

Income and Democracy*

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Abstract

We revisit one of the central empirical findings of the political economy literature that higher income per capita causes democracy. Existing studies establish a strong cross-country correlation between income and democracy, but do not typically control for factors that simultaneously affect income and democracy. We show that controlling for such factors removes the statistical association between income per capita and various measures of democracy. This is true both when we include country fixed effects, and when we control directly for potential historical determinants of political and economic development in a sample of former European colonies. Overall, these results provide little support for the hypothesis that income causes democracy. Rather, different countries appear to have followed different political and economic development paths, some leading to prosperity and democracy in the long run, others to relative poverty and non-democracy. Conditional on the development path, there is little effect of income on democracy. Consistent with this interpretation, there is a positive correlation between changes in income and democracy when we look over the last 500 years.

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

JEL Classification: P16, O10.

Still preliminary and incomplete.

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1 Introduction

One of the most notable empirical regularities in political economy is the relationship between income per capita and democracy. Today all OECD countries are democratic, while many of the nondemocracies are in the poor parts of the world, in Africa, the Caribbean, Central America and South Asia. This positive relationship is not only confined to a cross-country comparison. Most countries were nondemocratic before the modern growth process took off at the beginning of the 19th century. Democratization came together with growth.

This statistical association between income and democracy is consistent with the most famous theories of democracy. It is the cornerstone of the influential modernization theory, which sees a direct causal link between economic growth and democracy. Lipset (1959) not only popularized this theory but also undertook the first statistical analysis documenting this connection. Recent empirical work, by, among others, Londregan and Poole (1996), Przeworski and Limongi (1997), Barro (1997, 1999), and Przeworski, Alvarez, Cheibub, and Limongi (2000), documents that this is a robust regularity in the postwar data. Geddes (1999) emphasizes that this may be the only real empirical regularity that scholars have found on this topic. Even non-quantitative scholars such as Dahl (1971), Rueschemeyer, Stephens and Stephens (1992) or Huntington (1991) and economists working on institutions such as Glaeser, La Porta, Lopez-de-Silanes and Shleifer (2004) accept it as an established fact.

Correlation does not establish causation, however. First, there is the issue of reverse causality; perhaps democracy causes income rather than the other way round. Second, and more important, there is the potential for omitted variable bias. Some other factor may determine both the nature of the political regime and the potential for economic growth.

In this paper, we argue that in the postwar data, income per capita does not cause democracy. Instead the positive correlation between income and democracy reflects the fact that different societies have embarked on different *political and economic development paths*. Our approach and interpretation are motivated by theories where both the potential for economic growth and the form of government are determined as part of the political-economic equilibrium.¹ Sustained economic growth necessitates economic institutions that provide incentives to invest in physical capital, education and technology for a broad

¹See, among others, North and Thomas (1973), North (1981), Bates (1981), Jones (1981), Engerman and Sokoloff (1997), Bourguignon and Verdier (2000), Acemoglu, Johnson and Robinson (2002, 2004a,b), Nugent and Robinson (2000), and Acemoglu (2003).

cross-section of society. Such economic institutions are unlikely to develop and endure in societies where political power is in the hands of a small elite and can be exercised without effective constraints. This is because the elite would try to use their monopoly of political power to further their own interests by creating economic institutions that generate economic benefits for themselves, often at the cost of significant distortions and social inefficiencies. Economic institutions that enable a few to benefit at the expense of the many, in turn, contribute to the survival and persistence of an unequal distribution of political power in society. Consequently, there are multiple possible development paths for a society; one with economic institutions associated with economic growth and democracy, and another leading to relative economic backwardness and nondemocracy.

These considerations suggest that a satisfactory empirical strategy should control for country-specific factors affecting both economic growth and democratization. A natural approach is to organize the data as a panel and to control directly for these unobserved country-specific institutions using fixed effects. While fixed effect regressions are not a panacea against omitted variable biases,² they are well-suited to our problem. A major source of bias in a regression of democracy on income per capita are country-specific, historical factors influencing both political and economic development. If these omitted characteristics are, to a first approximation, time-invariant, the inclusion of fixed effects will remove them and this potential source of bias. Consider, for example, the comparison of the United States and Colombia. The United States is both richer and more democratic, so a cross-country comparison suggests that there is a relationship between democracy and income. The idea of fixed effects is to move beyond this comparison and investigate the “within-country variation”; i.e., whether as Colombia becomes relatively richer, it also tends to become more democratic relative to the United States. In addition to improving inference on the causal effect of income on democracy, this approach is also more closely related to the modernization theory articulated by Lipset (1959), which claims that countries should become more democratic as they become richer, not simply that rich countries should be more democratic.

²Fixed effects would not help inference if there are time-varying omitted factors affecting the dependent variable and correlated with the right-hand side variables (see the discussion below). They may also make problems of measurement error worse because they remove a significant portion of the variation in the right-hand side variables. Consequently, fixed effects are certainly no substitute for using an instrumental-variables approach with a valid instrument. The problem in this context is that it is difficult to find a variable that affects income per capita without also having a direct influence on democracy. A recent creative attempt is by Miguel, Satyanath and Sergenti (2004), who use the weather conditions as an instrument for income in Africa. Unfortunately, weather conditions are only a good instrument for relatively short-run changes in income, thus not necessarily ideal to study the relationship between income and democracy.

Our first main finding is that once fixed effects are introduced, the positive relationship between income per capita and various measures of democracy disappears. This finding is robust across different measures of democracy, the use of additional covariates, econometric specifications and estimation techniques. It therefore sheds considerable doubt on the claim that there is a strong *causal* effect of income on democracy.³

This result, while different from the conventional wisdom and at first surprising, is consistent with the hypothesis that democracy and prosperity are closely linked as part of the political and economic development process of a society. But what determines whether a country is on the (long-run) path to poverty and nondemocracy or to prosperity and democratic institutions?

To make further progress on these questions, we turn to a specific theory of political and economic development. Acemoglu, Johnson and Robinson (2001 and 2002) exploit the quasi-natural experiment provided by the colonization of many diverse societies by European powers after 1492, and document that the colonization experience led to significant divergence in the economic and political development paths of these societies. They show that the institutional differences created at these critical junctures persisted and significantly contributed to the large differences in both the form of government and economic success of these societies.

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, and did not generate incentives for investment, education or innovation, and 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

³In addition, contrary to many hypotheses in the literature including Lipset's (1959) modernization theory, Friedman's (1962) arguments in *Capitalism and Freedom*, or Dahl's (1971) arguments in *Polyarchy*, we find no evidence that the average education level of the population causes democracy.

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.

continued to benefit relatively small elites.⁴ 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, 2004). 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 (Morgan, 1975, Galenson, 1996, Keyssar, 2000).⁵ This institutional nexus provided relatively good economic incentives for the non-slave population, and 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 (Haber, 2002).

These ideas lead to the second main finding of the paper; both democracy and prosperity are closely linked to these historical processes. More explicitly, we document that in a sample of former European colonies, where we have a handle on the potential histor-

⁴For example, for Latin America, see Stein and Stein (1970), Coatsworth (1978, 1993), and Engerman and Sokoloff (1997).

⁵Though 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 Perú, Bolivia or México, while slaves constituted more than 90% of the population in most Caribbean islands (see, for example, Engerman and Sokoloff, 1997).

ical determinants of the development path, we estimate our econometric models without fixed effects, but controlling for these historical factors, in particular, for the mortality rates faced by European settlers, the indigenous population density before colonization, proxies for 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, 2000, 2001, 2002); constraint on the executive at independence is a proxy for the quality of early institutions; and date of independence is another measure of colonization strategy, since non-extractive colonies gained their independence typically earlier than the extractive ones. We find that once these historical characteristics are included in the regression as controls, income per capita again plays little role in determining regime type. These historical variables are themselves typically significant determinants of whether or not a country is democratic, and we show that they capture the same source of statistical variation as the fixed effects.

Our interpretation of these results is that events at critical historical junctures, such as the early stages of colonization, have a large influence on the political and economic development path of countries, in particular on whether a country embarks upon a path leading to higher income per capita and more democratic institutions, where democracy and income coevolve. *Conditional on the development path*, however, there is no causal effect of income per capita on democracy (the presumption is that in the postwar sample the fixed effects proxy for the different long-run development paths).⁶

This reasoning, however, suggests that we should find a positive covariation between democracy and income in the longer run, when we can observe some countries embarking on the development path to democracy and prosperity, while others stagnate relatively and remain nondemocratic.⁷ We investigate this question by looking at longer historical samples. Focusing on a sample of 28 countries during the period 1840-1940, we find no evidence of a significant relationship between income and democracy. The same sample of countries during 1840-2000 shows some weak evidence of a positive relationship. We find the most consistent results when we look at a much longer period, from 1500 to today. In this 500 year sample, there is a strong covariation between income and democracy, presumably because we observe countries embarking on different development paths. We

⁶This is in fact quite plausible. For example, if economic growth enriches the elites, who have much to gain from the continuation of a nondemocratic regime, why should we expect growth to lead to democracy?

⁷It is important to stress that, as explained below, this positive covariation corresponds to neither the causal effect of income on democracy nor of democracy on income, since other factors likely affect both outcomes, even conditional on the fixed effects.

view this pattern as consistent with our overall interpretation that the relationship between income and democracy is in the context of the long-run development path of a society, and not necessarily in the short run, or conditional on the development path.

We also investigate whether income has a causal effect on transitions to democracy and transitions from democracy differently from its effect on the overall level of democracy. Contrary to the findings in Przeworski et al. (2000), we find that, once we control for fixed effects, income seems to play little role either in explaining transitions to or away from democracy.

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 not explained by our historical variables, and even a larger amount of over-time variability in the democracy score of a country. For example, consistent with some theories of democratization (e.g., Haggard and Kaufman, 1995, Acemoglu and Robinson, 2000, 2001, and 2004), we find that severe economic crises make predict *higher* democracy scores, presumably because they lead to the collapse of dictatorships.

The paper proceeds as follows. In Section 2 we describe the data. In Section 3 we present some basic regressions similar to the pooled cross-sectional approach of the previous literature, documenting the positive correlation between income and democracy. Section 4 shows that with fixed effects there is no relationship between income and democracy in the postwar sample. In Section 5, we discuss the role of the historical determinants of the political and economic development paths of societies. Section 6 investigates the longer-run relationship between income and democracy. Section 7 decomposes our measures of democracy so as to distinguish between movements towards and away from democracy. Section 8 looks at the effect of crises and growth accelerations on democracy. In Section 9, we compare our results with the previous literature, in particular with the influential works by Londregan and Poole (1996), Barro (1999), and Przeworski et al. (2000). Section 10 concludes.

2 Data and Descriptive Statistics

The first question we face is how to measure democracy. There has been a great deal of controversy over this issue in political science mostly because there is some disagreement over what actually constitutes a democracy. Many scholars however accept the definition

proposed by Schumpeter (1950, p. 250) who argued that democracy was:⁸

“the institutional arrangement for arriving at political decisions in which individuals acquire the power to decide by means of a competitive struggle for the people’s vote.”

We follow much of the existing research in this area in adopting a Schumpeterian definition based on a number of institutional conditions. Our first and main 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.⁹ Following Barro (1999), we supplement this index with the related variable from Bollen (1990, 2001) for 1950, 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 postwar 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 therefore also look at the other widely-used measure of democracy, the composite Polity index, which is the difference between the Polity’s Democracy and Autocracy indices.¹⁰ 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. For instance, constraints on the executive is coded on a 7 point scale running from “unlimited authority” where “there are no regular limitations on the executive’s actions (as distinct from irregular actions such as the threat or actuality of coups and assassinations)” to

⁸Such an institutional definition is contested, with many scholars wishing to add other conditions for a democracy (see Linz and Stepan, 1996, Collier and Levitsky, 1997, and Schedler, 1998, for this debate).

