REEVALUATING THE MODERNIZATION HYPOTHESIS

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Abstract

This paper revisits and critically reevaluates the widely-accepted modernization hypothesis which claims that per capita income causes the creation and the consolidation of democracy. We argue that existing studies find support for this hypothesis because they fail to control for the presence of omitted variables. There are many underlying historical factors that affect both the level of income per capita and the likelihood of democracy in a country, and failing to control for these factors may introduce a spurious relationship between income and democracy. We show that controlling for these historical factors by including fixed country effects removes the correlation between income and democracy, as well as the correlation between income and the likelihood of transitions to and from democratic regimes. We argue that this evidence is consistent with another well-established approach in political science, which emphasizes how events during critical historical junctures can lead to divergent political-economic development paths, some leading to prosperity and democracy, others to relative poverty and non-democracy. We present evidence in favor of this interpretation by documenting that the fixed effects we estimate in the post-war sample are strongly associated with historical variables that have previously been used to explain diverging development paths within the former colonial world.

Keywords: democracy, economic growth, institutions, political development.

JEL Classification: P16, O10.

1 Introduction

At the heart of comparative politics is an attempt to understand why different societies are organized in different ways. Why are some societies democratic, while others are not? Why do some societies develop modern effective nation states, while others do not? Why do some societies experience revolutions, while others undertake more gradual change? And finally, why are some societies relatively prosperous, while others are not?

Two distinct approaches to these questions have been popular in empirical social science. The first focuses on the potentially causal channel from one of these variables, for example, income per capita, to the rest. The second emphasizes that many of the key outcomes of interest covary; at certain critical junctures societies make decisions that move them onto distinct *development paths* and along these paths various outcomes coevolve.

The seminal example of the first approach is Seymour Martin Lipset's (1959) *modernization hypothesis*. In Lipset's view, the level of economic development and variables closely associated with it, such as the level of educational attainment and urbanization, drive institutional change. They particularly shape the possibilities for the creation and consolidation of democracy.

The second approach, which we may refer to as the *critical junctures hypothesis*, is exemplified by Barrington Moore's famous (1966) thesis that the reasons why Britain moved gradually to democracy, Germany to fascism, and Russia to communist revolution are to be found in the differential organization of agriculture and the differential intensities of feudal legacies. Not only are the paths to the modern world through capitalist democracy, fascism, and communism distinct from one another, but they are also initiated by differences during a certain critical juncture, which in Moore's account is the end of the medieval world. Societies, like Britain, where feudalism had collapsed most comprehensively moved onto a path of successful capitalist development, prosperity, and democracy. Societies where the feudal legacy endured, such as Russia, had a weak bourgeoisie, thwarted capitalism, backward agriculture, and eventually, communist revolution. Other studies which share a similar methodological approach include Lipset and Rokkan (1967), Collier and Collier (1991), Scully (1992), Engerman and Sokoloff (1997), Acemoglu, Johnson, and Robinson (2001,2002), Mahoney (2002), as well as the essays in Steinmo, Thelen, and Longstreth (1992) and Mahoney and Rueschemeyer (2003).

Despite the appeal of the critical junctures hypothesis in many country studies, the modernization hypothesis and Lipset's work have been much more influential in the empirical political and social science literatures. Most importantly, the research on the creation and consolidation of democracy has stayed close to Lipset's causal scheme and investigated the role of income per capita and other socio-economic factors in accounting for variation in measures of democracy.

Most of this work has concluded that there is a *causal effect* of income per capita, prosperity or modernization on democracy or democratic consolidation. Examples of this work include Londregan and Poole (1996), Przeworski and Limongi (1997), Barro (1997, 1999), Przeworski, Alvarez, Cheibub, and Limongi (2000), Boix (2003), and Epstein, Bates, Goldstone, Kristensen, and O'Halloran (2006).[1]

In this paper, we demonstrate that the evidence supporting this conclusion and the modernization hypothesis is much weaker than the previous work claimed or presumed. Instead, we present evidence consistent with the existence and importance of critical junctures.

Most previous work on the determinants of democracy uses cross-sectional regression analysis in order to investigate the causal relationship between income and democracy.[2] However, to the extent that there is any validity to the critical junctures hypothesis, one would want to control for common variables affecting income and democracy. After all, not all correlation is a manifestation of a causal relationship. The simplest way of accomplishing this is to investigate the relationship between income and democracy in a panel of countries and to control for fixed effects. Controlling for fixed effects is not only a simple and transparent strategy, but is also in the spirit of the critical junctures hypothesis, since it takes out the effect of constant, potentially historical, factors.

Our first finding in this paper is that once fixed effects are introduced into standard regressions of democracy, the positive relationship between income per capita and democracy disappears.[3] Our main results then show that high levels of income per capita do not promote transitions to democracy from non-democracy, nor do they forestall transitions to non-democracy from democracy. Our findings are robust across different measures of democracy, the use of additional covariates, econometric specifications and estimation techniques. An important aspect of our result is that controlling for fixed effects removes omitted factors affecting both income and democracy, and this removes the influence of income per capita both on transitions away from democracy and transitions to democracy.

In addition to a linear specification, which we use to investigate both the effect of income

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[1] Przeworski et al. (2000, p. 89) focus on the effect of income on the consolidation of democracy rather than likelihood of democracy or the creation of democracy. They use Lipset's phrase "this means that the more well-to-do a nation, the greater the chances that it will sustain democracy" (Lipset, 1959, p. 75) to justify their claim that Lipset was connecting income per-capita only to democratic consolidation, as they do. Nevertheless, elsewhere in the article Lipset claims that higher income per-capita both creates and consolidates democracy. For example, (p. 83) "Increased wealth is not only related causally to the development of democracy..." See also his discussion on p.102 about whether Latin America will become democratic, where it is clear that he is talking about economic development causing countries to become more democratic.

[2] Among the papers mentioned above only Londregan and Poole (1996) include fixed country effects in their analysis. However, they fail to include time effects, and despite using annual data, they do not correct for serial correlation in the residuals. Their results are sensitive to these choices (see Section 6).

[3] For similar results focusing on the relationship between income and the *level* of democracy, see Acemoglu, Johnson, Robinson, and Yared (2007).

per capita on the level of democracy and on transitions, we develop and implement a double hazard model for the simultaneous estimation of transitions to democracy and transitions away from democracy. Though a major focus of political science research on democratization has been the study of transitions to and away from democracy, the econometrics of transition models is not entirely straightforward. Specifically, one cannot look at transitions to democracy or away from democracy as separate events because whether or not an observation finds itself in the at-risk sample is endogenously determined. We develop a framework to deal with this issue which allows for the incorporation of fixed effects in a straightforward and intuitive way, and we find that income per capita conditional on the fixed effect does not predict either transitions to democracy or transitions away from democracy.

Our initial results refer to the most-commonly used sample period of 1960-2000. One might argue, along the lines of Boix and Stokes (2003), that this post-war sample misses the democratization of much of Western Europe and North America in the late nineteenth century. Motivated by this concern, we also look at the relationship between income and democracy for a balanced sample during the period 1875-2000. Again we do not find a significant relationship between income and democracy once we control for fixed effects. Moreover, using our double hazard model, we also do not find a significant relationship between income and transitions to democracy or transitions away from democracy.

These empirical results show that while the level of per capita income and the level of democracy are correlated, there is no relationship between the *change* in income per capita and the *change* in democracy over the past 150 years. Interestingly, this point was made some time ago by O'Donnell (1973, p. 6) who noted in his discussion of the existing empirical literature on modernization that

> "the data used refer to a *set* of countries at a single *point* in time, while the postulated relationship refers to changes over a period of time in each of the countries ... The attempt to substitute "horizontal" data referring to many countries ... for this "longitudinal data" and still say something about causal, time-spanning processes *within each unit* [is a] fallacy."[4]

In essence, the finding that income per capita causes democracy comes only from the cross-sectional variation in the data. It is exactly this cross-sectional variation that fixed effects remove, and fixed effects estimators essentially exploit the *within* variation called for by O'Donnell

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[4]Przeworski et al. (2000, p. 99) object to O'Donnell's study of how increasing income per capita in Argentina induced a coup on the grounds that he "studied a country that turns out to be a distant outlier". Our empirical work shows that the patterns isolated by O'Donnell in the Southern Cone of Latin America are actually consistent with data from both the postwar era and throughout the 20th century.

by looking at the relationship between changes in income and changes in democracy. It is important to note that using fixed effects is not the same as looking at transitions, for example as in the work by Przeworski et al. (2000). Przeworski et al. (2000) find a relationship between income per capita and the propensity for coups because their key right-hand side variable is still the level of income, and thus they are documenting that richer democracies have fewer coups. Consequently, their econometric strategy does not deal with omitted variables affecting both democratization and economic growth, and these findings establish neither the presence of a causal effect of income on the likelihood of coups nor provides any evidence that democracies that *become* richer experience *fewer* coups.

Figures 1 and 2 provide a simple diagrammatic illustration of these point. Both figures use Przeworski et al.'s data and focus on the sample of democracies in every five year interval between 1955 and 1990. We then observe which of these democracies experience a coup five years later. In Figure 1, we group observations depending on whether log income per capita is above or below the average log income per capita in the world for the observation year, and we calculate the fraction of democracies in each group which experienced a coup. This figure corresponds to regressions without controlling for fixed effects, and consistent with Przeworski et al.'s findings, it shows that democracies with low income per capita are more likely to experience a coup than democracies with high income per capita. Figure 2, on the other hand, provides a visual representation of the patterns once we take out some of the time-invariant omitted variables. To do this, we group observations depending on whether log income per capita is above or below the average log income per capita for *that* country between 1955 and 1990.[5] In contrast to Figure 1, Figure 2 shows that democracies that are poorer than usual are not more likely to experience a coup. This figure therefore provides a preview of how the results are likely to change once we control for omitted variables affecting both income and democracy. Our empirical work will show in detail that the pattern shown in Figure 2 is very robust, and this leads us to conclude that the empirical support for and the strong conclusions drawn from the modernization hypotheses need to be reevaluated.

But if income does not cause democracy, then what does? The fact that including fixed effects removes the correlation between income and democracy suggests that relatively time-invariant, possibly historical factors are at the root of both the relative prosperity and the relative democratic experience of some countries. In the second part of the paper, we discuss whether, as emphasized by the critical junctures hypothesis, the relationship between prosperity and democracy is underpinned by the divergent development paths of the countries in our sample. We accomplish this by investigating whether the fixed effects estimated in the post-

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[5] Both of these values are demeaned from the world average to account for time trends.

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war regressions are systematically related to historical variables associated with political and economic divergence in history.

For this exercise, we focus on the sample of former European colonies, since for this sample there is a specific theory of political and economic development related to divergent development paths, and there is also data related to the determinants of these different paths during the critical junctures facing these former colonies. In particular, we build on Acemoglu, Johnson, and Robinson (2001, 2002) who exploit the quasi-natural experiment provided by the colonization of many diverse societies by European powers after 1492. They show that the institutional differences created at the critical juncture of European colonization persisted and significantly contributed to the large differences in both the form of government (particularly the extent of constraints on the executive) and the economic success of these societies. They also show that the different paths of economic and political development are systematically related to a number of historical variables which influenced the costs and benefits of different sets of institutions.

Institutional variation within the former colonies was influenced by the types of initial conditions that the European powers encountered. In colonies where there were initially large densities of indigenous peoples, where the mortality environment was unfavorable for European settlements, and which were relatively prosperous, extractive institutions designed to transfer rents to Europeans emerged. Such institutions did not create effective property rights except for small minorities, they did not generate incentives for investment, education, or innovation, and they consequently retarded economic growth. The political institutions in such societies were complementary to the extractive economic institutions; they were coercive, hierarchical, and authoritarian, aimed primarily at controlling indigenous populations, and focused on maintaining and perpetuating a fundamentally unequal order. Since institutions have a tendency to persist, the colonial economic and political institutions created in these extractive colonies persisted into the 19th and 20th centuries and continued to benefit relatively small elites. These elites had a lot to lose from democracy, not just because it would have directly taken away their formal political power, but also because the change in the distribution of power would have undermined their preferred set of economic institutions. Consequently, in these societies, elites were prepared to fight harder to stop democracy (see Acemoglu and Robinson, 2006). Moreover, given that such societies were based on relatively coercive institutions, elites were better able to repress those who pushed for democracy, and subsequently, if democracy was conceded, they were better able to undermine it by mounting coups. Therefore, the development path starting with extractive institutions was nondemocratic and associated with relatively slow economic growth.

In colonies with different initial conditions, where there were few indigenous peoples, where the disease environment was relatively benign for Europeans, and which were initially poor,

very different economic institutions emerged. Since there were few people to exploit and little to extract from indigenous peoples, relatively non-coercive societies emerged. Such societies, best exemplified by the settler colonies in North America and Australasia, developed economic institutions providing most inhabitants access to land, secure property rights, and equality before the law. They also quickly developed political institutions placing effective constraints on the exercise of power. The incentives for investment and innovation in these societies paved the way for economic growth. This situation is well illustrated by the development path of North America, where already during the colonial period a relatively egalitarian society emerged with representative assemblies in each state where free adult males could vote.[6] This institutional nexus provided relatively good economic incentives for the non-slave population and provided weaker incentives for the political elites to pursue strategies to block economic development or undermine democracy. Moreover, these initial institutions implied that later political elites, even when they tried, were unable to tilt the balance away from growth promoting and democratic institutions.

We confirm the importance of the critical junctures emphasized in Acemoglu, Johnson, and Robinson (2001, 2002) by showing that the fixed effects estimated in the post-war data are very strongly related to factors linked to the past colonization experiences of these countries. In particular, we show a very strong relationship between these fixed effects and the mortality rates faced by European settlers, the indigenous population density before colonization, the constraint on the executive at (or shortly after) independence, and the date of independence. Settler mortality and indigenous population density before colonization proxy for the initial conditions affecting the colonization strategy and the subsequent development path (Acemoglu, Johnson, and Robinson, 2001, 2002); constraint on the executive at independence is the closest variable we have to a direct measure of relevant institutions during the colonial period; and date of independence is another measure of colonization strategy, since non-extractive colonies gained their independence typically earlier than the extractive ones.

We also investigate the relationship between other variables, such as geography, religion and ethno-linguistic fragmentation, on the propensity of a society to be democratic. Interestingly, conditional on the historical variables related to the colonization strategy pursued by Europeans, these variables seem to have no correlation with the fixed effects for democracy.

In addition to the papers already mentioned, our work is most closely related to Acemoglu, Johnson, Robinson, and Yared (2007) who also investigate the relationship between income and

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[6] Though slavery was important in the South, the key U.S. institutions were formed in the 17th century when slavery was insignificant and at no time did slaves form more than 20% of the entire population. In contrast, indigenous peoples formed 80-90% of the populations of Peru, Bolivia or Mexico, while slaves constituted more than 90% of the population in most Caribbean islands (Engerman and Sokoloff, 1997).

democracy. Despite the similarities between the two papers, there are also major differences. Acemoglu, Johnson, Robinson, and Yared (2007) focus on instrumental variable estimates of the impact of income on democracy and also focus on the very long run relationship (between 1500 and 2000). The main innovations in the current paper include the focus on the modernization hypothesis (and the contrast to the critical junctures hypothesis), the investigation of the relationship between income and the dichotomous measures of democracy commonly used in the political science literature, and most importantly, a detailed analysis of the relationship between income and transitions to and away from democracy using a double hazard model. Our paper is also related to a large literature empirically investigating the modernization hypothesis. We discuss the relationship between our work in this literature in greater detail in Section 6.

The paper proceeds as follows. Section 2 discusses the data we use. In Section 3, we replicate some of the basic regression results in the literature using pooled OLS, and in Section 4, we show that the introduction of fixed effects into these models leads income per capita to become completely insignificant. After having focused on results where the level of democracy is the dependent variable we then move in Section 5 to consider various ways of measuring transitions towards and away from democracy, and we develop a double hazard model which allows for the incorporation of fixed effects. We find the same results. In Section 6, we discuss in detail the relationship between our results and the existing research in the political science literature. In Section 7, we investigate our interpretation of the fixed effects regressions, and in Section 8, we examine our basic findings over the period 1875-2000, rather than 1960-2000. Section 9 concludes.

