallel, driven by factors rooted in distant history. As early as 1500, some countries had good institutions that prompted rapid growth and democratization; others did not. Empirically, they showed that in panels of countries

[4] The literature on this is vast. For recent treatments, see Barro (1999), Boix and Stokes (2003), and Epstein et al. (2006). On the importance of education, see Glaeser et al. (2004) and Glaeser, Ponzetto, and Shleifer (2007); on value change, see Inglehart and Welzel (2005).

between 1960 and 2000 (and in a panel of 25 countries at 25-year intervals from 1875), the income-democracy link disappeared once country dummies were added to control for country-specific factors. However, two still more recent papers rediscover the relationship. Boix (2011) and Benhabib et al. (2011) report that using data that extend into the 19th Century, income is significant even with country fixed effects. Benhabib et al. (2011) also note that many countries—on average, 18 percent since 1900—reach the highest score of 10 on the commonly used Polity democracy scale and cannot rise any higher. Adjusting for such censoring also increases the significance.

I begin by replicating the main findings of AJRY (2008, 2009), Boix (2009), and Benhabib et al. (2011), using the latest national income estimates of Angus Maddison and his collaborators (Maddison 2010). As in these papers, I use two measures of democracy, one more or less continuous, the other dichotomous. The first is the Polity2 index from the Polity IV dataset (2009 version).[5] Constructed by scholars at George Mason University, this equals the difference between an index of democracy and an index of autocracy, both of which measure in different ways the openness and competitiveness of political participation and executive recruitment, along with the extent of constraints on the executive. The data include all countries with current populations over 500,000, starting in 1800 or the year of independence. Like AJRY (2008, 2009) and Boix (2011), I rescale the index, which runs from -10 to +10, to take values between 0 and 1. The dichotomous measure was constructed by Boix and Rosato (2001) and used in Boix and Stokes (2003) and AJRY (2009). This codes countries as democratic if elections are free and competitive, the executive is accountable (i.e. the president is directly elected or the head of government is answerable to

[5] For details, see www.systemicpeace.org/polity/polity4.htm. I use the Polity2 index, which, unlike the simple Polity index, includes estimates for years in which the regime was in transition.

parliament), and at least half the male population is enfranchised (Boix and Rosato 2001).[6]

Since I contend that long and short run effects of income differ, I construct panels at different frequencies. I show results for annual, 5-year, 10-year, 15-year, and 20-year panels, where relevant calculating the cumulative long-run effect.[7] Rather than averaging the data for the given period, which would introduce additional serial correlation, I follow AJRY (2008, 2009) and Boix (2011) in using observations from every fifth year for the five-year panel, and so on.[8] I include in each regression the lagged value of the dependent variable, again as in AJRY (2008, 2009) and Boix (2011), to capture persistence in democracy, reduce serial correlation, and pick up any tendency to revert to the mean. The basic model I estimate, as in AJRY (2008), can be written:

$$d_{it} = \alpha d_{it-1} + \gamma y_{it-1} + \mathbf{x'_{it-1}}\boldsymbol{\beta} + \mu_t + \delta_i + u_{it} \qquad (1)$$

where d_{it} is the extent of democracy in country *i* in period *t*; y_{it-1} is the natural log of per capita GDP in country *i* in the previous period; $\mathbf{x_{it-1}}$ is a vector of other covariates; δ_i is a full set of country dummies; μ_t a full set of year dummies; and u_{it} a random error with $E(u_{it}) = 0$ for all *i* and *t*. I calculate robust standard errors clustered by country.

---

[6] Coverage ranges from 22 countries in 1800 to 186 in 2000. I thank Carles Boix for sharing these data. Some studies have used ratings produced by the NGO Freedom House. However, since these begin in 1972, and even extensions go back only to 1950 (Bollen 1998), they cannot test arguments about the pre-World War II experience.

[7] In a model with a lagged dependent variable: $d_{it} = \alpha d_{it-1} + \gamma y_{it-1}$, the cumulative effect of income is $\gamma / (1-\alpha)$.

[8] That is, I include all years ending in 0 or 5 for which the data exist. I discuss varying the starting year of panels below.

In Table 1, panels A-C, I estimate this model by OLS, using the rescaled Polity2 index as dependent variable. Panel A includes just 1960-2000. As in AJRY (2008, Table 3, column 2; 2009, Table 1, Panel B, column 2), Boix (2009, Table 2, column 1), and Benhabib et al. (2011, Table 4, columns 3 and 4), income is statistically insignificant with estimated long-run impact close to zero. This is true at all panel lengths. Panel B includes all observations for 1820-2008. Now a new pattern emerges. In the 10-, 15-, and 20-year panels, income is significant, with a positive coefficient. The cumulative effect of income rises as the panel frequency falls, reaching .18 for the 20-year data.[9]

So far, I have not adjusted in any way for the fact that countries that reach the top of the Polity scale cannot rise any higher. The simplest way to do so is to reformulate the question to ask whether higher income predisposes *non-democracies* to become more democratic. The Polity creators recommend treating a Polity2 score of +6 as the lower bound for democracy. Panel C shows results estimated on just countries with Polity2 scores below six in the previous period. This increases the estimated effect of income.

Previous work showed that the determinants of levels of democracy may differ from those of transition to democracy (Przeworski et al. 2000). Panels D-F examine transitions. (This paper focuses on transitions *to* democracy, but I occasionally also show regressions for transitions away from it.) Panel D focuses on upward movement on the Polity2 scale. As in AJRY (2009), I estimate:

$$d_{it}^{+} = \alpha d_{it-1} + \gamma y_{it-1} + \mathbf{x'_{it-1}}\boldsymbol{\beta} + \mu_t + \delta_i + u_{it} \qquad (2)$$

where $d_{it}^{+} = \max(d_{it}, d_{it-1})$. This excludes any cases in which the democracy measure falls. Panels E and F use the dichotomous Boix-Rosato measure; I focus on just countries that were non-democracies in the previous period and so drop the lagged dependent variable. These regressions

[9] The data and STATA do files to replicate the analysis will be posted on the author's website by the date of publication.

thus capture the correlates of transitions from a score of 0 (non-democracy) to 1 (democracy).

The choice of statistical model for a panel with a binary dependent variable and unit and time fixed effects is not straightforward. Probit and (unconditional) logit with fixed effects are inconsistent because of the incidental parameters problem (Greene 2003). The conditional logit fixed effects model (CLFE; Chamberlain 1980) is consistent. However, it requires dropping all cases in which the dependent variable does not change. Besides the loss of up to two thirds of the data, eliminating the "dogs that don't bark" here produces estimates of the effect of income that are biased upward: all autocracies that became rich *without* democratizing are automatically excluded (and since only one country—the US—enters the data already a democracy, the non-democratizers are not balanced by countries coded 1 throughout). For instance, running CLFE on 5- and 10-year panels, I find a strong, significant effect of income on democratic transitions even in just the 1960-2000 period.[10] These problems have prompted many researchers to use the linear probability model (estimated by OLS, despite the binary dependent variable) when unit fixed effects are important.[11] This model is consistent under relatively weak assumptions (Wooldridge 2002, Chapter 15.2), although it has the disadvantage of sometimes predicting probabilities outside the 0-1 range. I do the same, and in any case focus on results using the Polity2 measure, where OLS is uncontroversial.

The transitions regressions show a similar pattern: once pre-1960 data are included income is significant, with the largest effects in the 10- to 20-year panels. Tables A1 and A2 in the appendix

[10] For reference—but with a strong caveat about bias—I show CLFE results in Appendix Table A2.

[11] For recent uses, see Besley and Reynal-Querol (2011), Boix (2011 Table 1, column 9); Acemoglu et al. (2009, Tables 1 and 2); Bruckner and Ciccione (2011); Pope and Schweitzer (2011). These articles were published in *Econometrica*, *The American Economic Review*, *The American Political Science Review*, and *The Journal of Monetary Economics*.

present some alternative formulations and extensions. Maddison's GDP data contain gaps in early years. Interpolating linearly to fill these—resulting in up to 27 percent more observations—renders the results slightly more significant (Table A1). Results are also similar or stronger if one replaces the Polity2 democracy cutoff of +6 with +10 or uses the estimator of Alan, Honoré, and Leth-Petersen (2008), which allows for censoring at top and bottom while also controlling for unobserved heterogeneity, as in Benhabib et al. (2011) (Table A2). I also estimate the models with Arellano and Bond's dynamic GMM estimator, as in AJRY (2008). Although results are not stable across panels, coefficients on income are positive and significant in the 10- and 20-year data. While this is reassuring, it is not clear one should have expected a significant result. The Arellano-Bond model estimates relationships between levels from a regression of first differences, using past levels as instruments. I argue here precisely that the effects of short-run changes in income differ from those of levels in the longer run. This also complicates the usual strategy of using regressions in first differences to avoid possible problems of non-stationarity. Thus, it is important to test for stationarity. Throughout the paper, I test the residuals of regressions using a Fisher test for non-stationarity in panels. In most—but not all—the models in Table 1, one can reject non-stationarity with high confidence. The results do not appear to represent merely the coincidence of parallel trends in income and democracy.

These checks reinforce the main finding observed so far. If one includes data that precede World War II, and especially if one also adjusts for censoring at the top of the Polity scale, higher income is significantly associated with movement towards greater democracy. In failing to detect a relationship in annual data—and usually in five-year panels—these results are in line with AJRY (2008, 2009). However, in finding a relationship in panels at lower frequency, the results echo those of Boix (2011) and Benhabib et al. (2011).

Table 1: Income and democracy

Level of democracy (Polity2 measure)

	(A) 1960-2000 all countries					*(B) 1820-2008 all countries*					*(C) 1820-2008, Polity2 $_{t-1}$ < 6*				
Type of panel:	*1-yr*	*5-yr*	*10-yr*	*15-yr*	*20-yr*	*1-yr*	*5-yr*	*10-yr*	*15-yr*	*20-yr*	*1-yr*	*5-yr*	*10-yr*	*15-yr*	*20-yr*
Democracy $_{t-1}$	.87***	.45***	.15*	-.01	-.17**	.92***	.62***	.33***	.21***	.03	.90***	.56***	.21**	.17	.04
	(.01)	(.05)	(.08)	(.10)	(.09)	(.01)	(.04)	(.06)	(.07)	(.08)	(.01)	(.06)	(.09)	(.13)	(.12)
Ln GDP per Capita $_{t-1}$	-.005	.007	.022	.022	.012	-.002	.009	.07*	.11*	.18**	-.00	.026	.14***	.17**	.26**
	(.007)	(.029)	(.051)	(.076)	(.114)	(.004)	(.019)	(.04)	(.05)	(.09)	(.01)	(.026)	(.05)	(.07)	(.12)
Implied cumulative effect of income	-.04	.01	.03	.02	.01	-.02	.02	.11*	.14**	.18**	-.00	.06	.18***	.21**	.27**
Fisher p level	[.00]	[.00]	[.00]	[1.00]	[.79]	[.00]	[.00]	[.00]	[.10]	[.01]	[.00]	[.00]	[.00]	[.00]	[.38]
Observations	5,377	1,103	562	330	267	10,305	1,933	884	531	391	6,594	1,291	616	381	275
Countries	160	159	137	135	131	164	160	138	136	132	142	138	124	123	116
R-squared	.9453	.8215	.7758	.8044	.8121	.9520	.8133	.7346	.7322	.7272	.8589	.6129	.5831	.6222	.6911

Transitions to democracy

	(D) 1820-2008 just upward movements of Polity2: Polity2 $_{t-1}$ < 6					*(E) 1960-2000: dichotomous Boix Rosato measure: non-democracies*					*(F) 1820-2000 dichotomous Boix Rosato measure: non-democracies*				
Type of panel:	*1-yr*	*5-yr*	*10-yr*	*15-yr*	*20-yr*	*1-yr*	*5-yr*	*10-yr*	*15-yr*	*20-yr*	*1-yr*	*5-yr*	*10-yr*	*15-yr*	*20-yr*
Democracy $_{t-1}$	.97***	.79***	.57***	.60***	.38***										
	(.01)	(.04)	(.07)	(.09)	(.13)										
Ln GDP per Capita $_{t-1}$	-.002	.02	.11***	.13**	.20**	.01	.07	.11	.06	.02	.005	.075**	.21***	.26***	.33**
	(.004)	(.02)	(.04)	(.06)	(.10)	(.01)	(.05)	(.08)	(.14)	(.24)	(.008)	(.032)	(.06)	(.10)	(.16)
Implied cumulative effect of income	-.07	.10	.26**	.33**	.32**										
Fisher p level	[.00]	[.00]	[.00]	[.02]	[.91]	[.00]	[.00]	[.00]	[.86]	[.00]	[.00]	[.00]	[.00]	[.00]	[.00]
Observations	6,594	1,291	616	381	275	3,545	733	376	227	182	5,735	1,169	594	363	264
Countries	142	138	124	123	116	126	125	114	111	111	141	137	126	124	118
R-squared	.9025	.7177	.6835	.7002	.7425	.1027	.3320	.5122	.6650	.7788	.0995	.2532	.4397	.5522	.6598

Sources: see Table A16 in Appendix.

Note: All regressions estimated by OLS including country and year fixed effects. Robust standard errors, clustered by country, in parentheses; * p<.10, ** p<.05, *** p<.01. Implied cumulative effect of income: coefficient on Ln GDP per Capita $_{t-1}$/(1 - coefficient on Democracy $_{t-1}$). "Fisher p level" is probability level at which one can reject H0: residuals are I(1), from Fisher test of residuals.

The new point I emphasize here is that the relationship between income and democracy is clearest and strongest *in the medium to long run*--i.e. panels of 10 to 20 years. Year on year, there is little change in measures of democracy. In annual data, the coefficient on lagged democracy is close to one. But as the interval between observations increases, the coefficient on lagged democracy falls; in 20-year panels, it is close to zero or even negative, suggesting strong regression to the mean. If one wants to predict how democratic a country will be next year, its current level of democracy is overwhelmingly important. But if one wants to know how democratic it will be in 20 years, its current democracy score helps little; its income level is far more informative.[12]

3 The importance of leadership change

Why might income matter for democracy mostly in the medium to long run? There may be several reasons. Here I focus on one. I hypothesize that the demand for democracy and the readiness of society to sustain it have a greater impact after turnover occurs in a country's top leadership.[13] Political change is discontinuous. In most years, a country's governing institutions are highly inertial. But when, for whatever reason, a dictator leaves office, constitutional questions suddenly come on the agenda. The direction and extent of political reform then depend on what level of economic development the country has reached under the previous ruler.

[12] The estimated effects are quite large. For instance, the difference between a per capita GDP of $2,000 and one of $20,000 would correspond to a long-term difference of .41 on the 0-to-1 Polity2 scale if one uses the estimate from Panel B (20-year data) or a difference of .62 points using the estimate from Panel C (20-year data).

[13] As Huntington noted, the decision of a failing authoritarian regime to democratize in the 1970s and 1980s "almost always first required a change of leadership " (1991, p.57) .

For years, a society may evolve under the surface, growing more complex, bourgeois, literate, interconnected, media savvy, tolerant, and difficult to control, without any alteration in the political superstructure—until a crack appears at the top and the latent demand for participation combines with the new potential for social organization. Leadership change by itself does not produce democracy: one dictator may just replace another. Economic development by itself only makes democracy more feasible. In the short run, growth may boost the incumbent's popularity, enabling him—if he wishes—to curb political freedom. It is the combination of economic development and leadership change that eases the path of political reform.

Table 2 provides evidence for this hypothesis. I examine whether the link between income and democracy differed in countries where the leader had recently changed.[14] Leadership data come from the Archigos dataset of Goemans, Gleditsch, and Chiozza (2009a, 2009b), which identifies the top leaders of all independent states between 1875 and 2004 and records how they left office. A country's "leader" is "the person that de facto exercised power"— in general, the prime minister in parliamentary regimes, the president in presidential and mixed ones, and the communist party chairman in communist states (Goemans et al. 2009a). Panel A uses the Polity2 measure, restricting attention as before to non-democracies (i.e. Polity2 < 6). Panels B and C examine transitions to democracy, using respectively the Boix-Rosato binary variable and the model of Equation (2) to explain upward movements on the Polity2 scale. The regressions suggest that income does indeed have a different effect in periods following turnover at the top. If the country's leader had not been replaced, there was generally no relationship between income and the level of democracy, controlling for democracy one period earlier (statistically insignificant coefficients, close to zero).

[14] For instance, in the 5-year panel, I distinguish cases in which the leader had been replaced in periods $t - 5$ through $t - 1$ from those in which he had not.

Table 2: Income, education, leadership change, and democracy

	----------*Level of Democracy*----------						----------*Transitions to Democracy*----------										
	(A) 1875-2004: Polity, Polity2 $_{t-1}$ < 6						*(B) 1875-2000: BR binary measure, only non-democracies*						*C) 1875-2004: Polity, Polity2 $_{t-1}$ < 6: just upward movements*				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)
Type of panel:	*1-yr*	*5-yr*	*10-yr*	*15-yr*	*20-yr*	*10-yr*	*1-yr*	*5-yr*	*10-yr*	*15-yr*	*20-yr*	*10-yr*	*1-yr*	*5-yr*	*10-yr*	*15-yr*	*20-yr*
Democracy $_{t-1}$	.90***	.47***	.11	.10	-.11	.25**							.97***	.72***	.49***	.59***	.25**
	(.01)	(.06)	(.10)	(.15)	(.11)	(.10)							(.01)	(.05)	(.07)	(.09)	(.12)
Leader replaced in previous period	-.07*	-.17	-.36	-.78*	-.76	.08*	-.14	-.50**	-.53	-1.55**	-1.72	-.06	-.07	-.11	-.33	-.78*	-.46
	(.04)	(.14)	(.28)	(.41)	(.82)	(.04)	(.09)	(.24)	(.38)	(.67)	(1.26)	(.05)	(.04)	(.13)	(.28)	(.40)	(.80)
Ln GDP per Capita $_{t-1}$	-.002	-.01	.07	-.00	.10		-.00	.02	.11*	-.00	.04		-.005	-.01	.05	-.03	.07
	(.006)	(.03)	(.05)	(.07)	(.14)		(.01)	(.03)	(.07)	(.10)	(.20)		(.005)	(.02)	(.05)	(.06)	(.13)
Ln GDP per Capita $_{t-1}$ * leader replaced	.010*	.04*	.07*	.13**	.14		.02*	.08**	.08	.22**	.26*		.010*	.03	.06	.13**	.10
	(.005)	(.02)	(.04)	(.06)	(.10)		(.01)	(.03)	(.05)	(.09)	(.15)		(.006)	(.02)	(.04)	(.05)	(.10)
Average yrs of schooling (age 15 and over) $_{t-1}$						.014						-.005					
						(.028)						(.036)					
Average yrs schooling * leader replaced $_{t-1}$						.041***						.080***					
						(.012)						(.016)					
Implied cumulative effect of income																	
-if leader replaced	.07	.04	.15**	.14	.22**		.02	.10**	.20***	.22**	.30*		.21	.05	.22**	.23	.23*
-if leader not replaced	-.02	-.03	.08	-.01	.09		-.00	.02	.11*	-.00	.04		-.17	-.04	.10	-.08	.09
Implied cumulative effect of schooling																	
-if leader replaced						.07*						.08**					
-if leader not replaced						.02						-.01					
Fisher p level	[.00]	[.00]	[.00]	[.12]	[.87]	[.00]	[.00]	[.00]	[.00]	[.54]	[.88]	[.00]	[.00]	[.00]	[.00]	[.82]	[.03]
Observations	5,815	1,173	551	342	246	416	5,261	1,061	535	329	239	400	5,815	1,173	551	342	246
Countries	136	133	119	120	113	65	137	133	122	121	115	64	136	133	119	120	113
R-squared	.8526	.6289	.6127	.6725	.7605	.5782	.1074	.2851	.4644	.6049	.7163	.4459	.8992	.7326	.7074	.7447	.8084

Sources: see Table A16 in Appendix.