⁹The main checklist includes 3 questions on the electoral process, 4 questions on the extent of political pluralism, and participation and 3 questions on the functioning of government. For each checklist question, 0 to 4 points are added, depending on the comparative rights and liberties present (0 represents the least, 4 represents the most) and these scores are added and used to determine where the country resides on the 1-7 scale. See Freedom House (2004), <http://www.freedomhouse.org/research/freeworld/2003/methodology.htm>

¹⁰See Marshall and Jaggers (2004) and <http://www.cidcm.umd.edu/inscr/polity/>

“executive parity or subordination” where “accountability groups have effective authority equal to or greater than the executive in most areas of activity”.¹¹ A country would be in the first category if “constitutional restrictions on executive action are ignored” or “there is no legislative assembly or there is one but it is called or dismissed at the executive’s pleasure.” A country would be in the latter category if “a legislature, ruling party or council of nobles initiates much or most important legislation” or “the executive is chosen by the accountability group and is dependent on its continued support to remain in office.” The Polity Autocracy Index also ranges from 0 to 10 and is constructed in a similar way to the democracy score based on scoring countries according to competitiveness of political participation, the regulation of participation, the openness and competitiveness of executive recruitment and constraints on the chief executive. 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 (e.g. Przeworski et al., 2000, chapter 1) who argue that a simple dichotomy between democracy and nondemocracy is the most useful empirical definition.¹² Another potential advantage of the dichotomous measures is that they may enable a clearer discussion of transitions from and to democracy. While there is a debate on the pros and cons of dichotomous measures (see, for example, the papers in Inkeles, 1991, Elkins, 2000), this issue is not central to our investigation, and in Section 9, we present regressions using the Boix-Rosato data set which extends the Przeworski et al. (2000) to 2000, with similar results to our baseline findings.

Using the Freedom House and the Polity data, 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

¹¹An “accountability group” in a monarchy might be a council or nobles; in a democracy, it would correspond to the legislature, or to an independent judicial system. The basic idea is to capture checks and balances in the exercise of power.

¹²In addition to the dichotomous classification, these authors add other provisos to the definition of a democracy, most importantly that a country cannot be democratic unless a political party has been observed to lose power. Hence, according to Przeworski et al. (2000), Botswana has never been a democracy because, even though all agree that elections are free and fair, that there is free entry into politics and that the government is accountable to the people, the Botswana Democratic Party has won every election since independence in 1966. Japan would not have been a democracy for most of the Post WWII period until the Liberal Democratic Party lost power and South Africa is not counted as a democracy because the African National Congress has formed the government since the end of apartheid.

correlation, making inference and estimation more difficult.¹³

Figures 1 and 2 plot the values of the normalized Freedom House (Figure 1) and Polity scores (Figure 2) for our basic 1960-2000 sample. These figures show that OECD countries entered the period almost fully democratic and stayed there. In contrast democracy declined in other parts of the world, particularly in Latin America and Africa, though from the mid 1970s onwards we can detect what Huntington (1991) calls the ‘third wave’ of democratization. Figure 3 uses the Polity data back to 1840 for all the countries which were independent over this period. This picture vividly displays the onward march of democracy in the OECD in the period leading up to WWI and shows evidence of the ‘first and second waves of democracy’, the first before WWI and the second after WWII.

In addition, we use GDP per capita data from the Summers-Heston dataset for the postwar period (Heston, Summers, and Aten, 2002), and 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), a measure of the relative average years of primary schooling for males compared to females, again from the Barro-Lee data (2000), and total population and the urbanization rate from the World Bank (2002).

Data for date of independence are from the CIA World Factbook,¹⁴ and those for constraint on the executive after independence are from the Polity IV dataset.¹⁵ 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 (e.g. Curtin, 1989, 1998).

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 historical 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

¹³For 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.

¹⁴There are different ways to code the date of independence. For example, for Perú the critical date was the defeat of the Spanish at the battle of Ayacucho in 1824. Different researchers code the date of independence in different ways, with the CIA coding the date of independence of Perú as 1821. Nevertheless, whether we take 1821 or 1824 for this date does not influence our results.

¹⁵The data on constraints 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.

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 where the process of institutional development, in particular during the 19th century and earlier, was shaped by European intervention (see Acemoglu, Johnson and Robinson, 2002).¹⁶ Nevertheless, we also show that our results are robust to the inclusion of these countries as former colonies.

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 Results with Pooled Cross Sections

We first replicate some of the basic results in the literature using a pooled cross-sectional approach. Tables 2a and 2b report estimates of the following model also used by Barro (1999):

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

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 whether income has an effect on democracy. All other potential covariates are included in the vector \mathbf{x}_{it-1} . μ_t here denotes a full set of time effects, which capture common shocks to (common 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.

¹⁶By 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, see, for example, Law (1977) for Nigeria, Harms (1981) for the Congo and Miller (1988) for Angola. During the same period, the Dutch and British East Indian Companies began to have a large impact on the organization of Asian societies (e.g., Reid, 1988, 1993).

Ordinary Least Square (OLS) regressions of (1) will lead to consistent estimates of the parameter of interest, γ , when $Cov(d_{it-1}, v_{it}) = Cov(y_{it-1}, v_{it}) = Cov(x_{it-1}^j, v_{it}) = 0$ for all j where x_{it-1}^j is the j th element of the vector \mathbf{x}_{it-1} . In other words, OLS estimation requires that there be no omitted variables correlated with the right-hand side variables in the regression.

It is also useful to note that, in contrast to (2), which will be our main focus below, equation (1) does not include any country fixed effects. Therefore, the only source of long-run differences in democracy across countries are the right hand side variables, y_{it-1} and \mathbf{x}_{it-1} . To see this more clearly, suppose that $\alpha < 1$, and that $y_{it-1} = y_i$ and $\mathbf{x}_{it-1} = \mathbf{x}_i$ for all t , and also ignore the common factor, μ_t (i.e., set $\mu_t = 0$). Then (1) implies that

$$\text{as } t \rightarrow \infty, E(d_{it}) \rightarrow d_{i\infty} \equiv \frac{\gamma y_i + \mathbf{x}_i' \boldsymbol{\beta}}{1 - \alpha}.$$

In other words, the only cross-country difference in the long-run democracy score will be due to differences in income or other covariates across countries. Whenever $d_{it} \neq d_{i\infty}$, there will also be “mean reverting” dynamics as d_{it} moves towards $d_{i\infty}$ (at a speed determined by α). This expression also highlights that estimates of the relationship between democracy and income from equation (1) will reveal the “cross-sectional” relationship between these two variables (i.e., they will capture the fact that richer countries are more democratic).

Table 2a uses the Freedom House data and Table 2b uses the Polity data to present pooled cross-sectional regressions of democracy on income. Results using the dichotomous Przeworski/Boix-Rosato index are also similar, and are discussed in Section 9. All columns pool the time-series and cross-sectional variation in the data, and include a full set of time effects, but no fixed effects. All standard errors in the paper (unless indicated otherwise) are robust against arbitrary heteroscedasticity in the variance-covariance matrix, and allow for clustering at the country level.¹⁷

Columns 1-3 are the most parsimonious specifications, including only one lag of democracy, log GDP per capita and time effects, and are estimated by Ordinary Least-Squares (OLS). Column 1 is for our entire (base) sample of 150 countries, over the period 1960-2000. Lagged democracy is highly significant, and shows a considerable degree of persistence (mean reversion) in democracy. More explicitly, the estimate of 0.706 in column 1, for example, implies that around the hypothetical $d_{i\infty}$, a 10% higher score of democracy

¹⁷Clustering is a simple strategy to 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).

five years ago is typically associated with a 7% higher score of democracy today.¹⁸

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 0.072 (standard error = 0.010) in column 1 implies that a 10 percent increase in GDP per capita is associated with an increase in the Freedom House score of less than 0.007, which is very small (for comparison, the gap between the United States and Colombia today is 0.5). Column 2 repeats the same regression on a sample of 136 countries between 1960 and 2000 omitting the first ten years after independence, with very similar results. Column 3 shows that using a balanced sample from 1970 to 2000 has little effect on the results.

Columns 4-6 add average years of schooling and log population to these basic specifications. In column 4, for example, the coefficient on income is now smaller, 0.053 (standard error = 0.017), but still statistically significant at 1 percent with the Freedom House measure. Educational attainment itself is significant, and shows a positive association between education and democracy (log population is insignificant). We can think of the estimates of the coefficients on log GDP per capita in columns 1 through 3 as corresponding to “the total effect of income,” for example, incorporating the indirect effect working through education (e.g., higher incomes increasing education, and higher education making democracy more likely). The estimates in columns 5 and 6 are similar.

Following the baseline specification in Barro (1999), column 7 adds the second lag of democracy, the male-female education gap, the urbanization rate and a major oil-producer dummy (following the definition in Barro, 1999) to the balanced panel regression of columns 3 and 6. The second lag of democracy is not significant, suggesting that a single lag is doing an adequate job of capturing the mean-reverting dynamics in democracy. As in Barro (1999), the male-female education gap is negative (significant at 5 percent) and the urbanization rate is also negative.¹⁹ The dummy for major oil producer, on the other hand, is insignificant. Finally, in column 8, we estimate the same model using Seemingly Unrelated Regression (SUR), for comparison with Barro’s (1999) results, once again with similar results.²⁰

¹⁸The fact that the democracy index takes discrete values creates no difficulty for inference with OLS, as long as standard errors are corrected for heteroscedasticity.

¹⁹As in Barro (1999), the urbanization rate has the opposite sign to what one may have expected (a natural conjecture would be that greater urbanization leads to greater democracy). This is presumably because urbanization is highly correlated with income per capita, causing a potential multicollinearity problem.

²⁰Barro’s use of SUR is motivated by the concern that the realizations of the democracy score for the same country over multiple time periods are correlated. The fixed effects and the clustering of the standard errors below take care of this correlation directly.

Table 2b has an identical structure but uses the Polity IV data, and shows similar results. Overall, the regressions in Tables 2a and 2b 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

4.1 Model and Interpretation

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

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

which differs from (1) because it includes a full set of country dummies, the δ_i 's.

These country dummies capture any time-invariant country characteristic that affect the equilibrium democracy level. Consequently, (2) allows for a very different behavior of democracy than equation (1) above. To highlight this difference, suppose again that $\alpha < 1$, $y_{it-1} = y_i$ and $\mathbf{x}_{it-1} = \mathbf{x}_i$ for all t , and $\mu_t = 0$. We then have that

$$\text{as } t \rightarrow \infty, E(d_{it}) \rightarrow d_{i\infty} = \frac{\gamma y_i + \mathbf{x}'_i \boldsymbol{\beta} + \delta_i}{1 - \alpha}.$$

Consequently, even if two countries have the same values of the covariates, they can have different long-run equilibrium values of democracy—because of the δ_i 's.

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, if $Cov(y_{it-1}, \delta_i + u_{it}) \neq 0$ (or $Cov(x_{it-1}^j, \delta_i + u_{it}) \neq 0$) but $Cov(y_{it-1}, u_{it}) = Cov(x_{it-1}^j, u_{it}) = 0$ for all j , then fixed effects estimator will be consistent.

This structure of correlation is particularly relevant in this context, because our reading of the historical evidence suggests that there are underlying political and social forces shaping both equilibrium political institutions and the potential for economic growth. Nevertheless, there should be no presumption that fixed effects regressions will necessarily estimate the causal effect of income on democracy.

To illustrate this point, consider a simplified version of (2), without the lagged dependent variable and the other covariates, but an additional error component (also changing the timing to simplify the discussion):

$$d_{it} = \gamma y_{it} + \delta_i^d + \eta_i^d t + u_{it}^d. \quad (3)$$

Moreover suppose that the statistical process for (log) income per capita can be represented as

$$y_{it} = \delta_i^y + \eta_i^y t + u_{it}^y, \quad (4)$$

with u_{it}^d and u_{it}^y orthogonal to each other, and δ_i^d and δ_i^y potentially correlated with each other. The parameter γ corresponds to the causal effect of income on democracy. The terms $\eta_i^d t$ and $\eta_i^y t$, as well as the δ_i^d and δ_i^y terms, capture the effect of underlying common factors on democracy and income. While δ_i^d and δ_i^y correspond to fixed differences, $\eta_i^d t$ and $\eta_i^y t$ affect the *evolution* in democracy and income, for example, because of factors encouraging economic growth and democratization over time in some countries.