2   Data and Descriptive Statistics

We follow the existing empirical research in the way we measure democracy. Though there is some controversy about how to exactly measure democracy, our strategy is to show that our results are robust to any of the measures that are widely used in the literature. Our first measure of democracy is the Freedom House Political Rights Index. This index ranges from 1 to 7, with 7 representing the least amount of political freedom and 1 the most freedom. A country gets a score of 1 if political rights come closest to the ideals suggested by a checklist of questions, beginning with whether there are free and fair elections, whether those who are elected rule, whether there are competitive parties or other political groupings, whether the opposition plays an important role and has actual power, and whether minority groups have reasonable self-government or can participate in the government through informal consensus.[7] Following Barro (1999), we supplement this index with the related variable from Bollen (1990, 2001) for 1950,

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[7] See Freedom House (2004), http://www.freedomhouse.org/research/freeworld/2003/methodology.htm

1955, 1960, and 1965. As in Barro (1999), we transform both indices so that they lie between 0 and 1, with 1 corresponding to the most democratic set of institutions.

The Freedom House index, even when augmented with Bollen's data, only enables us to look at the post-war era. The Polity IV dataset, on the other hand, provides information for all countries since independence starting in 1800. Both to look at pre-1940 events and as a check on our main measure, we also use the composite Polity index, which is the difference between the Polity's Democracy and Autocracy indices.[8] The Polity Democracy Index ranges from 0 to 10 and is derived from coding the competitiveness of political participation, the openness and competitiveness of executive recruitment, and constraints on the chief executive. The Polity Autocracy Index also ranges from 0 to 10 and is constructed in a similar way to the democracy score. To facilitate comparison with the Freedom House score, we also normalize the composite Polity index to lie between 0 and 1.

Both of these measures enable us to distinguish between different shades of democracy. An alternative empirical approach has been defended and used by Przeworski and his coauthors (Przeworski et al., 2000, chapter 1), who argue that a simple dichotomy between democracy and non-democracy is the most useful empirical definition. Dichotomous measures may also be better suited to analyses of transitions from and to democracy. Therefore, we present results using the Boix-Rosato dataset which extends the Przeworski et al. (2000) in which the index equals 1 if a country is a democracy and equals 0 otherwise. We also develop a simple double hazard model to deal with the simultaneous modeling of transitions to and from democracy. All of these exercises using the dichotomous measures give very similar results to those using the continuous measures.

We construct five-yearly and annual panels. For the five-year panels, we take the observation every fifth year. We prefer this procedure to averaging the five-yearly data, since averaging introduces additional serial correlation, making inference and estimation more difficult.[9]

In addition, we use GDP per capita data from the Summers-Heston dataset for the post-war period (Heston, Summers, and Atten, 2002), GDP per capita data from Maddison (2003) for the prewar and long samples, a measure of educational attainment from the Barro-Lee dataset (average years of schooling for people in the population over the age of 25), and total population from the World Bank (2002).

When we turn to the former European colonies sample, we obtain the date of independence from the CIA World Factbook and the constraint on the executive after independence from the

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[8] See Marshall and Jaggers (2004) and http://www.cidcm.umd.edu/inscr/polity/
[9] For the Freedom House data which begins in 1972, we follow Barro (1999) and assign the 1972 score to 1970 for the purpose of the five-year regressions. Moreover, we assign the 1994 score in the Boix-Rosato data to 1995 for the purpose of the five-year regressions.

Polity IV dataset.[10] Population density in 1500 is calculated by dividing the historical measures of population from McEvedy and Jones (1975) by the area of arable land (see Acemoglu, Johnson, and Robinson, 2002). Finally, data on settler mortality are from Acemoglu, Johnson, and Robinson (2001), who constructed it based on research by Philip Curtin and other historians.

Table 1 contains descriptive statistics for the key variables both for the whole world and for former European colonies, the sample we focus on for some of the regressions. Throughout the paper, we adopt the definition of former European colonies used in Acemoglu, Johnson, and Robinson (2001, 2002), which excludes the Middle Eastern countries that were briefly colonized by European powers during the 20th century. This definition is motivated by our interest in former colonies as a sample in which the process of institutional development, in particular during the 19th century and earlier, was shaped by European intervention (see Acemoglu, Johnson, and Robinson, 2002).[11]

Table 1 shows that there is significant variation in all the variables for both the entire sample and the former colonies sample. Countries in the former colonies sample are somewhat less democratic and substantially (about 30 percent) poorer than the average country in the whole sample.

3   Pooled Cross-Section Results

We first replicate the basic results in the literature using a pooled cross-sectional approach. The first column of Table 2 reports estimates of the following simple linear regression model:

$$d_{it} = \alpha d_{it-1} + \gamma y_{it-1} + \mu_t + v_{it} \tag{1}$$

where d_{it} is the democracy score of country i in period t. The lagged value of this variable on the right hand side is included to capture persistence in democracy and also potentially mean-reverting dynamics (i.e., the tendency of the democracy score to return to some equilibrium value for the country). The main variable of interest is y_{it-1}, the lagged value of log income per capita. The parameter γ therefore measures the impact of income per capita on democracy. In addition, the μ_t's denote a full set of time effects, which capture common shocks to (common

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[10] The data on constraint on the executive from Polity begins in 1800 or at the date of independence. In our former colonies sample only one country, the United States became independent before 1800. The United States broke with Britain in 1776 and was recognized as the new nation following the Treaty of Paris in 1783. We code the U.S. date of independence as 1800.

[11] By the time the Middle East was colonized, the whole 'colonial project' was on the retreat. The European powers had little influence on the institutions of these societies. In contrast, Latin American countries were colonies for almost 300 years and the Spanish colonial state penetrated into all areas of life. Though most African and Asian countries were not formally colonized until towards the end of the 19th century, they had been experiencing the effects of European colonialism since the mid 17th century. For example, in Africa the Atlantic slave trade took off in the first half of the 17th century and existing evidence suggests that this had a large impact on institutions even before formal colonization.

trends in) the democracy score of all countries, and v_{it} is an error term, capturing all other omitted factors, with $E(v_{it}) = 0$ for all i and t. The sample period is 1960-2000 and time periods correspond to five-year intervals.

Ordinary Least Square (OLS) regressions of (1) will lead to consistent estimates of the parameter of interest, γ, when $cov(d_{it-1}, v_{it}) = cov(y_{it-1}, v_{it}) = 0$. In other words, OLS estimation requires that there be *no* omitted variables correlated with the right-hand side variables in the regression.[12]

The panel A of Table 2 uses the Freedom House data, panel B uses the Polity data, and panel C uses the dichotomous Przeworski index to present pooled cross-sectional regressions of democracy on income. All panels pool the time-series and cross-sectional variation. All standard errors in the paper (unless indicated otherwise) are robust against arbitrary heteroskedasticity in the variance-covariance matrix, and they allow for clustering at the country level.[13]

The regressions include one lag of democracy, one lag of log GDP per capita, and time effects. The sample size varies because of data availability, and the panel is unbalanced. Lagged democracy is highly significant and shows a considerable degree of persistence (mean reversion) in democracy. Log GDP per capita is also significant and illustrates the well-documented positive relationship between income and democracy. Though statistically highly significant, the effect of income is quantitatively small. For example, the coefficient of 0.073 (standard error = 0.010) in column 1 of panel A implies that a temporary 10 percent increase in GDP per capita is associated with an increase in the Freedom House score of 0.0073, and a permanent increase in GDP per capita by 10 percent is associated with an increase in the (steady state) Freedom House score of 0.0073/(1-.703)≈0.025. This effect is quantitatively small (for comparison, the gap between the United States and Colombia today is 0.5).

Overall, the regressions in Table 2 confirm the main finding of the existing literature of a positive association between income and democracy. While the earlier literature has typically interpreted this as the causal effect of income on democracy, we next show that such an interpretation may not be warranted.

4 Panel Regressions with Fixed Effects

We now revisit the basic results of the last section in the panel set-up with fixed effects. In terms of equation (1), the presence of fixed effects implies that the error term can be represented as

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[12] The fact that the democracy index takes discrete values induces a special type of heteroscedasticity, but creates no difficulty for inference with OLS, as long as standard errors are corrected for heteroskedasticity (e.g., Wooldridge, 2002, Section 15.2).

[13] Clustering is a simple strategy to the correct the standard errors for potential correlation across observations both over time and within the same time period. See for example Moulton (1986) or Bertrand, Duflo, and Mullainathan (2004).

$v_{it} = \delta_i + u_{it}$, where now $E\left(u_{it}\right) = 0$ for all i and t. Taking this into account, the estimating equation becomes:

$$d_{it} = \alpha d_{it-1} + \gamma y_{it-1} + \mathbf{x}'_{it-1}\boldsymbol{\beta} + \mu_t + \delta_i + u_{it}, \tag{2}$$

which differs from (1) because it includes a full set of country dummies, the δ_i's, and because we now allow for other covariates captured by the vector $\mathbf{x}'_{it-1}$ with coefficient vector $\boldsymbol{\beta}$. The country dummies capture any time-invariant country characteristic that affect the equilibrium level of democracy.

The most important benefit of the fixed effect estimator is that, as well known, if the error term takes the form $v_{it} = \delta_i + u_{it}$, with the δ_i's, correlated with y_{it-1} or $\mathbf{x}_{it-1}$, then pooled OLS estimates are biased and inconsistent. In contrast, even if $cov\left(y_{it-1}, \delta_i + u_{it}\right) \neq 0$ (or $cov\left(x^j_{it-1}, \delta_i + u_{it}\right) \neq 0$ where x^j_{it-1} represents the j'th component of the vector $\mathbf{x}_{it-1}$) but $cov\left(y_{it-1}, u_{it}\right) = cov\left(x^j_{it-1}, u_{it}\right) = 0$ for all j, then the fixed effects estimator will be consistent.

This structure of correlation is particularly relevant in this context, because the critical junctures hypothesis suggests precisely the presence of historical factors affecting both political and economic development.[14]

In addition to the conceptual issues, there is also an econometric problem involved in the estimation of (2). The regressor d_{it-1} is mechanically correlated with u_{is} for $s < t$, so the standard fixed effects estimation is not consistent (e.g., Wooldridge, 2002, chapter 11). However, it can be shown that the fixed effects OLS estimator becomes consistent as the number of time periods in the sample increases. Here, we start with the fixed effects OLS estimates, and then consider various alternative estimation strategies to deal with this issue.

Table 2 column 2 presents our basic results using the Freedom House score (panel A), the Polity score (panel B), and the Przeworski index (panel C). In none of the panels is income per capita significant, and it typically has a very small coefficient. With the Freedom House data the coefficient in 0.008 (for example, compared to 0.073 in column 1 of Table 2) with a standard error of 0.034. With the Polity data in panel B, the estimate is basically zero, -0.003 (standard error=0.038).

A potential concern with fixed effects regressions is that once fixed effects are included, there may not remain enough variation in the right-hand side variables to obtain precise estimation.[15] The results in Table 2 show that this is not the case in our empirical investigation. The standard

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[14]Nevertheless, there should be no presumption that fixed effects regressions will necessarily estimate the causal effect of income on democracy, for example because there are time varying omitted variables. See Acemoglu, Johnson, Robinson, and Yared (2007) for instrumental variable strategies designed to estimate the causal effect of income on democracy.

[15]This issue is raised by a number of recent papers in the political science literature, debating the pros and cons of the fixed effects methodology. See, for example, Green, Kim, and Yoon (2001) and the accompanying symposium, as well as Beck (2001).

errors of the estimates of the effect of income on democracy are relatively small. For example, the standard error in column 2 of panel A of Table 2 is 0.034, compared to 0.010 in column 1. An effect of income on democracy of the same size as in column 1 (0.073), which, as noted in Section 3, is itself quantitatively very small, falls just outside the two standard error confidence interval of the fixed effect estimate. This shows that the lack of a positive effect of income per capita on democracy when we control for time-invariant omitted variables is not driven by imprecise estimates. Instead, it is likely due to the fact that these omitted variables are responsible for the positive relationship that previous cross-sectional (or pooled cross-section and time-series) studies have found.[16]

Furthermore, Figures 3-5 document that the lack of a significant relationship between income per capita and democracy is not driven by some econometric problems or some unusual feature of our data. Figures 3 and 4 plot the change in the Freedom House and Polity score for each country between 1970 and 1995 against the change in GDP per capita over the same period. These scatterplots correspond to the estimation of the fixed effects equation (2) in time differenced form without any covariates other than contemporaneous income, and using only two data points, 1970 and 1995 (these two dates are chosen to maximize sample size).[17] They show clearly that there is no strong relationship between income growth and changes in democracy over this period. Figure 5 performs a similar exercize using the Przeworski index. In contrast to the continuous Freedom House and Polity scores, this index is dichotomous so that change in democracy is either -1, 0, or 1. Therefore, we document the average change in democracy score for countries grouped by income per capita growth quintile. This figure shows that there is no relationship between the income per capita growth quintile and the change in the democracy score.[18]

These initial results show that once we allow for fixed effects, per capita income is not a major determinant of democracy. The remaining columns of the table consider alternative estimation strategies to deal with the potential biases introduced by the presence of the lagged dependent variable discussed above.

Our first strategy, adopted in column 3, is to use the Generalized Method-of-Moments Estimator (GMM) proposed by Arellano and Bond (1991). This builds on the approach first suggested by Anderson and Hsiao (1982) and uses second and higher order lags as instruments

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[16] Acemoglu, Johnson, Robinson, and Yared (2007) report similar results also instrumenting for income per-capita using various different instruments. They show that the lack of a positive relationship documented here is robust to these instrumental variables strategies.

[17] The regression of the change in Freedom House score between 1970 and 1995 on change in log income per capita between 1970 and 1995 yields a coefficient of 0.032, with a standard error of 0.058, while the same regression with Polity data gives a coefficient estimate of -0.024, with a standard error of 0.063.

[18] We have also investigated whether the lack of a statistical association between income and democracy once we condition on fixed effects is driven by some outliers in the data, and found no major outliers.

under the assumption of no serial correlation in the residual, u_{it}, in equation (2). With the Arellano-Bond's GMM estimator, the coefficient on income per capita is now negative in all panels.

Our second strategy, reproduced in column 4, estimates (2) with fixed effects OLS using annual observations. This is useful since the fixed effects OLS estimator becomes consistent as the number of observations becomes large. With annual observations, we have a reasonably large time dimension. However, estimating the same model on annual data with a single lag would induce significant serial correlation (since our results so far indicate that *five-year lags* of democracy predict changes in democracy). For this reason, we now include five lags of both democracy and log GDP per capita in these annual regressions. The table reports the p value of an F-test for the joint significance of these variables. The results show no evidence of a significant positive effect of income on democracy in any of the panels (while democracy is strongly predicted by its lags, as was the case in earlier columns).

In columns 5 and 6 of Table 2 we add average years of schooling and population as additional explanatory variables, and we repeat the regressions reported in columns 2 and 3 with very similar results. In particular, income never has a positive effect on democracy, and interestingly there is also no evidence of a positive relationship between education and democracy.

In addition, in regressions not reported here, we check for potential nonlinear interactions between income and other variables, and we find no evidence of such relationships.

Overall, the inclusion of fixed effects proxying for time-invariant and country-specific characteristics removes the entire cross-country correlation between income and democracy (and education and democracy). These results shed considerable doubt on the conventional wisdom that income has a strong causal effect on democracy.

5 Transitions to and from Democracy

In the previous two sub-sections we focused attention on the level of democracy as the dependent variable. Much of the empirical literature since the work of Przeworski and Limongi (1997) and Przeworski et al. (2000) has instead focused on estimating separate models for transitions to and away from democracy. In this section we investigate whether the findings in this literature are robust to the inclusion of fixed effects. We first investigate this question using a variety of linear models. We then develop and implement a double hazard model for the simultaneous estimation of transitions to democracy and transitions away from democracy. All of our various econometric strategies show that once fixed affects are included to control for time-invariant omitted variables simultaneously affecting both income and democracy, there is no evidence of an effect of income per capita on transitions to or away from democracy.