Note: All estimations by OLS with country and year fixed effects. Robust standard errors, clustered by country, in parentheses; * p<.10, ** p<.05, *** p<.01. "Fisher p level" is probability level at which one can reject H0: residuals are I(1), from Fisher test of residuals.

However, if the leader had been replaced, countries with higher income tended to move towards democracy faster. For periods of 1 to 15 years, the interaction term was statistically significant at least at $p < .10$ for the Polity measure. For transitions to democracy using the Boix-Rosato dichotomous measure (Panel B), the interaction was significant for all but the 10-year panel (for which $p = .16$). For upward movements on the Polity scale (Panel C), the results were similar although slightly less significant.[15] At extremely low per capita income leadership turnover has no effect or may even correlate with less democracy. But at higher income levels, a change in leader is associated with movement towards democracy that is larger, the more developed the country.[16]

Space constraints preclude exploring here by what channels development influences democratization, conditional on leader change. But (6) and (12) offer evidence for one plausible mechanism. As countries grow richer, their citizens become more educated, which increases their

[15] Note, however, that Fisher tests of the residuals raise doubts about the stationarity of some of the 15- and 20-year panels. Filling gaps in the Maddison income data by linear interpolation (Appendix Table A3), the coefficients are similar, but now statistically significant in all models in all three panels—and one can now reject non-stationarity in all models with high confidence.

[16] OLS with fixed effects and a lagged dependent variable can yield biased estimates because the lagged dependent variable is mechanically correlated with the error term for all earlier periods. To show that this is not driving the results, in Table A4 I estimate the models of Table 2, panel A, dropping the lagged dependent variable. (The cost of doing this is considerable autocorrelation and imprecise estimates in the high-frequency panels, but the clustered standard errors are still consistent.) Although significance is sometimes lower, the results are similar to those in Table 2, and statistically significant in the 10- and 15-year panels. Using the income data with interpolations, income interacted with leader change is also significant (at $p < .05$) in the five-year panel.

desire to participate in politics, their capacity to organize, and their commitment to tolerance and compromise. Lipset thought a high education level "close to being a necessary condition" for democracy (1959, p.80). Various evidence supports this (Barro 1999, Przeworski et al. 2000, Glaeser et al. 2004). However, Acemoglu et al. (2005) argue that once country and year fixed effects are added, the relationship disappears. To measure education levels, I use estimates of the average years of schooling among those aged 15 and older (available 1870-2010 at 10-year intervals for 74 countries; Morrisson and Murtin 2009). Just entering education into regressions of democracy with country and year fixed effects, it was not significant (not shown). But education correlated strongly with democracy in periods when the state's leader changed (columns 6 and 12). A more educated citizenry favors accountable government—but mostly after the ruler is replaced.

Another perspective on these results is offered by the record of countries where an authoritarian leader was lucky or skilled enough to preside over an extended period of rapid growth. The data include 15 leaders of non-democracies under whose rule income per capita increased by 150 percent or more. Under each, the average Polity2 score was negative, indicating quite repressive regimes. These "developmental dictators" are listed in Table A5 in the appendix.[17]

Except for Tunisia's President Bourguiba, who during 30 years in power raised his country's Polity2 score one point, none of these leaders left his state more democratic than he found it, and several exploited good economic conditions to curb political freedom. (Of course, this partly reflects a selection effect: those who *did* democratize early were more likely to fall before their countries could achieve large income gains.) What is noteworthy is what happened after these dictators lost power. In 10 of 15 cases, the next decade saw movement towards democracy—often

[17] To be clear, I do *not* argue that these leaders' policies explain the high growth. Easterly (2011) makes a strong case that they—and other so-called "benevolent autocrats"—were just lucky.

dramatic breakthroughs. Ten years after the deaths of Spain's Generalissimo Franco, Portugal's Prime Minister Salazar, and South Korea's General Park, their countries had become democracies (Polity2 > 5). A decade after the fall of Indonesia's President Suharto, Bulgaria's First Secretary Zhivkov, and Mongolia's General Secretary Tsedenbal, the same was true of their countries. Their successors quickly closed the gap that had opened between their countries' stagnant political institutions and higher levels of economic development.

Not all countries made such a large jump. Tunisia after Bourguiba and Iran after the Shah merely became slightly more pluralistic dictatorships. Libya after King Idris, China after Deng, Romania after Georghiu-Dej, Yugoslavia after Tito, and Singapore after Lee Kuan Yew saw no increase in political freedom at all. Still, the average rise in Polity2 in these 15 countries, +7.4, is much larger than the average change in all 10-year periods for non-democracies, +1.0.[18]

4 Robustness and extensions

It would be a problem for my argument if coders simply took leadership change as a sign of democratization. However, this is clearly not the case. Among the country-years for which the coders recorded an increase in the Polity2 score, more than half (324) occurred with no leader change that year and 41 percent (255) occurred with no leader change either that year or the previous year. Conversely, of all country-years in which leader change occurred, only 14 percent were coded as years in which democracy increased. Evidently, the coders do not equate the two.[19]

The proportion of cases of democratization without any prior leader change naturally falls as the panel interval increases. If the number fell too low, that could make it hard to estimate the

[18] Appendix Table A6 provides some descriptive statistics on leader turnover by era and income.

[19] Londregan and Poole (1996) found that leadership change and regime change were not related.

effect of income in cases without leader turnover. This might explain why significance is not higher in the 20-year panel, where, among non-democracies whose Polity2 score rose, only 7 percent (12 cases) had experienced no leader change in the preceding 20 years. It is much less of an issue in the lower-interval panels, where more non-democracies had Polity2 increases without prior leader change: 11 percent (22 cases) in the 15-year panel, 21 percent (53 cases) in the 10-year panel, 35 percent (126 cases) in the 5-year panel, and 77 percent (418 cases) in annual data. Without leader turnover, income is not just insignificant in the 20-year panel—it is insignificant in almost all.

Each panel is constructed starting from 1820, the earliest year with relatively consistent Maddison income data. Would starting in a different year change the findings? Appendix Table A7 shows the key results for 5- and 10-year panels beginning in each year of the cycle. Estimates do vary somewhat, although most coefficients remain significant. In the 10-year panels, the effects are stronger if the panel starts in the first half of the decade than if it starts in the second. This is because the influence of income varies over time in ways that result in some differences depending on which cross-sectional slices are included. Section 7 examines this variation.

Table 3 repeats the model from Table 2 column 1, and adds various controls that previous work suggests may affect democratization, using the one-year panel (Appendix Table A9 repeats these regressions in 10-year panels). Whether a country democratizes may depend on the extent of democracy in other countries, especially those nearby (Gleditsch and Ward 2006, Gleditsch and Choung 2004). I control for this in column 2, using a sophisticated measure of "foreign democratic capital"—essentially, the average level of democracy in other countries, weighted by their distance—constructed by Persson and Tabellini (2009).[20] Column 3 controls for foreign trade as a

[20] Their measure is: $f_{it} = (\sum_{j \neq i} (1 - a_{jt}) \varpi (\rho)_t^{ij}$, where *i* and *j* index countries, ϖ^{ij} measures the distance

share of GDP, which others contend affects political regimes (Li and Reuveny 2003, Lopez-Cordova and Meissner 2008). To capture the "resource curse," column 4 includes the logged income per capita earned from the country's sales of oil and gas, from Michael Ross's database.

As many have argued, differences in the nature of authoritarian regimes may affect their odds of democratization. Autocracies that use pseudo- or partly democratic institutions such as elected legislatures to coopt opposition may achieve greater stability (Gandhi and Przeworski 2007), while non-regime parties may help opponents mobilize for political change (Wright and Escribà-Folch 2012). Column 5 controls for these. Column 6 includes whether the head of state was a military officer or a monarch, as recorded by Banks (2007). Column 7 uses the more fine-grained and precise classifications of Geddes, Wright, and Frantz (2012: GWF), who distinguish military, monarchical, one-party, and personalistic regimes (but only since WWII).[21] A country's history of democracy and autocracy may affect its current regime. In column 8, I include Persson and Tabellini's measure of accumulated democratic experience, which they call "domestic democratic capital."[22] As a second check, column 9 contains a variable based on that used by Epstein et al.

---

between *i* and *j*, and ρ operationalizes a geographical limit beyond which influence falls to zero, which they, in fact, estimate from the data.

[21] I use "miscellaneous" for regimes that GWF do not consider non-democracies but which have a Polity2 score less than six; the excluded category is military regime.

[22] They assume this accrues at a fixed rate in each year a country is democratic (Polity2 > 0) and depreciates geometrically in years of autocracy. Specifically, domestic democratic capital, $z_{it} = (1-\delta) \sum_{\tau=0,\ldots,t_0} (1-a_{it-\tau})\delta^{\tau}$, where *i* indexes countries, *t* indexes year, t_0 is the initial year, *a* equals 1 for autocracies and 0 for democracies, and δ is a discount rate that they estimate from the data.

(2006) to capture the legacy of past democratic failures. Epstein et al. used the absolute value of the sum of a country's total downward movements on the Polity scale since 1960. I use the total since the start of the data, and normalize by the number of years.

Could leadership change be picking up the effect of something else? Suppose some other characteristic both predisposed countries to change their leaders often and, coincidentally, made them more likely to democratize. In fact, what matters is not just a predisposition to change leaders but *having just replaced one*. Column 10 controls for the country's rate of leader turnover in the previous 20 years (results are similar if one uses the rate going back to the start of data), as well as its interaction with income. The coefficient on income interacted with actual leader turnover is unchanged, although slightly less significant. Income interacted with the previous rate of leader turnover is insignificant with a counterintuitive *negative* sign. Evidently being prone to change leaders is not enough to render a country's regime sensitive to income: the leader must actually have changed. To control for political instability in another way, Table A8 in the appendix includes the percentage of previous leader changes in the country (since the start of the data) that were "irregular," according to the codings of Goemans et al. (2009a).

Perhaps it is not leader turnover that prompts democratization, but war that overthrows both leaders and regime. It was not just Hitler's suicide that secured West Germany's post-war democracy but the territory's occupation by armies of democratic powers. In Table A8, I control for whether the country had been in a war or civil war the previous year, and whether the government had won or lost such wars. Democratization was more likely if a civil war had been underway, and less likely if the government had just lost one. But this had little effect on the main results.

Some of the controls correlate with democracy in ways previously suggested, while others are not significant (or have the wrong signs) in the demanding setting of a fixed effects panel with

Table 3: Robustness

Dependent variable:	*Polity2 Level of Democracy, Polity2 $_{t-1}$ < 6, 1-yr panels*									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Democracy $_{t-1}$	.90***	.89***	.88***	.87***	.86***	.89***	.89***	.90***	.89***	.90***
	(.01)	(.01)	(.02)	(.02)	(.02)	(.01)	(.02)	(.01)	(.01)	(.01)
Leader replaced in previous period	-.07*	-.06	-.09*	-.06	-.08*	-.09**	-.09*	-.06	-.06	-.06
	(.04)	(.04)	(.05)	(.04)	(.05)	(.04)	(.05)	(.04)	(.04)	(.05)
Ln GDP per Capita $_{t-1}$	-.002	.00	.01	-.00	.00	.01	-.00	.00	-.01	.005
	(.006)	(.01)	(.01)	(.01)	(.01)	(.01)	(.01)	(.01)	(.01)	(.007)
Ln GDP per Capita $_{t-1}$ * leader replaced $_{t-1}$	.010*	.009	.013*	.010	.013**	.013**	.014**	.010*	.010*	.010
	(.005)	(.006)	(.007)	(.006)	(.007)	(.006)	(.007)	(.006)	(.006)	(.006)
Foreign democratic capital $_{t-1}$		.07								
		(.08)								
Trade/GDP			-.010							
			(.007)							
Log income from oil and gas				.001						
				(.002)						
Elected legislature					-.012*					
					(.006)					
Non-regime parties					.008*					
					(.005)					
Military regime $_{t-1}$						.022**				
						(.010)				
Monarchy $_{t-1}$						.014	-.03**			
						(.011)	(.01)			
One-party regime $_{t-1}$							-.04***			
							(.01)			
Personalistic regime $_{t-1}$							-.04***			
							(.01)			
Miscellaneous regime $_{t-1}$							-.04***			
							(.01)			
Domestic democratic capital $_{t-1}$								-.05**		
								(.02)		
Previous transitions									-.08***	
									(.01)	
Previous rate of leader turnover										.10
										(.08)
Ln GDP per Capita $_{t-1}$ * previous rate of turnover										-.015
										(.012)
Implied cumulative effect of income										
-if leader replaced	.07	.11	.21**	.04	.13*	.17**	.12	.13	.04	.10 [a]
-if leader not replaced	-.02	.02	.09	-.04	.03	.05	-.01	.03	-.05	.01 [a]
Fisher p level	[.00]	[.00]	[.00]	[.00]	[.00]	[.00]	[.00]	[.00]	[.00]	[.00]
Observations	5,815	5,425	4,024	4,215	4,020	5,082	4,237	5,425	5,815	4,926
Countries	136	131	124	126	124	134	120	131	136	126
R-squared	.8526	.8498	.8412	.8380	.8300	.8484	.8412	.8506	.8561	.8496

Sources: see Table A16 in Appendix.

Note: All estimations by OLS with country and year fixed effects. Robust standard errors, clustered by country, in parentheses; * p < .10, ** p < .05, *** p < .01. "Fisher p level" is probability level at which one can reject H0: residuals are I(1), from Fisher test of residuals. [a] assuming average rate of previous leader turnover.

a long data series. Military regimes are more prone to democratize (Geddes 1999), and elected legislatures correlate with less—and non-regime parties with more—movement towards democracy (Gandhi and Przeworski 2007, Wright and Escribà-Folch 2012). Past failures of democracy seem to inhibit reform (Epstein et al. 2006). The main point here is that all these controls at most slightly weaken the estimated effect of income conditional on leader change.[23]

In the same regressions run on 10-year panels (Appendix Table A9), the interaction of income with leader change is sometimes less significant, although its cumulative impact remains almost always significant. The weaker results are due almost entirely to the loss of observations because of poorer data coverage for various controls. Running the regressions without the controls but on the limited subset of data for which the control variables were available produces similar drops in estimated effects and significance.

Archigos distinguishes several ways leaders leave office. Besides dying from natural causes, committing suicide, retiring due to poor health, or being deposed by a foreign force, they may be replaced in a "regular" or an "irregular" manner. "Regular" replacements occur "according to the prevailing rules, provisions, conventions, and norms of the country" (Goemans et al. 2009, p.272). Although such turnovers are the rule in democracies, they also occur in authoritarian regimes, as, for instance, when an heir inherits the throne in a monarchy. "Irregular" replacements occur amid abnormal events such as military coups or popular revolts. Table 4 analyzes whether the mode of exit affects the influence of income on subsequent democratization. I examine the 1-, 5-, and 10-year panels, and in each case show regressions for levels (on the Polity2 scale) and transitions

[23] Note that including the country fixed effects is important. Even in a super-saturated model including all the aforementioned controls simultaneously (which requires a large loss of data), an F-test rejects the hypothesis that the country fixed effects are jointly zero at p = .0000.

Table 4: Democratization given different types of leader exit

Dependent variable	------*Polity2*-------		--*BR*--	------*Polity2*-------		-*BR*--	------*Polity2*------		--*BR*--
	Level	*Upward move-ments*	*Trans-itions*	*Level*	*Upward move-ments*	*Trans-itions*	*Level*	*Upward move-ments*	*Trans-itions*
Panel type	----------*1-yr*----------			----------*5-yr*----------			----------*10-yr*----------		
Democracy $_{t-1}$	.90***	.97***		.45***	.71***		.09	.49***	
	(.01)	(.01)		(.06)	(.05)		(.10)	(.07)	
Ln GDP per Capita $_{t-1}$	-.003	-.004	-.00	-.02	-.01	.00	.09*	.08*	.10
	(.006)	(.005)	(.01)	(.03)	(.02)	(.04)	(.05)	(.05)	(.07)
Leader left regular	-.13*	-.15**	-.24	-.03	-.01	-.50	-.07	-.05	-.77
	(.06)	(.06)	(.15)	(.23)	(.22)	(.42)	(.38)	(.39)	(.63)
Leader left irregular	-.07	-.02	-.09	-.13	-.15	-.15	.26	.14	-.03
	(.07)	(.07)	(.16)	(.21)	(.16)	(.22)	(.36)	(.32)	(.35)
Leader died in office of	-.07	-.04	-.14	-.45*	-.42**	-.45	.16	.08	.15
natural causes	(.10)	(.06)	(.16)	(.26)	(.21)	(.33)	(.36)	(.30)	(.35)
Leader deposed	.38	.04	-.43	.03	-.22	-.31	1.09	.75	.62
	(.37)	(.21)	(.44)	(.55)	(.65)	(.58)	(.80)	(.52)	(.47)
Leader retired due to poor	.29	-.08	-.25*	-.32	-.28	-1.32	.04	-.01	.63
health	(.43)	(.07)	(.13)	(.56)	(.37)	(1.35)	(.52)	(.25)	(1.78)
Regular leader exit *	.02**	.02**	.04*	.02	.02	.09	.03	.02	.13
ln GDP per Capita $_{t-1}$	(.01)	(.01)	(.02)	(.03)	(.03)	(.06)	(.05)	(.05)	(.09)
Irregular leader exit *	.01	.01	.02	.02	.03	.02	-.03	-.01	-.00
ln GDP per Capita $_{t-1}$	(.01)	(.01)	(.02)	(.03)	(.02)	(.03)	(.05)	(.05)	(.05)
Death from natural causes *	.01	.00	.02	.06*	.06**	.06	-.02	-.01	-.03
ln GDP per Capita $_{t-1}$	(.01)	(.01)	(.02)	(.04)	(.03)	(.05)	(.05)	(.04)	(.05)
Deposed by foreign force *	-.05	.00	.06	.01	.04	.03	-.15	-.10	-.10
ln GDP per Capita $_{t-1}$	(.05)	(.03)	(.07)	(.07)	(.09)	(.08)	(.10)	(.07)	(.06)
Leader retired *	-.04	.01	.03*	.05	.04	.18	-.02	-.02	-.06
ln GDP per Capita $_{t-1}$	(.06)	(.01)	(.02)	(.07)	(.05)	(.19)	(.07)	(.03)	(.22)
Cumulative effect of income if leader									
-exited regular	.15	.61	.04	.01	.01	.09	.13*	.20	.22**
-exited irregular	.09	.04	.02	.01	.06	.02	.07	.13	.09
-died of natural causes	.05	-.00	.02	.08	.15	.06	.08	.13	.06
-was deposed	-.49	-.16	.06	-.02	.09	.04	-.06	-.05	-.00
-retired due to health	-.46	.17	.03	.06	.09	.19	.08	.12	.04
-remained in office	-.03	-.17	-.00	-.03	-.05	.00	.10*	.15	.10
Fisher p level	[.00]	[.00]	[.00]	[.00]	[.00]	[.00]	[.00]	[.00]	[.00]
Observations	5,815	5,815	5,261	1,159	1,159	1,048	534	534	518
Countries	136	136	137	133	133	133	118	118	122
R-squared	.8531	.8996	.1104	.6388	.7362	.3281	.6225	.7120	.5369

Sources: see Table A16 in appendix.