In this case, pooled OLS regression of d_{it} on y_{it} will lead to inconsistent estimates. Fixed effects estimators will lead to consistent estimates, however, only if η_i^d and η_i^y are orthogonal. In the presence of factors that affect the joint evolution of democracy and income, there is no reason to expect that the fixed effects estimates will be consistent—such factors will most likely imply that η_i^d and η_i^y are present and correlated. Nevertheless, under plausible assumptions, the inclusion of fixed effects will lead to estimates that are less biased than the pooled OLS estimates. To see this, suppose that we only have data for two periods, $t = 0$ and $t = 1$. Then, the probability limit of the pooled OLS estimator $\hat{\gamma}$ is:

$$\text{plim} \hat{\gamma} = \gamma + \frac{\text{Cov}(\delta_i^d + \eta_i^d, \delta_i^y + \eta_i^y)}{\text{Var}(\delta_i^y + \eta_i^y + u_{it}^y)},$$

whereas the fixed effects estimator $\tilde{\gamma}$ will have

$$\text{plim} \tilde{\gamma} = \gamma + \frac{\text{Cov}(\eta_i^d, \eta_i^y)}{\text{Var}(\eta_i^y + u_{it}^y - u_{it-1}^y)}.$$

As long as $\text{Cov}(\delta_i^d, \delta_i^y)$ is positive and large, which will be the case when there are important omitted common factors, such as historical influences, we expect the bias in the pooled OLS estimator to be larger. Moreover, under the plausible assumption that $\text{Cov}(\eta_i^d, \eta_i^y) \geq 0$, which means that the trends in democracy and income are *not* negatively correlated, the fixed effects estimator will be biased upwards, so it will provide an upper bound on the causal effect of income on democracy.

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 effect 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 OLS estimates, and then consider various alternative estimation strategies.

4.2 Fixed Effect Estimates

Tables 3a and 3b present our basic results, the first using the Freedom House score and the second the Polity score. Column 1 is identical to column 1 of Tables 2a and 2b, with only the lagged dependent variable and lagged income per capita. In neither table is income per capita significant, and has a very small coefficient, 0.010, with a standard error of 0.035 (for example, compared to 0.072 in column 1 of Table 2a). With the Polity data in Table 3b, the estimate has in fact the wrong (negative) sign, -0.006 (standard error=0.039).

That these results are not driven by some econometric problems or some unusual feature of the data is shown in Figures 4 and 5 which 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).²¹ They show clearly that there is no strong relationship between income growth and changes in democracy over this period.²²

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 tables 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 2, is to use the methodology proposed by Anderson and Hsiao (1982), which is to time difference equation (2), which yields

$$\Delta d_{it} = \alpha \Delta d_{it-1} + \gamma \Delta y_{it-1} + \Delta \mathbf{x}'_{it-1} \boldsymbol{\beta} + \Delta \mu_t + \Delta u_{it}, \quad (5)$$

²¹The 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.

²²We 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.

where the fixed country effects are removed by time differencing. Although equation (5) cannot be estimated consistently by OLS, in the absence of serial correlation in the original residual, u_{it} (i.e., no second order serial correlation in Δu_{it}), d_{it-2} is uncorrelated with Δu_{it} , so can be used as instrument for Δd_{it-1} to obtain consistent estimates (and similarly, y_{it-2} is used as an instrument for Δy_{it-1} , see Anderson and Hsiao, 1982. We find that this procedure leads to negative estimates (e.g., -0.104, standard error = 0.107 with the Freedom House data), and shows no evidence of a positive effect of income on democracy.

Although the instrumental variable estimator of Anderson and Hsiao (1982) leads to consistent estimates, it is not efficient, since, under the assumption of no further serial correlation in u_{it} , not only d_{it-2} , but all further lags of d_{it} are uncorrelated with Δu_{it} , and can also be used as additional instruments. Arellano and Bond (1991) develop a Generalized Method-of-Moments Estimator (GMM) using all of these moment conditions. When all these moment conditions are valid, this GMM estimator is more efficient than the Anderson and Hsiao's (1982) estimator. We use this GMM estimator in column 3. The coefficient is now even more negative and more precisely estimated, -0.129 (standard error = 0.76).²³ In addition, the presence of multiple instruments in the GMM procedure allows us to investigate whether the assumption of no serial correlation in u_{it} can be rejected and also test for overidentifying restrictions. With the Freedom House data, the AR(2) test and the Hansen J test indicate that there is no further serial correlation and the overidentifying restrictions are not rejected.

With the Polity data, both the Anderson and Hsiao (1982) and Arellano and Bond (1991) procedures lead to more negative (and statistically significant) estimates. However, in this case, though there continues to be no serial correlation in u_{it} , the overidentification test is rejected, so we need to be more cautious in interpreting the results with the Polity data.

Finally, column 4 estimates (2) with OLS using annual observations. This is useful since the fixed effect 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

²³Additional time differenced instruments in the level equation can be included following Arellano and Bover (1995). However, these instruments would only be valid if the distribution of fixed effects is independent from the distribution of initial values, or more put more simply, if the time differenced instruments are independent of the fixed effect. Since this is clearly not appealing in this context, we do not include these additional instruments.

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 (while democracy is strongly predicted by its lags, as was the case in earlier columns).

In columns 5-8 of both tables, we add average years of schooling and population as additional explanatory variables, and repeat the four estimation strategies, with very similar results. In particular, income never has a positive effect on democracy, and there is also no evidence of a positive relationship between education and democracy.

Table 4 shows the robustness of these results in alternative samples. To save space, we only report the robustness checks for the Freedom House data (the results with Polity are similar and are available upon request). Columns 1-3 show the regressions corresponding to columns 1, 3 and 4 of Table 3a for a sample without observations during the first ten years after independence. This is useful to check whether the lack of correlation is driven by the political instability that some countries experience after independence. Columns 4-6 repeat these regressions for the balanced sample from 1970 to 2000. All of these columns provide very similar results. Columns 7-9 exclude sub-Saharan Africa, and show that the results are not driven by unusual behavior of African countries, whereby many became democratic immediately after independence and later lapsed into nondemocracy. There is no evidence of a significant positive effect of income on democracy in any of the specifications.

In addition, in regressions not reported here, we checked for non-linear and non-monotonic effects of income on democracy and for potential non-linear interactions between income and other variables, and found no evidence of such relationships.²⁴

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.

²⁴The only subsample where we find a positive association between income per capita and democracy conditional on fixed effects is the postwar sample with 18 West European countries. However, this relationship is only true with the Freedom House data, and not with the Polity data, and also disappears when we look at a longer sample than the postwar period alone. Details are available upon request.

5 Understanding the Fixed Effects Results

In the introduction, we argued 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 (see the discussion in Acemoglu, Johnson and Robinson, 2004a). 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 these 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 nondemocracy.²⁵

According to this theory, the paths of economic and political development are intertwined, and we expect democracy and income to evolve jointly. Nevertheless, conditional on a given development path, economic growth does not necessarily lead to democratization.²⁶

As an example, consider the experience of economic growth based on forced labor working in the coffee plantations of Central America; there is no natural reason to expect that this process of economic growth enriching the plantation owners should lead to democracy. Instead, societies embarked on a development path based on free labor and the rule of law will become democratic and also generate economic growth. 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 followed. We now investigate this question.

²⁵See the discussion of the role of critical junctures in Collier and Collier (1991).

²⁶Similarly, there is no natural presumption that, conditional on a particular development path, a temporary improvement in the democracy score should lead to higher incomes.

5.1 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. They emphasize that two factors affecting colonial strategies were the mortality rate faced by European settlers and the population density of indigenous peoples before colonization (in practice around 1500). Higher mortality rates discouraged Europeans from settling, and made an extractive strategy, associated with coercive and non-participatory institutions more likely. More densely-settled areas also discouraged European settlements, and even conditional on settlements, encouraged the establishment of coercive institutions designed to control the indigenous population and to transfer resources from them.

We next use these ideas in the sample of former European colonies, where European intervention created a potentially exogenous source of divergence in political and economic development paths. We examine the effects of both the density of the indigenous population in 1500 (population density in 1500, for short) and of settler mortality. 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 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 path on current outcomes. For this reason, we look at the measure of constraints 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.²⁷

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, so it is important to control for the date of independence. In addition and potentially more

²⁷For example, Perú had a constraint on the executive score equal to 0.33, while the United States's score was 1 in 1800. These numbers are clearly indicative of the institutions that these countries had within the colonial period itself.

important, 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.

5.2 Historical Variables and Fixed Effects

Our basic results with the former colony sample are in Tables 5a and 5b. Table 5a uses the Freedom House measure of democracy while Table 5b uses the Polity index. The first two columns of both tables show the pooled cross-sectional OLS results and the basic fixed effects regressions on this sample of countries.²⁸ Column 1 shows a strong correlation between income and democracy, which again disappears once fixed effects are introduced.

The remaining six columns of the table drop the fixed effects and control for various variables which might capture institutional differences. A glimpse of the overall patterns can be obtained from Figures 6-9, which show the bivariate correlation between the fixed effects estimated in column 2 of Table 5a (with Freedom House data) and the four historical variables: settler mortality rates, the density of the population in 1500, constraint on the executive at (shortly after) independence, and year of independence. In all four cases, there is a very close association between the fixed effects and these historical variables, suggesting that the fixed effects were indeed related to the divergent development paths of these countries.

The regressions document this relationship in greater detail and investigate whether the inclusion of these variables removes the cross-sectional association between income and democracy. In column 3, we start with log settler mortality.²⁹ This variable is highly significant and indicates that countries with higher settler mortality rates more than 200 years ago are now less democratic. Nevertheless, income is still significant (though with a smaller coefficient than in column 1). Column 4 repeats this regression with log population density in 1500, which is also negative and significant (only at 5 percent), indicating that areas more densely settled before colonization are now less democratic. Once again income

²⁸Since we construct the constraint on the executive at independence using the first ten years of independence data, in Tables 5a, 5b and 6, a country enters the sample only ten years after its independence, as in Table 4, columns 1-3.

²⁹We use the log of this variable rather than the level, since otherwise some African countries, which have very high European mortality rates, have a disproportionate effect on the relationship (see Acemoglu, Johnson and Robinson, 2001).

is still positively correlated with democracy.

Column 5 adds the constraint on executive at independence and date of independence, which are also highly significant (both at 1 percent), and show that countries that became independent earlier and those that had more constraints on their political elites (executives) in the first ten years after independence are significantly more democratic today, and the inclusion of these two variables significantly weakens the correlation between democracy and income today. The coefficient of income is now at 0.025 with a standard error of 0.013.

In column 6, we include all four historical variables together. Now income is no longer significant, and has a coefficient of 0.016, with a standard error of 0.018 (compared to 0.059 in column 1). In this regression, independence year and the constraint on the executive at independence are significant at 1 percent, though log population density and log settler mortality are not statistically significant.

Column 7 is our baseline historical specification, with only independence year, constraint on the executive at independence and log population density in 1500. All three of these historical variables are statistically significant at 5 percent or less, and income per capita is highly insignificant (coefficient 0.012, standard error= 0.014).

Finally, column 8 shows similar results when education and population are included. Neither of these two variables are significant, and there is still no significant relationship between income and democracy (though population density in 1500 is no longer significant in this specification either).

The results with the Polity IV data in Table 5b are generally similar, though all variables including income and population density are somewhat weaker in this table than in Table 5a.