5.1 Linear Models

Standard analyses of transitions to and from democracy use dichotomous measures such as the Przeworski/Boix-Rosato data. Here we start with a more straightforward, and to the best of our knowledge novel, approach using the democracy scores in the Freedom House and Polity data. Our strategy is to modify the model in equation (2) as follows:

$$d_{it}^+ = \alpha d_{it-1} + \gamma y_{it-1} + \mathbf{x}_{it-1}' \boldsymbol{\beta} + \mu_t + \delta_i + u_{it} \tag{3}$$

for transitions to democracy and

$$d_{it}^- = \alpha d_{it-1} + \gamma y_{it-1} + \mathbf{x}_{it-1}' \boldsymbol{\beta} + \mu_t + \delta_i + u_{it} \tag{4}$$

for transitions from democracy, where $d_{it}^+ = \max\{d_{it}, d_{it-1}\}$ and $d_{it}^- = \min\{d_{it}, d_{it-1}\}$.[19] This procedure implies that for d_{it}^+, we only consider upward movements on the democracy score, and thus ignore declines in democracy. For d_{it}^-, we only consider deterioration in the democracy score. This approach therefore enables us to study increases and decreases in democracy separately, while still maintaining linearity.

Table 3 reports estimates of (3) while Table 4 reports estimates of (4). In both tables, panel A uses the Freedom House data while panel B uses the Polity data. Panel C of both tables uses the Przeworski dichotomous index where the formulas (3) and (4) also represent the model we estimate. Columns 1-6 of these two tables are analogous to columns 1-6 of Table 2 with the only differences being in the left hand side variable.

In the first columns of both tables we start with regressions without the fixed effects, the δ_i's, to replicate the results of the previous literature in our framework. The results in Table 3 using the pooled OLS approach show that, surprisingly and contrary to the claims of Przeworski and Limongi (1997), income per capita is positively associated with transitions to democracy. In line with Elkins (2000), Table 3 shows that even the basic results of Przeworski and Limongi (1997) are not entirely robust. In Table 4, we also find statistically significant correlations between income and transitions away from democracy with all three types of data (note that a positive coefficient in the transitions to non-democracy regressions means that higher income countries are less likely to experience coups).

Our main results, which add fixed effects, are presented in column 2 of Tables 3 and 4. The findings here are similar to those reported in Table 2. Once we introduce the fixed effects, income per capita is never significant. In Table 3, the coefficient on income in panel A is

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[19] Although (3) and (4) are nonlinear in d_{it}, they are linear in the parameters and in particular, in the fixed effects, the δ_i's. This implies that the fixed effects can be differenced out to achieve consistent estimation (without creating an incidental parameters problem).

similar to column 1, but no longer significant, whereas in Table 4 the coefficient is negative and insignificant.

Column 3 of both tables then turns to GMM estimation of the models with fixed effects. The estimates again show no evidence of an effect of income on either transitions to democracy or away from democracy. In fact, the estimated impact of income per capita on the likelihood of the transition to democracy or of remaining a democracy is negative in all specifications except in panel C of Table 3.

In column 4 we turn to the alternative strategy of using annual data. We again report the level of significance of an F-test on the joint significance of the lags of income per capita. The general picture here is that income per capita is insignificant although in panel B of Table 3 using the Polity dataset the variables are jointly significant at the 11% level. The final two columns of the table repeat columns 2 and 3 adding the same covariates as in Table 2 columns 5 and 6. The same message comes through here, once fixed effects are included income per capita is insignificant.

The results are consistent with those reported in Section 4. Though with pooled OLS the coefficient on income per capita is significant on transitions to and transitions away from democracy, once we add fixed effects, income is never significant in any specification.

In summary, when we control for the presence of country-specific omitted factors, there is little effect of income per capita either on transitions to democracy, or contrary to the emphasis in Przeworski et al. (2000), on transitions away from democracy.

5.2   Nonlinear Models

We have so far reported linear probability models of transitions to and away from democracy rather than nonlinear models of transitions because they are more transparent, simpler, and consistent under a weaker set of assumptions (see Wooldridge, 2002, chapter 15.2). Another advantage of the linear probability model is that standard panel data techniques can be used for consistent estimation in the presence of fixed effects (with large T). In contrast, because the conditional mean function in a nonlinear model is not linear in the parameters, the model with fixed effects *cannot* be estimated consistently (see, for example, Wooldridge, 2002, chapter 15.8). This makes the linear probability model with fixed effects a natural starting point for the analysis of transitions. Nevertheless, the political science literature and parts of the economics literature focus on nonlinear models of transitions. Though more complicated and somewhat more difficult to interpret, these nonlinear models also have advantages. In particular, they provide a better approximation to the structural form that might be generating the data on transitions to and away from democracy.

We now develop and estimate a nonlinear double hazard model to measure the impact of income on transitions to democracy and transitions away from democracy. The reason why we need to turn to a double hazard model rather than use existing approaches relying on probit or duration model analysis is that transitions to democracy or away from democracy are jointly determined events. They cannot be treated as separate events because whether or not an observation finds itself in the *at-risk* sample is endogenously determined. Our modest methodological contribution here is to develop a framework to deal with this issue which also allows the incorporation of fixed effects in a straightforward manner.

Our double hazard model can be expressed in terms of two conditional mean functions for the probability of transitioning to democracy and the probability of remaining in democracy:[20]

$$\Pr\left(d_{it} = 1 \mid d_{it-1} = 0, y_{it-1}, t\right) = \Phi\left(\gamma^{pos} y_{it-1} + \mu_t^{pos}\right) \tag{5}$$

$$\Pr\left(d_{it} = 1 \mid d_{it-1} = 1, y_{it-1}, t\right) = \Phi\left(\gamma^{neg} y_{it-1} + \mu_t^{neg}\right), \tag{6}$$

where Φ is an increasing function with a range between 0 and 1. Equation (5) describes the probability that a dictatorship collapses (transitions to democracy), and equation (6) describes the probability that a democracy survives, which is negatively related to the probability of a coup (transitions away from democracy). Together, these two equations characterize the law of motion of democracy for a given country, so that we can think of these equations as constituting a "double hazard model". The parameters γ^{pos} and γ^{neg} represent the effect of income on positive and negative transitions respectively, and μ_t^{pos} and μ_t^{neg} represent the time effects on positive and negative transitions, respectively. Note that equations (5) and (6) model the appropriate transitions to and away from democracy, but they do not yet introduce fixed country effects.

To make further progress, let us also assume that $\Phi\left(\cdot\right)$ is the normal cumulative distribution function, so that the system described by (5) and (6) is an *exponential* double hazard model. Since this system of equations characterizes the entire motion of democracy, it can easily be estimated by maximum likelihood.[21]

Table 5 reports estimates of (5) and (6) using the Przeworksi/Boix-Rosato dichotomous measures of democracy. Column 1 of Table 5 estimates (5) and (6) simultaneously on a balanced

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[20]Instead of (6), we could have alternatively written

$$\Pr\left(d_{it} = 0 \mid d_{it-1} = 1, y_{it-1}, t\right) = \Phi\left(\bar{\gamma}^{neg} y_{it-1} + \bar{\mu}_t^{neg}\right),$$

in which case we would have

$$\Pr\left(d_{it} = 1 \mid d_{it-1} = 1, y_{it-1}, t\right) = 1 - \Phi\left(\bar{\gamma}^{neg} y_{it-1} + \bar{\mu}_t^{neg}\right).$$

While these two specifications are econometrically equivalent, the interpretation of the parameters $\bar{\gamma}^{neg}$ and $\bar{\mu}_t^{neg}$ is less intuitive, making us prefer the system of equations given by (5) and (6).

[21]The likelihood function is straightforward to compute. For example, for a given country i, we have that $\Pr\left\{d_{i1}, ..., d_{iT} \mid y_{i0}, ..., y_{iT-1}\right\} = \Pr\left\{d_{iT} \mid d_{iT-1}, y_{iT-1}, T\right\} \times \Pr\left\{d_{iT-1} \mid d_{iT-2}, y_{iT-2}, T-1\right\} ... \times \Pr\left\{d_{i1} \mid d_{i0}, y_{i0}, 1\right\}$.

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panel and reports the estimates of the marginal effect of lagged income.[22] In panel A, we constrain $\gamma^{pos} = \gamma^{neg}$ and $\mu_t^{pos} = \mu_t^{neg}$. The estimates show a significant (cumulative) effect of income per capita on transitions to and away from democracy. In panel B, we allow $\gamma^{pos} \neq \gamma^{neg}$, while still constraining $\mu_t^{pos} = \mu_t^{neg}$. This is useful as a check of whether the impact of income differs in the two equations as emphasized by Przeworski and Limongi (1997) and Przeworski et al. (2000). Income per capita is significant for both transitions to and transitions away from democracy, though the coefficient on transitions away from democracy is higher and more significant, which is in line with the basic finding of these works. In panel C, we estimate the most flexible specification with $\gamma^{pos} \neq \gamma^{neg}$ and $\mu_t^{pos} \neq \mu_t^{neg}$. The estimates are again similar.

The major problem of the double hazard model, like all other models that are nonlinear in parameters, is that it cannot accommodate fixed effects. For example, if fixed effects are added, the right hand side of equation (5) changes to $\Phi\left(\gamma^{pos} y_{it-1} + \mu_t^{pos} + \delta_i^{pos}\right)$, and the right hand side of equation (6) changes to $\Phi\left(\gamma^{neg} y_{it-1} + \mu_t^{neg} + \delta_i^{neg}\right)$, where the δ_i's are the fixed effects for observation i. This specification creates an incidental parameters problem in the estimation of the δ_i's (because the number of parameters to be estimated increases at the same rate as the number of observations in the cross-section). This incidental parameters problem makes consistent estimation impossible. One solution to this problem is suggested by Mundlak (1978) and Chamberlain (1980), and it involves imposing a functional form on the δ_i's. Specifically, Chamberlain (1980) posits that

$$\Pr\left(\delta_i^j = \delta \mid y_{i1}, ... y_{iT}\right) = \Phi\left(\overline{\alpha}^j + \overline{y}_i \overline{\beta}^j\right), \, j = pos, neg \tag{7}$$

where $\overline{\alpha}^j$ and $\overline{\beta}^j$ are exogenous parameters, and $\overline{y}_i$ is the average of $y_{i\tau-1}$ for $\tau = 1, ..., T$. The important assumption is that the component of δ_i^j which is uncorrelated with $\overline{y}_i$ will be random in that it will not be correlated with d_{it}. As a consequence, we can write (incorporating the constant term $\overline{\alpha}^j$ into the time effects μ_t^j)

$$\Pr\left(d_{it} = 1 \mid d_{it-1} = 0, y_{it-1}, t\right) = \Phi\left(\gamma^{pos} y_{it-1} + \mu_t^{pos} + \overline{y}_i \overline{\beta}^{pos}\right) \tag{8}$$

$$\Pr\left(d_{it} = 1 \mid d_{it-1} = 1, y_{it-1}, t\right) = \Phi\left(\gamma^{neg} y_{it-1} + \mu_t^{neg} + \overline{y}_i \overline{\beta}^{neg}\right). \tag{9}$$

This specification is less flexible than including a full set of fixed effects, which was our strategy in the linear models, because it imposes considerable amount of structure on how unobserved heterogeneity (omitted time-invariant factors) affects democratic transitions. Consequently, this specification makes it less likely that we will be able to fully control for the effect of omitted variables simultaneously affecting income and democracy. Thus it makes it more likely that we

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[22] As is typically the case in studies like this we focus on a balanced panel. Our results do not change if we instead modify the exercise to consider an unbalanced panel. Details available upon request.

may still find a spurious positive effect of income on transitions to and away from democracy. Nevertheless, column 2 of Table 5 shows that even with this more restrictive Chamberlain hazard model, there is no effect of income per capita on transitions to or away from democracy. Once again, in panel A, we constrain $\gamma^{pos} = \gamma^{neg}$, $\mu_t^{pos} = \mu_t^{neg}$, and $\overline{\beta}^{pos} = \overline{\beta}^{neg}$. In panel B, we allow $\gamma^{pos} \neq \gamma^{neg}$ but we constrain $\mu_t^{pos} = \mu_t^{neg}$ and $\overline{\beta}^{pos} = \overline{\beta}^{neg}$. In panel C, we allow $\gamma^{pos} \neq \gamma^{neg}$, $\mu_t^{pos} \neq \mu_t^{neg}$, and $\overline{\beta}^{pos} \neq \overline{\beta}^{neg}$. In all of these panels, the effect of income per capita is reduced and is insignificant. Overall, there is no evidence that income per capita has a causal effect on transitions to or away from democracy once we include controls for omitted variables simultaneously affecting the evolution of income and democracy.

Columns 3 and 4 are analogous to columns 1 and 2 on an annual balanced sample, and achieve similar results. Column 5 adds lagged population and lagged education to the sample of columns 1 and 2, where the averages of lagged population and lagged education are used in the calculation of (7), and again, income per capita has no effect on transitions to democracy or transitions away from democracy.

These results thus establish that the source of the difference between our results and those of Przeworski et al. (2000) are due to the presence of fixed effects, and are not related to differences in the measures of democracy or estimation methods. All in all, the results in the last two sections show that no matter what estimation approach one takes, controlling for omitted variables simultaneously affecting income and democracy–either by including a full set of fixed country effects or by using the parameterized approach of Chamberlain, 1980– removes the empirical relationship between income per capita and democracy. These results shed considerable doubt on the existence of a causal effect of income on democracy which has been the plinth of modernization theory for almost 50 years.

6   Comparison to the Existing Literature

The empirical results we present in this paper stand in contrast to the conventional wisdom and the previous literature. In this section we discuss in more detail why our results differ.

Beginning with Lipset (1959) most scholars have adopted a simple empirical strategy to evaluate the determinants of democracy. Much cited papers by Jackman (1973), Bollen (1979), and Muller (1995) look for the cross-sectional relationship between per capita income and other covariates and some measure of democracy. In such studies, a particular date is chosen, and each country constitutes one observation. An advance over these approaches was the addition of time-series data in the paper by Burkhart and Lewis-Beck (1994) who pooled time-series data with cross-sectional data. This latter approach is common in many contemporary studies (Barro, 1997, 1999, Ross, 2001, Fish, 2003). That our results are different from this literature is

not very surprising given that these papers do not investigate the possibility that the correlation between income and democracy may be generated by omitted variables affecting both variables. The results we have presented so far make it clear that the source of difference between our findings and these studies is the presence of fixed effects in our models. Both because there are likely to be many potential omitted factors, and also because the alternative critical junctures hypothesis emphasizes the importance of time-invariant historical factors, we believe that a fixed effects specification is more appropriate, and that our results are likely to be closer to the causal effect of income on democracy.[23]

Several more recent empirical studies have used dynamic probit models to investigate the determinants of transitions to and away from democracy. Przeworski et al. (2000), Boix (2003), Boix and Stokes (2003), Epstein, Bates, Goldstone, Kristensen, and O'Halloran (2004) and Papaioannou and Siourounis (2004), for example, find a positive relationship between income and democracy using such an approach (though Przeworski et al., 2000, emphasizes the effect of income on coups rather than on democratizations). As the last sub-section documented, the critical difference is that these previous empirical specifications do not include fixed effects and do not control for other cross-country differences potentially affecting income and democracy.

Another important and influential paper relevant to our investigation is Londregan and Poole (1996), which is the only paper we are aware of that estimates models of democracy with fixed effects. A natural conjecture would have been that their results would parallel ours. However, they also report positive and significant effects of income on democracy. Although Londregan and Poole (1996) use maximum likelihood estimation in a two-equation model, their equation for democracy is basically linear. Appendix Table A2 reports their basic result in column 1, together with our linear estimation of their model in column 2.[24] The estimates are very close, in fact practically identical, to those in their original paper. In column 3, we omit Londregan and Poole's "transition" variable, which is an index giving a value of 1 to countries with missing lagged democracy values, making their specification more comparable to ours.[25] This shows that the results do not change much as a result of this exclusion.

So why are these estimates so different from our results in the previous tables? The answer is twofold. First, Londregan and Poole (1996) estimate their model on annual data, but do not correct the standard errors for the serial correlation in the residuals. Second, they do not

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[23] In fact, to the extent that there are time-varying omitted variables, we would expect changes in these omitted variables to have a positive effect on both income and democracy, so that even fixed effects estimators may lead to results that are upwardly biased. Therefore, one may want to interpret our results as providing an upper bound on the causal effect of income on democracy.

[24] We use their data, both to increase comparability (there have been revisions to the data since then), and also to include the additional variables they have put together.