Note: All estimations by OLS with country and year fixed effects. Robust standard errors, clustered by country, in parentheses; * p<.10, ** p<.05, *** p<.01. "Fisher p level" is probability level at which one can reject H0: residuals are I(1), from Fisher test of residuals. "BR": Boix-Rosato dichotomous measure. Too few cases of leader suicide to estimate effects. If more than one leader turnover during the panel interval, type of turnover refers to last one.

(upward movements on the Polity2 scale and jumps from 0 to 1 on the Boix-Rosato measure).[24]

For the most part, the statistical tests lack sufficient power to distinguish between the effects of income after different types of leader exit. However, contra Miller (2010), they do not suggest that income matters most after leaders are violently overthrown. In fact, the strongest and most significant effect in a one-year window is that after *regular* leader replacement. In a five-year window, income is most influential and statistically significant after a leader dies of natural causes. (It makes sense that the effect would take longer to show up after a natural death than after a regular transition since the former will often come as a surprise.) No interaction terms are significant in the 10-year panel, but the cumulative impact of income after a regular transition is significant. Although violent leader replacements may—like other types—sometimes foster democratization, they are clearly not driving the results. That income has a significantly stronger impact (within five years) if the leader died from natural causes than if no turnover occurred should increase confidence that the results are not marred by endogeneity. The timing of leaders' deaths by natural causes can be considered quite exogenous—and previous papers have used this as a compelling source of identification in assessing the effects of leaders' actions (Jones and Olken 2005).

5 The effects of economic growth

If higher income increases the odds of a transition to democracy when a leader leaves office, why

[24] In a subset of cases in the 5- and 10-year panels, more than one leader change occurs within the interval. In such cases, I focus on the mode of the final turnover. If a regular turnover is followed by a revolution that sweeps away the old leader, one would expect the revolution to affect the type of regime at the end of the period more than the earlier peaceful transition. An alternative—to drop all but the cases with at most one change—produces generally similar but less significant results.

do authoritarian rulers ever promote economic growth? Of course, some might not care what kind of regime emerges after their exit. But many clearly do. Monarchs hope to pass on the patrimony to their heirs. Dictators like Hafez al-Assad, Kim Jong Il, and Hosni Mubarak scheme to hand power to their sons. Others worry about the security of broader circles of associates or co-ethnics who risk prosecution or persecution under a majoritarian regime.

So why hasten the transition? The simple answer is that, even though economic development makes anointing a successor harder, in the short run high growth increases the dictator's odds of retaining power. Several papers find that higher growth boosts an autocrat's survival chances (Londregan and Poole 1996, Bueno de Mesquita and Smith 2010). In Appendix Table A10, I demonstrate this in a more historically extensive dataset than previously examined. The relationship is clear using a variety of models (OLS with country and year fixed effects, CLFE with year dummies, Weibull hazard model), instrumenting for growth using trade-weighted growth rates in other countries, and controlling for many confounding factors (income, regime type, war, civil war, leader turnover elsewhere, and the leader's age, tenure, and previous times in office).

If lower growth increases the odds of leader turnover, does it also speed up the rate of democratization? (This need not follow, since leader turnover is associated with democratization only in richer autocracies.) In Appendix Table A11, I explore this question. The results suggest that *when* lower growth leads to or accompanies leader change, democratization is, indeed, more likely. (Higher growth without leader change may also slow democratization (A11, column 2), but the effect is weaker and less significant; the impact of the growth rate *after* leader change is unclear.)

To sum up, lower growth is associated with—and the IV regressions suggest it causes—faster leader turnover in autocracies. When low growth is followed or accompanied by such leader change, it is associated with—and may cause—more rapid democratization. The logic for a dictator

is clear. The higher is his country's growth rate in a given year, the more likely he will be to survive in office and avoid a democratic revolution. When—or if—the time comes to step down, higher growth that year will enhance his chances of handing the regime intact to a chosen successor.[25] The irony is that over time faster growth results in significantly higher national income. And in high income countries, leader transitions *more* frequently end in democracy.

That low economic growth increases the odds of leadership change—and, indirectly, of democratization—casts light on Kennedy's (2010) finding. Kennedy argued that slower growth predisposes countries to make major institutional changes, the direction of which is then determined by the country's income level. I contend that the growth rate affects institutional change mostly through its effect on—and interaction with—leader replacement. Low economic growth first increases the odds that the incumbent leader will fall (Table A10) and then, if he does, influences the new leader's willingness to open up the political system.

Table A12 provides evidence for this. The dependent variable is Kennedy's measure of "major institutional change"—a dummy that equals 1 if the country's Polity2 score changed that year by three or more points. I estimate models with both OLS and IV, instrumenting for growth with trade-weighted growth in other countries. Column 1 shows that, even with country and year fixed effects, growth is negatively related to institutional change, as Kennedy argued. (The instrumented coefficient in column 4 is also negative, albeit not statistically significant.) Column 2 adds dummies for leader turnover in the previous and current year (coding as 0 cases where the leader changed after or on the same day as change in the Polity2 score, to focus on leader change

[25] Pursuing growth is only one of several survival strategies. If dictators control plentiful free resources, for instance in the form of oil rents, they may instead focus on patronage and repression (Bueno de Mesquita and Smith 2009; Wright 2012).

prior to regime change). Growth still has a negative effect. However, it is dwarfed by that of current year leader turnover. If the leader exits, the chance of major institutional change that year goes up by 6.5 percentage points (column 2) or 9 points (column 5). To cause an increase in the probability this large, the growth rate would have to fall by 32.5 percentage points (column 2) or 20 percentage points (column 5).[26] Moreover, the impact of growth on institutional change is greater if followed by leader turnover (columns 3 and 6). With no leader change, the effect of growth is small and insignificant (-.11 in column 3); if followed by leader turnover, growth has an impact almost five time as large (-.11 + -.43).[27] In short, lower growth is associated with major institutional change, but mostly by prompting leader turnover and catalyzing the effect of such turnover if it occurs.

6 Why does leadership change matter?

Why does leadership turnover promote democratization in countries that have become relatively rich? Several reasons are possible, some related to characteristics of the leaders, others to the setting. First, the new ruler may be more highly educated than his predecessor. Across individuals, education correlates with tolerance and more liberal attitudes; the same may be true of leaders. In the Soviet Union under Leonid Brezhnev, the share of adults with a high school diploma increased from 17 to almost 60 percent (Hough 1997, p.44). Yet it was only after Brezhnev's death—and those of two decrepit successors—that a member of the new generation, Mikhail Gorbachev, the most highly educated leader since Lenin, began political reform.

Second, new leaders may be younger than the dictators they replace. Various studies suggest

[26] That is, .065/(-.20) = -.325, or 32.5 percentage points (column 2) or .09/(-.45) = -.20 or 20 percentage points (column 5).

[27] Again, estimates are not statistically significant when instrumented, but the pattern is similar.

that aging inculcates conservatism (e.g. Truett 1993). Leaders from a generation that grew up during modernization may also have been socialized into more democratic values. Third, it might be that leaders' conservatism grows not with age, but with their time in office. Newly installed leaders might be readier to embrace risky policies; the longer they serve, the more invested they become in existing arrangements and the more reluctant to shake things up.

These factors concern characteristics of leaders. But the key point might be that new rulers face a distinctive environment. Especially if the previous dictator was overthrown by street protests, his successor may encounter a mobilized opposition. Acemoglu and Robinson (2006) argue that democracy emerges precisely under the pressure of such mass movements because rulers cannot commit to future concessions that are not enforced by popular representation. In such a crisis, the new leader may see appealing to newly active groups as his best bet for survival. After Indonesian dictator General Suharto was ousted in 1998, his vice-president, B.J. Habibie, promptly relaxed controls over the press, legalized opposition parties, and promised democratic elections the following year. By doing so, he diverted the opposition, which had sought to force his resignation, into preparing electoral campaigns (Liddle 1999). Even if the old dictator was not toppled by mass unrest, his disappearance may become a focal point for the release of latent frustration.[28]

Taking these points in turn, I examined leaders' educational levels using data assembled by Besley and Reynal-Querol (2011), who coded whether 1,468 leaders in 197 countries since 1874 had a college degree, a graduate degree, or none at all.[29] They found that, although the education level of leaders in both democracies and autocracies rose in the 20th Century, the level in

[28] Another possibility, which I lack data—and space—to examine here, is that democracy emerges by default as a means of sharing power between equally balanced, contending social forces.

[29] I thank Marta Reynal-Querol for sharing these data.

democracies was consistently higher. I created an index of educational attainment coded 0 if the leader had no higher degrees, 1 for just college, and 2 for graduate degree. It turns out that in non-democracies movement up the education scale has been slow and uneven. After turnovers, the new leader was better educated 27 percent of the time, worse educated 24 percent, and the same level 49 percent.[30] Do increases in leaders' educational attainment catalyze the effect of income on democracy? Table 5 column 1 shows that a jump in the leader's education level does correlate with more democratization (although the result is not robust: see below), but the interaction of increased education with national income was not significant (column 2).

What about age? New heads of state do tend to be younger—on average the leader's age increases by .58 years for each year in office in authoritarian states (.75 years in democracies).[31] The average leader takes office at 51 in autocracies (55 in democracies). When a leader is replaced, his successor is on average 3.6 years younger in autocracies (1.9 years younger in democracies). Do such age differences explain why income correlates most strongly with the political regime early in an authoritarian leader's term? If so, one might expect the interaction of age and national income to be significant in regressions of democracy and to reduce the estimated effect of leader change interacted with income. Table 5, columns 3 and 4, present evidence consistent with this; the estimates suggest that if the leader is younger than fifty, higher income is associated with more democratization (but these results will also prove non-robust—see below).[32]

Are freshly-appointed leaders more willing to embrace risky policies? We saw already that

[30] Of course, it could be that a finer-grained scale would reveal a more consistent pattern.

[31] Based on regressions with country and year fixed effects. For democracies, I use only tenure up to 15 years; after that the pattern gets erratic because few democratic leaders survive so long.

[32] From .02*ln income -.0004*ln income*age = 0.

new leaders are more likely to democratize, at least in richer countries. In Table 6, I seek further evidence. If time in office breeds conservatism, we might expect the odds of democratic reform not just to be higher in a leader's first years, but to decline monotonically with his tenure. Column 1 shows that they do. Each year a leader serves, the probability he will democratize falls by 0.3 percentage points (compared to an average probability of about 6 percent). Does this reflect a general increase in risk aversion as tenure increases? To explore this, I examined leaders' propensities to take several other risky actions. Table 6 column 2 shows that more seasoned leaders are not only less likely to democratize, they are also less likely to *de*-democratize. Each year a leader serves, the probability he will reduce his country's Polity2 score falls by 0.08 percentage points (average probability about 3 percent). In other words, leaders who have served for some time are less likely to undertake either liberal or repressive reforms. To show this another way, column 3 regresses a dummy for no change in the Polity2 score on the usual variables, including leader's tenure. The longer a leader has served, the more likely he will preserve the status quo.

Some evidence also suggests that new leaders are more likely to start military conflicts. In columns 4 and 5, the dependent variable is a dummy for whether the state initiated at least one militarized interstate dispute (MID) during the year.[33] I include an interaction term to check whether the effect of tenure differs in more and less democratic states. Besides country and year fixed effects and level of democracy, I control in column 5 for other factors that previous work suggests might matter—trade dependence (Oneal, Russett and Berbaum 2003), economic growth (Oneal and Tir

[33] Data are from the Correlates of War project (Ghosn, Palmer, and Bremer 2004, version 3). A militarized interstate dispute occurs when one state directs "the threat, display or use of military force short of war" explicitly towards "the government, official representatives, official forces, property, or territory of another state" (Jones, Bremer, and Singer 1996).

Table 5: Why does leader turnover matter?

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	----------Polity2 Level of Democracy, Polity2 $_{t-1}$ < 6, 1-year panel ----------								
Democracy $_{t-1}$	.89***	.89***	.89***	.89***	.88***	.88***	.87***	.87***	.86***
	(.01)	(.01)	(.01)	(.01)	(.01)	(.01)	(.02)	(.02)	(.02)
Leader replaced $_{t-1}$	-.05	-.05	-.06*	-.05	-.06	-.03	-.08	-.07	-.04
	(.04)	(.04)	(.04)	(.04)	(.05)	(.04)	(.05)	(.05)	(.05)
Ln GDP per Capita $_{t-1}$	-.003	-.003	-.002	.02*	-.001	.006	.004	.004	.019
	(.006)	(.006)	(.006)	(.01)	(.006)	(.007)	(.008)	(.008)	(.015)
Ln GDP per Capita $_{t-1}$ *	.008	.008	.010*	.008	.008	.004	.013*	.012	.005
leader replaced $_{t-1}$	(.006)	(.006)	(.005)	(.005)	(.006)	(.006)	(.007)	(.007)	(.007)
Education of leader $_{t-1}$	.002	.002							.002
	(.003)	(.003)							(.004)
Change in education of leader $_{t}$	.020***	.042							.13
	(.007)	(.080)							(.09)
Ln GDP per Capita $_{t-1}$ *		-.003							-.015
change in education of leader		(.010)							(.012)
Age of leader			-.0003**	.003**					.002
			(.001)	(.001)					(.002)
Ln GDP per Capita $_{t-1}$ *				-.0004**					-.0002
age of leader				(.0002)					(.0003)
Leader's years in office					-.0009***	.004**			.004
					(.0002)	(.002)			(.003)
Ln GDP per Capita $_{t-1}$ *						-.0007***			-.0006
leader's years in office						(.0002)			(.0004)
Previous leader's years in office					.0013*	.0013*			.0016*
if replaced last year					(.0007)	(.0007)			(.0009)
Δ antigovernment							.006***	-.020	-.023
protests $_{t-1}$							(.002)	(.018)	(.018)
Ln antigovernment							.025***	.024***	.022***
protests $_{t-2}$							(.006)	(.006)	(.006)
Ln GDP per Capita $_{t-1}$ *								.003	.004
Δ antigovernment protests $_{t-1}$								(.002)	(.002)
Implied cumulative effect of									
-education of leader $_{t-1}$	.02	.02							
-change in education of leader $_{t}$	.18***								
-change in ed: GDP p.c. = $6,000		.15							.001
-age of leader			-.003**						
-age: GDP p.c. = $6,000				-.007***					.000
-leader's years in office					-.008***				
-leader's years: GDP p.c. = $6,000						-.014***			-.012***
-Δ antigovernment protests $_{t-1}$							.049***		
-Δ protests $_{t-1}$: GDP p.c. = $6,000								.067***	.059***
Fisher p level	[.00]	[.00]	[.00]	[.00]	[.00]	[.00]	[.00]	[.00]	[.00]
Observations	5,651	5,651	5,814	5,814	5,693	5,693	4,296	4,296	4,271
Countries	136	136	136	136	136	136	128	128	128
R-squared	.8528	.8528	.8527	.8528	.8525	.8528	.8312	.8314	.8336

Sources: see Table A16 in Appendix.

Note: All estimations by OLS with country and year fixed effects. Robust standard errors, clustered by country, in parentheses; * p<.10, ** p<.05, *** p<.01. "Fisher p level" is probability level at which one can reject H0: residuals are I(1), from Fisher test of residuals.

2006), antigovernment protests (Miller 1999), military power (Bremer 1992; I use the COW project's Composite Index of National Capability), the country's past rate of starting international conflicts (from the beginning of the data), the leader's age (Horowitz, McDermott and Stam 2005), and whether he was a military officer (Lai and Slater 2006). In columns 6 and 7, I show similar regressions for whether the country started a war. (Overall, MIDs were initiated in 13 percent of country years and wars in 0.7 percent.)

The results suggest that leaders in authoritarian states *are* more likely to initiate military conflicts early in their terms than later on. (This is not the case for democratic leaders, whose propensity may even increase with tenure.) The effect is relatively modest, but statistically significant, especially among the harsher autocracies (I report the total impact for those with Polity2 of -6).[34] The longer a relatively repressive dictator has been in power, the less likely he is to start a militarized dispute or a war.[35] Most of the controls were not significant. Economic crises may boost the odds of a militarized dispute and generals in power may start fewer wars than civilian leaders—

[34] Gelpi and Grieco (2001) argue that inexperienced leaders are more likely to be targeted by challengers, but note that I examine whether a leader himself *initiates* an MID or war. In a few such cases, an inexperienced newcomer might have been provoked into taking the first step, but this is unlikely to be driving the results.

[35] Chiozza and Choi (2003, p.273), in a study of territorial disputes in 1950-90, found similarly that nondemocratic leaders were "slightly more conflict prone in the early phases of their tenure and slightly more inclined to seek a peaceful resolution later in their careers than their democratic counterparts." Horowitz et al. (2005) found that leaders became more prone to participate in MIDs as they aged (except in personalist autocracies), but that their tenure was insignificant. However, they did not distinguish tenure of leaders in democracies from that in autocracies, and examined the

whether because they are more restrained or more able to dominate without actually using force.

If leaders grow more conservative the longer they serve, can this explain why income matters most for democracy early on? I lack a direct measure of leaders' conservatism over time, but the evidence does confirm that the tendency to democratize declines continuously with a leader's years in office (Table 5, column 5). Moreover, the effect of income does fall as the leader's tenure rises (column 6), reaching zero around his ninth year.[36] Testing the hypothesis in another way, if what matters is the difference in conservatism between long-serving and new leaders, the democratic impulse under a new leader should be stronger, the longer the previous leader had served. I check this by including a variable that in a new leader's first year takes the value of the previous leader's tenure, and equals zero in other years. As expected, it is positive and significant: for each year that the *previous* leader had served, the level of democracy at the end of his successor's first year is .13 percentage points higher. Finally, the influence of income interacted with leader turnover falls sharply once one includes the leader's tenure interacted with income, suggesting that the immediate post-turnover effect is part of a more continuously operating process.