The robustness of these historical results are investigated in Table 6, which focuses on the results with the Freedom House data (results with Polity are similar and are available upon request). Many scholars have emphasized the importance of other underlying factors, which might be important both for democracy and economic prosperity. Particularly widespread is the argument that countries formally colonized by the British inherit a culture more compatible with democracy (Churchill, 1956, Weiner, 1987, Lipset et al., 1993, Muller, 1995). Column 1 of Table 6 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 they are jointly insignificant. 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.³⁰ When these colonial dummies are added the impact of the variables measuring the historical development process is largely unchanged, and there continues to be no effect of income per capita on democracy.

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 democracy. None of the individual coefficients are significant and an F-test again shows them to be jointly insignificant. Again as long as the variables proxying for the historical development process are controlled for, per capita GDP has no influence on democracy.

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 affects neither the relationship between the historical variables and democracy nor the (lack of) correlation between income per capita and democracy. Latitude itself is insignificant.

Column 4 repeats the baseline regression for the balanced panel, with similar results. Column 5 adds data for Jordan, Lebanon and Syria, and Yemen which were not counted as former colonies in our basic specifications for reasons discussed above.³¹ This again has no effect on the results.

Finally, column 6 adds two more lags of democracy and shows that the effect of historical variables, in particular of constraint on the executive at independence is robust. This result shows that constraint on the executive at independence does not capture some slow dynamics in democracy, but is related to the political and economic development path of the colonies, most likely determined at a critical juncture in their histories.

Table 7 complements Figures 6-9 and shows that the regressions using historical institutions are capturing the same phenomena as those that control for fixed effects. Table 7 regresses the fixed effects in column 2 of Tables 5a and 5b on our historical variables,

³⁰This 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. 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.

³¹We do not have GDP data for Bahrain, Iraq, Kuwait, Oman, Qatar, Saudi Arabia, and United Arab Emirates.

constraint on the executive at independence, date of independence, log population density in 1500, and log settler mortality. The top panel shows that in all cases, the four variables are all individually significant and explain a considerable fraction of the variation in the fixed effects, confirming our interpretation of the results in Tables 3a and 3b that the fixed effects are capturing historical characteristics affecting both political and economic development. The bottom panel of Table 7 tests whether the coefficient estimates in Tables 5a and 5b, columns 2 and 6, and columns 2 and 7, are statistically distinguishable. More explicitly, we test the hypothesis that the coefficient estimates on log GDP per capita in Tables 5a and 5b, columns 2 and 6, and columns 2 and 7, are equal. The numbers in the table show that in no case can we reject this hypothesis at 5 percent significance or less. These results together suggest that for the former colonies sample the fixed effects in Table 3a,b were capturing some country characteristics very closely related to the three historical variables investigated in Table 5, thus bolstering our argument that there are some underlying institutional factors affecting both the potential for economic growth and the political equilibrium of these countries.

5.3 Interpretation

The regression results discussed above are consistent with our reading of the historical evidence. Acemoglu, Johnson and Robinson (2001, 2002) document that the mortality rates faced by Europeans and population density at the time of colonization were major determinants of European colonization strategy, and the subsequent political and economic development paths on the countries.

To illustrate the mechanisms at work, we now briefly discuss the impact of initial population density on the historical creation of institutions within the Americas.³² In societies such as Perú and México with large indigenous populations to exploit, Spanish colonizers created a set of economic institutions, such as the *encomienda* and *mita*, to extract rents from these peoples. Because they significantly weakened the economic incentives for the vast mass of the population, these institutions discouraged the economic development of the society. Nevertheless, they were highly attractive for the Spanish colonists and crown, who were the beneficiaries of the extraction from these colonies. These economic institutions were supported by highly authoritarian political institutions. Even after independence, the political institutions of these societies endowed power to precisely those groups who benefitted from the extractive economic institutions. Coatsworth's (1978, p.

³²See Acemoglu, Johnson and Robinson (2001) for a detailed discussion of the role of settler mortality and its impact on institutions.

95) analysis of the continuity of Mexican institutions after independence supports this view:

“Méxican independence came through a virtual coup d’état by the colony’s Creole elite, carried out largely to separate México from the liberalizing process under way in the Mother country ... The principal proponent of these conservative efforts was a limited social group of major landowners and industrialists in the center of the country ... who had been the principal beneficiaries in the colony of the crown’s interventionism or who, like the large merchant houses of the capital, sought to regain privileges the crown itself had abolished in the reforms of the late Bourbon era.”

The experience of many other Latin American countries is also similar, particularly the Guatemalan case (see Acemoglu, Johnson and Robinson, 2004a). Moreover, where population density was lower within the Spanish empire, different institutions emerged. For example, the lowest level of Spanish administration was the *cabildo*, a council that at least nominally had the power to levy taxes and was responsible for administering certain services such as schools. The members of the *cabildo* were generally appointed but there was the possibility of a *cabildo abierto* (and open council) where all the citizens could express their opinions and attempt to influence policies. This institution was as close to real democracy as existed within the Spanish empire. Such meetings were rarely held in the central areas with high densities of indigenous peoples such as Perú or México, but they were regularly held in peripheral and insignificant areas such as Santiago and Buenos Aires (see Lang, 1975, p. 28). Relative insignificance in the Spanish empire was closely related to low population density (Lockhart and Schwartz, 1983, pp. 33-35).

These early institutions had major implications for political development. Democracy, by its very nature, would have re-allocated political power in ways highly unfavorable to such elites. The anticipated consequences of democracy therefore often gave them strong incentives to engage in repression and coercion to sustain non-democratic regimes.

The situation could not have been more different in North America. Jamestown and subsequent colonies quickly evolved into quasi-democracies, with economic opportunities, such as access to land, much more equally distributed than in high-population density colonies. Population density played a major role in this difference. With few Amerindians to use as labor and no existing systems of tribute to take over, the only way to make money in the new colony was to induce British labor to move there. However, as Galenson (1996,

pp. 136-137) notes about the initial colonizing attempts by the Virginia Company in Jamestown,

“what the company failed to anticipate ... was that its protection was not essential to the workers, who could consequently rebel against their harsh treatment by running away to live with the Indians or simply by starting their own small settlements. Faced with this effective competition for the workers’ labor, the company had to recognize that it did not have the monopsony position it had anticipated as the only employer in the region’s labor market and was forced to respond by offering higher wages and better living and working conditions. . .

The directors of the Virginia company were only the first of many who had to adapt to a world completely unlike the one they knew and had assumed to be universal. 17th century Englishmen lived in an economy in which land had long been scarce and labor abundant, and employers simply took for granted the availability of workers at very low wages. In the course of the settlement of English America, a succession of employers and workers would be surprised by the full social and economic implications of a new world in which factor proportion were radically different.”

The low population density in the United States therefore had a crucial impact on early institutions, directly as well as indirectly by placing effective constraints on the elites who wanted to introduce more extractive institutions. Low population density necessitated the use of indentured labor, but it also made it very hard to control the laborers. It was this factor that was behind the creation of the General Assembly in Jamestown in 1619 (Acemoglu and Robinson, 2004, chapter 6).

The impact of population continued to be felt after Jamestown. Galenson (1996, p. 143) notes; “The extreme labor shortage ... allowed many early settlers to gain their economic independence from the manorial lords, and establish separate farms ... Thus just as in Virginia, in Maryland the colonial labor problem undermined the initial plans for a rigid social hierarchy, as Lord Baltimore’s blueprints for a manorial society were largely swept away and early Maryland became an open and fluid society, which offered considerable economic and social opportunity.”

The other historical variables that are significant in the regressions also have natural interpretations along the same lines. The measure of early institutions (constraints on the executive at independence) is as close as we can get to an independent measure of the political institutions that the country had when it emerged from being a colony. Since

institutions of the former colonies at independence were heavily influenced by the colonial legacy, a measure of early institutions directly proxies for the types of institutions that were in force during the colonial period. This is useful because many factors other than settler mortality and population density affected the evolution of colonial institutions.

The date of independence is another potential measure of historical institutions and of the development path of a colony, especially since settler colonies, and colonies where Europeans developed economic and political institutions conducive to economic and political development were the ones that became independent early on. Moreover, many European colonies experienced considerable amounts of political instability in the decades after independence because it took time for central authority to be established. This was true in the majority of Latin American countries up until at least the 1860's and 1870's, and has also been the case in much of Africa since the 1960's. We therefore expect that the longer a country has been independent, the more politically stable it is and the more likely that democracy will be to emerge.

The connection between these historical variables and current economic and political outcomes is therefore plausible. The settler mortality and the density of the indigenous population affected the profitability and feasibility of the different colonization strategies, leading to the divergent development paths; measures of early institutions proxy for the political organization of the society during colonial times, and the date of independence is closely linked to the colonization strategy, since non-extractive colonies typically gained their independence earlier than extractive colonies.

6 Democracy and Income in the Long Run

We have so far followed much of the existing literature in focusing on the postwar period, where the democracy and income data are of higher quality. Nevertheless, it is important to investigate whether the relationship between income and democracy may have been different before the war, or whether the long-run relationship is different from that which we find in a 40-year sample. The first issue is of interest because when Lipset (1959) formulated the modernization theory in the late 1950s, he had in mind the connection between industrialization, income growth and democracy that seemed to characterize the development experiences of the late 19th and early 20th centuries. The second issue is potentially more important. Recall that according to our interpretation, there is a limited effect of income on democracy conditional on the development path of a society, but common factors possibly affect both long-run political and economic development. If

so, we should find “more covariation” between economic growth and democratizations as we look at longer samples where we can observe countries *along* different development paths.³³

6.1 Democracy-Income Covariation in the Long Run

To elaborate on this point, let us return to the statistical model Section 4, equations (3) and (4). Our emphasis on political and economic development paths diverging at some critical juncture would correspond to $\text{Cov}(\eta_i^d, \eta_i^y) > 0$, i.e., to a scenario where countries marching towards democracy are also those that are becoming relatively prosperous. As noted above, this structure implies that even the fixed effect estimates of the impact of income on democracy would be biased upwards.

Now imagine we have data for two time periods again, $t = 0$ and $t = s \geq 1$. In this case, time-differencing equations (3) and (4), we have:

$$d_{is} - d_{i0} = \gamma (y_{is} - y_{i0}) + \eta_i^d s + u_{is}^d - u_{i0}^d,$$

and

$$y_{is} - y_{i0} = \eta_i^y s + u_{is}^y - u_{i0}^y.$$

Let us suppose that the u_i^d and u_i^y 's are iid, and denote the variance of η_i^y by $\sigma_{\eta^y}^2$ and of u_i^y by $\sigma_{u^y}^2$. It can then be shown that the probability limit of the fixed effect estimator using these two data points is:

$$\text{plim} \tilde{\gamma} = \gamma + \frac{\text{Cov}(\eta_i^d, \eta_i^y)}{\sigma_{\eta^y}^2 + 2\sigma_{u^y}^2/s^2}. \quad (6)$$

Equation (6) implies that if $\gamma = 0$, we should still find a positive association (“covariation”) between income and democracy when we look at long samples.

Moreover, if $\sigma_{u^y}^2$ is large, i.e., if there is sufficient variation in income, then as $s \rightarrow 1$, we should find $\text{plim} \tilde{\gamma}$ approach γ . In contrast as $s \rightarrow \infty$, i.e., as we look at longer intervals, we should find a positive relationship between changes in democracy and changes in income even when $\gamma = 0$. This reasoning motivates our investigation of the relationship between income and democracy in the longer historical samples, to see whether as we look at longer intervals we find a stronger positive association between the evolution of democracy and economic growth.

³³It is important to emphasize that because, as noted above, fixed effect regressions do not solve all omitted variable bias problems, a regression of democracy on income with fixed effects over a long panel does *not* reveal the long-run causal effect of income on democracy. Instead, it is informative about their *joint evolution*, whereby some other factor might be responsible for their positive comovement.