[25] Our results are similar if we do not drop the transition variable, and in column 5 add five lags of this variable together with five lags of GDP per capita and democracy.

include time effects. Lack of time effects in their models implies that their estimate of the effect of income may partly pick up the common trend in democracy and income. Equally important, their standard errors are significantly biased downward because of serial correlation in the residual.[26] Column 4 adds a full set of time effects, and corrects the standard errors by clustering on country. Now, log income per capita is no longer significant, though it continues to be positive. However, this positive estimate is also difficult to interpret, since the regression also includes income per capita growth, which is estimated to have a negative coefficient. For this reason, in column 5, we estimate their equation in line with our annual regressions (column 4 in Table 2), including five lags of democracy and five lags of income per capita (which is naturally much more flexible than including the one lag and two growth terms). Now, all of the income terms are insignificant. We therefore conclude that Londregan and Poole's (1996) results are also consistent with ours.

7 Understanding the Fixed Effects Results

In the introduction, we argued that the fixed effects results are consistent with the hypothesis that the (long run) political and economic development paths of societies are intimately linked. Political institutions and the distribution of resources in a society determine the distribution of political power which then influences both the economic institutions today and the evolution of future political institutions. Economic institutions, in turn, determine both the aggregate economic performance of the society and how its resources will be distributed. There is therefore a natural complementarity between political and economic institutions. Economies grow if their economic institutions encourage investment and innovation, for example, by providing secure property rights and equality before the law; but this can only happen when those controlling political power (the political elites) are constrained. We should thus expect democracy to be associated with economic institutions that foster growth. Moreover, if events at some critical juncture create a divergence in the political and economic institutions of a set of societies, we may expect these differences to persist over time; some of these societies may embark on a path to high income and democracy, while others experience relative stagnation and non-democracy.[27]

According to this theory, the paths of economic and political development are intertwined,

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[26] To see why this is an important concern, consider a hypothetical dataset in which we have minute-by-minute data on democracy and income. Since the number of observations in this dataset would be near infinite, the standard error estimates without correcting for serial correlation would be essentially equal to zero and any estimated coefficient would be highly significant. However, it is clear that the standard errors are not really equal to zero in this case, because democracy and income from one minute to another are not independent observations; they are highly serially correlated. To obtain an unbiased estimate of the variance-covariance matrix, one needs to allow for an arbitrary pattern of serial correlation by country, which is what clustering achieves.

[27] See the discussion of the role of critical junctures in Collier and Collier (1991).

and we expect democracy and income to evolve jointly. Nevertheless, conditional on a given development path, economic growth does not necessarily lead to democratization.[28] This reasoning suggests that the fixed effects estimated in the previous section should be closely linked to the underlying institutional development paths and to the factors affecting what type of path a society has followed. We now investigate this question.

7.1 Divergent Development Paths Among the Colonies

Acemoglu, Johnson, and Robinson (2001, 2002) document that factors affecting the profitability of different institutional structures for European colonizers had a major impact on early institutions and on subsequent political and economic development in former European colonies. Based on their ideas, we expect countries with high rates of settler mortality and higher indigenous population density in 1500 to have experienced greater extraction of resources and repression by Europeans, and consequently to be less democratic today. However, both population density in 1500 and European settler mortality rates are subject to a large amount of measurement error, and they are only some of the influences on the ultimate choice of development path. For example, for various reasons, Europeans opted for extractive institutions in many areas, such as Brazil, with low population density. Therefore, a direct measure of institutions immediately after the end of the colonial period is also useful to gauge the effect of the historical development paths on current outcomes. For this reason, we look at the measure of constraint on the executive from the Polity IV dataset right after independence for each former colony, measured as the average score during the first ten years after independence. This is the closest variable we have to a measure of institutions during colonialism. We normalize this score to a 0 to 1 scale like democracy, with 1 representing the highest constraint on the executive.[29] Finally, we also control for the date of independence. This is useful because constraint on the executive at different dates of independence may mean different things. In addition and potentially more importantly, countries where Europeans settled and developed secure property rights and more democratic institutions typically gained their independence earlier than colonies with extractive institutions. Another important effect of the date of independence on political and economic development might be that former colonies undergo a relatively lengthy period of instability after independence, adversely affecting both growth prospects and democracy.

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[28]Similarly, there is no natural presumption that, conditional on a particular development path, a temporary improvement in the democracy score should lead to higher incomes.

[29]For example, Peru had a constraint on the executive score equal to 0.33, while the United States's score was 1 at independence. These numbers are clearly indicative of the institutions that these countries had within the colonial period itself.

7.2 Historical Variables and Fixed Effects

Our basic results with the former colony sample are in Table 6. Table 6 has three panels which take the fixed effects from our basic fixed effects OLS regressions in Table 2, column 2. As a caveat, we remind the reader that in the regressions in Table 2, the fixed effects are not consistently estimated, because the time dimension, T, is small. Therefore, these results should only be interpreted as suggestive of a general pattern. The first column of panel A shows that the fixed effect for the level of democracy using the Freedom House measure of democracy is negatively correlated with population density in 1500. The higher was historical population density, the lower is the fixed effect today. Population density is highly significant with a t-statistic of almost 4. In the column 2 panel A we then investigate the effect of settler mortality. This again has the expected sign and greater historical mortality of European settlers is negatively associated with the democracy fixed effect. Column 3 then introduces our two variables which should capture historical institutions more directly. The results in this column show that greater constraint on the executive at independence is significantly associated with a greater fixed effect, and a more recent independence date is significantly associated with a lower fixed effect. In column 4 we include all of the historical variables. When we do so, settler mortality loses significance, and so in column 5 we include just the three significant variables from column 4. The historical variables are highly significant with the expected signs and explain 63% of the variation in the fixed effects.

The robustness of the relationships found in Table 6 can be illustrated with scatterplots. In Figures 6-9 we plot the fixed effect from Table 2 column 2 panel A against the four historical variables. These figures show that there are distinct relationships between these historical variables and the fixed effects which strongly supports our interpretations of our findings.

Panels B and C of Table 6 are similar to panel A and examine the fixed effects from regressions which use Polity and Przeworski measures of democracy. The results are very similar to those in panel A. The historical variables are all jointly significant but not always individually significant. The main difference is that population density loses significance when it is included with constraint on the executive and independence year when we consider the Przeworski measures of democracy.

Many other time-invariant slow-moving characteristics of a society may also influence its propensity to be democratic. In Table 7, we investigate whether some obvious candidates might be responsible for some of our findings and whether they alter the explanatory power of our historical variables in the sample of former colonies. In particular, we add a number of additional control variables to the three regressions from column 5 of Table 6. In each panel, we report the coefficient or the F-test for significance of the additional variables, and at the bottom, we

report the F-test for the joint significance of our historical variables from Table 6 to demonstrate the robustness of the findings there (we do not report the coefficient of each of these historical variables to save space).

Many scholars have argued that countries formally colonized by the British inherit a culture more compatible with democracy (Weiner, 1987, Muller, 1995). Column 1 of Table 7 investigates this issue by adding a set of dummy variables capturing the identity of the colonial power. None of these variables are individually significant (the coefficient estimates are not reported to save space) and an F-test also reveals that they are jointly insignificant in all of the three regressions. Contrary to these widespread views in the literature, there is no evidence here that having been colonized by the British relative to some other power tends to promote democracy, facilitate democratizations, or reduce the propensity of a society to experience coups.[30] When these colonial dummies are added the impact of the variables proxying for the determinants of the development path is unchanged.

Another popular argument points to religion as an important determinant of political development, suggesting that countries whose population is predominantly Muslim are less likely to be democratic (e.g. Huntington, 1991, Fish, 2002). Column 2 investigates this issue by adding the proportions of the population that are Catholic, Protestant and Muslim. As with variables capturing colonial origins, there is no evidence here that religion influences the democracy fixed effect in the sample of former colonies.[31] None of the individual coefficients are significant and an F-test again shows them to be jointly insignificant.

Column 3 adds the absolute value of latitude (distance from the equator), a popular proxy for geographic effects on economic development, and shows that latitude is insignificant and does not alter the joint impact of the historical variables. Column 4 then examines the significance of a dummy variable which captures whether or not the country is a major oil producer. It has been widely argued in political science that countries dominated by natural resources tend to be less democratic (Ross, 2001), though recent work contests this (e.g., Dunning, 2007). This variable is

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[30] This is despite the fact that there appears to be an oft-emphasized correlation between democracy and having been a British colony. Britain became a colonial power later than Spain and Portugal and even the Netherlands. When it finally expanded into the world many of the places which were promising colonies, such as Latin America, were taken. Thus, by being a latecomer, Britain was forced to colonize what at the time appeared relatively unappealing places to colonize, such as the United States or subsequently Australia. It was precisely these relatively unattractive places which had initial conditions which did not lend themselves to the creation of extractive colonies. But it was not because it was colonized by the British that led the United States to become democratic, but rather how the initial conditions moulded the formation of institutions. Once the influence of these initial conditions is controlled for, there is no additional positive effect of British colonization on democracy.

[31] A dummy variable denoting whether the fraction of the population which is Muslim exceeds 20% is nevertheless significant if population density is excluded. Note that religion might still be an important determinant of the development path in the entire world sample without being important in the former colony sample. In addition, since these results are for a set of former European colonies which exclude the Middle East, our results cannot convincingly settle the issue of the relationship between Islam and democracy.

also not significant in any panels of the table. Finally we examine the impact of ethnolinguistic fragmentation. The final column of Table 7 shows that conditional on the historical variables, greater ethnolinguistic fragmentation is negatively correlated with the fixed effects in panel A, though it is only statistically significant when we use the Freedom House measure of democracy. In all panels, the addition of this variable has no impact on the significance or explanatory power of our historical variables.

Overall, this section has provided evidence that is consistent with our interpretation of the fixed effects results as capturing the effect of time-invariant, historical variables simultaneously affecting the evolution of income and democracy. It has documented that various historical variables that proxy for the factors influencing the subsequent evolution of institutions in former European colonies are closely related to the fixed effects from the democracy regressions. This pattern is consistent with the general thrust of the critical junctures hypothesis.

8   Democracy and Income in the Long Run

We have so far followed much of the existing literature in focusing on the post-war period, where the democracy and income data are of higher quality. Nevertheless, it is important to investigate whether the relationship between income and democracy emerges only over a longer period of time. This is of interest because when Lipset (1959) formulated modernization theory in the late 1950s, he probably had in mind the connection between industrialization, income growth, and democracy that seemed to characterize the development experiences of the late nineteenth and early twentieth centuries. This point is emphasized by Boix and Stokes (2003), who argue that the post-war sample misses the democratization of much of Western Europe and North America in the late nineteenth century.

Although historical data are typically less reliable, the Polity IV dataset extends back to the beginning of the nineteenth century for all independent countries, as does the Boix-Rosato extension of Przeworski et al.'s dataset, and Maddison (2003) gives estimates of income per capita for many countries during this period. We therefore construct a data set starting from 1875, where we study the data in 25 year intervals in order to maximize the cross-section of countries which can be observed. We construct a balanced panel of countries for which democracy, lagged democracy (calculated 25 years earlier), and lagged income (calculated 25 years earlier) are available for every 25th year between 1875 and 2000.[32] The result is a sample of 25 countries for

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[32] For reasons of data availability, we assign income per capita in 1820 to 1850, income per capita in 1870 to 1875, and income per capita in 1929 to 1925. All of our results are robust to dropping the 1875 observation so as to not use the 1850 estimate of income per capita as the value of lagged income. For all observations, if income per capita is not available for a particular observation, it is estimated at the lowest aggregation level which it is available, and the regressions are clustered by the highest aggregation level assigned to a particular country. We

the regressions using the Polity measure and a sample of 30 countries for the regressions using the Przeworski/Boix-Rosato measure.[33]

In Table 8 we present our fixed effects results with this long run panel. The specifications of columns 1-3 in Table 8 are identical to the specifications of columns 1-3 of Table 2 over the long 25 year sample where the dependent variable is the Polity index. In columns 4-6, the dependent variable is the Przeworski/Boix-Rosato index. The results in this table are very similar with either measure of democracy. Columns 1 and 4 report the basic pooled OLS regressions without fixed effects. These show the usual findings since income per capita has a positive coefficient and is strongly significant. Columns 2 and 5 then add the fixed effects, and the introduction of fixed effects makes income per capita insignificant. In columns 3 and 6, the use of the Arellano-Bond estimator causes income to have the wrong (negative) sign.

In Table 9 we examine whether there is a relationship between transitions to democracy and transitions away from democracy in this long run panel using the dichotomous Przeworski/Boix-Rosato measure of democracy. We again implement the double hazard model introduced in Section 5.2. As before, we estimate the three possible models with differing degrees of flexibility in cross-equation restrictions.[34] As in the post-war panel, without fixed effects the effect of income is large and significant on transitions to democracy and transitions away from democracy. However, once again when we include fixed effects to control for omitted variables simultaneously affecting the evolution of income and democracy, the relationship between income per capita and transitions to and away from democracy becomes insignificant.

The conclusion from this investigation is that the long run historical evolution of countries is similar to the evolution of countries in the post-1960 sample. Once we control for fixed effects, there is no significant relationship between income per capita and democracy.

9   Conclusion

There is a general consensus in the empirical political science literature that the modernization hypothesis provides a good approximation of the relationship between economic growth and democratization. In particular, most empirical studies conclude that there is a causal effect of per capita income on democracy, though this may perhaps only work through the process

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also assign the 1994 Przeworski/Boix-Rosato democracy score to 2000.

[33] Countries in both samples are Argentina, Austria, Belgium, Brazil, Chile, China, Colombia, Costa Rica, Denmark, El Salvador, Greece, Guatemala, Honduras, Mexico, Netherlands, Nicaragua, Norway, Sweden, Switzerland, Thailand, Turkey, United Kingdom, United States, Uruguay, Venezuela. The sample with Przeworski/Boix-Rosato measure additionally includes France, Japan, Peru, Portugal, and Spain.

[34] Specifically, Columns 1 and 2 correspond to the specifications of columns 1 and 2 of panel A of Table 5; columns 3 and 4 correspond to the specifications of columns 1 and 2 of panel B of Table 5; and columns 5 and 6 correspond to the specifications of columns 1 and 2 of panel C of Table 5.

of democratic consolidation. In this paper, we argue that, though income and democracy are positively correlated, there is no evidence of a significant causal effect of income on democracy. Instead, omitted, most probably historical, factors appear to have shaped the divergent political and economic development paths of various societies, leading to the positive association between economic performance and democracy.

The previous literature did not reach this conclusion because most papers did not control for the endogeneity of income per capita. This empirical methodology is potentially problematic because it fails to control for omitted variables correlated with both income and democracy. This is particularly concerning, since an alternative hypothesis, the critical junctures theory, implies that such omitted variables are responsible for the correlation between income and democracy. We show that when we control for these omitted variables by including fixed effects or by using other econometric techniques, the effect of income per capita on democracy disappears. The most plausible interpretation of our findings is that previous research has suffered from omitted variable biases. We then show that in the sample of former European colonies, where we have measures of the historical sources of variation in development paths, the fixed effects indeed capture the impact of historical differences which researchers have shown can account for economic and institutional divergence.

Our overall conclusion is that the relationship between income and democracy and the widely-accepted modernization hypothesis need to be reevaluated, with much greater emphasis on the underlying factors affecting both variables and the political and economic development path of societies. Our results indicate that countries have embarked upon different development paths, most likely at some critical junctures during their histories, and while some paths have led to democracy and prosperity, some others involved non-democracy and relative poverty. Although democracy and prosperity coevolve along the "virtuous" development path, there is no evidence that income has a causal effect on democracy conditional on the development path. Consequently, there is no reason to expect income changes over 5, 10 or even 20 year intervals observed during the post-war era to lead to significant changes in regimes above and beyond those experienced by the world as a whole. Though this conceptual approach differs from that proposed by Lipset (1959) and intensively pursued by the empirical literature, it is close to that developed by Moore (1966) and elaborated by many non-quantitative scholars.

Our results should not be interpreted as implying that historical factors (or time-invariant factors captured by fixed effects) are the only or the major determinant of democracy today. There is a large amount of variability in democracy across countries that is not explained by our historical variables and also a substantial amount of over-time variability in the democracy score of a country that still needs to be understood and accounted for. For example, it remains true

that over time there is a general tendency towards greater incomes and education, and increased political participation across the world. In our regressions, time effects capture these general (world-level) tendencies. Our estimates suggest that these world-level movements in democracy are unlikely to be driven by the causal effect of income and education on democracy. Just what is causing them is a subject for future research. One possibility, for example, is that there are important diffusion effects (see for instance, Simmons and Elkins, 2004) so that the democracy level of one country interacts with that of its neighbors.