To explore whether popular mobilization contributes to the explanation, one needs a measure of mobilization. I follow some previous papers (e.g. Bueno de Mesquita and Smith 2010, Alemán and Yang 2011), in using data on the incidence of antigovernment demonstrations (with at least 100 participants) compiled by Banks (2007). These data are based on newspaper reports, which raises the obvious question whether limits on the press in more authoritarian countries might reduce reporting of civil unrest. It is hard to rule this out, but one can check whether reports of

---

average extent of hostilities between a given state and all others, rather than the probability that the leader of a given state engages in hostilities with *at least one* other, which may explain the difference.

[36] From: 0.006*(ln income) - .0007*(ln income)*(years in office) = 0, years in office = 8.6.

Table 6: Are new leaders less risk-averse?

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent variable:	*Dummy for Polity2 moved up*	*Dummy for Polity2 moved down*	*Dummy forPolity2 unchanged*	*Dummy for initiated a militarized interstate dispute* [a]		*Dummy for initiated a war* [a]	
Countries:	*$Pol2_{t-1} < 10$*	*$Pol2_{t-1} > -10$*	*$-10 < Pol2_{t-1} < 10$*	*all*	*all*	*all*	*all*
Democracy $_{t-1}$	-.13***	.11***		-.12***	-.13***	-.011*	-.022**
	(.02)	(.02)		(.04)	(.05)	(.006)	(.009)
Ln GDP per Capita $_{t-1}$	-.020	-.016	.035*		-.047		-.018**
	(.018)	(.011)	(.018)		(.037)		(.008)
Leader's years in office	-.0030***	-.0008**	.0032***	-.0034***	-.0042**	-.0003**	-.0006**
	(.0007)	(.0003)	(.006)	(.0009)	(.0016)	(.0001)	(.0002)
Leader's years in office * democracy $_{t-1}$				.0055***	.010**	.0004	.0011**
				(.0020)	(.004)	(.0004)	(.0005)
Polity2 unchanged $_{t-1}$			.13***				
			(.02)				
Country's past rate of initiating wars						-.21*	-.21
						(.11)	(.15)
Country's past rate of initiating MIDs				.25**	-.13		
				(.11)	(.17)		
Leader's age					-.000		-.0001
					(.001)		(.0001)
Growth rate $_{t-1}$					-.003***		.00
					(.001)		(.00)
State's military capability $_{t-1}$					-.51		.36
					(1.85)		(.33)
Trade as share of GDP $_{t-1}$					.02		.002
					(.05)		(.005)
Ln antigovernment demonstrations $_{t-1}$					-.001		.002
					(.012)		(.003)
Head of state a military officer					.035		-.016**
					(.034)		(.007)
Total effect of leader's years in office if Polity2 $_{t-1}$ = -6				-.002***	-.002*	-.0002*	-.0004*
Fisher p level	[.00]	[.00]	[.00]	[.00]	[.00]	[.00]	[.00]
Observations	7,201	8,712	6,861	10,755	5,267	11,095	5,215
Countries	150	156	148	167	149	168	148
R-squared	.0895	.0782	.0937	.2321	.2744	.0549	.0987

Sources: see Table A16 in Appendix.

Note: All estimations by OLS with country and year fixed effects. Robust standard errors, clustered by country, in parentheses; * p<.10, ** p<.05, *** p<.01. "Fisher p level" is probability level at which one can reject H0: residuals are I(1), from Fisher test of residuals; [a] years in which state does not initiate new war or MID but continues one it previously initiated are excluded. Cases where lagged Polity2 score equals 10 and/or -10 excluded in columns 1-3 to adjust for fact that countries cannot move beyond the limit of the scale.

unrest correlate negatively with measures of restrictions on the press. In fact, I show in Appendix Table A13 that Banks' measure of antigovernment demonstrations is either unrelated to press freedom or, if one controls for other determinants of protest, actually *higher* in countries where the press is more restricted. Although this may alleviate concern, it does not eliminate it. Bueno de Mesquita and Smith make the plausible argument that even if countries differ in their medias' coverage of civil unrest, these differences should not usually change radically from one year to the next. Thus, the interyear *change* in the number of demonstrations may be a more reliable indicator than the number itself. I therefore use change in demonstrations as my measure of mobilization.

How does the dynamic of antigovernment protests relate to the leader's tenure? Controlling for country and year fixed effects, the increase in antigovernment demonstrations falls every year a dictator remains in power (Table A13, column 4; this is not true of democratic leaders—column 5). That said, the pace of protests picks up in the few years *preceding* a leader turnover. Do such popular pressures correlate with democratization? Table 5 column 7 shows that increases in protests do predict movement towards democracy. The interaction with income is not quite significant (column 8), but at higher incomes the total impact is: surges of demonstrations are most clearly linked to political reform in more economically developed autocracies. However, such protests cannot explain why richer countries are more likely to democratize after leader turnover. In fact, the effect of income conditional on leader turnover is even stronger than before.

Column 9 of Table 5 includes the various measures together in the same regression. The estimated cumulative effects of age and education are insignificant and close to zero (calculated for a non-democracy with GDP per capita of $6,000).[37] However, the cumulative effect of the leader's

[37] Leaders' education might still be important if the effect of income is to favor the selection of well-educated leaders.

time in office is still significant (for countries with income above about $1,200). At the same time, the longer the previous dictator ruled, the more likely is a subsequent move towards democracy. And increases in antigovernment demonstrations are still associated with democratization, more and more strongly as income rises. When the number of antigovernment demonstrations is rising fast, whether democracy results becomes more sensitive to the country's level of development.

The data available are less than ideal, and I cannot hope here to adequately address problems of endogeneity and strategic interaction. Still, the results are suggestive. Newly installed leaders in authoritarian states tend to be readier than their more seasoned predecessors to embrace risky new initiatives. Sometimes, these involve creating more repressive institutions to crush opposition. But in richer countries, they more often involve opening the political system to broader participation.

7 Democratization over time

As others have noted, the impact of income on democracy varies across time periods. Income is commonly thought to have been less important in the post-WWII period, on which AJRY (2008, 2009) focused. This certainly seems the case, comparing panels A and E to the others in Table 1.

On closer examination, however, this conclusion seems dubious. While the coefficient on income estimated with a 10-year panel on 1960-2000 is just .02 (Table 1, Panel A, third column), the coefficient from an identical regression estimated on 1950-1990 is .12 ($p < .10$) and that from the same regression on 1940-1980 is .19 ($p < .05$). The influence of income varies considerably over time, even between overlapping periods. Running similar regressions for various 40-year periods from 1840 on, it turns out that the income effect since the war is not historically low.

Another way to assess the influence of income in different eras is to regress democracy on lagged democracy, income, country and year fixed effects, and interactions between income and

each year fixed effect:

$$d_{it} = \alpha d_{it-1} + \gamma y_{it-1} + \beta_t \mu_t * y_{it-1} + \mu_t + \delta_i + u_{it} \quad (5)$$

From this, one can calculate the effect of income on democratization in each year, *t*: $\gamma + \beta_t$. I estimated Equation 5 in 1-year, 5-year, 10-year, and 20-year panels, for non-democracies, and plot the (short-run) effects of income in Figure 1. Several points are immediately apparent. First, however one estimates, the effect of income varies over time.[38] Second, as in Table 1, the estimates from low-frequency panels tend to be higher than those from high-frequency ones (income has a stronger impact after 20 years than after one year). Third, the 1960-2000 period does not stand out. Fourth, certain peaks are evident—around 1848, 1918-20, 1945, 1960, and 1990.[39]

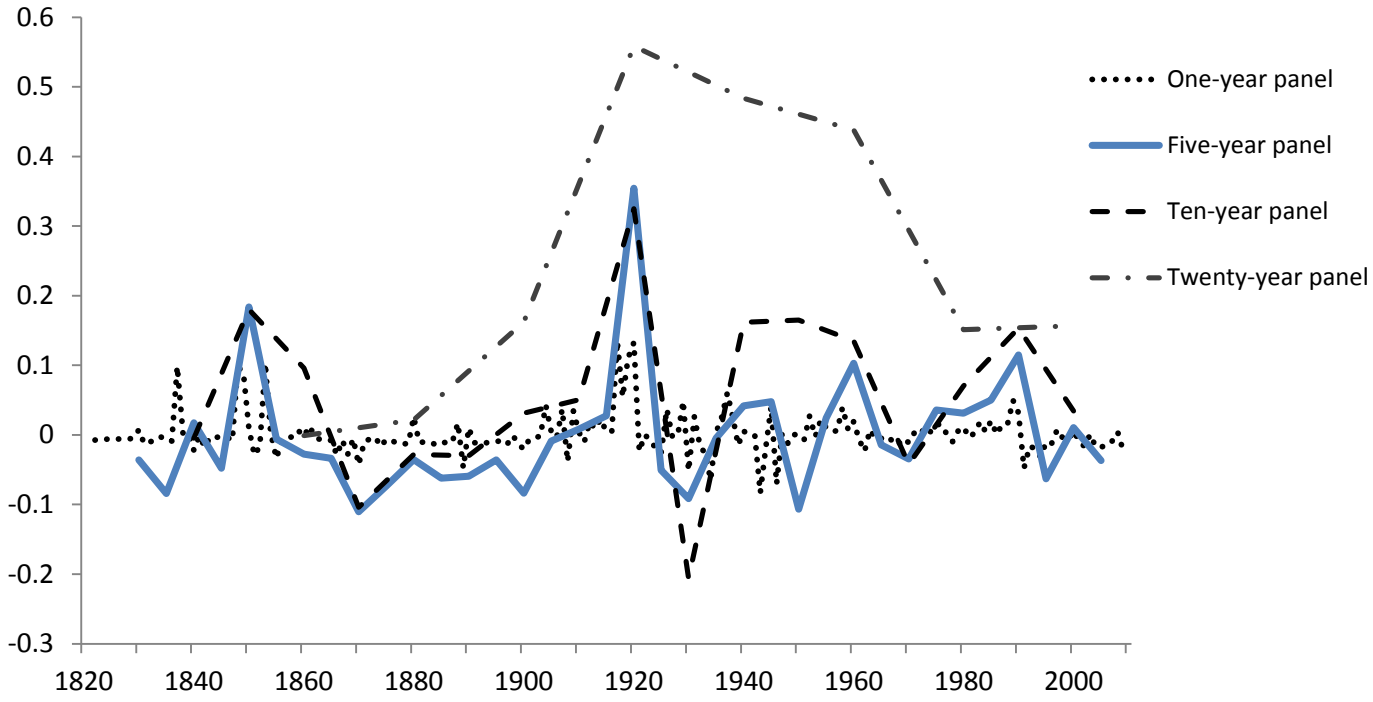
What might explain this pattern? Boix (2011) argues that the international distribution of power has at different times favored or hindered democratization. He divides the period since 1800 into four categories. Least conducive to democracy were the years of "authoritarian hegemony"

[38] This can explain why the estimated impact of income varies somewhat depending on in which year in the cycle a panel begins (recall Table A7). Depending on which years are included and excluded by a particular way of slicing the data, the estimated total impact of income will be slightly different.

[39] Boix, Miller and Rosato (2012) also present a graph with estimated effects of income by year. They estimate for separate 5-year slices of the data, with a control for year but no fixed effects. The pattern thus derived differs somewhat from that in Figure 1, which is estimated using the full data in a single regression with interaction terms and country and year fixed effects, as well as the lagged dependent variable (because regime type exhibits considerable inertia). Since the range of countries included changes dramatically over time, not controlling for country characteristics risks conflating change in the effect of income with change in the composition of the sample.

(1800-1848), in which autocracies dominated the international scene. Next most anti-democratic was the "polarized order" of 1933-1990, in which one authoritarian superpower confronted a democratic one. In the "neutral" order of 1849-1918, the great powers joined in non-ideological alliances based on realpolitik. Finally, a "pro-democratic order" in which "all the great powers are democratic" existed at the height of the Wilsonian project (1918-1933) and again after 1990.

Figure 1: Effect of income on democracy by year



Source: Author's calculations .

Appendix Table A14 shows estimates of the cumulative impact of income on democracy in each of these periods, calculated for 1- to 20-year panels. I run regressions similar to those in Boix (2011), with interaction terms to capture the effects of income in the separate periods (as well as period dummies, and country and year fixed effects). I follow Boix in controlling also for whether the country was under Soviet control (this changes the results only slightly). However, unlike Boix, I restrict attention to countries where the lagged Polity2 score is less than six (or, using the

dichotomous Boix-Rosato democracy variable, where the country was a "non-democracy"). Not to adjust for this would artificially reduce the effect of income in later periods as more countries reach the top of the Polity scale and cannot rise further despite higher income.

In some regards, Table A14 confirms Boix's findings. Under "authoritarian hegemony" before 1848, income has the weakest effect. However, the two "pro-democratic" periods (1918-1933 and after 1990) turn out to be quite distinct.[40] The first consistently exhibits the strongest effect of income (note the huge spike around 1920 in Figure 1). But after 1990, income has a much weaker effect, and in the one-year panel it is actually significantly negative (Table A14, panel A). Against expectations, the "polarized" era of 1933-90 has in all panels stronger income effects than the "neutral" period of 1849-1918, although the differences are not always statistically significant.[41] A similar pattern is evident focusing on transitions (both to and from democracy) and using the Boix-Rosato dichotomous democracy measure (panels C-E).

In sum, the evolving influence of income over time does not fit closely with changes in the international system. Income continued to matter in the post-war era despite the polarized competition of the Cold War. It played a much smaller role in the period of democratic hegemony

[40] Boix (2011) did not examine this difference since the regressions in his Table 5 use data only up to 1990. Note that the post-1990 period *is* unusually hospitable to democracy: the coefficient on the dummy for this period in the one-year panel is positive and significant. The point is that democratization in this period is not very sensitive to *income*; rich and poor states alike democratized at relatively high rates.

[41] The two are closer—and the "neutral" period often has the stronger effect—if all countries are used (Panel B), but the effect is still larger for the "polarized" period (although insignificantly so) if one excludes only those countries that already have the top score of 10 (not shown).

after 1990. It is hard to reconcile the sharp spikes in Figure 1 with the notion that long-lived international configurations of power exercised relatively consistent influence throughout their existence. These spikes often come not in the middle of pro-democratic eras but in the border years—1848, 1918-20, 1945, 1989—when the international system and domestic orders of many states were in flux.

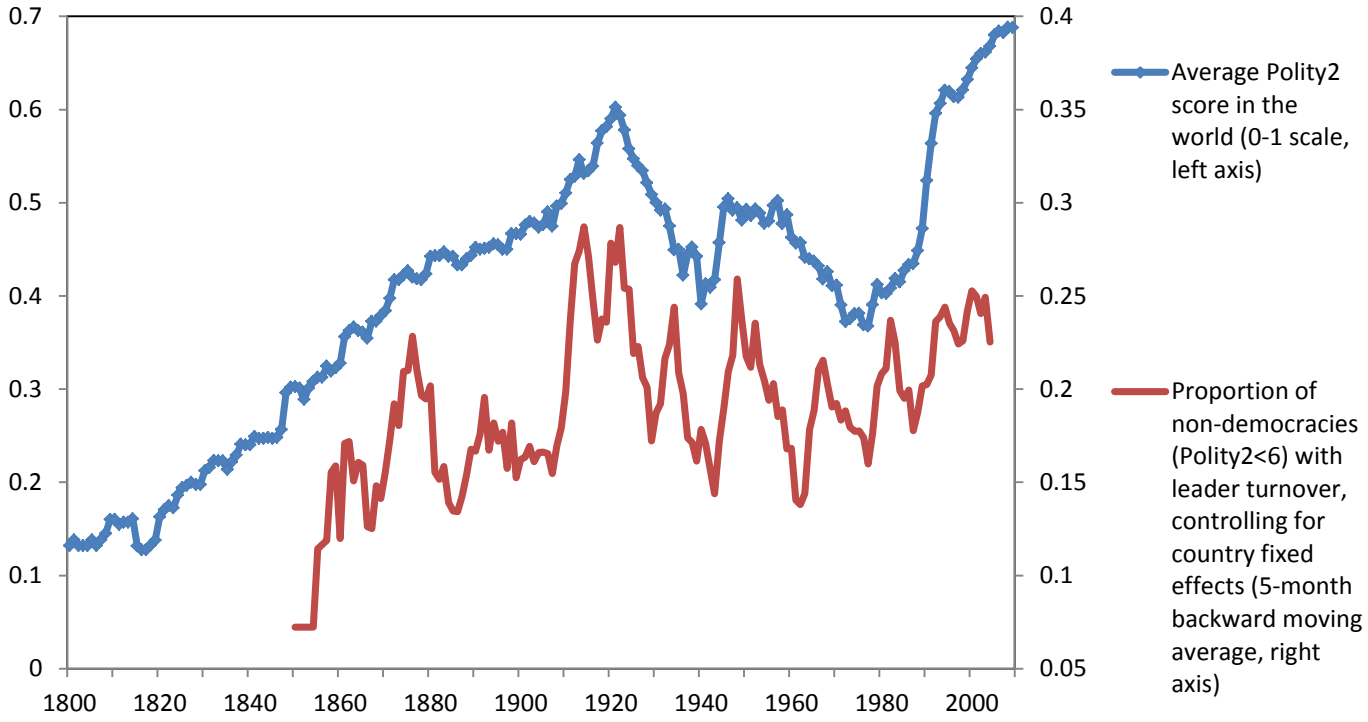
Although leader replacement is probably not the whole explanation, it is noteworthy that the biggest spikes coincide with times of high turnover. Of all years from 1840 to 2004, 1848 and 1920 had respectively the second and the fourth highest rates of leader change in non-democracies.[42] Figure 2 shows the trends over time in the global level of democracy (average Polity2 score) and in the frequency of leader turnover in non-democracies. The trends often coincide. Of course, visual impressions can deceive. In Appendix Table A15, I present three error correction models, using the time series in Figure 2, which demonstrate the statistical significance of three relationships (tests of the residuals allow one to reject non-stationarity with high confidence). There is strong evidence of both long run and short run relationships between: (a) the frequency of leader turnover in non-democracies and the global level of democracy (positive), (b) the average growth rate in non-democracies and the frequency of leader turnover in those countries (negative), and (c) the proportion of non-democracies with a negative growth rate—one measure of recession—and the frequency of leader turnover in those countries (positive). These correlations do not prove a causal connection, but they are consistent with the view that global recessions prompt higher than usual rates of leader turnover in the authoritarian world, which in turn produce bursts of democratization.