6.2 Democracy and Income Over the past 200 Years

Although historical data are typically less reliable, the Polity IV dataset extends back to the beginning of the 19th century for all independent countries, and Maddison (2003) gives estimates of income for many countries during this period. Using these data, we first construct a 5-yearly and a 10-yearly dataset between 1840 and 1940. Countries that have been independent before 1900 and have more than 5 observations in the 1840 to 1940 data period are included in this dataset. The result is an unbalanced panel with a country entering when there are observations from both Polity and Maddison.³⁴

Table 8, Panel A reports some basic regressions with this dataset. In column 1, pooled cross-sectional OLS regressions again show the conventional result, with income per capita having a positive and significant sign. Column 2 adds fixed effects and similar to our results above, the coefficient estimate on income per capita becomes insignificant.

Columns 3 and 4 repeat these regressions using ten-year instead of five-year intervals. We use this strategy to check whether the lack of a significant effect of income on democracy is caused by measurement error or country noise in democracy over the five-year horizon, and also to investigate whether there could be an effect of income on democracy at lower frequencies than at 5 years. The basic results are identical to those in the first four columns of the table.

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

We further investigate these ideas in Panels B and C by taking the same sample of countries and extending the dataset to 2000 (thus constructing a 160-year panel with 28 countries). Without fixed effects, higher per capita income is again strongly associated with greater democracy. In this 160 year sample, there is also a positive coefficient on log GDP per capita even once we control for fixed effects. Figure 10 depicts the change in the Polity composite index versus the change in log GDP per capita between 1870 and 1995 (dates chosen to maximize sample size), and shows a weak but positive relationship between these dates.³⁵

Nevertheless, further investigation suggests that the results in the 1840-2000 panel

³⁴The countries in this section are Argentina, Austria, Belgium, Brazil, Bulgaria, Canada, Chile, China, Colombia, Denmark, France, Germany, Greece, Hungary, Italy, Japan, México, Netherlands, Norway, Perú, Portugal, Spain, Sweden, Switzerland, United Kingdom, United States, Uruguay, Venezuela.

³⁵The corresponding regression yields the coefficients of 0.076, with a standard error of 0.123.

are partly driven by one country, Venezuela, especially in the postwar correlation between income per capita and democracy in Venezuela.³⁶ The correlation between the Venezuelan GDP and democracy over the relevant period appears to be related to increases in oil revenues—not the type of income variation generally thought to promote democratization. This is illustrated in Figure 11 which depicts the deviations of the democracy score, oil and non-oil GDP in Venezuela from the sample means since 1950.³⁷ This figure shows a close association between oil income and democracy, but not necessarily between non-oil income and democracy. Panel C therefore repeats the regressions in Panel B without Venezuela. In this case, though the estimate of the coefficient on income per capita remains positive, it is no longer statistically significant.

Overall, we interpret the pattern of the results in Table 8 as consistent with the argument encapsulated in equation (6), that the covariation between income and democracy should only manifest itself when we look at sufficiently long time spans where we can see the variation *across development paths*. In the 100-year interval between 1840 and 1940, we still do not see a positive association between democracy and income, and in the 160-year panel, there is some positive relationship.

6.3 Democracy and Income Over the past 500 Years

Next we push the reasoning in equation (6) further; equation (6) suggests that if we could construct a longer dataset, we should obtain a stronger relationship. Although a systematic dataset spanning more than 160 years cannot be constructed, there exist rough estimates of income per capita for almost all areas of the world in 1500. Moreover, we also know that around the turn of the 16th century, no country was “democratic” according to the current definitions. Nevertheless, there were some significant differences in the political institutions of countries around the world even at this date. While most countries outside Europe were ruled by absolutist regimes, some European countries had developed certain constraints on the behavior of their monarchs. Acemoglu, Johnson and Robinson (2004b) provide a coding of constraint on the executive for European countries (based on the Polity definition) going back to 1500 from various sources. It also appears reasonable to assume that constraint on the executive for non-European countries and the other components of the Polity index (competitiveness of executive recruitment, openness

³⁶In Tables 3-7, Venezuela did not have a disproportionate effect on the results because these regressions included a considerably larger set of countries than Table 8.

³⁷The breakdown of GDP into oil and non-oil is not available before. Both oil and non-oil GDP are deviations from the sample mean of GDP, in all cases the sample consisting of the 28 countries in the long-run sample.

of executive recruitment, and competitiveness of political participation) both for European and non-European countries should take the lowest score in 1500. Based on this information, we can construct the Composite Polity index for 1500.³⁸ Combining these data with estimates of income per capita, we can get a glimpse of the variation between income and democracy over this 500 year interval.

This is done in Figure 12 using Maddison (2003) estimates of income per capita in 1500.³⁹ The figure shows a strong positive relationship between changes in democracy and changes in income for 143 countries.⁴⁰ This plot corresponds to a fixed effect regression with only two time periods. This regression yields the following estimates:

$$\Delta d_{i,1995-1500} = \text{constant} + \begin{array}{c} 0.139 \\ (0.033) \end{array} \Delta y_{i,1995-1500},$$

where $\Delta d_{i,1995-1500}$ is the change in our estimate of democracy between 1500 and 1995, and $\Delta y_{i,1995-1500}$ is the change in log income per capita between the same dates ($R^2 = 0.17$). As also shown in Figure 12, there is now a strong relationship between income and democracy.

We repeat the same exercise using an estimate for income per capita in 1500 calculated from urbanization rates along the lines of the arguments in Acemoglu, Johnson and Robinson (2002). This paper documented a strong association between urbanization rates and income per capita, suggesting that urbanization rates can be used to proxy income per capita. The results in their Table 2 suggest that we can form an estimate of log income per capita in 1500, y_{i1500} , as

$$\hat{y}_{i1500} = 0.038u_{i1500},$$

where u_{i1500} is the urbanization rate in 1500. Figure 13 shows the relationship between change in democracy and change in log income per capita between 1500 and 1995 using this method of estimating income per capita in 1500 for a sample of 84 countries. The corresponding regression now gives:

$$\Delta d_{i,1995-1500} = \text{constant} + \begin{array}{c} 0.163 \\ (0.034) \end{array} \Delta y_{i,1995-1500},$$

with an R^2 of 0.23, which is similar to the results with the income estimates from Maddison (2003).⁴¹

³⁸Details are available upon request. Below we also report results assigning the lowest score of democracy to all countries in 1500, with very similar results.

³⁹For this exercise, when Maddison gives estimates for individual countries, we use these estimates. When he gives estimates for broad geographic areas, we assign this estimate to all countries now occupying these territories.

⁴⁰1995 is used as the end date to maximize sample size.

⁴¹We repeated these regressions assigning a value of 0 to the Polity index in 1500. Using Polity and

This exercise shows that even though there is no causal effect of income on democracy conditional on the long-run development path of a society, over the very long run democracy and income show significant correlation. This is consistent with our interpretation that the cross-country correlation between income and democracy we observe today reflects the long-run divergent development paths of these societies.

7 Transitions to and from Democracy

Following the research of Przeworski and Limongi (1997), Przeworski et al. (2000), and Boix and Stokes (2003), it is also interesting to decompose the democracy scores into positive and negative changes. The first two studies claim that the main effect of income per capita on democracy comes through its effects on coups or movements away from democracy, not movements towards democracy, while Boix and Stokes (2003) find income affects both transition to and away from democracy in a longer sample.

To investigate these ideas, we distinguish between transitions to and from democracy by separating positive and negative changes in the democracy scores. The most straightforward way of doing this is to modify the model in equation (2) as follows:

$$d_{it}^+ = \alpha d_{it-1} + \gamma y_{it-1} + \mathbf{x}'_{it-1} \boldsymbol{\beta} + \mu_t + \delta_i + u_{it} \quad (7)$$

for transitions to democracy and

$$d_{it}^- = \alpha d_{it-1} + \gamma y_{it-1} + \mathbf{x}'_{it-1} \boldsymbol{\beta} + \mu_t + \delta_i + u_{it} \quad (8)$$

for transitions from democracy, where $d_{it}^+ = \max\{d_{it}, d_{it-1}\}$ and $d_{it}^- = \min\{d_{it}, d_{it-1}\}$.⁴² 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. We only report these results using the Freedom House data, while Section 9 reports similar results using the dichotomous measure of Boix and Rosato, using both linear probability models and probit-logit models.

income per capita from Maddison, the coefficient is 0.148 (standard error=0.035), $N = 143$, $R^2 = 0.18$. Using Polity and income per capita derived from urbanization estimates, the coefficient is 0.175 (standard error=0.034), $N = 84$, $R^2 = 0.25$. We also repeated this exercise using the Freedom House index in 1995 and assigning a score of 0 to all countries in 1500, with similar results. With the Freedom House index and income per capita from Maddison, the coefficient is 0.202 (standard error=0.038), $N = 155$, $R^2 = 0.32$. Finally, using income per capita derived from urbanization estimates, the coefficient in this case is 0.255 (standard error=0.031), $N = 85$, $R^2 = 0.44$.

⁴²Although (7) and (8) are non-linear equations, the fixed effects, the δ_i 's, enter additively and can be differenced out to achieve consistent estimation.

In Tables 9 and 10, 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 using the pooled OLS approach (without fixed effects) show that income is strongly correlated with transitions to nondemocracy (coups) as originally reported in Przeworski and Limongi (1997) and later investigated in detail in Przeworski et al. (2000), but we also find statistically significant, though weaker, correlation between income and transitions to democracy (recall that a positive coefficient in the transitions to nondemocracy regressions means that higher income countries are less likely to experience coups).⁴³ However, in column 2 of Tables 9 and 10 we add fixed effects. The findings are similar to those reported in Table 3a,b. Once we introduce the fixed effects, income per capita is never significant. In Table 9, the coefficient on income is similar to column 1, but no longer significant, whereas in Table 10, the coefficient is negative and insignificant. Column 3 of both tables shows that once we estimate these equations with GMM, the estimates are negative and show no evidence of an effect of income on either transitions to democracy or away from democracy.

Columns 4-6 repeat the same regressions for the former colonies sample, with similar results. Finally, column 7 reports a specification similar to those in Tables 5 and 6 for the former colonies sample. Now instead of fixed effects, the three baseline historical characteristics, constraint on the executive at independence, date of independence and density of the indigenous population in 1500 are added. The results are broadly similar, though income is now significant for transitions to nondemocracy.⁴⁴

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.

8 Growth Spurts, Crises and Democracy

Our analysis so far shows that a number of historical variables have significant predictive power for the political development path of a society (at least for the sample of former European colonies), but neither income nor education nor population seem to have predictive power for the over-time variation (at the five or ten-year frequencies) in democracy

⁴³This is different from the results in Przeworski and Limongi (1997) and Przeworski et al. (2000). Section 9 below shows that this is because of the difference in the measures of democracy. There, using the dichotomous measure of democracy with the additional requirements imposed by these authors, we also find that income is not correlated with transitions to democracy.

⁴⁴This result disappears when education and population density are included. These results are not reported here to save space, and are available upon request.

in the postwar sample. In this section, we document that there are some predictable movements in changes in democracy.

A number of theories, including Haggard and Kaufman (1995) and Acemoglu and Robinson (2001, 2004), emphasize economic crises as events destabilizing both democratic and nondemocratic regimes, and leading to regime transitions. The notable feature of these theories is that, contrary to the modernization hypothesis and the existing literature, they do *not* predict that economic crises should be associated with declines in democracy, instead crises should make both transitions to and away from democracy more likely. The overall effect on the level of democracy is ambiguous.

We investigate this issue in Table 11. We define an economic crisis as a sudden and sharp decline in growth relative to five years ago. More specifically, there is an economic crisis at time $t - 1$ if the five-yearly average growth rate of GDP per capita in any year in the five-year period between $t - 1$ and $t - 2$ is less than a certain threshold $-\overline{\Delta y}$.⁴⁵ We choose the threshold $\overline{\Delta y}$ as 3 percent (columns 1 and 2), 4 percent (columns 3 and 4) or 5 percent (columns 5 and 6). These thresholds are motivated by the fact that one standard deviation of this variable is equal to 4 percent. The dependent variable in Panel A is the Freedom House democracy score, and Panels B and C look at transitions to and away from democracy using the same methodology as in Tables 9 and 10.