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Table 1
Descriptive Statistics

	All countries	Former Colonies	High Income Countries	Low Income Countries
	(1)	(2)	(3)	(4)
Freedom House Measure of Democracy	0.55 (0.36)	0.47 (0.34)	0.74 (0.31)	0.35 (0.29)
Polity Measure of Democracy	0.54 (0.38)	0.47 (0.36)	0.75 (0.34)	0.34 (0.31)
Przeworski Measure of Democracy	0.43 (0.49)	0.32 (0.47)	0.68 (0.47)	0.18 (0.38)
Log GDP per Capita (Chain Weighted 1996 Prices)	8.14 (1.03)	7.82 (0.89)	9.01 (0.58)	7.27 (0.51)
Constraint on the Executive at Independence		0.40 (0.35)		
Independence Year		1918 (63)		
Log Population Density in 1500		0.45 (1.58)		
Log Settler Mortality		4.76 (1.24)		
Observations	822	557	411	411

Values are averages during sample period, with standard deviations in parentheses, where the sample is from 1960-2000 and data is included in five year intervals. All countries are those for which democracy--using all three potential measures--and income per capita in five-year intervals are available at least once during 1960-2000. Column 1 refers to the world sample, and column 2 refers to the sample of former colonies. Columns 3 and 4 split the sample in column 1 by the median income (from Penn World Tables 6.1) in the sample of column 1. Freedom House Measure of Democracy is the Political Rights Index, augmented following Barro (1999). Polity Measure of Democracy is Democracy Index minus Autocracy Index from Polity IV. Przeworski Measure of Democracy is from Boix-Rosato (2001). GDP per capita in 1996 prices with PPP adjustment is from the Penn World Tables 6.1. Constraint on the Executive at Independence is from Polity. Year of independence is from the CIA World Factbook. Log Population Density in 1500 is from Acemoglu, Johnson, and Robinson (2002). Log Settler Mortality is from Acemoglu, Johnson, and Robinson (2001). Former colonies is the subsample colonized by European powers before 1900. For detailed definitions and sources, see Appendix Table A1.

Table 2
Fixed Effects Results using Democracy

	Base Sample, 1960-2000					
	5-year data			Annual data	5-year data	
	Pooled OLS	Fixed Effects OLS	Arellano-Bond GMM	Fixed Effects OLS	Fixed Effects OLS	Arellano-Bond GMM
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A	Dependent Variable is Freedom House Measure of Democracy					
Democracy $_{t-1}$	0.703	0.377	0.489	[0.00]	0.362	0.508
	(0.036)	(0.052)	(0.085)		(0.056)	(0.093)
Log GDP per Capita $_{t-1}$	0.073	0.008	-0.129	[0.33]	-0.038	-0.153
	(0.010)	(0.034)	(0.076)		(0.042)	(0.133)
Log Population $_{t-1}$					-0.019	0.016
					(0.083)	(0.119)
Education $_{t-1}$					-0.012	-0.025
					(0.019)	(0.024)
Observations	955	955	838	2896	685	589
Countries	150	150	127	148	96	92
R-squared	0.72	0.79		0.93	0.76	
Panel B	Dependent Variable is Polity Measure of Democracy					
Democracy $_{t-1}$	0.748	0.447	0.590	[0.00]	0.453	0.633
	(0.034)	(0.063)	(0.106)		(0.068)	(0.112)
Log GDP per Capita $_{t-1}$	0.053	-0.003	-0.351	[0.53]	-0.006	-0.229
	(0.010)	(0.038)	(0.127)		(0.044)	(0.186)
Log Population $_{t-1}$					0.160	0.156
					(0.081)	(0.106)
Education $_{t-1}$					-0.028	-0.027
					(0.021)	(0.028)
Observations	856	856	747	3705	643	541
Countries	136	136	114	134	93	91
R-squared	0.77	0.82		0.96	0.80	
Panel C	Dependent Variable is Przeworski Measure of Democracy					
Democracy $_{t-1}$	0.679	0.318	0.457	[0.00]	0.293	0.389
	(0.046)	(0.058)	(0.092)		(0.062)	(0.106)
Log GDP per Capita $_{t-1}$	0.097	0.051	-0.017	[0.77]	0.052	0.107
	(0.018)	(0.055)	(0.138)		(0.088)	(0.233)
Log Population $_{t-1}$					0.066	0.301
					(0.144)	(0.206)
Education $_{t-1}$					-0.012	-0.045
					(0.045)	(0.040)
Observations	862	862	792	3720	619	524
Countries	123	123	118	119	95	93
R-squared	0.67	0.76		0.92	0.75	

Pooled cross-sectional OLS regression in column 1, with robust standard errors clustered by country in parentheses. Fixed effects OLS regressions in columns 2, 4, and 5, with country dummies and robust standard errors clustered by country in parentheses. Columns 3 and 6 use GMM of Arellano and Bond (1991), with robust standard errors; in both columns we instrument for income using a double lag. Year dummies are included in all regressions. Dependent variable is Freedom House Measure of Democracy in panel A; Polity Measure of Democracy in panel B; and Przeworski Measure of Democracy in panel C. Base sample in columns 1, 2, 3, 5, and 6 is an unbalanced panel, 1960-2000, with data at 5-year intervals, where the start date of the panel refers to the dependent variable (i.e., t=1960, so t-1=1955); column 4 uses annual data from the same sample. In column 4, each right hand side variable has five annual lags; we report the p-value from an F-test for the joint significance of all 5 lags. For detailed data definitions and sources see Table 1 and Appendix Table A1.

Table 3
Fixed Effects Results using Transitions to Democracy

| | Base Sample, 1960-2000 | | | | | |
| | 5-year data | | | Annual data | 5-year data | |
	Pooled OLS (1)	Fixed Effects OLS (2)	Arellano-Bond GMM (3)	Fixed Effects OLS (4)	Fixed Effects OLS (5)	Arellano-Bond GMM (6)
Panel A	*Dependent Variable is Freedom House Measure of Transitions to Democracy*					
Democracy $_{t-1}$	0.820	0.656	0.761	[0.00]	0.634	0.738
	(0.024)	(0.031)	(0.049)		(0.035)	(0.057)
Log GDP per Capita $_{t-1}$	0.021	0.019	-0.022	[0.30]	-0.009	0.033
	(0.007)	(0.023)	(0.046)		(0.025)	(0.075)
Log Population $_{t-1}$					-0.003	0.083
					(0.045)	(0.065)
Education $_{t-1}$					-0.013	-0.033
					(0.012)	(0.020)
Observations	955	955	838	2896	685	589
Countries	150	150	127	148	96	92
R-squared	0.84	0.88		0.96	0.86	
Panel B	*Dependent Variable is Polity Measure of Transitions to Democracy*					
Democracy $_{t-1}$	0.851	0.674	0.813	[0.00]	0.668	0.829
	(0.023)	(0.043)	(0.061)		(0.049)	(0.066)
Log GDP per Capita $_{t-1}$	0.015	-0.016	-0.237	[0.11]	-0.013	-0.056
	(0.008)	(0.031)	(0.073)		(0.032)	(0.106)
Log Population $_{t-1}$					0.130	0.176
					(0.052)	(0.069)
Education $_{t-1}$					-0.029	-0.045
					(0.014)	(0.022)
Observations	856	856	747	3705	643	541
Countries	136	136	114	134	93	91
R-squared	0.85	0.88		0.97	0.87	
Panel C	*Dependent Variable is Przeworski Measure of Transitions to Democracy*					
Democracy $_{t-1}$	0.842	0.582	0.753	[0.00]	0.568	0.721
	(0.027)	(0.042)	(0.064)		(0.047)	(0.079)
Log GDP per Capita $_{t-1}$	0.040	0.009	0.009	[0.91]	0.012	0.220
	(0.014)	(0.042)	(0.112)		(0.059)	(0.174)
Log Population $_{t-1}$					0.034	0.274
					(0.087)	(0.134)
Education $_{t-1}$					-0.027	-0.080
					(0.029)	(0.035)
Observations	862	862	792	3720	619	524
Countries	123	123	118	119	95	93
R-squared	0.77	0.84		0.95	0.84	

Pooled cross-sectional OLS regression in column 1, with robust standard errors clustered by country in parentheses. Fixed effects OLS regressions in columns 2, 4, and 5, with country dummies and robust standard errors clustered by country in parentheses. Columns 3 and 6 use GMM of Arellano and Bond (1991), with robust standard errors; in both columns we instrument for income using a double lag. Year dummies are included in all regressions. Dependent variable is Freedom House Measure of Transitions to Democracy in panel A; Polity Measure of Transitions to Democracy in panel B; and Przeworski Measure of Transitions to Democracy in panel C, where the construction of the variables is described in Section 5.1 of the text. Base sample in columns 1, 2, 3, 5, and 6 is an unbalanced panel, 1960-2000, with data at 5-year intervals, where the start date of the panel refers to the dependent variable (i.e., t=1960, so t-1=1955); column 4 uses annual data from the same sample. In column 4, each right hand side variable has five annual lags; we report the p-value from an F-test for the joint significance of all 5 lags. For detailed data definitions and sources see Table 1 and Appendix Table A1.

Table 4
Fixed Effects Results using Transitions away from Democracy

| | Base Sample, 1960-2000 | | | | | |
| | 5-year data | | | Annual data | 5-year data | |
	Pooled OLS (1)	Fixed Effects OLS (2)	Arellano-Bond GMM (3)	Fixed Effects OLS (4)	Fixed Effects OLS (5)	Arellano-Bond GMM (6)
Panel A	*Dependent Variable is Freedom House Measure of Transitions away from Democracy*					
Democracy $_{t-1}$	0.882	0.721	0.728	[0.00]	0.729	0.770
	(0.022)	(0.035)	(0.056)		(0.037)	(0.055)
Log GDP per Capita $_{t-1}$	0.052	-0.011	-0.108	[0.19]	-0.029	-0.186
	(0.007)	(0.017)	(0.054)		(0.026)	(0.102)
Log Population $_{t-1}$					-0.016	-0.067
					(0.049)	(0.085)
Education $_{t-1}$					0.001	0.008
					(0.011)	(0.014)
Observations	955	955	838	2896	685	589
Countries	150	150	127	148	96	92
R-squared	0.90	0.92		0.97	0.91	
Panel B	*Dependent Variable is Polity Measure of Transitions away from Democracy*					
Democracy $_{t-1}$	0.898	0.773	0.776	[0.00]	0.786	0.805
	(0.022)	(0.040)	(0.067)		(0.039)	(0.067)
Log GDP per Capita $_{t-1}$	0.038	0.013	-0.114	[0.34]	0.006	-0.172
	(0.008)	(0.016)	(0.073)		(0.027)	(0.120)
Log Population $_{t-1}$					0.030	-0.020
					(0.047)	(0.073)
Education $_{t-1}$					0.001	0.017
					(0.013)	(0.014)
Observations	856	856	747	3705	643	541
Countries	136	136	114	134	93	91
R-squared	0.92	0.94		0.98	0.93	
Panel C	*Dependent Variable is Przeworski Measure of Transitions away from Democracy*					
Democracy $_{t-1}$	0.837	0.736	0.705	[0.00]	0.725	0.668
	(0.031)	(0.044)	(0.054)		(0.047)	(0.061)
Log GDP per Capita $_{t-1}$	0.057	0.042	-0.026	[0.48]	0.040	-0.113
	(0.013)	(0.028)	(0.092)		(0.046)	(0.168)
Log Population $_{t-1}$					0.032	0.027
					(0.077)	(0.137)
Education $_{t-1}$					0.015	0.034
					(0.024)	(0.022)
Observations	862	862	792	3720	619	524
Countries	123	123	118	119	95	93
R-squared	0.88	0.91		0.97	0.90	

Pooled cross-sectional OLS regression in column 1, with robust standard errors clustered by country in parentheses. Fixed effects OLS regressions in columns 2, 4, and 5, with country dummies and robust standard errors clustered by country in parentheses. Columns 3 and 6 use GMM of Arellano and Bond (1991), with robust standard errors; in both columns we instrument for income using a double lag. Year dummies are included in all regressions. Dependent variable is Freedom House Measure of Transitions away from Democracy in panel A; Polity Measure of Transitions away from Democracy in panel B; and Przeworski Measure of Transitions away from Democracy in panel C. The construction of the variables is described in Section 5.1 of the text. Base sample in columns 1, 2, 3, 5, and 6 is an unbalanced panel, 1960-2000, with data at 5-year intervals, where the start date of the panel refers to the dependent variable (i.e., t=1960, so t-1=1955); column 4 uses annual data from the same sample. In column 4, each right hand side variable has five annual lags; we report the p-value from an F-test for the joint significance of all 5 lags. For detailed data definitions and sources see Table 1 and Appendix Table A1.

Table 5

Hazard Model using Przeworski Measure of Transitions to and away from Democracy

	Balanced Panel, 1965-1995				
	5-year data		Annual data		5-year data
	Exponential Hazard (1)	Chamberlain Exponential Hazard (2)	Exponential Hazard (3)	Chamberlain Exponential Hazard (4)	Chamberlain Exponential Hazard (5)
Panel A: Constrained Model	*Dependent Variable is Transitions to and away from Democracy*				
Log GDP per Capita$_{t-1}$	0.412	0.014	0.397	-0.052	-0.044
	(0.047)	(0.099)	(0.046)	(0.095)	(0.139)
Log Population$_{t-1}$					-0.263
					(0.277)
Education$_{t-1}$					0.030
					(0.071)
Observations	735	735	3180	3180	588
Countries	105	105	106	106	88
Panel B: Partially Constrained Model	*Dependent Variable is Transitions to and away from Democracy*				
Log GDP per Capita$_{t-1}$ on Transitions to Democracy	0.180	-0.050	0.080	-0.157	-0.081
	(0.039)	(0.113)	(0.028)	(0.089)	(0.148)
Log GDP per Capita$_{t-1}$ on Transitions away from Democracy	0.288	0.056	0.265	0.027	0.017
	(0.034)	(0.112)	(0.026)	(0.089)	(0.148)
Log Population$_{t-1}$					0.012
					(0.293)
Education$_{t-1}$					0.018
					(0.084)
Observations	735	735	3180	3180	588
Countries	105	105	106	106	88
Panel C: Unconstrained Model	*Dependent Variable is Transitions to and away from Democracy*				
Log GDP per Capita$_{t-1}$ on Transitions to Democracy	0.147	-0.101	0.085	-0.112	-0.135
	(0.049)	(0.108)	(0.029)	(0.077)	(0.148)
Log GDP per Capita$_{t-1}$ on Transitions away from Democracy	0.344	0.341	0.208	-0.010	0.308
	(0.089)	(0.279)	(0.049)	(0.099)	(0.336)
Log Population$_{t-1}$					0.017
					(0.085)
Education$_{t-1}$					0.219
					(0.349)
Observations	686	686	2062	2062	540
Countries	105	105	106	106	88

Exponential double hazard model in columns 1 and 3 and Chamberlain exponential double hazard model in columns 2, 4, and 5. Coefficients correspond to average marginal effects. Robust standard errors clustered by country in parentheses. Year dummies are included in all regressions. Dependent variable is Przeworski Measure of Democracy. Base sample in columns 1 and 2 is a balanced panel 1965-1965 with data at 5-year intervals, where the start date of the panel refers to the dependent variable (i.e., t=1965, so t-1=1960). Column 5 is the same panel for which population and education data is available. Columns 3 and 4 is a balanced panel 1965-1994 in annual intervals, where the start date of the panel refers to the dependent variable (i.e., t=1965, so t-1=1964). In columns 1 and 3, in panel A, the coefficients in equations (5) and (6) are constrained to be identical; in panel B, the coefficient on income is allowed to be different; in panel C, the coefficient on income and time effects are allowed to be different. In columns 2, 4, and 5, in panel A, the coefficients in equations (8) and (9) are constrained to be identical; in panel B, the coefficient on income is allowed to be different; in panel C, the coefficient on income, time effects, and country fixed effects are allowed to be different. For detailed data definitions and sources see Table 1 and Appendix Table A1. See text for a detailed discussion of estimation technique

	OLS (1)	OLS (2)	OLS (3)	OLS (4)	OLS (5)
		Former European Colonies			
		5-year data			
Panel A	Dependent Variable is Fixed Effect from panel A column 2 of Table 2				
Constraint on the Executive at Independence			0.308 (0.036)	0.266 (0.042)	0.288 (0.036)
Independence Year/100			-0.163 (0.020)	-0.136 (0.023)	-0.142 (0.021)
Log Population Density in 1500	-0.045 (0.011)			-0.016 (0.008)	-0.021 (0.008)
Log Settler Mortality		-0.086 (0.014)		-0.017 (0.013)	
Observations	90	73	82	68	80
R-squared	0.15	0.35	0.59	0.69	0.63
Panel B	Dependent Variable is Fixed Effect from panel B column 2 of Table 2				
Constraint on the Executive at Independence			0.293 (0.033)	0.283 (0.041)	0.266 (0.034)
Independence Year/100			-0.147 (0.018)	-0.139 (0.023)	-0.131 (0.019)
Log Population Density in 1500	-0.042 (0.010)			-0.017 (0.008)	-0.018 (0.008)
Log Settler Mortality		-0.074 (0.013)		0.001 (0.013)	
Observations	80	70	82	68	80
R-squared	0.18	0.31	0.59	0.67	0.62
Panel C	Dependent Variable is Fixed Effect from panel C column 2 of Table 2				
Constraint on the Executive at Independence			0.276 (0.062)	0.294 (0.084)	0.255 (0.065)
Independence Year/100			-0.220 (0.034)	-0.221 (0.048)	-0.228 (0.037)
Log Population Density in 1500	-0.032 (0.017)			-0.005 (0.017)	0.005 (0.015)
Log Settler Mortality		-0.077 (0.023)		0.012 (0.027)	
Observations	83	71	81	68	79
R-squared	0.04	0.14	0.39	0.39	0.40

Weighted cross-sectional OLS in all columns. Dependent variable in panels A, B, and C is the country fixed effect calculated column 2 of Table 2 in panels A, B, and C, respectively. Weights correspond to the inverse non-robust standard error of the country fixed effect calculated in column 2 of Table 2. In all columns, data represents all available values in the former colonies sample, where a country can only enter once. For detailed data definitions and sources see Table 1 and Appendix Table A1.