A final question about timing concerns the often-noted phenomenon of waves of democratization followed by reversals (Huntington 1991). An explanation emerges from the

[42] The data for leaders in 1848 contain only five countries and so cannot be completely trusted.

preceding analysis. In each country, the zig-zag pattern is inherent in the conditional impact of income. This implies an increase in democracy early in a new leader's term, as economic advance under his predecessor translates into political opening, followed by partial backsliding as the impact of income weakens. In fact, simulating any of the models in Table 2, panel A, for a country initially in equilibrium produces a sharp jump in the Polity2 score after a leader change, followed by a reverse wave that undoes some of the advance. The reason for waves in individual countries is, thus, the same as the reason why the effects of income are felt mostly in the early years of a new leader.

Figure 2: Democracy and leader change in non-democracies, 1800-2008



Sources: see Table A16 in Appendix.

In the aggregate, such waves might cancel out if leader turnover were random across countries. But it is not (Figure 2). Bursts of leader turnover may result from global recessions or wars, which destabilize autocrats in many countries simultaneously. Such bursts, when they occur

in countries that have grown richer under authoritarian rule, produce temporary synchronized surges of democratization that then weaken as new leaders consolidate their positions.

8 Conclusion

In *Lectures on the Philosophy of World History*, Hegel gave the name "the cunning of reason" to the way great leaders, driven by the "energy of their ego," and seeking their own self-aggrandizement, are unwittingly used by history for its own purposes (Hegel 1980 [1837], pp.86-9). A Napoleon Bonaparte or a Julius Caesar sought only personal power and glory. Yet such men became the tools by which outmoded political orders were replaced by ever more universal forms of the state. Of course, the logic identified here is not exactly Hegel's; the notion that economic development drives political change is closer to Marx. But that autocrats, merely by trying to ensure their supremacy and survive a little longer, end up preparing their countries—and the world—for breakthroughs to democracy has a distinctly Hegelian flavor.

Dictators like Tunisia's Ben Ali promote their countries' economic development because higher growth each year decreases the odds they will be overthrown. Although some pretend otherwise, most do not see themselves as preparing the ground for democracy after they depart. In the short run, rising incomes allow them to freeze or even curtail political freedoms. But when they do eventually leave office, the level of income that their survival strategy has produced over the years influences whether or not the country jumps to a higher level of democracy.

Worldwide, this self-interested behavior of dictators produces a kind of democratic ratchet. During periodic global or regional recessions, incumbent leaders are turfed out at unusually high rates. When recession comes after years of modernization within autocratic regimes, the extra turnover translates into a wave of democratization. Neither economic development nor leadership change by itself produces democracy, but together they increase the odds of transition. For a while,

the levels of democracy and economic development can get out of synch. But this just creates a gap to be crossed quickly when leadership change again puts constitutional questions on the agenda. As long as countries continue getting richer, better educated, and more modern in other respects, the screw turns in one direction. Recessions may also overturn incumbents in rich democracies, but such turnover usually strengthens rather than undermines such regimes.

This logic can explain the pattern of "two steps forward, one step back," in which waves of democratization are followed by stasis or temporary reversions. In their early years, authoritarian leaders are more apt to risk reforms—and if their country grew richer under the previous dictator, the direction tends to be towards greater political freedom. As they settle into power, leaders become more conservative, while implementation of those reforms already begun bogs down.[43] The drop in public mobilization may also reduce pressure for change. Such waves within individual countries become coordinated when common shocks—global recession, world wars, etc.—prompt simultaneous leader turnover in many places.

The logic also explains why modernization theory often seems at odds with current events, and why breakthroughs to more accountable government frequently come as a surprise. Under Brezhnev, Soviet society grew more educated, urban, and differentiated, without any sign of democratization. In retrospect, we see that this prepared the ground for a significant leap forward under Mikhail Gorbachev. In Indonesia under Suharto, per capita GDP more than tripled. Yet, two

[43] Leaders' increased conservatism might have psychological underpinnings; alternatively, leaders may become objectively more secure over time and therefore see less need for diversionary wars or political concessions. I lack data to adjudicate between these possibilities here. The main point is that time in office rather than the leader's age or education seems to matter most, and that the tendency to conservatism operates alongside the impact of popular pressures.

years before the general was forced out by street protests, his autocratic regime seemed to informed observers more secure than ever (Liddle 1996). In rapidly modernizing autocracies, a stability that observers take for granted can evaporate suddenly when the leader changes.

The perspective of this paper suggests a bridge between two seemingly conflicting approaches to accounting for regime change. One tradition, exemplified by Lipset, emphasizes underlying characteristics of countries such as economic development or political culture. Other scholars, dissatisfied with the failure of such approaches to explain the timing and process of transitions, focus on the strategic interactions of key players (e.g. O'Donnell et al. 1986). The results presented here suggest the need to integrate both levels of analysis—and how to do so. Underlying factors such as economic development have effects that are crucial but not uniform. They are switched off and on by twists in the contingent games of politics, and even by chance events, that determine the timing of leader turnover. Of course, the games of politics are played within a social context, but they are not reducible to it; conversely, social factors predict outcomes well in the long run, but the timing and force of their effects depend on vicissitudes of the games.

Of course, much work remains to test more systematically the many links for which only suggestive evidence could be provided here. And there is no guarantee that past patterns will continue to hold. At the same time, that economic development is associated with democracy does not mean that historical junctures and country-specific factors do not matter. In the estimations, the country and year dummies, which capture such effects, are often large and statistically significant. The enduring challenge is to incorporate the distinctive historical patterns into theoretical accounts that are as general as possible—but not more so.

References

Acemoglu, Daron, and James A. Robinson. 2006. *Economic Origins of Dictatorship and Democracy*, New York: Cambridge University Press.

Acemoglu, Daron, Simon Johnson, James A. Robinson, and PierreYared. 2005. "From Education to Democracy," *American Economic Review Papers and Proceedings* 95, 2: 44-49.

Acemoglu, Daron, Simon Johnson, James A. Robinson, and PierreYared. 2008. "Income and Democracy," *American Economic Review* 98, 3: 808-42.

Acemoglu, Daron, Simon Johnson, James A. Robinson, and PierreYared. 2009. "Revisiting the Modernization Hypothesis," *Journal of Monetary Economics* 56: 1043-58.

Alan, Sule, Bo E. Honoré, and Sören Leth-Petersen. 2008. "Estimation of Panel Data Models with Twosided Censoring," Princeton University: unpublished.

Alemán, José and David Yang. 2011. "A Duration Analysis of Democratic Transitions and Authoritarian Backslides," *Comparative Political Studies* 44, 9: 1123-51.

Banks, Arthur. 2007. *Cross-National Time-Series Data Archive: 1815-2007*, Jerusalem, Israel: Databanks International.

Barro, Robert J. 1999. "Determinants of Democracy," *Journal of Political Economy* 107, 6: 157-83.

Benhabib, Jess, Alejandro Corvalan, and Mark M. Spiegel. 2011. "Reestablishing the Income-Democracy Nexus," Cambridge, MA: NBER, Working Paper 16832.

Besley, Timothy and Marta Reynal-Querol. 2011. "Do Democracies Select More Educated Leaders?" *American Political Science Review* 105, 3: 552-66.

Boix, Carles. 2011. "Democracy, Development, and the International System," *American Political Science Review* 105, 4: 809-28.

Boix, Carles, Michael Miller, and Sebastian Rosato. 2012. "A Complete Data Set of Political Regimes, 1800-2007," forthcoming, *Comparative Political Studies.*

Boix, Carles and Sebastian Rosato. 2001. "A Complete Data Set of Political Regimes, 1800-1999," Department of Political Science, The University of Chicago, Chicago.

Boix, Carles and Susan Stokes. 2003. "Endogenous Democratization," *World Politics* 55, 4: 517-49.

Bollen, Kenneth A. 1998. "Cross-National Indicators of Liberal Democracy, 1950 to 1990," University of North Carolina: manuscript.

Bremer, Stuart A. 1992. "Dangerous Dyads: Conditions Affecting the Likelihood of Interstate War, 1816-1965," *Journal of Conflict Resolution* 36, 3: 309-41.

Brückner, Markus and Antonio Ciccone. 2011. "Rain and the Democratic Window of Opportunity," *Econometrica* 79, 3: 923-47.

Bueno de Mesquita, Bruce, and Alastair Smith. 2009. "Political Survival and Endogenous Institutional Change," *Comparative Political Studies* 42, 2: 167-97.

Bueno de Mesquita, Bruce, and Alastair Smith. 2010. "Leader Survival, Revolutions, and the Nature of Government Finance," *American Journal of Political Science* 54, 4: 936-50.

Chamberlain, G. 1980. "Analysis of covariance with qualitative data," *Review of Economic Studies* 47: 225–238.

Chiozza, Giacomo and Ajin Choi. 2003. "Guess Who DidWhat: Political Leaders and the Management of Territorial Disputes, 1950-1990," *Journal of Conflict Resolution* 47: 251-78.

Easterly, William. 2011. "Benevolent Autocrats," New York University: manuscript.

Epstein, David L., Robert Bates, Jack Goldstone, Ida Kristensen and Sharyn O'Halloran. 2006. "Democratic Transitions," *American Journal of Political Science* 50, July: 551-569.

Gandhi, Jennifer, and Adam Przeworski. 2007. "Authoritarian Institutions and the Survival of Autocrats," *Comparative Political Studies* 40, 11: 1279-1301.

Geddes, Barbara. 1999. "Authoritarian Breakdown: Empirical Test of a Game Theoretic Argument," UCLA: manuscript.

Geddes, Barbara, Joseph Wright, and Erica Frantz. 2012. "Authoritarian Regimes: A New Data Set," UCLA: manuscript.

Gelpi, Christopher, and Joseph M. Grieco. 2001. "Attracting trouble: Democracy, leadership tenure, and the targeting of militarized challenges, 1918-1992," *Journal of Conflict Resolution* 45: 794-817.

Ghosn, Faten, Glenn Palmer, and Stuart Bremer. 2004. "The MID3 Data Set, 1993–2001: Procedures, Coding Rules, and Description," *Conflict Management and Peace Science* 21: 133-154.

Giglio, Mike. 2011. "The Cyberactivists Who Helped Topple a Dictator," *Newsweek.com*, January 15, http://www.newsweek.com/2011/01/15/tunisia-protests-the-facebook-revolution.html.

Glaeser, Edward L., Rafael La Porta, Florencio Lopez-de-Silanes, and Andrei Shleifer. 2004. "Do Institutions Cause Growth?" *Journal of Economic Growth* 9: 271-303.

Glaeser, Edward L., Giacomo A. M. Ponzetto, and Andrei Shleifer. 2007. "Why Does Democracy Need Education?" *Journal of Economic Growth* 12: 77-99.

Gleditsch, Kristian S. 2008. *Modified Polity P4 and P4D Data*, Version 3.0., http://privatewww.essex.ac.uk/~ksg/Polity.html.

Gleditsch, Kristian S. and Jinhee L. Choung. 2004. "Autocratic Transitions and Democratization," University of California, San Diego: manuscript.

Gleditsch, Kristian S. and Michael D. Ward. 2006. " Diffusion and the International Context of Democratization," *International Organization* 60, 4: 911-933.

Goemans, Henk E., Kristian Skrede Gleditsch, and Giacomo Chiozza. 2009a. "Introducing Archigos: A Dataset of Political Leaders," *Journal of Peace Research* 46, 2: 269-83.

Goemans, H.E., Kristian Skrede Gleditsch, and Giacomo Chiozza. 2009b. *Archigos: A Data Set on Leaders 1875-2004*, Version 2.9, University of Rochester, http://www.rochester.edu/college/faculty/hgoemans/data.htm.

Greene, W. 2003. *Econometric Analysis*, New York: Prentice Hall.

Hegel, Georg W.F. 1980 [1837]. *Lectures on the Philosophy of World History*, trans. H.B. Nisbet, New York: Cambridge University Press.

Horowitz, Michael, Rose McDermott and Allan C. Stam. 2005. "Leader Age, Regime Type, and Violent International Relations," *Journal of Conflict Resolution* 49, 5: 661-85.

Hough, Jerry F. 1997. *Democratization and Revolution in the USSR, 1985-1991*, Washington, DC: Brookings Institution Press.

Huntington, Samuel P. 1991. *The Third Wave: Democratization in the Late Twentieth Century*, Norman: University of Oklahoma Press.

Inglehart, Ronald and Christian Welzel. 2005. *Modernization, Cultural Change, and Democracy: The Human Development Sequence*, New York: Cambridge University Press.

Jones, Benjamin F. and Benjamin A. Olken. 2005. "Do Leaders Matter? National Leadership and Growth since World War II," *Quarterly Journal of Economics* 120, 3: 835-864.

Jones, Benjamin F. and Benjamin A. Olken. 2009. "Hit or Miss? The Effect of Assassinations on Institutions and War," *American Economic Journal: Macroeconomics* 1, 2: 55-87.

Kennedy, Ryan. 2010. "The Contradiction of Modernization: A Conditional Model of Endogenous Democratization," *Journal of Politics* 72, 3: 785-98.

Jones, Daniel M., Stuart A. Bremer and J. David Singer. 1996. "Militarized Interstate Disputes, 1816-1992: Rationale, Coding Rules, and Empirical Patterns," *Conflict Management and Peace Science* 15, 2: 163-213.

Lai, Brian and Dan Slater. 2006. "Institutions of the Offensive: Domestic Sources of Dispute Initiation in Authoritarian Regimes, 1950-1992," *American Journal of Political Science* 50, 1: 113-26.

Li, Quan and Rafael Reuveny. 2003. "Economic Globalization and Democracy: An Empirical Analysis," *British Journal of Political Science* 33, 1: 29-54.

Liddle, R. William. 1996. "Indonesia: Suharto's Tightening Grip," *Journal of Democracy* 7, 4: 58-72.

Liddle, R. William. 1999. "Indonesia's Democratic Opening," *Government and Opposition* 34, 1: 94-116.

Lipset, Seymour Martin. 1959. " Some Social Requisites of Democracy: Economic Development and Political Legitimacy," *American Political Science Review* 53, 1: 69-105.

Londregan, John B. and Keith T. Poole. 1990. "Poverty, The Coup Trap, and the Seizure of Executive Power," *World Politics* 42, 2: 151-83.

Londregan, John B. and Keith T. Poole. 1996. "Does High Income Promote Democracy?" *World Politics* 49, 1, October: 1-30.

Lopez-Cordova, J. Ernesto and Christopher M. Meissner. 2008. "The Impact of International Trade on Democracy: A Long-Run Perspective," *World Politics* 60: 539-75.

Maddison, Angus. 2010. *Historical Statistics of the World Economy, 1-2008 AD*, www.ggdc.net/MADDISON/oriindex.htm.

Miller, Michael K. 2010. "Economic Development, Violent Leader Removal, and Democratization," Princeton University: manuscript.

Miller, Ross, 1999. "Regime Type, Strategic Interaction, and the Diversionary Use of Force," *Journal of Conflict Resolution* 43, 3: 388–402.

Morrisson, Christian and Fabrice Murtin. 2009. "The Century of Education," *Journal of Human Capital* 3, 1: 1-42.

O'Donnell, Guillermo A. 1988. *Bureaucratic Authoritarianism: Argentina, 1966-1973, in Comparative Perspective*, Berkeley, CA: University of California Press.

O'Donnell, Guillermo, Philippe Schmitter and Laurence Whitehead, eds. 1986. *Transitions from Authoritarian Rule: Comparative Perspectives*, Baltimore, MD: Johns Hopkins University Press.

Oneal, J.R. and J. Tir. 2006. "Does the Diversionary Use of Force Threaten the Democratic Peace? Assessing the Effect of Economic Growth on Interstate Conflict, 1921–2001," *International Studies Quarterly* 50: 755–779.

Persson, Torsten, and Guido Tabellini. 2009. "Democratic Capital: The Nexus of Political and Economic Change," *American Economic Journal: Macroeconomics* 1, 2: 88–126.

Pope, Devin G., and Maurice E. Schweitzer. 2011. "Is Tiger Woods Loss Averse? Persistent Bias in the Face of Experience, Competition, and High Stakes," *American Economic Review* 101, 1: 129-57.

Przeworski, Adam, Michael Alvarez, José Antonio Cheibub, and Fernando Limongi. 2000. *Democracy and Development: Political Institutions and Well-Being in the World, 1950-1990*, New York: Cambridge University Press.

Robinson, James A. 2001. "When is a State Predatory," Harvard University: manuscript.

Russett, Bruce, John R. Oneal, and Michael L. Berbaum. 2003. "*Causes* of Peace: Democracy, Interdependence and International Organizations, 1885-1992," *International Studies Quarterly* 47: 371-93.

Singer, J. David, Stuart Bremer, and John Stuckey. 1972. "Capability Distribution, Uncertainty, and Major Power War, 1820-1965," in Bruce Russett, ed., *Peace, War, and Numbers*, Beverly Hills: Sage: 19-48.

Truett, K.R. 1993. "Age Differences in Conservatism," *Personality and Individual Differences* 14: 405-411.

World Bank. 2001. *World Development Indicators*. Washington, DC: World Bank.

Wright, Joseph. 2011. "Curses and Conditionality: Do Oil and Aid Affect Democracy Differently?" Penn State University: manuscript.

Wright, Joseph and Abel Escribà-Folch. 2012. "Authoritarian Institutions and Regime Survival: Transitions to Democracy and Subsequent Autocracy," *British Journal of Political Science*, forthcoming.