The results in Panel A show that, again contrary to the predictions of modernization theory economic crises make democracy *more likely*. Panels B and C show why this is so: economic crises have a small, and typically not statistically significant, effect on transitions away from democracy, but a large effect on transitions away from nondemocracy to democracy. In other words, consistent with Haggard and Kaufman (1995) and Acemoglu and Robinson (2001, 2004), economic crises cause dictatorships to fall, thus increasing the likelihood of democracy.⁴⁶ The results in Panel B show that this effect is robust to the various threshold values that we use and to the use of OLS with fixed effect or the Arellano-Bond GMM estimator.⁴⁷

⁴⁵Recall that time periods, t refers five year intervals. The years s between $t - 2$ and $t - 1$ are $s \in [5t - 10, 5t - 6]$. We denote the forward average five-year growth rate in year s by $\Delta \tilde{y}_s = \sum_{j=1}^5 \Delta y_{s+j} / 5$, and define a crisis during year s as occurring if $\Delta \tilde{y}_s - \Delta \tilde{y}_{s-5} < -\overline{\Delta y}$.

⁴⁶Previous empirical papers have examined the effects of growth and crises. Contrary to our results, Londregan and Poole (1990) find that higher growth rates reduce the likelihood of coups, and Gasiorowski (1995) finds that the rate of economic growth does not influence democratizations, but that higher growth rates do reduce the chance of coups. Przeworski et al. (2000) also report a number of results along these lines. The problem with these findings however is that none of these papers attempted to control for omitted variables.

⁴⁷We obtain similar results using the Polity and the Boix-Rosato indices of democracy. Details are available upon request.

A natural question is whether growth accelerations have a similar effect on regime change. A recent paper by Hausmann, Pritchett and Rodrik (2004) emphasizes the importance of growth accelerations. We define growth accelerations in a symmetric way to economic crises, and create a dummy for acceleration if the change in the five-yearly average growth rate of GDP per capita in any year between $t - 2$ and $t - 1$ is above a certain threshold $\overline{\Delta y}$, symmetrically chosen to be 3 percent, 4 percent or 5 percent.⁴⁸ The results in Table 12 show that, again contrary to the view that higher incomes and more rapid economic growth should lead to beneficial change in regimes, we find no evidence of an effect of growth accelerations on democracy.

9 Comparison to the Existing Literature

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

Beginning with Lipset (1959) most scholars have adopted a simple empirical strategy to evaluate the determinants of democracy. Following Lipset, much cited papers by Jackman (1973), Bollen (1979) and Muller (1995) look for the cross-sectional relationship between per capita income, 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 papers by Londregan and Poole (1990) and 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 results found are generated by omitted variable bias. The results in Table 2a, especially, columns 7 and 8, are very similar to the main specifications in Barro (1999), which is the most careful and well-known study of the determinants of democracy based on pooled cross-sectional data. A comparison of Table 3a to Table 2a shows that the source of difference between our results and these studies is the presence of fixed effects in our models. Since there are many potential omitted factors, we believe that a fixed effects

⁴⁸In other words, there is an acceleration if $\Delta \tilde{y}_s - \Delta \tilde{y}_{s-5} \geq \overline{\Delta y}$, using the same notation as above, for any year $s \in [5t - 10, 5t - 6]$. Hausmann, Pritchett and Rodrik (2004) use a more complicated definition of a growth acceleration, also incorporating information from future growth experiences, which would not be appropriate to use as a right-hand side variable. Nevertheless, we repeated the regressions in Table 12 using their measure and obtained identical results.

specification is more appropriate, and our results are likely to be closer to the causal effect of income on democracy (or at least they would provide an upper bound on the causal effect of income on democracy).⁴⁹

Several more recent empirical studies have abandoned the pooled cross-sectional time series approach and instead used dynamic probit models to investigate the determinants of transitions to and away from democracy. Przeworski et al. (2000), 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, as we noted earlier, the work of Przeworski et al., 2000, emphasizes the effect of income on coups rather than on democratizations). Since they do not control for country fixed effects, the approach in Przeworski et al. (2000) and Boix and Stokes (2003) is most similar to our cross-country regressions in Tables 2a and 2b and especially the transitions regressions in Tables 9 and 10, though there are two significant differences; first, focusing on the dichotomous distinction between democracy and nondemocracy, these authors use a dummy variable for democracy, and second, they use dynamic probit models rather than linear models. These differences are not essential, however. The major difference is that their empirical specifications do not include fixed effects and do not control for other cross-country differences potentially affecting income and democracy.

Appendix Table A2 substantiates these claims. It first estimates models similar to those in Table 2 with the dichotomous democracy variable from the Boix-Rosato (2001) dataset (which extends Przeworski et al.'s data to the present). We start with linear probability models rather than probit models, which are simpler, consistent under a weaker set of assumptions, and also easier to interpret (see Wooldridge, 2002, chapter 15.2). The table is divided into three panels, the top panel looks at the relationship between income and democracy, while the other two panels look at transitions to and away from democracy.

Without fixed effects, the results are very similar to those in Przeworski et al. (2000). There is a positive association between income and democracy, and this is driven by the

⁴⁹One concern with fixed effects regressions is that once fixed effects are included, there may not remain enough variation in the right-hand side variables. This is not so in our case. First, the standard error of the estimates of the effect of income on democracy remained relatively small in Table 3a and 3b. For example, the standard error in column 1 of Table 3a is 0.035, compared to 0.010 in Table 2a. An effect of income on democracy of the same size as in Table 2a, which is itself quantitatively very small as noted in Section 3, would have been statistically significant with this standard error. Moreover, as the analysis in Section 8 shows, other factors, such as economic crises, are still found to have a robust effect on democracy with this methodology.

positive association between income and transitions out of democracy (and differently from the results of Table 9 using the Freedom House data, now there is no positive association between income and transitions into democracy; this reflects the differences between the Freedom House and Przeworski/Boix-Rosato indices of democracy). However, once fixed effects are included, this relationship disappears. These results show that the source of the difference between our findings and those in the earlier papers is again our inclusion of fixed effects.

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 the probit models is nonlinear, 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 the natural choice in our context.

Nevertheless, since Przeworski et al. (2000) place special emphasis on probit (and thus on discrete response models), in Appendix Table A3, we also look at discrete response models. Models without fixed effects are estimated both by probit and logit. Essentially, for all binary response models, the conditional mean function can be written in the compact form:

$$\Pr(y_{it} = 1 \mid \mathbf{z}_i) = G(\mathbf{z}'_{it}\boldsymbol{\beta}), \quad (9)$$

where \mathbf{z}_{it} is the vector of right-hand side variables, and \mathbf{z}_i is the vector obtained by stacking all \mathbf{z}_{it} 's together. The probit model is the special case when $G(s) = \Phi(s)$, where $\Phi(\cdot)$ is the normal cumulative distribution function. The logit model is obtained under different functional form assumptions, in particular, when $G(s) = \exp(s) / [1 + \exp(s)]$.

Appendix Table A3 shows that without fixed effects the probit and logit models give very similar results, except that in the logit model the marginal affects cannot be calculated without further assumptions, so we simply show the parameter estimates, which do not have the same quantitative scale. Both probit and logit estimates of the effect of income without fixed effects are positive (and the implied magnitudes are similar).

As noted above, the major problem of the probit model is that it cannot accommodate fixed effects. If fixed effects are added, equation (9) changes to $\Pr(y_{it} = 1 \mid \mathbf{z}_i, \delta_i) = G(\mathbf{z}'_{it}\boldsymbol{\beta} + \delta_i)$, where δ_i is the fixed effect for observation i . This specification creates an incidental parameters problem in the estimation of the δ_i 's and consistent estimation is no longer possible. In contrast, consistent estimation with fixed effects is possible in the (conditional) logit model.⁵⁰ Appendix Table A3, therefore, also reports (conditional) logit

⁵⁰This is because the transformation $\Pr(y_{it} = 1 \mid \mathbf{z}_i, \delta_i) / \prod_{\tau=1}^T \Pr(y_{i\tau} = 1 \mid \mathbf{z}_i, \delta_i)$ removes the fixed

estimates with fixed effects. These estimates again show no significant effect of income on democracy or on transitions (again the magnitudes cannot be interpreted directly, but simple calculations show that they are of similar magnitudes to the effects of income in the linear probability model with fixed effects). 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).

Finally, we also look at the effect of income on democracy with probit regressions (without fixed effects) when the effect of historical variables are controlled for in the former colonies sample. Here results are again similar to those in Tables 5a,b, 6, 9 and 10.

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 A4 report uses their basic result in column 1, together with our linear estimation of their model in column 2.⁵¹ The estimates are very close, almost 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 lag democracy values, making their specification more comparable to ours.⁵² 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 that Londregan and Poole (1996) estimate their model on annual data, without correcting the standard errors for the serial correlation in the residuals, and do not include time effects in their model. This creates two potential problems: first, their estimate of the effect of income may partly pick up the common trend in democracy and income, and second, the standard errors are biased downward because of serial correlation. Column 4

effects. This transformation is only valid when all the variables in \mathbf{z}_i are strictly exogenous, which is not the case here because of the lagged dependent variable. Nevertheless, similar, and more complicated, techniques can be developed for the logit model with fixed effects and lagged dependent variables, see Wooldridge (2002, chapter 15.8).

⁵¹We use their data, both to increase comparability (there have been revisions to data since then), and also to include the additional variables they have put together.

⁵²Our 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.

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 (columns 4 and 8 in Tables 3a,b), 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 entirely consistent with ours.

10 Conclusion

There is a general consensus in the political economy literature that there is a causal effect of per capita income on democracy. In this paper, we argue that, though income and democracy are positively correlated, there is no evidence of a 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 democracy and economic performance.

The previous literature did not reach this conclusion because most papers did not control for the endogeneity of income per capita. The simple strategy of including fixed effects to control for time-invariant country characteristics entirely removes the positive relationship between income and democracy. We then show that, at least in the sample of former European colonies where we have good measures of the historical sources of variation in development paths, the fixed effects indeed capture the impact of these historical differences.

Our overall conclusion is that the relationship between income and democracy needs 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 nondemocracy 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 postwar era to lead to significant changes in regimes.

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Table 1
Descriptive Statistics

	All countries			Former Colonies	High Income Countries	Low Income Countries
	(1)	(2)	(3)	(4)	(5)	(6)
Freedom House Political Rights Index	0.57 (0.36)	0.61 (0.35)	0.55 (0.37)	0.47 (0.34)	0.76 (0.31)	0.36 (0.30)
Polity Composite Democracy Index	0.57 (0.38)	0.63 (0.37)	0.58 (0.38)	0.50 (0.35)	0.78 (0.32)	0.37 (0.32)
Boix/Rosato/Przeworski Democracy Index	0.44 (0.50)	0.50 (0.50)	0.45 (0.50)	0.34 (0.47)	0.70 (0.46)	0.20 (0.40)
Log GDP per Capita (Chain Weighted 1996 Prices)	8.26 (1.05)	8.39 (1.04)	8.34 (1.10)	7.90 (0.92)	9.15 (0.56)	7.37 (0.56)
Log GDP per Capita (1990 dollars)	8.00 (1.09)	8.19 (1.08)	8.11 (1.14)	7.67 (0.97)	8.91 (0.70)	7.16 (0.60)
Log Population (Thousands)	8.91 (1.78)	9.19 (1.54)	9.29 (1.48)	9.12 (1.47)	8.77 (1.90)	9.06 (1.64)
Average Years of Schooling in Adult Population	4.71 (2.83)	4.75 (2.86)	4.94 (2.88)	3.93 (2.59)	6.57 (2.36)	2.59 (1.52)
Constraint on the Executive at Independence	0.39 (0.37)	0.41 (0.39)	0.35 (0.37)	0.38 (0.35)	0.44 (0.42)	0.34 (0.31)
Independence Year	1899 (71)	1887 (71)	1890 (69)	1911 (65)	1875 (70)	1925 (62)
Log Population Density in 1500				0.41 (1.60)		
Log Settler Mortality				4.75 (1.20)		
Observations	945	677	630	541	483	462

Values are averages during sample period, with standard deviations in parentheses. All countries are those for which democracy, lag democracy, and lag income in five-year intervals are available at least once during 1960-2000, for the Freedom House measure of democracy. A country must be independent for at least 5 years. Column 1 refers to the sample in the regression in Table 2a, column 1. Column 2 refers to the sample in the regression in Table 2a, column 4. Column 3 refers to the sample in Table 2a, column 3. Column 4 refers to the sample in Table 5a column 1. Columns 5 and 6 split the sample in column 1 using the median income in the world during the sample period. GDP per capita in 1996 prices is from the Penn World Tables 6.1; GDP per capita in 1990 Geary-Khamis dollars is from Maddison (2003). Freedom House Political Rights Index is augmented following Barro (1999). Polity Composite Democracy Index and Boix/Rosato/Przeworski Democracy Index are alternative measures of democracy. Log Population are from the World Bank (2002). Average years of schooling in adult population aged 25 and above, from Barro and Lee (2000). 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 are the subsample colonized by European powers before 1900. For detailed definitions and sources, see Appendix Table A1.