Table 7
Effect of Alternate Historical Factors on Democracy: Former Colonies

	Former European Colonies				
	5-year data				
	OLS	OLS	OLS	OLS	OLS
	(1)	(2)	(3)	(4)	(5)
Panel A	*Dependent Variable is Fixed Effect from panel A column 2 of Table 2*				
Former Colonizer F-test	[0.54]			.	
Religion F-test		[0.55]			
Latitude			0.124		
			(0.104)		
Significant Oil Producer				0.000	
				(0.048)	
Ethno-Linguistic Fragmentation					-0.104
					(0.042)
Historical Institutions F-test	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
Observations	80	80	80	80	77
R-squared	0.64	0.64	0.63	0.63	0.68
Panel B	*Dependent Variable is Fixed Effect from panel B column 2 of Table 2*				
Former Colonizer F-test	[0.71]				
Religion F-test		[0.41]			
Latitude			0.005		
			(0.097)		
Significant Oil Producer				-0.001	
				(0.045)	
Ethno-Linguistic Fragmentation					-0.067
					(0.042)
Historical Institutions F-test	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
Observations	80	80	80	80	77
R-squared	0.62	0.63	0.62	0.62	0.65
Panel C	*Dependent Variable is Fixed Effect from panel C column 2 of Table 2*				
Former Colonizer F-test	[0.85]				
Religion F-test		[0.29]			
Latitude			-0.023		
			(0.187)		
Significant Oil Producer				0.025	
				(0.087)	
Ethno-Linguistic Fragmentation					-0.132
					(0.082)
Historical Institutions F-test	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
Observations	79	79	79	79	77
R-squared	0.40	0.43	0.40	0.40	0.42

Weighted cross-sectional OLS in all columns. Dependent variable in panels A, B, and C is the country fixed effect calculated column 2 of Table 2 in panels A, B, and C, respectively. Weights correspond to the non-robust standard error of the country fixed effect calculated in column 2 of Table 2. In all columns, data represents all available values in the former colonies sample, where a country can only enter once. Constraint on the Executive at Independence, Independence Year/100, and Log Population Density in 1500 are included in all columns but not displayed; the historical institutions F-test reports the p-value for their joint significance. Former colonizer F-test reports the p-value for the joint significance of British, French, and Spanish colony dummies which are included but not displayed in column 1. Religion F-test reports the p-value for the joint significance of fraction Catholic, fraction Protestant, and fraction Muslim which are included but not displayed in column 2. For detailed data definitions and sources see Table 1 and Appendix Table A1.

Table 8

Fixed Effects Results using Democracy in the Long Run

Balanced Panel, 1875-2000

25-year data

	Pooled OLS (1)	Fixed Effects OLS (2)	Arellano-Bond GMM (3)	Pooled OLS (4)	Fixed Effects OLS (5)	Arellano-Bond GMM (6)
	Dependent Variable is Polity Measure of Democracy			*Dependent Variable is Przeworski Measure of Democracy*		
Democracy $_{t-1}$	0.487	0.192	0.439	0.311	0.042	0.215
	(0.085)	(0.119)	(0.143)	(0.102)	(0.119)	(0.143)
Log GDP per Capita $_{t-1}$	0.116	-0.020	-0.495	0.259	0.163	-0.692
	(0.034)	(0.093)	(0.266)	(0.048)	(0.104)	(0.198)
Observations	150	150	125	180	180	150
Countries	25	25	25	30	30	30
R-squared	0.55	0.65		0.53	0.63	

Pooled cross-sectional OLS regression in columns 1 and 4, with robust standard errors clustered by highest level of aggregation for income data in parentheses. Fixed effects OLS regressions in columns 2, and 5, with country dummies and robust standard errors clustered by highest level of aggregation for income data in parentheses. Column 3 and 6 use GMM of Arellano and Bond (1991), with robust standard errors; we instrument for income using a double lag. Year dummies are included in all regressions. Dependent variable is Polity Measure of Democracy in columns 1-3 and Przeworski Measure of Democracy in columns 4-6. Base sample is a balanced panel 1875-2000. All columns use 25-year data where the start date of the panel refers to the dependent variable (i.e., t=1875, so t-1=1850). GDP per capita is from Maddison (2003). For detailed data definitions and sources see Table 1 and Appendix Table A1.

Table 9

Hazard Model using Transitions to and away from Democracy in the Long Run

	Balanced Panel, 1875-2000					
			25-year data			
	Exponential Hazard	Chamberlain Exponential Hazard	Exponential Hazard	Chamberlain Exponential Hazard	Exponential Hazard	Chamberlain Exponential Hazard
	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent Variable is Transitions to and away from Democracy					
Log GDP per Capita$_{t-1}$	0.612	0.035				
	(0.082)	(0.159)				
Log GDP per Capita$_{t-1}$ on Transitions to Democracy			0.455	0.056	0.508	0.153
			(0.066)	(0.146)	(0.079)	(0.177)
Log GDP per Capita$_{t-1}$ on Transitions away from Democracy			0.509	0.103	0.533	0.206
			(0.064)	(0.145)	(0.082)	(0.226)
Observations	180	180	180	180	173	173
Countries	30	30	30	30	30	30

Exponential double hazard model in columns 1, 3, and 5, and Chamberlain exponential double hazard model in columns 2, 4, and 6. Robust standard errors clustered by highest level of aggregation for income data in parentheses. Year dummies are included in all regressions. Dependent variable is Przeworski Measure of Democracy. Base sample is a balanced panel 1875-2000. All columns use 25-year data where the start date of the panel refers to the dependent variable (i.e., t=1875, so t-1=1850). In column 1 the coefficients in equations (5) and (6) are constrained to be identical; in column 3, the coefficient on income is allowed to be different; in column 5, the coefficient on income and time effects are allowed to be different. In column 2 the coefficients in equations (8) and (9) are constrained to be identical; in column 4, the coefficient on income is allowed to be different; in column 6, the coefficient on income, time effects, and country fixed effects are allowed to be different. GDP per capita is from Maddison (2003). For detailed data definitions and sources see Table 1 and Appendix Table A1.

Appendix Table A1

VARIABLE	DESCRIPTION	SOURCE
Freedom House Measure of Democracy	Data for 1972-2000 in Freedom House Political Rights Index, original range 1,2,3,...,7 normalized 0-1. Data for 1972 used for 1970 in the five-year sample. Data for 1950, 1955, 1960 and 1965, in Bollen, original range 0.00,0.01,...0.99,1. Transitions to democracy are calculated as $\max((d_{it}, d_{it-1})$ and transitions away from democracy are calculated as $\min(d_{it}, d_{it-1})$.	http://www.freedomhouse.org/ratings/, and Bollen (2001) "Cross National Indicators of Liberal Democracy 1950-1990" available on ICPSR
Polity Measure of Democracy	Data for 1840-2000 in Polity IV. The composite index is the democracy score minus the autocracy score. Original range -10,-9,...10, normalized 0-1. Transitions are coded in the same fashion as the Freedom House Measure of Democracy	http://www.cidcm.umd.edu/inscr/polity/
Przeworski Measure of Democracy	Data for 1840-1995 in Boix and Rosato (2001). Range is 0,1. Data for 1994 is used for 1995 in the 5-year sample. Data for 2000 in the 25-year sample. Transitions are coded in the same fashion as the Freedom House Measure of Democracy.	Boix and Rosato (2001)
GDP per Capita (Chain Weighted 1996 Prices)	Data for 1950-2000 measured as Log Real GDP per Capita (Chain Method in 1996 prices) from Penn World Tables 6.1.	http://pwt.econ.upenn.edu/
GDP per Capita (1990 dollars)	Data for 1800-2000 measured as Log Real GDP per Capita (1990 Geary-Khamis dollars) from Maddison (2003). Countries are assigned values at the lowest possible aggregation. Data in 1820 is used for 1850. Data in 1870 is used for 1875. Data in 1929 is used for 1925.	http://www.eco.rug.nl/~Maddison/
Population	Total population in thousands.	World Bank (2002)
Education	Average total years of schooling in the population aged 25 and over. Data for 1960, 1965,..., 1995 from Barro and Lee.	Barro and Lee (2000) available at http://www.cid.harvard.edu/ciddata/ciddata.html
Constraint on the Executive at Independence	Data in Polity IV, original range 1,2,3...7, normalized 0-1. Calculated as the average of constraint on the executive in a country during the first 10 years after its independence (ignoring missing data). If data for the first 10 years after independence is missing, we find the first year these data are available in Polity, then average over the following ten years (ignoring missing data).	http://www.cidcm.umd.edu/inscr/polity/
Independence year	Year when country became independent, with any year before 1800 coded as 1800. We coded Taiwan's independence year to 1948 and changed Zimbabwe's independence year to 1964. Classification of countries follows Polity.	CIA World Factbook (2004) available at http://www.cia.gov/cia/publications/factbook/
Population Density in 1500	Indigenous population divided by arable land in 1500.	Acemoglu et al (2002)
Settler mortality	Historical mortality rates of potential European settlers.	Acemoglu et al (2001)
Colonial Origin	Dummies for whether the country was a (1) French colony, (2) British colony, (3) Spanish colony, or (4) Other (Portuguese, Belgian).	La Porta et al (1999)

VARIABLE	DESCRIPTION	SOURCE
Religion	Percent of population in 1980 which is (1) Catholic, (2) Protestant, or (3) Muslim.	La Porta et al (1999)
Latitude	Distance from equator (absolute value).	La Porta et al (1999)
Significant Oil Producer	Dummy=1 for Algeria, Gabon, Indonesia, Libya, Nigeria, and Venezuela and dummy=0 otherwise. Middle Eastern countries are not included in the sample of former colonies.	
Ethno-Linguistic Fragmentation	Average of five different indices of ethno-linguistic fragmentation.	La Porta et al (1999)

Appendix Table A2
Fixed Effects Results: Investigating Londregan and Poole (1996)

	Table 3, Londregan & Poole (1996) (1)	Fixed Effects OLS (2)	Fixed Effects OLS (3)	Fixed Effects OLS (4)	Fixed Effects OLS (5)
	Londregan and Poole Sample, 1952-1985				
	Dependent Variable is Democracy				
Democracy $_{t-1}$	0.848	0.848	0.858	0.858	[0.00]
	(0.014)	(0.015)	(0.014)	(0.031)	
Log GDP per Capita F-test		[0.00]	[0.00]	[0.12]	[0.32]
Log GDP per Capita $_{t-1}$	0.119	0.119	0.117	0.126	
	(0.032)	(0.033)	(0.033)	(0.080)	
Log GDP per Capita Growth $_t$	-0.305	-0.305	-0.341	-0.302	
	(0.161)	(0.164)	(0.164)	(0.172)	
Log GDP per Capita Growth $_{t-1}$	-0.027	-0.027	-0.038	0.012	
	(0.156)	(0.159)	(0.160)	(0.156)	
Transition	0.197	0.197			
	(0.069)	(0.071)			
Nonconstitutional Leader	0.020	0.020	0.009	0.013	0.002
	(0.040)	(0.041)	(0.040)	(0.062)	(0.081)
Leader is a Prime Minister	0.221	0.221	0.226	0.228	0.205
	(0.086)	(0.088)	(0.088)	(0.152)	(0.213)
Leader had a Military Career	0.093	0.093	0.086	0.078	0.093
	(0.036)	(0.037)	(0.037)	(0.068)	(0.082)
Time Since Leader Came to Power	-0.004	-0.004	-0.004	-0.004	-0.005
	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)
Leader has a CMIP	-0.020	-0.020	-0.075	-0.087	-0.151
	(0.063)	(0.065)	(0.062)	(0.103)	(0.127)
Time Remaining in Leader's CMIP	-0.017	-0.017	-0.017	-0.015	-0.010
	(0.013)	(0.013)	(0.013)	(0.012)	(0.012)
Leader's CMIP Expires This Year	-0.027	-0.027	-0.027	-0.025	-0.024
	(0.045)	(0.046)	(0.046)	(0.047)	(0.046)
Leader's Age/10	0.011	0.011	0.011	0.009	0.007
	(0.012)	(0.013)	(0.013)	(0.016)	(0.017)
Time Effects F-test				[0.04]	[0.02]
Residual AR(1) Test		[0.00]	[0.00]	[0.00]	[0.00]
Observations	2798	2798	2798	2798	2364
Countries		100	100	100	99
R-squared	0.96	0.96	0.96	0.96	0.96

In both columns 1 and 2, non-robust standard errors are in parentheses. Country dummies are included in all columns. Columns 3 and 4 include time effects and cluster the standard errors (included in parentheses) to account for potential residual serial correlation. Column 5 includes lags of democracy up to t-5 and includes up to 5 lags of income and removes growth terms so that income variables correspond more closely to the variables we include in Tables 2, column 4. Dependent Variable: Polity Composite Index (Logistic Transform), from Londregan and Poole (1996). This takes the original Polity score, ranging from -10 to 10, and creates a variable ln(Polity+10.5)-ln(10.5-Polity) ranging from -3.71 to 3.71. Sample is unbalanced panel from 1952 and 1985, from Londregan and Poole. Log GDP per Capita Growth is calculated on an annual basis. Column 1 reports coefficients from Table 3, columns 3 and 4, on p.19 of Londregan and Poole (1996), which is generated using maximum likelihood estimation on a two equation model. The Income F-test corresponds to a joint test of the significance of coefficients on all income variables listed at the top of table in columns 2, 3, and 4. The Democracy F-test does the same for democracy variables. Columns 2-5 include a test of first order autocorrelation in the residual term; see Wooldridge (2002), section 10.6.3. CMIP stands for constitutionally mandated interelection period. See Londregan and Poole (1996) for data and definitions.