Appendix

Table A1: Income and democracy, with interpolations to fill gaps in Maddison income data

Level of democracy (Polity measure)

	(A) 1960-2000 all countries					*(B) 1820-2008 all countries*					*(C) 1820-2008, Polity2 $_{t-1}$ < 6*				
Type of panel:	*1-yr*	*5-yr*	*10-yr*	*15-yr*	*20-yr*	*1-yr*	*5-yr*	*10-yr*	*15-yr*	*20-yr*	*1-yr*	*5-yr*	*10-yr*	*15-yr*	*20-yr*
Democracy $_{t-1}$	.87***	.45***	.15*	-.01	-.18**	.92***	.65***	.36***	.27***	.05	.91***	.60***	.27***	.26**	.08
	(.01)	(.05)	(.08)	(.10)	(.09)	(.01)	(.03)	(.05)	(.06)	(.07)	(.01)	(.06)	(.09)	(.11)	(.10)
Ln GDP per Capita $_{t-1}$	-.005	.007	.022	.022	-.001	.001	.02	.08**	.10**	.17**	.002	.03	.14***	16***	.24**
	(.007)	(.029)	(.051)	(.076)	(.111)	(.003)	(.02)	(.03)	(.05)	(.07)	(.004)	(.02)	(.04)	(.05)	(.09)
Implied cumulative effect of income	-.04	.01	.03	.02	-.00	.02	.06	.13**	.14**	.18**	.03	.08	.18***	.21***	.26***
Fisher p level	[.00]	[.00]	[.00]	[.02]	[.84]	[.00]	[.00]	[.00]	[.00]	[.00]	[.00]	[.00]	[.00]	[.00]	[.00]
Observations	5,377	1,103	562	330	278	11,996	2,219	993	627	454	8,165	1,561	722	469	336
Countries	160	159	137	135	131	164	160	138	136	132	142	138	124	123	117
R-squared	.9453	.8215	.7758	.8044	.8097	.9547	.8167	.7306	.7299	.7249	.8703	.6198	.5631	.6047	.6686

Transitions to democracy

	(D) 1820-2008 just upward movements, Polity2 $_{t-1}$ < 6					*(E) 1960-2000: dichotomous Boix Rosato Just non-democracies*					*(F) 1820-2000 dichotomous Boix Rosato Just non-democracies*				
Type of panel:	*1-yr*	*5-yr*	*10-yr*	*15-yr*	*20-yr*	*1-yr*	*5-yr*	*10-yr*	*15-yr*	*20-yr*	*1-yr*	*5-yr*	*10-yr*	*15-yr*	*20-yr*
Democracy $_{t-1}$	.98***	.82***	.61***	.65***	.47***										
	(.01)	(.04)	(.06)	(.07)	(.10)										
Ln GDP per Capita $_{t-1}$	-.00	.03	.11***	.12***	.15**	.01	.07	.11	.06	.01	.01	.07**	.21***	.24***	.31***
	(.00)	(.02)	(.03)	(.04)	(.08)	(.01)	(.05)	(.08)	(.14)	(.22)	(.01)	(.03)	(.05)	(.07)	(.12)
Implied cumulative effect of income	-.01	.14	.28***	.34**	.29**										
Fisher p level	[.00]	[.00]	[.00]	[.00]	[.00]	[.00]	[.00]	[.00]	[.44]	[.00]	[.00]	[.00]	[.00]	[.01]	[.01]
Observations	8,165	1,561	722	469	336	3,545	733	376	227	193	7,284	1,435	699	449	328
Countries	142	138	124	123	117	126	125	114	111	111	141	137	126	124	118
R-squared	.9115	.7299	.6813	.7001	.7337	.1027	.3320	.5122	.6650	.7721	.0877	.2321	.4209	.5282	.6211

Sources: see Table A16.

Note: This table exactly replicates Table 1, but using a version of the Maddison data on GDP per capita in which gaps have been filled by linear interpolation. All regressions estimated by OLS including country and year fixed effects. Robust standard errors, clustered by country, in parentheses; * p<.10, ** p<.05, *** p<.01. Implied cumulative effect of income: coefficient on Ln GDP per Capita $_{t-1}$/(1 - coefficient on Democracy $_{t-1}$). "Fisher p level" is probability level at which one can reject H0: residuals are I(1), from Fisher test of residuals.

Table A2: Income and democracy, robustness checks and extensions

	(A) 1820-2008, Polity2 $_{t-1}$ < 10					(B) 1820-2008: Polity2 $_{t-1}$ >- 6: just downward movements					(C)1820-2008, Polity2				
	OLS, country and year fixed effects					OLS, country and year fixed effects					Honoré Two Side Estimator				
Type of panel:	*1-yr*	*5-yr*	*10-yr*	*15-yr*	*20-yr*	*1-yr*	*5-yr*	*10-yr*	*15-yr*	*20-yr*	*1-yr*	*5-yr*	*10-yr*	*15-yr*	*20-yr*
Democracy $_{t-1}$	.90***	.54***	.21***	.09	-.11	.97***	.88***	.85***	.84***	.72***	.97***	.79***	.30***	.17*	-.06
	(.01)	(.04)	(.07)	(.08)	(.09)	(.01)	(.03)	(.06)	(.06)	(.09)	(.00)	(.03)	(.08)	(.09)	(.09)
Ln GDP per Capita $_{t-1}$	.001	.027	.12**	.15**	.24**	.001	.002	.05*	.09*	.26***	.016***	.10***	.21***	.26***	.40***
	(.005)	(.024)	(.05)	(.07)	(.10)	(.004)	(.014)	(.03)	(.05)	(.08)	(.002)	(.01)	(.05)	(.06)	(.10)
Implied cumulative effect of income	.01	.06	.15**	.16**	.22***	.04	.02	.35	.55	.96**					
Observations	8,349	1,584	733	441	327	6,735	1,219	535	307	229	10,305	1,933	884	531	391
Countries	159	154	134	132	128	151	144	104	83	79	164	160	134	121	100
Fisher p level	[.00]	[.00]	[.00]	[.12]	[.15]	[.00]	[.00]	[.00]	[.00]	[.00]					
R-squared	.9229	.7245	.6510	.6625	.7011	.9445	.8627	.8479	.8772	.8725					
Pct. censored below											19	19	19	21	21
Pct. censored above											3	3	4	3	3

	(D) 1820-2008, Polity2 $_{t-1}$ <6				(E) 1960-2000, Dichotomous Boix-Rosato measure					(F) 1820-2000, Dichotomous Boix-Rosato measure				
	Arellano-Bond GMM				Fixed effects conditional logit					Fixed effects conditional logit				
Period of panel:	*5-yr*	*10-yr*	*15-yr*	*20-yr*	*1-yr*	*5-yr*	*10-yr*	*15-yr*	*20-yr*	*1-yr*	*5-yr*	*10-yr*	*15-yr*	*20-yr*
Democracy $_{t-1}$	.14*	-.16**	-.55**	-.55**										
	(.08)	(.10)	(.16)	(.24)										
Democracy $_{t-2}$			.31	-.22										
			(.26)	(.19)										
Ln GDP per Capita $_{t-1}$	-.31**	.56***	-.18	.75**	1.06	3.18***	4.21**	5.76***	7.53**	.95*	2.24***	3.91***	3.32	6.11
	(.14)	(.17)	(.25)	(.31)	(.85)	(1.20)	(1.81)	(1.78)	(3.01)	(.54)	(.74)	(1.32)	(2.18)	(4.50)
Implied cum. effect of inc.	-.36**	.41	-.15	.42**										
AR(2) test	[.74]	[.75]	[.10]	[.08]										
Hansen J-test	[.93]	[.65]	[.39]	[.85]										
Fisher p level														
Observations	1,103	463	187	113	1,357	286	143	74	59	3,358	702	356	210	152
Countries	133	118	66	49	54	49	39	31	26	68	65	58	50	46
R-squared														

Sources: see Table A16.

Note: standard errors in parentheses; * p<.10, ** p<.05, *** p<.01. Panels A, B, D: robust standard errors, clustered by country in A and B. All regressions except first two columns of Panel C and last two of Panel E include year dummies (in other cases, convergence not achieved with year fixed effects). Implied cumulative effect of income, all except last two columns of Panel D: coefficient on Ln GDP per Capita $_{t-1}$/(1 - coefficient on Democracy $_{t-1}$); cumulative effect of income, last two columns of Panel D: coefficient on Ln GDP per Capita $_{t-1}$/(1 - coefficient on Democracy $_{t-1}$ - coefficient on Democracy $_{t-2}$). Arellano-Bond regressions: Democracy and Ln GDP per Capita instrumented with second lags (5- and 10-yr regressions) and Democracy by 3rd lag and Ln GDP per capita by 2nd lag (15- and 20-yr regression); in 15-yr and 20-yr panels, second lag of Democracy included to reduce autocorrelation. "Fisher p level" is probability level at which one can reject H0: residuals are I(1), from Fisher test of residuals.

Table A3: Income, education, democracy, and leadership change, with interpolations to fill gaps in Maddison income data

	Level of Democracy					*Transitions to Democracy*									
	(A) 1875-2004: Polity, Polity2 $_{t-1}$ < 6					*(B) 1875-2000: BR binary measure, only non-democracies*					*C) 1875-2004: Polity, Polity2 $_{t-1}$ < 6: just upward movements*				
Type of panel:	*1-yr*	*5-yr*	*10-yr*	*15-yr*	*20-yr*	*1-yr*	*5-yr*	*10-yr*	*15-yr*	*20-yr*	*1-yr*	*5-yr*	*10-yr*	*15-yr*	*20-yr*
Democracy $_{t-1}$	.90***	.50***	.13	.15	-.03						.98***	.74***	.51***	.61***	.37***
	(.01)	(.06)	(.09)	(.13)	(.10)						(.01)	(.04)	(.06)	(.07)	(.10)
Leader replaced in previous period	-.07**	-.21*	-.42*	-.84***	-1.11**	-.15*	-.54**	-.58*	-1.46***	-2.24***	-.06*	-.15	-.36	-.82***	-1.05**
	(.04)	(.12)	(.25)	(.32)	(.48)	(.08)	(.21)	(.33)	(.47)	(.56)	(.04)	(.12)	(.24)	(.29)	(.48)
Ln GDP per Capita $_{t-1}$	-.001	-.01	.06	.00	.07	.00	.01	.11*	-.00	-.01	-.004	-.01	.04	-.03	-.01
	(.005)	(.02)	(.05)	(.06)	(.10)	(.01)	(.03)	(.06)	(.08)	(.13)	(.004)	(.02)	(.04)	(.05)	(.08)
Ln GDP per Capita $_{t-1}$* leader replaced	.011**	.04**	.07**	.14***	.18***	.02**	.08***	.09*	.21***	.32***	.010**	.03*	.06*	.13***	.17***
	(.005)	(.02)	(.03)	(.04)	(.07)	(.01)	(.03)	(.05)	(.06)	(.07)	(.005)	(.02)	(.03)	(.04)	(.06)
Implied cumulative effect of income															
if leader replaced	.10	.06	.16***	.17**	.25***	.03*	.09**	.21***	.21***	.32**	.27	.10	.22**	.27**	.26**
if leader not replaced	-.01	-.02	.07	.00	.07	.00	.01	.11*	-.00	-.01	-.14	-.03	.09	-.07	-.01
Fisher p level	[.00]	[.00]	[.00]	[.00]	[.00]	[.00]	[.00]	[.00]	[.00]	[.00]	[.00]	[.00]	[.00]	[.00]	[.00]
Observations	6,667	1,329	615	406	293	6,103	1,214	597	391	288	6,667	1,329	615	406	293
Countries	136	133	119	120	113	137	133	122	121	115	136	133	119	120	113
R-squared	.8580	.6257	.5923	.6520	.7241	.0982	.2738	.4546	.5658	.6606	.9040	.7372	.7034	.7397	.7771

Sources: see Table A16.

Note: This table replicates Table 2, but using a version of the Maddison data on GDP per capita in which gaps have been filled by linear interpolation. All estimations by OLS with country and year fixed effects. Robust standard errors, clustered by country, in parentheses; * p<.10, ** p<.05, *** p<.01. "Fisher p level" is probability level at which one can reject H0: residuals are I(1), from Fisher test of residuals.

Table A4: Income, education, leadership change, and democracy—without the lagged dependent variable

	----------*Level of Democracy*----------					
	(A) 1875-2004: Polity, Polity2 $_{t-1}$ < 6					
	(1)	(2)	(3)	(4)	(5)	(6)
Type of panel:	*1-yr*	*5-yr*	*10-yr*	*15-yr*	*20-yr*	*10-yr*
Leader replaced in previous period	.04 (.08)	-.13 (.16)	-.34 (.28)	-.78* (.41)	-.75 (.81)	.11** (.04)
Ln GDP per Capita $_{t-1}$	.00 (.03)	-.02 (.03)	.07 (.05)	-.00 (.07)	.09 (.14)	
Ln GDP per Capita $_{t-1}$* leader replaced	.004 (.011)	.04 (.02)	.06* (.04)	.13** (.06)	.14 (.10)	
Average yrs of schooling (age 15 and over) $_{t-1}$						.015 (.034)
Average yrs schooling * leader replaced $_{t-1}$						.041*** (.013)
Implied cumulative effect of income						
if leader replaced	.004	.02	.14***	.13*	.23**	
if leader not replaced	.000	-.02	.07	-.00	.09	
Implied cumulative effect of schooling						
if leader replaced						.06*
if leader not replaced						.02
Fisher p level	[.00]	[.00]	[.00]	[.00]	[.95]	[.00]
Observations	5,815	1,173	551	342	246	416
Countries	136	133	119	120	113	65
R-squared	.5155	.5610	.6100	.6709	.7588	.5589

Sources: see Table A16.

Note: All estimations by OLS with country and year fixed effects. Robust standard errors, clustered by country, in parentheses; * p<.10, ** p<.05, *** p<.01. "Fisher p level" is probability level at which one can reject H0: residuals are I(1), from Fisher test of residuals.

Table A5: Political change under "developmental dictators" and their successors

Country	Leader	Year in	Year out	Change in GDP per capita (times)	Change in Polity2 score under dictator	Change in Polity2 score after dictator
Libya	Idris	1951	1969	9.78	0	0
Singapore	Lee Kuan Yew	1959	1990	6.50	-9	0
Yugoslavia	Tito	1945	1980	4.53	0	0 [b]
Spain	Franco	1939	1975	4.36	0	+17
Taiwan	Chiang Kai-shek	1950	1975	3.85	0	+1
Venezuela	Gomez	1908	1935	3.78	-6	+6
S. Korea	Park Chung-hee	1961	1979	3.44	-1	+14
Indonesia	Suharto	1966	1998	3.29	-1	+15
Iran	Mohammad Reza	1953	1979	3.03	-6	+4
Portugal	Salazar	1932	1968	2.97	0	+18
Bulgaria	Zhivkov	1956	1989	2.92	0	+15
China	Deng Xiaoping	1980	1997	2.84	0	0
Romania	Georghiu-Dej	1947	1965	2.78	0	0
Tunisia	Bourguiba	1957	1987	2.73	+1 [a]	+5
Mongolia	Tsedenbal	1952	1984	2.70	0	+16

Sources: see Table A16.

Note: Table includes all leaders out of power by 2004 during whose tenure the average Polity2 score was less than 6 and GDP per capita increased by at least 150 percent. "Change in Polity2 score under dictator": on 21-point scale, from leader's entry year to his last full year in office. "Change in Polity2 score after dictator": on 21-point scale, from last full year in office to 10 years later. [a] from 1959 (first year in data). When unclear, I verified that the changes in the Polity2 scores did not precede the change of leader, using Gleditch's (2008) dating of the Polity changes. [b] the new leader started *after* the date of a 2-point increase in the Polity2 score; I record here the change in Polity2 in the first 10 years *under* the new leader.

Table A6: Descriptive statistics on leader turnover

	Authoritarian states (Polity2 < 6)	*Democracies (Polity2 > 5)*
Percent with leader turnover in given year		
By period		
1850-1900	16	25
1901-1950	21	37
1951-2000	13	26
By GDP per capita, 1990 $		
0-3,000	15	28
3,001-6,000	14	33
6,001-10,000	14	26
> 10,000	8	26
Percent of authoritarian states that had higher Polity2 score		
-1 year after leader turnover	11	
-5 years after leader turnover	24	
-10 years after leader turnover	36	

Sources: See Table A16.

Table A7: Effect of changing starting year in panel on estimated effect of income conditional on leader turnover

	5-Year Panel									
Panel of years ending in:	*0 or 5*	*1 or 6*	*2 or 7*	*3 or 8*	*4 or 9*					
Coefficient on Ln GDP per Capita $_{t-1}$ * leader replaced	.04**	.05***	.04**	.02	.03*					
	(.02)	(.02)	(.02)	(.02)	(.02)					
Cumulative impact of income										
-when leader replaced	.06	.09*	.08*	.06	.09*					
-when leader not replaced	-.02	-.01	.01	.02	.03					
	10-Year Panel									
Panel of years ending in:	*0*	*1*	*2*	*3*	*4*	*5*	*6*	*7*	*8*	*9*
Coefficient on Ln GDP per Capita $_{t-1}$ * leader replaced	.07**	.09***	.09***	.09***	.10***	.07**	.06*	.06*	.04	.02
	(.03)	(.02)	(.03)	(.03)	(.03)	(.03)	(.03)	(.03)	(.03)	(.03)
Cumulative impact of income										
-when leader replaced	.16***	.15***	.12***	.12**	.10*	.06	.04	.04	.04	.09
-when leader not replaced	.07	.05	.02	.01	-.03	-.05	-.05	-.03	-.01	.07

Sources: see Table A16.

Notes: Robust standard errors, clustered by country, in parentheses; * p<.10, ** p<.05, *** p<.01. Estimates from regressions identical to those in Table 2 column 2 (5-Year Panel) and column 3 (10-Year Panel). I use the version of the Maddison income data with linear interpolation to fill gaps, since otherwise starting on years not divisible by 10 results in significant loss of data.

Table A8: Additional Robustness checks, one-year panel

Dependent variable:	*Polity2 Level of Democracy, Polity2 $_{t-1}$ < 6, 1-yr panels*	
	(1)	(2)
Democracy $_{t-1}$	.90***	.89***
	(.01)	(.01)
Leader replaced in previous period	-.07*	-.06
	(.04)	(.04)
Ln GDP per Capita $_{t-1}$	-.003	-.001
	(.006)	(.006)
Ln GDP per Capita $_{t-1}$ * leader replaced $_{t-1}$	.010*	.010*
	(.005)	(.005)
Percent of previous leader changes irregular	-.003	
	(.011)	
Interstate war in progress $_{t-1}$		-.005
		(.009)
Won interstate war $_{t-1}$		.002
		(.013)
Lost interstate war $_{t-1}$		.044
		(.032)
Civil war in progress $_{t-1}$		.033***
		(.009)
Government won civil war $_{t-1}$		-.021
		(.015)
Government lost civil war $_{t-1}$		-.051*
		(.028)
Implied cumulative effect of income		
-if leader replaced	.07	.08
-if leader not replaced	-.02	-.01
Fisher p level	[.00]	[.00]
Observations	5,815	5,815
Countries	136	136
R-squared	.8526	.8533

Sources: see Table A16.

Note: All estimations by OLS with country and year fixed effects. Robust standard errors, clustered by country, in parentheses; * p<.10, ** p<.05, *** p<.01. "Fisher p level" is probability level at which one can reject H0: residuals are I(1), from Fisher test of residuals.