Table 2a
Pooled Cross-Section Results using Freedom House Measure of Democracy

	Pooled Cross-Section OLS							SUR
	Base sample, 1960-2000 (1)	Base sample, 1960- 2000, with 10 years of independence (2)	Balanced Panel, 1970-2000 (3)	Base sample, 1960-2000 (4)	Base sample, 1960- 2000, with 10 years of independence (5)	Balanced Panel, 1970-2000 (6)	Balanced Panel, 1970-2000 (7)	Balanced Panel, 1970-2000 (8)
	<i>Dependent Variable is Democracy</i>							
Democracy _{t-1}	0.706 (0.035)	0.706 (0.037)	0.665 (0.049)	0.633 (0.043)	0.641 (0.042)	0.602 (0.054)	0.580 (0.072)	0.596 (0.039)
Democracy _{t-2}							-0.003 (0.064)	0.003 (0.040)
Log GDP per Capita _{t-1}	0.072 (0.010)	0.068 (0.010)	0.083 (0.014)	0.053 (0.017)	0.049 (0.017)	0.049 (0.019)	0.058 (0.024)	0.057 (0.020)
Log Population _{t-1}				0.000 (0.006)	0.001 (0.005)	0.003 (0.007)	0.007 (0.007)	0.006 (0.006)
Education _{t-1}				0.015 (0.005)	0.015 (0.005)	0.019 (0.006)	0.017 (0.006)	0.015 (0.006)
Male/Female Education Gap _{t-1}							-0.044 (0.021)	-0.054 (0.017)
Urbanization Rate _{t-1}							-0.086 (0.061)	-0.104 (0.063)
Significant Oil Producer							-0.036 (0.055)	-0.067 (0.039)
Observations	945	876	630	677	650	497	427	427
Countries	150	136	90	96	96	71	61	61
R-squared	0.73	0.73	0.72	0.70	0.70	0.70	0.68	

Pooled cross-sectional OLS regressions in columns 1 through 7, with robust standard errors clustered by country in parentheses. Column 8 uses Seemingly Unrelated Regression (SUR), which allows for arbitrary correlation structure of errors between time periods but does not allow for heteroskedasticity across countries. Year dummies in all regressions. Dependent variable is augmented Freedom House Political Rights Index. Base sample in columns 1 and 4 is an unbalanced panel, 1960-2000, with data at 5-year intervals; countries can enter panel 5 years after democracy data are available. Columns 2 and 5 use same sample but drop any observations within 10 years of independence. Columns 3, 6, 7, and 8 use a balanced panel between 1970 and 2000. In columns 3 and 6, countries must have been independent by 1965. In columns 7 and 8, countries must have been independent by 1960. For detailed data definitions and sources see Table 1 and Appendix Table A1.

Table 2b
Pooled Cross-Section Results using Polity Measure of Democracy

	Pooled Cross-Section OLS							SUR
	Base sample, 1960-2000 (1)	Base sample, 1960- 2000, with 10 years of independence (2)	Balanced Panel, 1970-2000 (3)	Base sample, 1960-2000 (4)	Base sample, 1960- 2000, with 10 years of independence (5)	Balanced Panel, 1970-2000 (6)	Balanced Panel, 1970-2000 (7)	Balanced Panel, 1970-2000 (8)
<i>Dependent Variable is Democracy</i>								
Democracy _{t-1}	0.749 (0.034)	0.755 (0.036)	0.730 (0.047)	0.718 (0.039)	0.724 (0.038)	0.710 (0.053)	0.572 (0.109)	0.592 (0.044)
Democracy _{t-2}							0.137 (0.108)	0.160 (0.042)
Log GDP per Capita _{t-1}	0.053 (0.010)	0.047 (0.010)	0.053 (0.013)	0.029 (0.015)	0.022 (0.015)	0.023 (0.018)	0.017 (0.022)	0.011 (0.017)
Log Population _{t-1}				0.000 (0.005)	0.002 (0.005)	0.007 (0.005)	0.009 (0.005)	0.007 (0.004)
Education _{t-1}				0.012 (0.004)	0.012 (0.004)	0.011 (0.006)	0.006 (0.006)	0.005 (0.005)
Male/Female Education Gap _{t-1}							-0.027 (0.016)	-0.035 (0.014)
Urbanization Rate _{t-1}							0.017 (0.050)	-0.029 (0.045)
Significant Oil Producer							-0.016 (0.039)	-0.001 (0.029)
Observations	854	795	518	641	615	413	350	350
Countries	136	122	74	93	93	59	50	50
R-squared	0.77	0.77	0.77	0.76	0.75	0.76	0.76	

Pooled cross-sectional OLS regressions in columns 1 through 7, with robust standard errors clustered by country in parentheses. Column 8 uses Seemingly Unrelated Regression (SUR), which allows for arbitrary correlation structure of errors between time periods but does not allow for heteroskedasticity across countries. Year dummies in all regressions. Dependent variable is Polity Composite Democracy Index. Base sample in columns 1 and 4 is an unbalanced panel, 1960-2000, with data at 5-year intervals; countries can enter panel 5 years after democracy data are available. Columns 2 and 5 use same sample but drop any observations within 10 years of independence. Columns 3, 6, 7, and 8 use a balanced panel between 1970 and 2000. In columns 3 and 6, countries must have been independent by 1965. In columns 7 and 8, countries must have been independent by 1960. For detailed data definitions and sources see Table 1 and Appendix Table A1.

Table 3a
Fixed Effects Results using Freedom House Measure of Democracy

	Base Sample, 1960-2000							
	5-year data			Annual data		5-year data		Annual data
	Fixed Effects OLS (1)	Anderson-Hsiao IV (2)	Arellano-Bond GMM (3)	Fixed Effects OLS (4)	Fixed Effects OLS (5)	Anderson-Hsiao IV (6)	Arellano-Bond GMM (7)	Fixed Effects OLS (8)
<i>Dependent Variable is Democracy</i>								
Democracy _{t-1}	0.379 (0.051)	0.469 (0.100)	0.489 (0.085)	[0.00]	0.364 (0.054)	0.470 (0.131)	0.508 (0.093)	[0.00]
Log GDP per Capita _{t-1}	0.010 (0.035)	-0.104 (0.107)	-0.129 (0.076)	[0.33]	-0.032 (0.043)	-0.534 (0.479)	-0.153 (0.133)	[0.19]
Log Population _{t-1}					-0.013 (0.085)	-0.301 (0.301)	0.016 (0.119)	[0.11]
Education _{t-1}					-0.014 (0.020)	-0.006 (0.033)	-0.025 (0.024)	
Time Effects F-test	[0.00]	[0.00]	[0.00]	[0.01]	[0.01]	[0.00]	[0.00]	[0.01]
Hansen J Test			[0.26]				[0.28]	
AR(2) Test			[0.45]				[0.88]	
Observations	945	838	838	2895	677	589	589	2867
Countries	150	127	127	148	96	92	92	147
R-squared	0.80			0.93	0.76			0.93

Fixed effects OLS regressions in columns 1, 4, 5, and 8, with country dummies and robust standard errors clustered by country in parentheses. Columns 2 and 6 use instrumental variables method of Anderson and Hsiao (1982), with clustered standard errors, and columns 3 and 7 use GMM of Arellano and Bond (1991), with robust standard errors; in both methods we instrument for income using a double lag. Year dummies are included in all regressions, and the time effects F-test gives the p-value for their joint significance. Dependent variable is augmented Freedom House Political Rights Index. Base sample in all columns 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). Columns 4 and 8 use annual data from the same sample; Freedom House data begin in 1972, as we require 5 lags, this panel starts in 1977. In columns 4 and 8, each right hand side variables 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 3b
Fixed Effects Results using Polity Measure of Democracy

	Base Sample, 1960-2000							
	5-year data			Annual data	5-year data			Annual data
	Fixed Effects OLS (1)	Anderson-Hsiao IV (2)	Arellano-Bond GMM (3)	Fixed Effects OLS (4)	Fixed Effects OLS (5)	Anderson-Hsiao IV (6)	Arellano-Bond GMM (7)	Fixed Effects OLS (8)
<i>Dependent Variable is Democracy</i>								
Democracy _{t-1}	0.449 (0.063)	0.582 (0.127)	0.590 (0.106)	[0.00]	0.455 (0.068)	0.595 (0.184)	0.633 (0.112)	[0.00]
Log GDP per Capita _{t-1}	-0.006 (0.039)	-0.413 (0.163)	-0.351 (0.127)	[0.53]	-0.012 (0.046)	-0.561 (0.668)	-0.229 (0.186)	[0.22]
Log Population _{t-1}					0.155 (0.082)	-0.104 (0.322)	0.156 (0.106)	[0.01]
Education _{t-1}					-0.027 (0.021)	-0.008 (0.047)	-0.027 (0.028)	
Time Effects F-test	[0.00]	[0.19]	[0.00]	[0.01]	[0.04]	[0.07]	[0.09]	[0.09]
Hansen J Test			[0.03]				[0.08]	
AR(2) Test			[0.39]				[0.38]	
Observations	854	747	747	3701	641	541	541	3396
Countries	136	114	114	134	93	91	91	133
R-squared	0.82			0.96	0.80			0.96

Fixed effects OLS regressions in columns 1, 4, 5, and 8, with country dummies and robust standard errors clustered by country in parentheses. Columns 2 and 6 use instrumental variables method of Anderson and Hsiao (1982), with clustered standard errors, and columns 3 and 7 use GMM of Arellano and Bond (1991), with robust standard errors; in both methods we instrument for income using a double lag. Year dummies are included in all regressions, and the time effects F-test gives the p-value for their joint significance. Dependent variable is Polity Composite Democracy Index. Base sample in all columns 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). Columns 4 and 8 use annual data from the same sample. In columns 4 and 8, each right hand side variables 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 Freedom House Measure of Democracy: Alternative Samples

	Base Sample, 1960-2000, with 10 years of independence				Balanced Panel, 1970-2000			Base Sample, 1960-2000, without sub- saharan Africa			
	5-year data		Annual data		5-year data		Annual data		5-year data		Annual data
	Fixed Effects OLS (1)	Arellano- Bond GMM (2)	Fixed Effects OLS (3)	Fixed Effects OLS (4)	Arellano- Bond GMM (5)	Fixed Effects OLS (6)	Fixed Effects OLS (7)	Arellano- Bond GMM (8)	Fixed Effects OLS (9)		
	<i>Dependent Variable is Democracy</i>										
Democracy _{t-1}	0.378 (0.054)	0.483 (0.085)	[0.00]	0.283 (0.058)	0.472 (0.092)	[0.00]	0.410 (0.060)	0.520 (0.096)	[0.00]		
Log GDP per Capita _{t-1}	0.013 (0.038)	-0.167 (0.079)	[0.34]	-0.031 (0.049)	-0.262 (0.128)	[0.52]	0.069 (0.043)	0.044 (0.084)	[0.80]		
Time Effects F-test	[0.00]	[0.00]	[0.01]	[0.00]	[0.00]	[0.05]	[0.01]	[0.00]	[0.19]		
Hansen J Test		[0.27]			[0.40]			[0.31]			
AR(2) Test		[0.50]			[0.73]			[0.83]			
Observations	876	828	2763	630	567	2328	659	591	1942		
Countries	136	127	135	90	81	97	106	85	104		
R-squared	0.80		0.93	0.80		0.93	0.78		0.93		

Fixed effects OLS regressions in columns 1, 3, 4, 6, 7 and 9, with country dummies and robust standard errors clustered by country in parentheses. Columns 2, 5, and 8 use GMM of Arellano and Bond (1991) with robust standard errors. Year dummies are included in all regressions. Dependent variable is augmented Freedom House Political Rights Index. The base sample is an unbalanced panel, 1960-2000, with data at 5-year intervals. Columns 3, 6, and 9 use annual data from the same sample. Columns 4, 5, and 6 use a balanced panel between 1970 and 2000. In columns 3, 6, and 9, each right hand side variables has five annual lags; values in brackets are the p-value from an F-test of the joint significance of all 5 lags. For detailed data definitions and sources see Table 1 and Appendix Table A1.