Appendix Table A3
## Codes Used to Represent Countries in Figures

Country	Code	Country	Code	Country	Code
Andorra	ADO	Ghana	GHA	Netherlands	NLD
Afghanistan	AFG	Guinea	GIN	Norway	NOR
Angola	AGO	Gambia, The	GMB	Nepal	NPL
Albania	ALB	Guinea-Bissau	GNB	New Zealand	NZL
United Arab Emirates	ARE	Equatorial Guinea	GNQ	Oman	OMN
Argentina	ARG	Greece	GRC	Pakistan-post-1972	PAK
Armenia	ARM	Grenada	GRD	Pakistan-pre-1972	PAK_1
Antigua	ATG	Guatemala	GTM	Panama	PAN
Australia	AUS	Guyana	GUY	Peru	PER
Austria	AUT	Honduras	HND	Philippines	PHL
Azerbaijan	AZE	Croatia	HRV	Papua New Guinea	PNG
Burundi	BDI	Haiti	HTI	Poland	POL
Belgium	BEL	Hungary	HUN	Korea, Dem. Rep.	PRK
Benin	BEN	Indonesia	IDN	Portugal	PRT
Burkina Faso	BFA	India	IND	Paraguay	PRY
Bangladesh	BGD	Ireland	IRL	Qatar	QAT
Bulgaria	BGR	Iran	IRN	Romania	ROM
Bahrain	BHR	Iraq	IRQ	Russia	RUS
Bahamas	BHS	Iceland	ISL	Rwanda	RWA
Bosnia and Herzegovina	BIH	Israel	ISR	Saudi Arabia	SAU
Belarus	BLR	Italy	ITA	Sudan	SDN
Belize	BLZ	Jamaica	JAM	Senegal	SEN
Bolivia	BOL	Jordan	JOR	Singapore	SGP
Brazil	BRA	Japan	JPN	Solomon Islands	SLB
Barbados	BRB	Kazakhstan	KAZ	Sierra Leone	SLE
Brunei	BRN	Kenya	KEN	El Salvador	SLV
Bhutan	BTN	Kyrgyz Republic	KGZ	Somalia	SOM
Botswana	BWA	Cambodia	KHM	Sao Tome and Principe	STP
Central African Republic	CAF	Kiribati	KIR	Suriname	SUR
Canada	CAN	St. Kitts and Nevis	KNA	Slovakia	SVK
Switzerland	CHE	Korea, Rep.	KOR	Slovenia	SVN
Chile	CHL	Kuwait	KWT	Sweden	SWE
China	CHN	Lao PDR	LAO	Swaziland	SWZ
Cote d'Ivoire	CIV	Lebanon	LBN	Seychelles	SYC
Cameroon	CMR	Liberia	LBR	Syrian Arab Republic	SYR
Congo, Rep.	COG	Libya	LBY	Chad	TCD
Colombia	COL	St. Lucia	LCA	Togo	TGO
Comoros	COM	Liechtenstein	LIE	Thailand	THA
Cape Verde	CPV	Sri Lanka	LKA	Tajikistan	TJK
Costa Rica	CRI	Lesotho	LSO	Turkmenistan	TKM
Cuba	CUB	Lithuania	LTU	Tonga	TON
Cyprus	CYP	Luxembourg	LUX	Trinidad and Tobago	TTO
Czech Republic	CZE	Latvia	LVA	Tunisia	TUN
Germany	DEU	Morocco	MAR	Turkey	TUR
Djibouti	DJI	Moldova	MDA	Taiwan	TWN
Dominica	DMA	Madagascar	MDG	Tanzania	TZA
Denmark	DNK	Maldives	MDV	Uganda	UGA
Dominican Republic	DOM	Mexico	MEX	Ukraine	UKR
Algeria	DZA	Macedonia, FYR	MKD	Uruguay	URY
Ecuador	ECU	Mali	MLI	United States	USA
Egypt, Arab Rep.	EGY	Malta	MLT	Uzbekistan	UZB
Eritrea	ERI	Myanmar	MMR	St. Vincent and the Grenadines	VCT
Spain	ESP	Mongolia	MNG	Venezuela, RB	VEN
Estonia	EST	Mozambique	MOZ	Vietnam	VNM
Ethiopia	ETH	Mauritania	MRT	Vanuatu	VUT
East Timor	ETM	Mauritius	MUS	Western Samoa	WSM
Finland	FIN	Malawi	MWI	Yemen	YEM
Fiji	FJI	Malaysia	MYS	Yugoslavia - post 1991	YUG
France	FRA	Namibia	NAM	South Africa	ZAF
Gabon	GAB	Niger	NER	Congo, Dem. Rep.	ZAR
United Kingdom	GBR	Nigeria	NGA	Zambia	ZMB
Georgia	GEO	Nicaragua	NIC	Zimbabwe	ZWE

Figure 1

## Transition to Non-Democracy and Income Level, 1960-1995
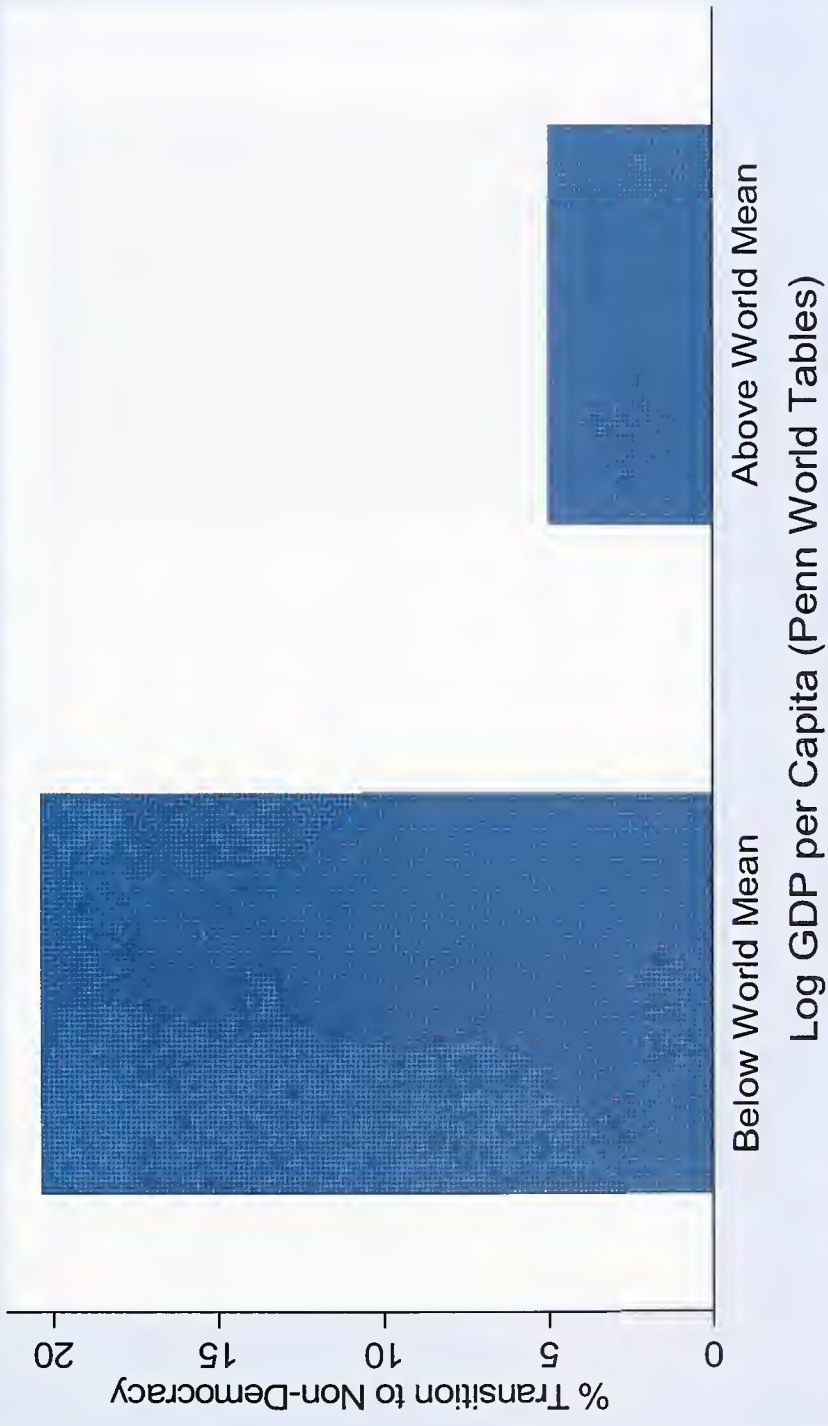
See Appendix Table A1 for data definitions and sources. Sample includes all countries in five year intervals between 1955 and 1990 which are democratic according to the Przeworski Measure of Democracy for which at least two observations are available. Observations are grouped depending on whether log income per capita is above or below the average log income per capita in the world for the observation year. Each column measures the fraction of observations within each group which experience a transition to non-democracy five years later.

Figure 2

Transition to Non-Democracy and Income Level, 1960-1995
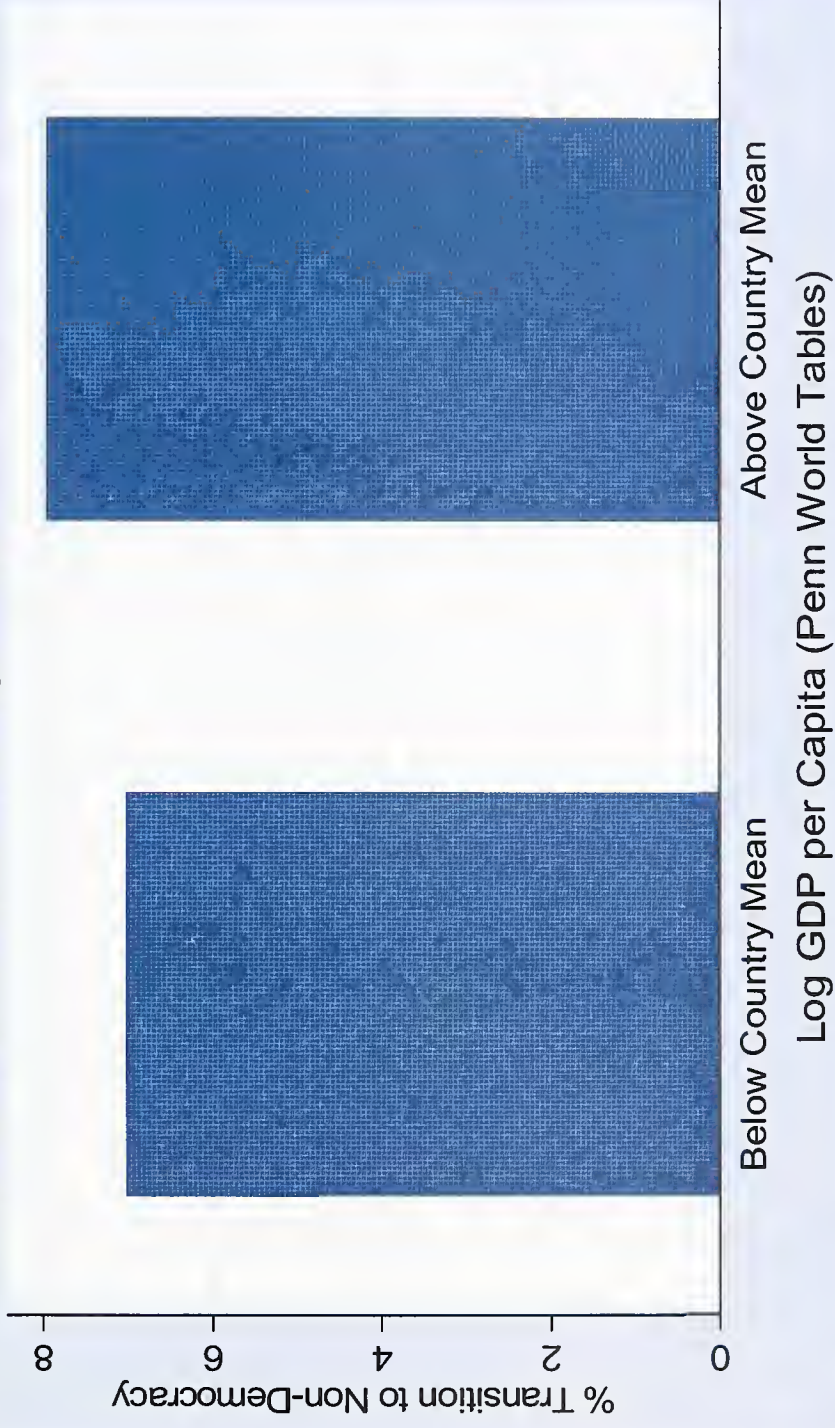
See Appendix Table A1 for data definitions and sources. Sample is the same as in Figure 1. Log income per capita for every observation is demeaned from the average log income per capita in the world for the observation year. Observations are grouped depending on whether demeaned log income per capita is above or below the average demeaned log income per capita in the country between 1955 and 1990. Each column measures the fraction of observations within each group which experience a transition to non-democracy five years later.

Figure 3
Change in Democracy and Change in Income, 1970-1995
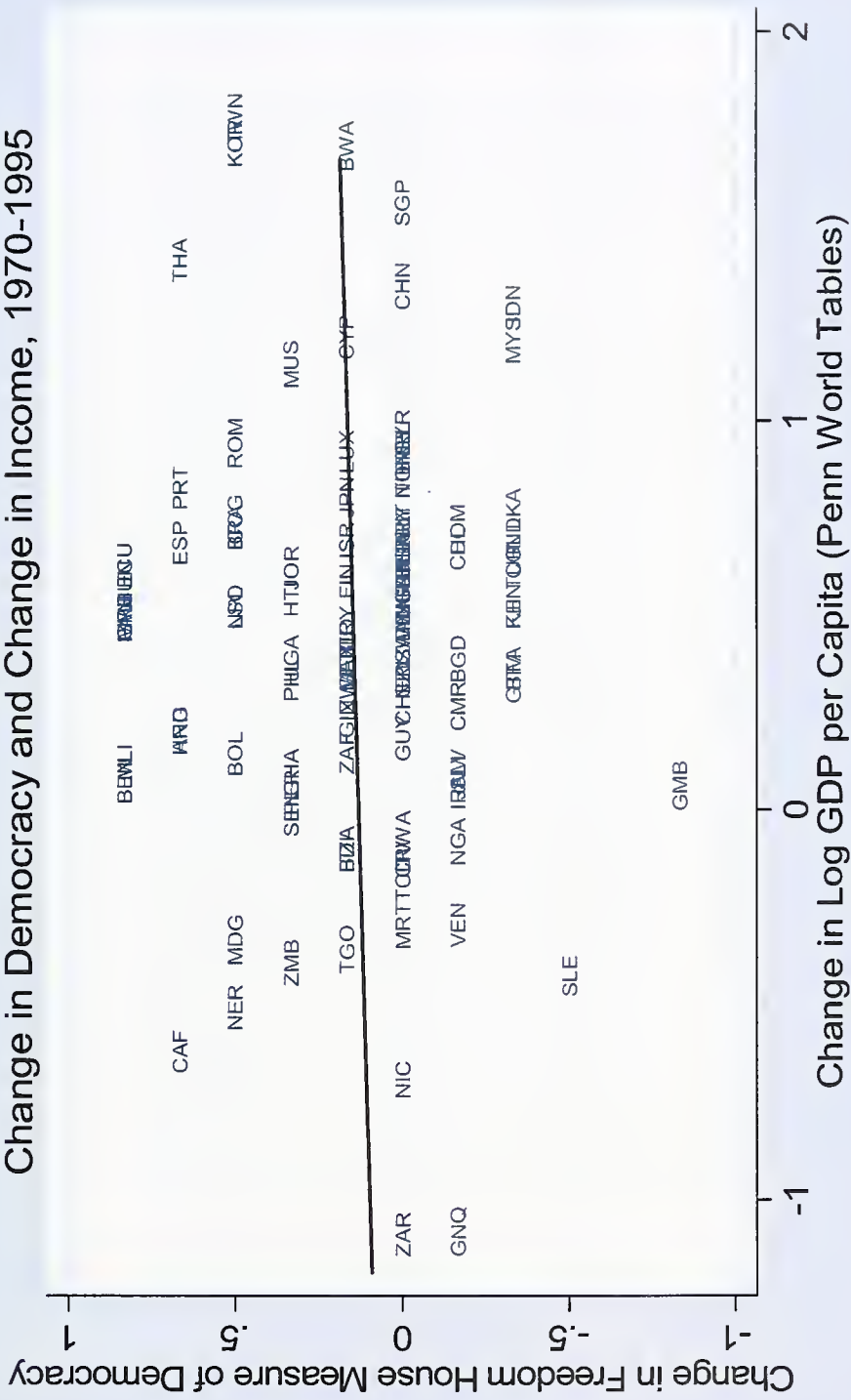
See Appendix Table A1 for data definitions and sources. Changes are total difference between 1970 and 1995. Start and end dates are chosen to maximize the number of countries in the cross-section. The regression represented by the fitted line yields a coefficient of 0.032 (standard error=0.058), N=103, R^2=0.00.