Table A9: Robustness checks, 10-year panel

1875-2004: Level of Democracy, Polity2 $_{t-1}$ < 6									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Democracy $_{t-1}$	.11	.11	.09	-.05	.04	.13	.08	.12	.14
	(.10)	(.10)	(.10)	(.11)	(.12)	(.09)	(.14)	(.10)	(.10)
Leader replaced in previous period	-.36	-.31	-.23	.02	-.15	-.19	-.36	-.37	-.37
	(.28)	(.29)	(.31)	(.31)	(.32)	(.28)	(.36)	(.29)	(.28)
Ln GDP per Capita $_{t-1}$	.07	.08	.09	.13*	.08	.08	.03	.08	.06
	(.05)	(.05)	(.06)	(.08)	(.07)	(.05)	(.08)	(.05)	(.05)
Ln GDP per Capita $_{t-1}$ * leader replaced $_{t-1}$	.07*	.06	.05	.02	.04	.04	.06	.07*	.07*
	(.04)	(.04)	(.04)	(.04)	(.04)	(.04)	(.05)	(.04)	(.04)
Foreign democratic capital $_{t-1}$		.81							
		(.86)							
Trade/GDP			-.01						
			(.05)						
Log income from oil and gas				-.02					
				(.02)					
Elected legislature					-.03				
					(.04)				
Non-regime parties					.01				
					(.04)				
Military regime $_{t-1}$						.03			
						(.05)			
Monarchy $_{t-1}$						.10*	.09		
						(.06)	(.10)		
One-party regime $_{t-1}$							-.07		
							(.08)		
Miscellaneous regime $_{t-1}$							-.03		
							(.11)		
Personalistic regime $_{t-1}$							.04		
							(.11)		
Domestic democratic capital $_{t-1}$								-.08	
								(.10)	
Previous transitions									-.40**
									(.18)
Implied cumulative effect of income									
-if leader replaced	.15**	.16***	.15**	.14*	.11	.14**	.10	.16**	.15**
-if leader not replaced	.08	.10	.10	.13*	.08	.09	.03	.09	.07
Fisher p level	[.00]	[.00]	[.00]	[.00]	[.74]	[.00]	[.00]	[.00]	[.00]
Observations	551	541	440	358	372	500	353	541	551
Countries	119	116	111	109	106	114	103	116	119
R-squared	.6127	.6145	.6229	.6966	.6662	.6282	.6724	.6139	.6246

Table A9: Robustness checks, 10-year panel (cont.)

1875-2004: Level of Democracy, Polity2 $_{t-1}$ < 6			
	(10)	(11)	(12)
Democracy $_{t-1}$	.04	.09	.11
	(.11)	(.10)	(.10)
Leader replaced in	-.51	-.35	-.38
previous period	(.33)	(.28)	(.28)
Ln GDP per Capita $_{t-1}$	.11	.04	.06
	(.08)	(.05)	(.05)
Ln GDP per Capita $_{t-1}$ *	.09*	.06*	.07*
leader replaced $_{t-1}$	(.05)	(.04)	(.04)
Previous rate of leader	.25		
turnover	(.88)		
Ln GDP per Capita $_{t-1}$ *	-.03		
previous rate of turnover	(.12)		
Percent of previous		-.24**	
leader changes irregular		(.10)	
Interstate war in			.09
progress $_{t-1}$			(.07)
Won interstate war $_{t-1}$			.06
			(.07)
Lost interstate war $_{t-1}$			.14
			(.10)
Civil war in progress $_{t-1}$			.01
			(.06)
Government won civil war $_{t-1}$			-.01
			(.08)
Government lost civil war $_{t-1}$			.08
			(.13)
Implied cumulative			
effect of income			
-if leader replaced	.20***	.12**	.15**
-if leader not replaced	.11	.05	.07
Fisher p level	[.00]	[.00]	[.00]
Observations	433	534	551
Countries	111	118	119
R-squared	.6447	.6213	.6184

Sources: see Table A16.

Note: All estimations by OLS with country and year fixed effects. Robust standard errors, clustered by country, in parentheses; * p<.10, ** p<.05, *** p<.01. "Fisher p level" is probability level at which one can reject H0: residuals are I(1), from Fisher test of residuals.

Economic growth and leader survival

Table A10 regresses a dummy for leader turnover on the economic growth rate and a variety of controls. What other factors might affect the odds of turnover? The nature of the regime and its formal procedures for selecting top officials are obviously relevant (Bueno de Mesquita et al. 2003). In democracies—especially those with short term limits—leaders are likely to change more often than in autocracies. Among authoritarian regimes, turnover may be greater in some types than in others (Geddes 1999). Dynastic monarchies aim to limit change to the aftermath of a ruler's natural death. In military states, generals may rotate in and out of political posts. Autocracies that use institutions such as elected legislatures to coopt opposition may achieve greater stability (Gandhi and Przeworski 2007). On the other hand, it might be only regimes that already feel threatened that resort to such strategies.

Characteristics of individual leaders may also affect their tenure. Older rulers may be more vulnerable to challenges, although those with greater experience may handle threats more adeptly (Londregan and Poole 1996, Bienen and van de Walle 1991). The passage of time may help incumbents to secure themselves, but discontent may also cumulate, rendering the effect of time unclear (Londregan and Poole 1996). Wars are bound to matter (Bueno de Mesquita and Siverson 1995, Chiozza and Goemans 2004). During a civil war, rulers are more likely to be overthrown. Almost by definition, a ruler who loses a civil war is likely to fall, and one who wins is more likely to survive. The implications of external war are less obvious. They may cause citizens to rally behind their commander-in-chief, but they may also destabilize the incumbent. Victory should improve the leader's prospects, while defeat may prompt externally imposed or internally generated change. Finally, stability or instability may spread across borders: the fall of one country's ruler may encourage regime opponents in others, producing regional waves of turnover.

Table A10: Why leaders leave office

Data format:	*country/year*		*country/year*		*country/year*		*leader/year*	
Method:	*OLS, country and year fixed effects*		*IV, country and year fixed effects*		*Fixed effects conditional logit, year dummies*		*Weibull hazard model*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Ln GDP per Capita $_{t-1}$	-.01	.01	-.02	-.02	-.08	.03	-.09*	-.00
	(.02)	(.02)	(.02)	(.03)	(.12)	(.14)	(.05)	(.06)
GDP per Capita Growth Rate	-.34***	-.38***	-1.48**	-1.40**	-5.08***	-3.73***	-4.54***	-2.56***
	(.10)	(.09)	(.63)	(.70)	(1.00)	(.74)	(.67)	(.59)
Democracy $_{t-1}$								
Rescaled Polity2 score	.20***		.17***		1.44***		.76***	
Dummy for Polity2>5	(.03)	.07*	(.04)	.06	(.15)	.37**	(.19)	-.07
		(.04)		(.04)		(.16)		(.20)
Democracy $_{t-1}$ * Growth Rate	-.10	-.15	.80	.003	3.03*	.27	3.25***	.67
	(.24)	(.24)	(.72)	(.006)	(1.56)	(1.32)	(1.11)	(.91)
Proportion of other countries		-.02		-.00		-.31		1.44***
in region that replaced their leaders		(.06)		(.05)		(.35)		(.26)
Leader's age		.004***		.004***		.026***		.012***
		(.001)		(.001)		(.004)		(.004)
Previous times in office		-.01		-.01		-.07		.15***
		(.01)		(.01)		(.06)		(.04)
Leader's years in office this time		-.003***		-.003***		-.026***		-.028***
		(.001)		(.001)		(.007)		(.009)
Monarchy $_{t-1}$		.01		.01		.01		-.12
		(.04)		(.04)		(.17)		(.15)
Military regime $_{t-1}$		.06*		.05*		.41**		.20
		(.03)		(.03)		(.20)		(.17)
Authoritarian regime with		.00		.00		-.08		-.10
elected parliament $_{t-1}$		(.02)		(.02)		(.14)		(.13)
Civil war in progress		.08***		.06***		.56***		.28**
		(.02)		(.02)		(.17)		(.13)
Government won civil war		-.09***		-.11***		-.79**		-.49

this year or last year		(.03)		(.04)		(.39)		(.33)
Government lost civil war		.27***		.26***		1.36***		.57***
this year or last year		(.06)		(.07)		(.38)		(.15)
Interstate war in progress		-.00		-.04		-.04		.11
		(.02)		(.03)		(.20)		(.18)
Country won interstate war		-.08*		-.06		-.59**		-.16
this year or last year		(.04)		(.04)		(.30)		(.24)
Country lost interstate war		.09**		.05		.66**		.31
this year or last year		(.05)		(.05)		(.32)		(.21)
Constant							-.67*	-1.80***
							(.38)	(.46)
Ancillary parameter (ln(p))								
Democracy (Polity2 > 5)							.17***	.29***
							(.05)	(.07)
Constant							-.39***	-.38***
							(.04)	(.04)
Stock Yogo test			p = .10	p = .10				
Cragg-Donald statistic			84.0	56.14				
Fisher p level	[.00]	[.00]	[.00]	[.00]				
Observations	8,929	7,781	7,449	6,862	8,539	7,403	10,757	9,444
Countries	158	156	145	145	143	139		
Leaders							2,329	2,117
R-squared	.1825	.1912	.0026	.0043				

Sources: see Table A16.

Note: Robust standard errors, clustered by country, in parentheses; * p<.10, ** p<.05, *** p<.01. All data are annual. "Fisher p level" is probability level at which one can reject H0: residuals are I(1), from Fisher test of residuals. Models 3 and 4: growth instrumented with trade-weighted growth in other countries. Here (but nowhere else in the paper!) the dependent variable is leader exit excluding exit due to death from natural causes, suicide, or retirement due to poor health, because such forms of exit are not likely to be influenced by economic growth.

All these factors have been examined in previous work so I control for them here. But my key hypothesis is that economic growth increases a leader's odds of survival. I also look to see if the level of economic development has a direct effect. And, motivated by earlier work, I check whether growth affects turnover differently in democracies and non-democracies (Bueno de Mesquita et al. 2003).

Scholars have used various statistical methods to analyze leadership change. I show results with four alternative models. The dependent variable in each is a dummy that equals 1 if the leader is replaced and 0 otherwise.[44] First, to control for country and year in a way that parallels the previous analysis, I estimate the relationship by OLS with country and year fixed effects. Second, to better accommodate the non-linear nature of the dependent variable, I use a conditional logit fixed effects model, including year dummies. (Since almost no countries completely avoid leader change or change leaders every year, there are very few cases that are automatically dropped because the dependent variable is constant, and the problem of bias that arises in the case of democracy regressions does not arise.) I run both of these models on country-year data. Some papers have analyzed leader-year data with hazard models (e.g., Chiozza and Goemans 2004). These have a number of attractive features. For instance, besides gauging the impact of independent variables, one can calculate a hazard rate at which leaders are replaced on average, other things equal. As in Bueno de Mesquita and Smith (2010), I fit a Weibull hazard model, which allows the hazard rate to change over time; how it changes depends on

[44] Here, but nowhere else in the paper, I code as 0 cases in which the leader died in office of natural causes, committed suicide, or retired because of ill health as I wish to explain removal through social action; of course, suicide and ill health might at times be prompted by the stress of leadership in the face of low growth, but they will generally be exogenous to such processes. In fact, this makes little difference: results for growth are very similar and still significant if one includes all leader changes.

an "ancillary parameter," *p*, which is estimated from the data. I model this parameter as a function of whether the country is a democracy (Polity2 greater than 5).[45]

One concern is that regressions of leader replacement on economic growth might pick up the opposite causal process: more leadership change might, by creating uncertainty for investors, inhibit growth. To address this, I estimate a model instrumenting for the growth rate with the average growth rate in *other* countries, weighted by their trade shares with the given country in the previous year:

$$\hat{g}_{at} = \sum_{b \neq a} \omega_{abt-1} I_{bt} g_{bt} \Big/ \sum_{b \neq a} \omega_{abt-1} I_{bt} \qquad \text{(A1)}$$

where g_{bt} is the growth rate of GDP per capita in country *b* in *t*; I_{bt} is an indicator that equals one if the dataset includes data on growth in country *b* in period *t*, 0 otherwise; and $\omega_{abt-1} = X_{abt-1} / Y_{at-1}$, where X_{abt-1} is trade between *a* and *b* in *t-1*, and Y_{at-1} is country *a*'s GDP in *t-1*. The trade data come from Russett, Oneal, and Berbaum (2003); since these data end in 1992, I use the trade weights from 1992 for the years 1993-2008.[46]

To satisfy the exclusion restriction, this instrument should be unrelated to leader turnover by any path other than via growth.[47] It is possible that economic performance in other countries affects the

[45] Bueno de Mesquita and Smith (2010) model this as a function of what they call "coalition size". The Weibull function can be written: $h(t) = p \exp(X\beta) t^{p-1}$, where $h(t)$ is the hazard at time *t*, p is the ancillary "shape" parameter, X is a vector of explanatory factors, and β is a vector of their coefficients.

[46] This instrument is similar to one AJRY (2008) use for per capita income. I tried to instrument for income using an instrument corresponding to theirs (and one other instrument used in Boix (2011)), but in the dataset used here the instruments were too weakly correlated with income to serve adequately, particularly in regressions including interactions with leader turnover.

[47] With just one instrument, one cannot test this.

incidence of war, which, if it involves the given country, could influence leader change there. Fortunately, I already control for interstate war. If economic conditions in other countries cause *them* to replace their leaders at higher rates, the demonstration effect might conceivably influence leader turnover in the given country. Again, I control already for the proportion of other countries that replaced their leaders. I use the test devised by Stock and Yogo (2005) to check that the instrument is not weak (i.e. that it correlates highly with the variable instrumented). This test consists of comparing the Cragg-Donald statistic to a set of critical values. In both models 3 and 4, we can reject the hypothesis of weak instruments with high confidence.

The main finding in Table A10 is that, as hypothesized, economic growth is a highly significant determinant of the turnover of leaders. Where growth is higher, leaders are less likely to be replaced. The coefficients cannot be compared directly across different methods of estimation, but growth is statistically significant in all. In columns 5 and 7, the interaction of growth with democracy is also significant and positive, implying that the effect of growth on leader survival is greater in non-democracies than in democracies. (However, the interaction is not significant in the models with multiple controls.)[48] The estimates from the Weibull models in columns 7-8 imply that each additional

[48] Bueno de Mesquita et al. (2003) similarly find that economic growth has a greater effect in non-democracies (small-coalition systems); however, they argue that the incentive to pursue growth will still be stronger in democracies because they have a much higher baseline hazard rate (from estimates of the ancillary parameter). My aim here is not to compare the motivation to promote growth under democracy and autocracy but just to show that for dictators securing a higher growth rate is an effective way to reduce the odds of being deposed.

percentage point of growth reduces the hazard rate by about 3-5 percentage points for leaders of non-democracies, and by about 1-2 percentage points for leaders of democracies.[49]

Columns 3 and 4 suggest that the impact of growth on leader survival may, indeed, be causal. No instrument is perfect. Still, the results in columns 3 and 4 increase confidence that lower growth causes more frequent leader replacement, and the estimated effect when instrumented is considerably larger than that in columns 1 and 2. Leadership turnover does not appear to be related to the level of income. With controls included—and generally even without—the coefficient on log GDP per capita is close to zero and statistically insignificant.

As expected, regime type also matters. Consistent with previous work, leaders are replaced more often in democracies (Londregan and Poole 1996, Bueno de Mesquita and Smith 2010). I use the rescaled Polity2 index in the basic regressions, but a dummy for Polity2 > 5 in those that control for types of authoritarian regime, since these controls overlap with what Polity2 is supposed to measure. Besides the coefficients on the democracy terms, estimates of the ancillary parameter suggest the shape of the hazard function differs for democracies and non-democracies: the odds of losing office fall over time in both, but much faster in non-democracies. For example, using model 8, in a non-democracy the

[49] It could be that growth over a longer period is even more important than growth in the previous year. I explored this, alternately replacing the one-year growth rate with the two-year and five-year growth rates in models 1 and 2, as well as a measure of the average growth rate in the incumbent leader's term in office so far (not shown here, results available upon request). The alternative specifications were often also significant, but the size of the effect tended to decrease (along with significance) as the period of growth lengthened. This is consistent with views of retrospective accountability that assume that voters—or other social actors—respond more to recent economic performance than to performance from earlier years.

leader's hazard rate after five years is less than one third of his hazard rate after one month; in a democracy, the rate after five years is still more than two-thirds of the one-month rate.[50]

Among the other controls, those related to war were most significant. A leader fighting a civil war—or who had just lost one—was more likely to be deposed.[51] Victory in a civil war was associated with lower odds. The results for interstate war were less clear, but winning one may have increased—and losing decreased—the leader's tenure. Older leaders were more likely to be replaced, but controlling for age, the longer they had been in office, the lower were the odds of exit.[52] Military regimes replace leaders more often than other types of autocracy. These findings are generally consistent with those of Londregan and Poole (1996) and Bueno de Mesquita and Smith (2010), who also analyzed the causes of leadership turnover. This is interesting, in itself, since I am able to include a much larger number of data points, extending further back in time, and to show that various results hold using a number of different estimation methods.[53] Neither of these papers instrumented for economic growth.[54]

---

[50] The parameter p is exp(-.38 + .29) = .91 for democracies and exp(-.38) = .68 for non-democracies. Thus, the ratio of the hazard rate after 60 months to that after one month is

$.91\exp(X\beta)60^{-.09} / .91\exp(X\beta)1^{-.09} = .70$ for democracies and $.68\exp(X\beta)60^{-.32} / .68\exp(X\beta)1^{-.32} = .27$ for non-democracies.

[51] As in prior papers, I focus on civil wars that cause 1,000 battle-related combatant deaths within 12 months.

[52] This is consistent with Bienen and van de Walle (1991), but not Londregan and Poole (1996); Bueno de Mesquita and Smith (2010) found that age lowered survival for leaders of non-democracies, but not democracies.

[53] In their leadership change regressions, Londregan and Poole (1996) report 2,707-2,798 observations, from 1952-1985; Bueno de Mesquita and Smith's (2010) regressions include 1,452-5,831 observations,

Economic growth and democratization

In Table A11, column 1, I regress the measure of non-negative movement on the Polity2 scale, $d_{it}^{+} = \max(d_{it}, d_{it-1})$, on the usual variables plus the country's lagged growth rate, using annual data. Growth proves significant: on average, countries with lower growth tend to increase their democracy ratings more in the following year. But why? Does economic performance affect democratization just via its influence on leader turnover, which—in higher income states—leads to democratization? Or does growth have a direct effect? I investigate this in column 2. Including interaction terms, I separate out the effects of growth that precedes, follows, and coincides with leader change.[55] Although low

---

from 1960-2000. Bueno de Mesquita et al. (2003, p.303) report regressions for the impact of growth on leader survival that include 5,821 data points. Using Maddison's GDP estimates, I am able to include 6,872-10,757 observations, going back to 1875 in some cases. At times, broader time coverage requires dropping controls for which data availability is a problem. My strategy is to include full sets of country and year dummies to pick up much of the unobserved heterogeneity. Londregan and Poole also included country but not year fixed effects. Bueno de Mesquita and Smith (2010) and Bueno de Mesquita et al. (2003) used only hazard models without fixed effects.

[54] Campante and Chor (2011) argue that the frustrated ambitions of over-educated youths increase the pressure on incumbent leaders, accelerating turnover. In a panel of countries between 1976 and 2010, they show that, for a given level of national income, the more educated the population was, the more frequently its leaders were replaced. Since using even the most comprehensive data available on education levels would require a drastic drop in coverage, I do not include controls for education and its interaction with income here.

[55] For this, I need to use data from both year *t-1* and year *t*. To avoid mistaking leadership change that coincided with or followed regime change for a cause of that regime change, when both occurred in the

growth that occurs without any leader replacement is associated with slightly more democratization, low growth followed by leader change has a much larger and more significant influence. Columns 4 and 5 present similar results for the 5-year panel. Here it is not possible to model the order of events so precisely, but column 5 does distinguish low growth that occurs without any leader change in the preceding 5 years from low growth in a 5-year period with leader turnover. The results suggest it is low growth accompanied by leader change that speeds democratization.