Figure 4

Change in Democracy and Change in Income, 1970-1995

See notes to Figure 3. The regression represented by the fitted line yields a coefficient of -0.024 (standard error=0.063), N=98, R^2=0.00.
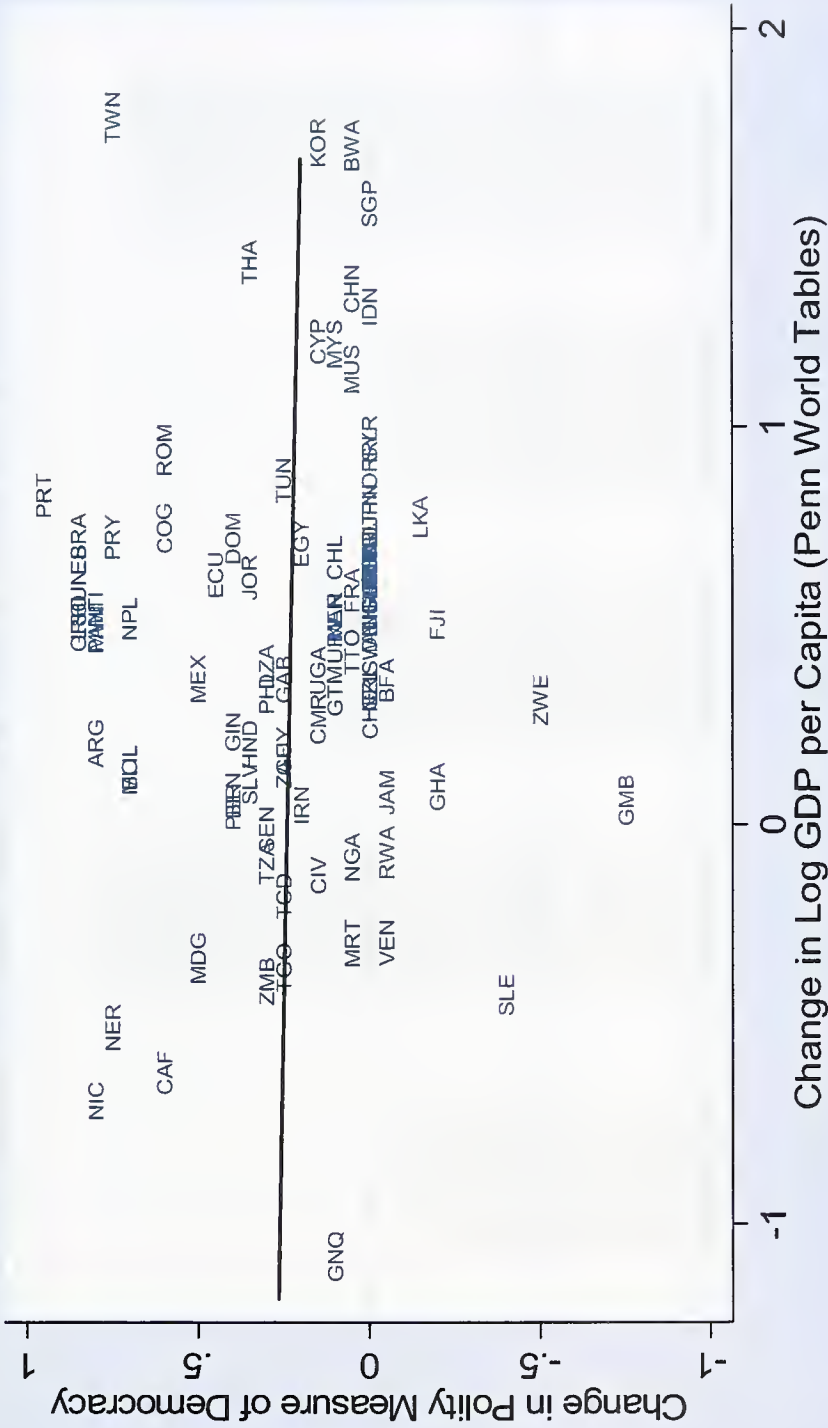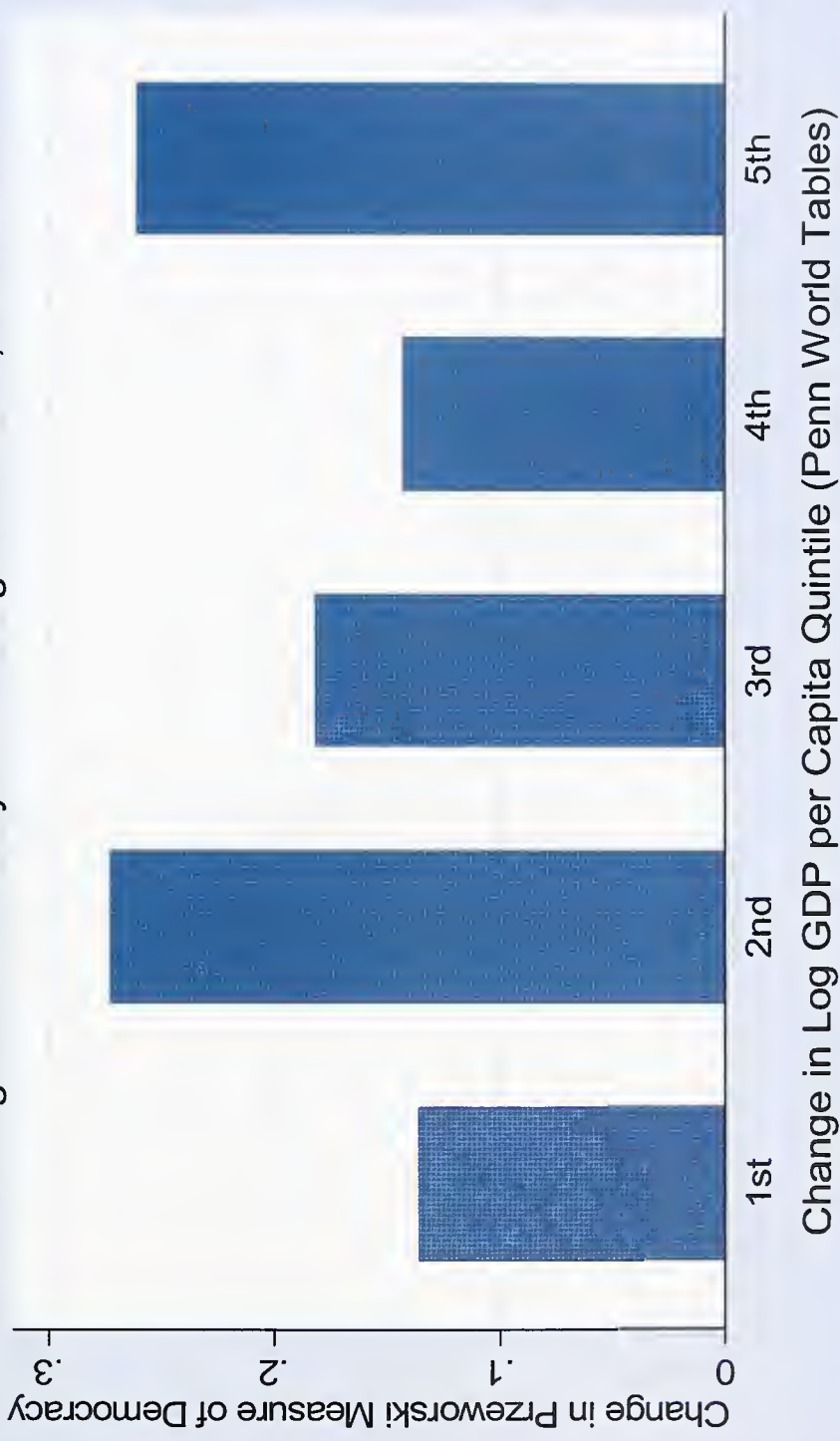
Figure 5

Change in Democracy and Change in Income, 1970-1995

See notes to Figure 3. Countries are grouped into quintiles corresponding to the total change in Log GDP per Capita between 1970 and 1995. Change in democracy is the average change within each quintile.

Figure 6

Democracy Fixed Effect and Log Settler Mortality

Country fixed effects are those estimated in the specification of Table 2, column 2 using Freedom House measure of democracy. PAK_1 refers to Pakistan pre-1972. The regression represented by the fitted line yields a coefficient of -0.086 (standard error=0.013), N=73, R^2=0.35.

Figure 7

Democracy Fixed Effect and Log Population Density in 1500

See notes to Figure 6. The regression represented by the fitted line yields a coefficient of -0.412 (standard error=0.012), N=90, R^2=0.12.

Figure 8

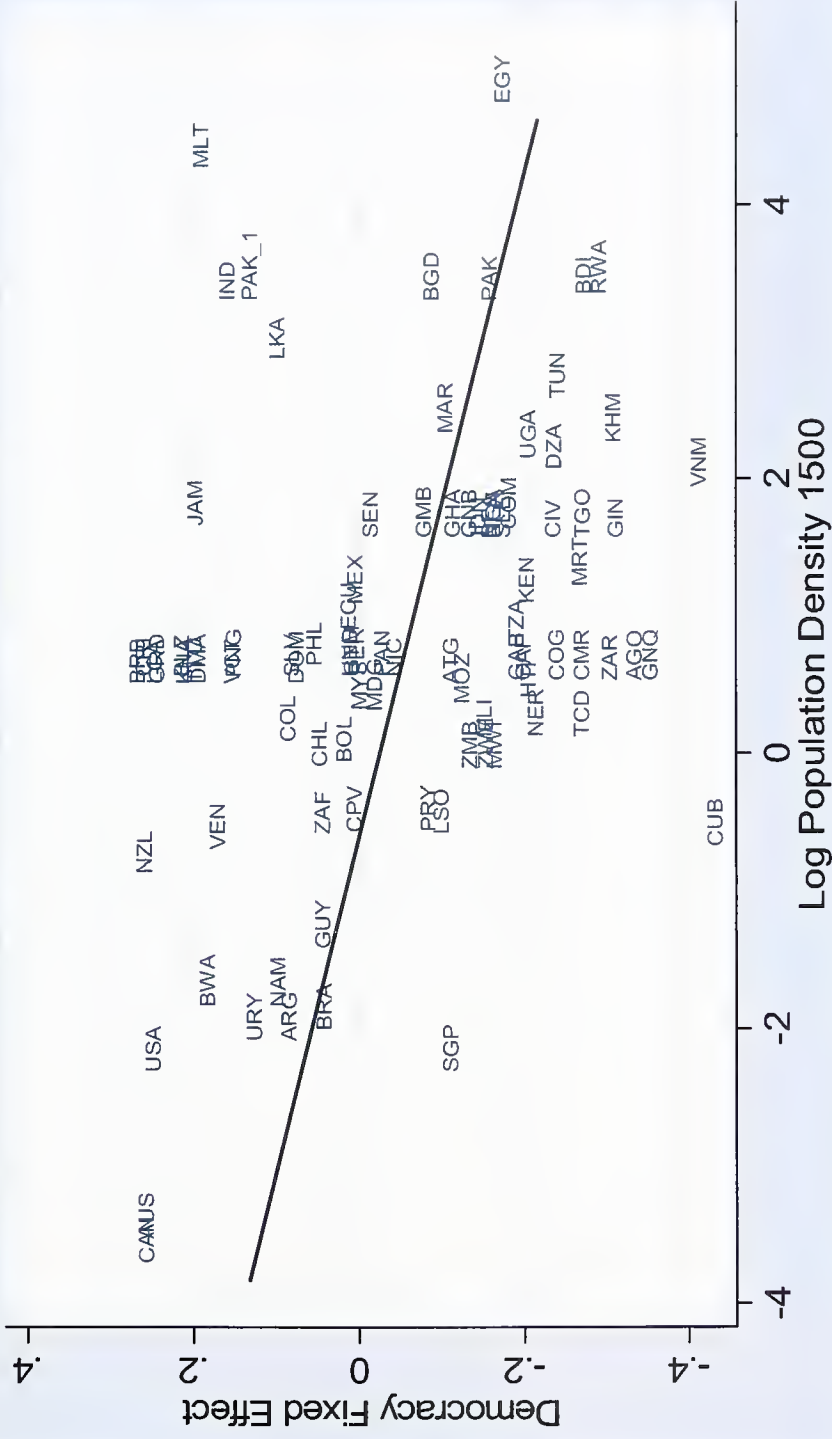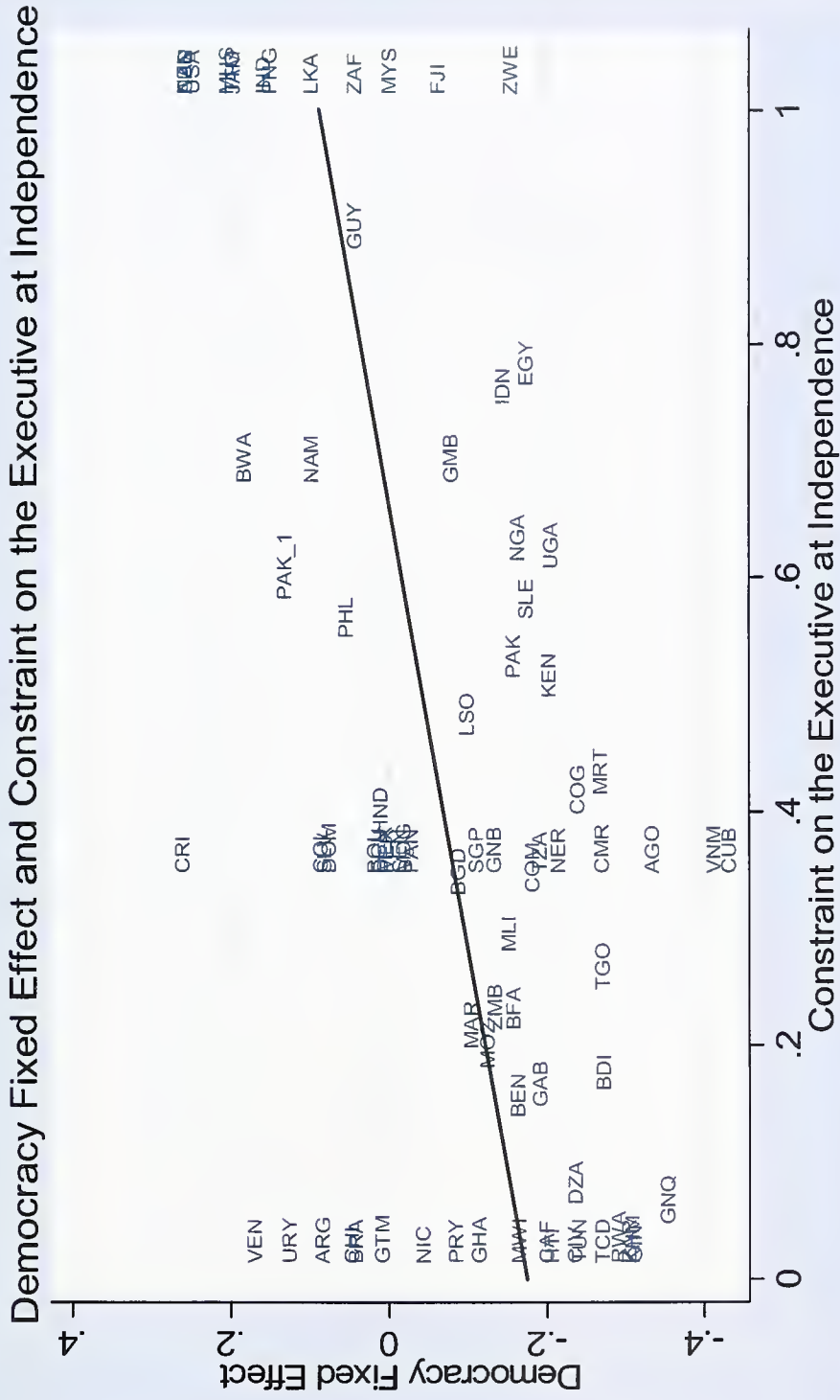Democracy Fixed Effect and Constraint on the Executive at Independence

See notes to Figure 6. The regression represented by the fitted line yields a coefficient of 0.263 (standard error=0.047), N=82, R^2=0.27.

Figure 9

Democracy Fixed Effect and Independence Year

See notes to Figure 6. The regression represented by the fitted line yields a coefficient of -0.081 (standard error=0.025), N=94, R^2=0.07.