The growth rate might itself be influenced by leader change and democratization. In models (3) and (6), I instrument for growth using the same strategy as in Table A10. The instrument is the average growth rate in *other* countries, weighted by their trade shares with the given country in the previous year (Equation A1). Does this instrument satisfy the exclusion restriction in this context? One problem might arise if higher growth in other countries caused them to democratize and this created a more conducive environment for democracy in the given country. I therefore control for the level of democracy elsewhere in the world, using Persson and Tabellini's index (used in Table 3, column 2). If economic performance elsewhere affected the incidence of war, this could influence the country's political regime; so I also control for interstate war. In both models 3 and 6, we can reject the hypothesis of weak instruments with high confidence, based on Stock-Yogo tests. It is not possible to include multiple, correlated interaction terms with the single instrument available, so I focus on the one effect that was significant in OLS—that of growth followed by leader change. In the one-year panel, the estimated effect of growth, conditional on subsequent leader turnover, is not significant, although its magnitude is larger than before. In the five-year panel, however, the estimate is both larger than using OLS and highly significant.

---

same year I code leader change as 1 only if it *preceded* the regime change (using the dates for Polity regime changes in Gleditsch 2008).

Table A11: Economic Growth and Democratization

	--1-yr panel: upward movement in Polity2---			*--5-yr panel: upward movement in Polity2---*		
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	IV	OLS	OLS	IV
Democracy $_{t-1}$	.97***	.97***	.96***	.72***	.72***	.42***
	(.01)	(.01)	(.01)	(.05)	(.04)	(.06)
Ln GDP per Capita $_{t-1}$	-.002	-.003	.008	-.016	-.011	.053
	(.005)	(.005)	(.008)	(.027)	(.025)	(.039)
Leader replaced $_{t-1}$	-.07*	-.07	-.04	-.09	-.13	-.19
	(.04)	(.04)	(.05)	(.13)	(.14)	(.20)
Leader replaced $_{t}$ [a]		.03***	.03***			
		(.01)	(.01)			
Ln GDP per Capita $_{t-1}$ *	.011*	.010*	.007	.024	.032*	.047*
leader replaced $_{t-1}$	(.006)	(.006)	(.007)	(.018)	(.019)	(.027)
Growth rate $_{t-1}$	-.07***	-.03*	.06	-.07**	-.01	.09
	(.02)	(.02)	(.13)	(.03)	(.02)	(.19)
Growth rate $_{t}$		-.031				
		(.019)				
Growth rate $_{t-1}$*		-.00			-.23***	-.79***
leader replaced $_{t-1}$		(.00)			(.05)	(.18)
Growth rate $_{t-1}$*		-.20**	-.41			
leader replaced $_{t}$ [a]		(.10)	(.31)			
Growth rate $_{t}$*		.04				
leader replaced $_{t-1}$		(.05)				
Growth rate $_{t}$*		-.03				
leader replaced $_{t}$		(.09)				
Foreign democratic			.15			.86*
capital $_{t-1}$			(.09)			(.47)
Interstate war			-.005			.10***
			(.006)			(.02)
Implied cumulative effect of:						
growth rate $_{t-1}$	-2.46**			-.26**		
-with no leader change		-1.07*	1.53		-.02	.16
-with leader change $_{t-1}$		-1.07			-.84***	-1.20***
-with leader change $_{t}$		-7.21**	-9.40			
growth rate $_{t}$						
-with no leader change		-.98				
-with leader change $_{t-1}$		.22				
-with leader change $_{t}$		-1.83				
Fisher p level	[.00]	[.00]	[.00]	[.00]	[.00]	[.00]
Cragg-Donald			39.06			9.07
Stock Yogo (size)			p = .10			p = .10
Observations	5,724	5,718	4,374	1,101	1,101	815
Countries	136	136	124	133	133	120
R-squared	.8996	.9015	.7785	.7354	.7422	.4051

Sources: see Table A16.

Note: Dependent variable is $d_{it}^{+} = \max(d_{it}, d_{it-1})$ All estimations include country and year fixed effects. Robust standard errors, clustered by country, in parentheses; * p<.10, ** p<.05, *** p<.01. "Fisher p level" is probability level at which one can reject H0: residuals are I(1), from Fisher test of residuals. "Stock Yogo (size)": test rejects null hypothesis that actual size of test is greater than given p-level (when significance level is .05). Growth instrument in (3) is average growth rate in other countries, weighted by previous year's trade share; in (6) it is the cumulative growth rate in other countries over the five preceding years, weighted in each year by the previous year's trade share. [a] coding as 0 years in which leader changed but on the same day as or after regime changed, in order to focus on leader change prior to regime change.

Table A12: Income, Growth, and "Major Institutional Change"

Dependent Variable:	*"Major Institutional Change": 1 if $\|Polity2_t - Polity2_{t-1}\| \geq 3$, 0 otherwise*					
Panel type:	*----------1-yr panel----------*					
	OLS	OLS	OLS	IV	IV	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Institutional Change $_{t-1}$	.17***	.18***	.18***	.15***	.15***	.15***
	(.03)	(.03)	(.03)	(.03)	(.03)	(.03)
Ln GDP per Capita $_{t-1}$	-.01	-.01	-.02	.03	.03	.03
	(.02)	(.02)	(.02)	(.03)	(.03)	(.03)
Democracy $_{t-1}$	.01	.02	.02	.06	.04	.04
	(.02)	(.03)	(.03)	(.04)	(.04)	(.04)
Leader replaced $_{t-1}$		-.01	-.33**		-.01	-.22
		(.01)	(.14)		(.02)	(.17)
Leader replaced $_t$ [a]		.065***	.07***		.09***	.09***
		(.014)	(.01)		(.02)	(.02)
Ln GDP per Capita $_{t-1}$ * leader replaced $_{t-1}$			.043**			.028
			(.019)			(.022)
Growth rate $_{t-1}$	-.21***	-.20***	-.11	-.48	-.45	-.39
	(.07)	(.08)	(.07)	(.44)	(.44)	(.43)
Growth rate $_{t-1}$* leader replaced $_{t-1}$			-.15			.16
			(.15)			(.61)
Growth rate $_{t-1}$* leader replaced $_t$ [a]			-.43*			-.66
			(.23)			(1.01)
Foreign democratic capital $_{t-1}$				.36	.40	.38
				(.32)	(.31)	(.31)
Interstate war				-.02	-.02	-.02
				(.03)	(03)	(.03)
Implied cumulative effect of growth rate $_{t-1}$	-.25***	-.25***		-.56	-.53	
-with no leader turnover			-.14			-.46
-with turnover in t-1			-.32*			-.28
-with turnover in t			-.65**			-1.24
Implied cumulative effect of leader turnover $_{t-1}$		-.01			-.01	
Implied cumulative effect of leader turnover $_t$ [a]		.079***			.10***	
Cragg-Donald				104.3	102.2	30.04
Stock Yogo (size)				p = .10	p = .10	p = .10
Fisher p level	[.00]	[.00]	[.00]	[.00]	[.00]	[.00]
Observations	5,676	4,953	4,953	3,651	3,642	3,642
Countries	142	138	138	123	123	123
R-squared	.1309	.1363	.1395	.0241	.0370	.0412

Sources: see Table A16.

Note: All estimations include country and year fixed effects. Robust standard errors, clustered by country, in parentheses; * p<.10, ** p<.05, *** p<.01. "Fisher p level" is probability level at which one can reject H0: residuals are I(1), from Fisher test of residuals. "Stock Yogo (size)": test rejects null hypothesis that actual size of test is greater than given p-level (when significance level is .05). Growth instrument in (4)-(6) is average growth rate in other countries, weighted by previous year's trade share; [a] coding as 0 years in which leader changed but on the same day as or after regime changed. Data in all regressions annual: -8 < Polity2 $_{t-1}$ < 8, since the dependent variable is censored beyond these bounds.

Table A13: Antigovernment demonstrations

Method	*OLS cross-section, robust standard errors*		*OLS panel, country and year fixed effects, robust standard errors clustered by country*		
Dependent variable	*Ln number of antigovernment demonstrations (average)*		*Ln number of antigovernment demonstrations*	*Δ Antigovernment demonstrations*	
Years	*1994-2008*	*1994-2008*	*1994-2008*	*1920-99*	*1920-99*
Countries	*All*	*All*	*All*	*Authoritarian Polity2 $_{t-1}$ < 6*	*Democracy Polity2 $_{t-1}$ > 5*
	(1)	(2)	(3)	(4)	(5)
Press Freedom	.000	-.007***			
average 1994-2008	(.001)	(.002)			
Growth rate average		-.004			
1994-2008		(.008)			
Democracy average		.68***			
1994-2008		(.15)			
Ln GDP per Capita		.003			
average 1994-2008		(.034)			
Press Freedom $_{t-1}$			-.001		
			(.002)		
Growth rate $_{t-1}$			-.005*		
			(.003)		
Democracy $_{t-1}$			-.19		
			(.32)		
Ln GDP per Capita $_{t-1}$			.27		
			(.24)		
Leader's tenure				-.014***	-.004
				(.005)	(.008)
Leader turnover $_{t}$				.17**	.09
				(.07)	(.10)
Year before leader turnover				.18**	.04
				(.08)	(.09)
Two years before leader				.12**	.10
turnover				(.06)	(.07)
Ln antigovernment				-1.90***	-2.25***
demonstrations $_{t-1}$				(.14)	(.13)
Fisher p level			[.00]	[.00]	[.00]
Observations	177	151	740	4,860	2,476
Countries	177	151	150	137	102
R-squared	.0002	.1107	.4785	.2520	.2840

Sources: see Table A16.

Notes: Columns (1) and (2): all variables averages for 1994-2008, years for which Freedom of Press index available. Ln of number of antigovernment demonstrations used because distribution is right skewed. I have reversed the scale on Freedom House's index of press freedom so that higher values indicate more freedom. p<.10, ** p<.05, *** p<.01. "Fisher p level" is probability level at which one can reject H0: residuals are I(1), from Fisher test of residuals.

Table A14: Cumulative impact of income in different periods

	(A)					(B)					(C)				
	----------Level: Polity2----------					----------Level: Polity2----------					-Transition: Polity2 upward movement-				
	Polity2 $_{t-1}$ < 6					All countries					Polity2 $_{t-1}$ < 6				
Type of panel:	*1-yr*	*5-yr*	*10-yr*	*15-yr*	*20-yr*	*1-yr*	*5-yr*	*10-yr*	*15-yr*	*20-yr*	*1-yr*	*5-yr*	*10-yr*	*15-yr*	*20-yr*
Periods by Boix (2011) classification															
"Pro-democracy": 1918-32	.17	.22	.23	.35**	.51***	.27***	.30***	.27***	.37***	.42***	.30	.36	.32*	.50**	.44**
"Pro-democracy": after 1990	-.11**	-.06	.07	.10	.17	-.06	-.04	.01	.03	.13*	-.26**	-.10	.07	.13	.22
"Neutral": 1848-1917	.07	-.03	.04	-.15	.03	.16**	.14*	.16*	.13	.27*	-.05	-.17	-.04	-.39	-.09
"Polarized": 1933-90	.08	.11**	.17***	.17*	.24*	.10**	.12**	.15**	.16**	.26***	.15	.19*	.26**	.26	.26
"Authoritarian hegemony": 1800-48	.01	-.25	-.21	-.37	-.02	.05	-.05	-.05	-.06	.19	-.18	-.43*	-.33	-.81*	-.15

	(D)					(E)				
	-------Transition: Polity2 downward movement--------					--Transition: Boix-Rosato dichotomous measure--				
	Polity2 $_{t-1}$ > -6					Non-democracies (Boix-Rosato)				
Type of panel:	*1-yr*	*5-yr*	*10-yr*	*15-yr*	*20-yr*	*1-yr*	*5-yr*	*10-yr*	*15-yr*	*20-yr*
Periods by Boix (2011) classification										
"Pro-democracy": 1918-32	.59**	.53*	.58	.67	1.25*	.08**	.42***	.64***	.70***	.76***
"Pro-democracy": after 1990	-.03	-.05	.32	.88	1.04*	.00	.05	.15*	.14	.14
"Neutral": 1848-1917	.19	.04	.09	-.15	.63**	-.01	-.03	-.09	-.02	.17
"Polarized": 1933-90	.48**	.30**	.46*	.83	1.26*	.01	.07**	.16***	.15	.15
"Authoritarian hegemony": 1800-48	.13	-.08	-.24	-.51	.48	-.03	-.17*	-.36*	-.52	-.53

Sources: see Table A16.

Note: Figures are the estimated cumulative impact of Ln GDP $_{t-1}$, from OLS regressions of the dependent variable on Polity2 $_{t-1}$ (panels A-D), Ln GDP $_{t-1}$, country and year fixed effects, a dummy for countries under Soviet control, and the relevant period dummies and interactions between period dummies and Ln GDP $_{t-1}$; * p < .10, ** p < .05, *** p < .01 (robust standard errors, clustered by country). Each regression includes dummies and interaction terms for four of the five Boix-classification periods (one is excluded category). Fisher tests of residuals sometimes fail to reject non-stationarity in 20-year and 15-year panels at p < .10.

Table A15: The global economy, leader turnover, and democracy, 1850-2004

Dependent variable:	*Δ Average Polity2 score, rescaled*	*Δ Rate of leader turnover, non-democracies, adjusted for country fixed effects*	
	(1)	(2)	(3)
Average Polity2 score $_{t-1}$	-.028*	.35***	.34***
	(.016)	(.07)	(.07)
Rate of leader turnover, non-democracies, adjusted for country fixed effects $_{t-1}$	.059**	-1.09***	-1.07***
	(.023)	(.08)	(.08)
Δ Rate of leader turnover, non-democracies, adjusted for country fixed effects	.044***		
	(.016)		
Average Ln GDP per capita, non-democracies $_{t-1}$	.012		
	(.011)		
Average growth rate, non-democracies $_{t-1}$	-.14**	-.59**	
	(.06)	(.23)	
Δ average growth rate, non-democracies		-.47**	
		(.21)	
Proportion of non-democracies with negative growth $_{t-1}$			.08**
			(.04)
Δ Proportion of non-democracies with negative growth			.12***
			(.04)
Year	-.000	.000	.000
	(.000)	(.000)	(.000)
Constant	-.076	-.28	-.29
	(.075)	(.22)	(.21)
Observations	155	155	155
R-squared	.1388	.5420	.5584
ADF test of H0: residuals are I(1)	-7.25***	-9.05***	-9.08***
Phillips-Perron test of H0: residuals are I(1)	-10.34***	-12.13***	-11.98***

Sources: see Table A16.

Note: Robust standard errors in parentheses; * p<.10, ** p<.05, *** p<.01. "Non-democracies": Polity2 < 6. "Rate of leader turnover" is proportion of countries where leader changed in given year.

Table A16: Data sources

Variable	Notes	Source
Democracy: close to continuous measure	Polity2, rescaled to take values from 0 to 1.	Polity IV Dataset, Version 2009, http://www.systemicpeace.org/inscr/inscr.htm
Democracy: binary measure	Dummy: 1 = democracy; 0 = non-democracy.	Constructed by Boix and Rosato (2001), for 1800-2000, provided by Carles Boix.
GDP, GDP per capita, GDP per capita growth	In 1990 international Geary-Khamis dollars.	Maddison (2010), downloaded from http://www.ggdc.net/MADDISON/oriindex.htm
Trade	Trade between dyads of countries, in 1990 dollars.	Dataset for Russett, Oneal, and Berbaum (2003), downloaded from Bruce Russett's website at: http://pantheon.yale.edu/~brusset/.
Domestic democratic capital, foreign democratic capital	Definitions in Persson and Tabellini (2009)	Dataset for Persson and Tabellini (2009), downloaded from Guido Tabellini's website at http://didattica.unibocconi.it/mypage/index.php?IdUte=48805&idr=7569&lingua=ita.
Average schooling	Average years of schooling in population aged 15 and over	Morrisson and Murtin (2009), downloaded www.pse.ens.fr/data/index.html.
Leader turnover, timing and type; leaders' ages, other characteristics		Archigos, downloaded from Henk Goemans' website http://www.rochester.edu/college/faculty/hgoemans/data.htm.
War, civil war, initiators of war, militarized interstate disputes, military capacity		Correlates of War intrastate and interstate wars datasets, v.4.0, Militarized interstate disuptes v.3.10, National material capabilities, v.4.0, downloaded from http://www.correlatesofwar.org/datasets.htm
Military regime	Head of State coded as "military" in Banks dataset.	Arthur Banks' "Cross- National Time-Series Data Archive," as reproduced in Bueno de Mesquita et al. (2003) dataset, downloaded from http://www.nyu.edu/gsas/dept/politics/data/bdm2s2/bdm2s2_nation_year_data_may2002_webversion.zip.
Oil and gas income per capita		Michael L. Ross, 2011-04, "Replication data for: Oil and Gas Production and Value, 1932-2009", http://hdl.handle.net/1902.1/15828 UNF:5:Hwe3jAjxG7fgOMzpGQXOxw== V4
Monarchy	Head of State coded as "monarch" in Banks dataset.	Banks (see above)
Military, personalist, one-party, monarchical autocracies		Geddes, Barbara, Joseph Wright and Erica Frantz. 2012. ''Authoritarian Regimes: A New Data Set.'' Manuscript.
Antigovernment protests	Antigoverment protests involving more than 100 people	Banks (see above)
Elected parliament	Legislative selection = "elective" in Banks dataset.	Banks (see above)
Non-regime parties	"defacto 2": existence of parties outside of regime front	Democracy and Dictatorship Revisited dataset, José Cheibub, Jennifer Gandhi, James Vreeland (Georgetown University), September 2009 (v.1)
Press freedom index	Freedom House	Downloaded from www.freedomhouse.org
Education of leaders	Besley and Reynal-Querol (2011)	Provided by Marta Reynal-Querol.

Additional Appendix References

Bienen, Henry and Nicholas van de Walle.1991. *Of Time and Power: Leadership Duration in the Modern World*, Stanford, CA: Stanford University Press.

Bueno de Mesquita, Bruce, and Randolph M. Siverson. 1995. "War and the Survival of Political Leaders: A Comparative Study of Regime Types and Political Accountability," *American Political Science Review* 89, 4: 841-855.

Bueno de Mesquita, Bruce, Alastair Smith, Randolph Siverson, and James D. Morrow. 2003. *The Logic of Political Survival*, Cambridge, MA: MIT Press.

Campante, Filipe R. and Davin Chor.2011. "'The People Want the Fall of the Regime': Schooling, Political Protest, and the Economy," Harvard University: manuscript.

Chiozza, Giacomo and H.E. Goemans. 2004. "International Conflict and the Tenure of Leaders: Is War Still 'Ex Post' Inefficient?" *American Journal of Political Science* 48, 3, July: 604-19.