Table 5a
Effect of Historical Institutions on Democracy: Former Colonies, using Freedom House Measure of Democracy

	Former European Colonies, 1960-2000							
	Pooled OLS	Fixed Effects	Pooled Cross-Section OLS					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	<i>Dependent Variable is Democracy</i>							
Democracy _{t-1}	0.672 (0.048)	0.283 (0.062)	0.636 (0.050)	0.668 (0.050)	0.573 (0.047)	0.538 (0.049)	0.567 (0.047)	0.538 (0.049)
Log GDP per Capita _{t-1}	0.059 (0.014)	-0.088 (0.050)	0.039 (0.017)	0.046 (0.015)	0.025 (0.013)	0.016 (0.018)	0.012 (0.014)	0.014 (0.021)
Constraint on the Executive at Independence					0.170 (0.030)	0.178 (0.030)	0.175 (0.028)	0.141 (0.036)
Independence Year/100					-0.101 (0.016)	-0.101 (0.018)	-0.098 (0.016)	-0.079 (0.020)
Log Population Density in 1500				-0.013 (0.007)		-0.011 (0.007)	-0.013 (0.006)	-0.007 (0.008)
Log Settler Mortality			-0.033 (0.011)			-0.009 (0.010)		
Log Population _{t-1}								-0.002 (0.008)
Education _{t-1}								0.011 (0.007)
Time Effects F-test	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.01]
Observations	541	541	486	541	541	486	541	412
Countries	80	80	68	80	80	68	80	63
R-squared	0.62	0.72	0.63	0.62	0.65	0.66	0.65	0.61

OLS regressions, with robust standard errors clustered by country in parentheses. Time effects are included in all columns. Country dummies in column 2 only. Dependent variable is augmented Freedom House Political Rights Index. In all columns, data represents all available values in the former colonies sample for which Constraint on the Executive at Independence, Independence Year, and Log Population Density in 1500 are available. Former colonies are an unbalanced panel, 1960-2000, with data at 5-year intervals, so first lagged value is for 1955. Countries enter the panel when they have been independent for at least ten years. For detailed data definitions and sources see Table 1 and Appendix Table A1.

Table 5b
Effect of Historical Institutions on Democracy: Former Colonies, using Polity Measure of Democracy

	Former European Colonies, 1960-2000							
	Pooled OLS	Fixed Effects	Pooled Cross-Section OLS					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	<i>Dependent Variable is Democracy</i>							
Democracy _{t-1}	0.723 (0.047)	0.354 (0.078)	0.711 (0.050)	0.720 (0.048)	0.643 (0.054)	0.622 (0.057)	0.637 (0.053)	0.623 (0.058)
Log GDP per Capita _{t-1}	0.043 (0.014)	-0.056 (0.060)	0.032 (0.017)	0.032 (0.014)	0.015 (0.012)	0.009 (0.018)	0.004 (0.013)	0.004 (0.020)
Constraint on the Executive at Independence					0.131 (0.037)	0.153 (0.037)	0.137 (0.036)	0.118 (0.041)
Independence Year/100					-0.083 (0.016)	-0.087 (0.018)	-0.081 (0.016)	-0.072 (0.017)
Log Population Density in 1500				-0.010 (0.006)		-0.009 (0.008)	-0.011 (0.007)	-0.004 (0.008)
Log Settler Mortality			-0.019 (0.010)			-0.001 (0.010)		
Log Population _{t-1}								0.000 (0.007)
Education _{t-1}								0.007 (0.007)
Income and Education F-test								[0.37]
Time Effects F-test	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
Observations	514	514	462	514	514	462	514	393
Countries	80	80	68	80	80	68	80	62
R-squared	0.68	0.76	0.7	0.69	0.7	0.71	0.7	0.68

OLS regressions, with robust standard errors clustered by country in parentheses. Time effects are included in all columns. Country dummies in column 2 only. Dependent variable is Polity Composite Democracy Index. In all columns, data represents all available values in the former colonies sample for which Constraint on the Executive at Independence, Independence Year, and Log Population Density in 1500 are available. Former colonies are an unbalanced panel, 1960-2000, with data at 5-year intervals, so first lagged value is for 1955. Countries enter the panel when they have been independent for at least ten years. For detailed data definitions and sources see Table 1 and Appendix Table A1.

Table 6

Effect of Historical Institutions on Democracy: Former Colonies, using Freedom House Measure of Democracy

	Former European Colonies, Pooled Cross-Section OLS					
	Unbalanced Panel, 1960-2000			Balanced Panel, 1970-2000	Unbalanced Panel, 1960- 2000	
	(1)	(2)	(3)	(4)	(5)	(6)
	<i>Dependent Variable is Democracy</i>					
Democracy _{t-1}	0.560 (0.049)	0.566 (0.047)	0.565 (0.048)	0.471 (0.064)	0.577 (0.046)	[0.00]
Log GDP per Capita _{t-1}	0.015 (0.015)	0.013 (0.014)	0.007 (0.014)	0.008 (0.020)	0.009 (0.013)	0.015 (0.016)
Constraint on the Executive at Independence	0.143 (0.036)	0.165 (0.030)	0.170 (0.028)	0.212 (0.036)	0.171 (0.028)	0.138 (0.034)
Independence Year/100	-0.100 (0.030)	-0.104 (0.031)	-0.097 (0.016)	-0.135 (0.022)	-0.098 (0.016)	-0.083 (0.017)
Log Population Density in 1500	-0.012 (0.007)	-0.011 (0.007)	-0.013 (0.007)	-0.019 (0.010)	-0.015 (0.006)	-0.014 (0.006)
Former Colony F-test	[0.67]					
Religion F-test		[0.87]				
Latitude			0.102 (0.067)			
Time Effects F-test	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
Observations	541	541	541	336	561	465
Countries	80	80	80	48	84	78
R-squared	0.65	0.65	0.65	0.63	0.65	0.63

OLS regressions, with robust standard errors clustered by country in parentheses. Dependent variable is augmented Freedom House Political Rights Index. Data are at 5-year intervals, from former European colonies. Columns 1, 2, 3, 5, and 6 are an unbalanced panel, 1960-2000; a country must be independent for 10 years to enter the panel. Column 4 is a balanced panel from 1970 to 2000; a country must be independent by 1960 to enter the panel. Column 1 includes dummies for former French, Spanish, Portuguese, Belgian, and other non-English colonies; English colonies are the base case. Column 2 includes percent of the population in 1980 that is Catholic, Protestant, and Muslim. Column 3 includes the absolute value of latitude, measured as distance from the equator. Column 5 adds Jordan, Lebanon, Syria and Yemen. Column 6 has three lags of democracy and reports the p-value of an F-test for their joint significance. For detailed data definitions and sources see Table 1 and Appendix Table A1.

Table 7
Explaining the Fixed Effect: Former Colonies

	Former European Colonies, 1960-2000			
	Using Freedom House Measure of Democracy		Using Polity measure of Democracy	
	(1)	(2)	(3)	(4)
	<i>Dependent Variable is Fixed Effect from column 2 of Table 5a</i>		<i>Dependent Variable is Fixed Effect from column 2 of Table 5b</i>	
<i>Panel A</i>				
Constraint on the Executive at Independence	0.311 (0.058)	0.397 (0.051)	0.318 (0.052)	0.341 (0.043)
Independence Year/100	-0.178 (0.033)	-0.221 (0.029)	-0.164 (0.029)	-0.179 (0.025)
Log Population Density in 1500	-0.035 (0.012)	-0.042 (0.012)	-0.029 (0.011)	-0.030 (0.010)
Log Settler Mortality	-0.055 (0.019)		-0.023 (0.017)	
Observations	68	80	68	80
R-squared	0.73	0.66	0.69	0.64
<i>Panel B</i>	<i>Wald Test of Coefficient Equality, testing coefficients from:</i>			
	Table 5a: column 2 vs. column 6	Table 5a: column 2 vs. column 7	Table 5b: column 2 vs. column 6	Table 5b: column 2 vs. column 7
Equality of Coefficient Test: Log GDP per Capita _{t-1}	[0.06]	[0.08]	[0.34]	[0.33]

Panel A: cross-sectional OLS, with non-robust standard errors in parentheses. Dependent variable is countries' fixed effect, from column 2 of Table 5a, using augmented Freedom House Political Rights Index (for columns 1 and 2) and from column 2 of Table 5b (for columns 3 and 4) using the Polity measure of democracy. Fixed effects from Tables 5a and 5b are weighted by their inverse non-robust standard errors. Panel B: equality of coefficient test using results from indicated columns in Table 5a and 5b; p-values reported are for non-robust estimates of the variance-covariance matrix. For detailed data definitions and sources see Table 1 and Appendix Table A1.

Table 8
Democracy in the Long Run

	5-year data		10-year data	
	Pooled Cross- Section OLS (1)	Fixed Effects OLS (2)	Pooled Cross- Section OLS (3)	Fixed Effects OLS (4)
<i>Panel A: All Countries</i>				
<i>Dependent Variable is Democracy, 1840-1940</i>				
Democracy _{t-1}	0.841 (0.039)	0.652 (0.089)	0.752 (0.049)	0.455 (0.136)
Log GDP per Capita _{t-1}	0.072 (0.017)	-0.076 (0.064)	0.133 (0.031)	-0.035 (0.135)
Time Effects F-test	[0.01]	[0.00]	[0.08]	[0.23]
Observations	358	358	188	188
Countries	28	28	28	28
R-squared	0.78	0.81	0.70	0.75
<i>Panel B: All Countries</i>				
<i>Dependent Variable is Democracy, 1840-2000</i>				
Democracy _{t-1}	0.772 (0.043)	0.682 (0.070)	0.573 (0.055)	0.407 (0.082)
Log GDP per Capita _{t-1}	0.081 (0.015)	0.066 (0.032)	0.151 (0.025)	0.134 (0.070)
Time Effects F-test	[0.00]	[0.00]	[0.00]	[0.02]
Observations	656	656	342	342
Countries	28	28	28	28
R-squared	0.79	0.80	0.66	0.70
<i>Panel C: Without Venezuela</i>				
<i>Dependent Variable is Democracy, 1840-2000</i>				
Democracy _{t-1}	0.771 (0.045)	0.673 (0.074)	0.576 (0.057)	0.400 (0.085)
Log GDP per Capita _{t-1}	0.078 (0.015)	0.041 (0.030)	0.144 (0.025)	0.076 (0.068)
Time Effects F-test	[0.00]	[0.00]	[0.00]	[0.04]
Observations	637	637	331	331
Countries	27	27	27	27
R-squared	0.78	0.80	0.65	0.69

OLS pooled cross-section regressions in columns 1 and 3, and fixed effects regressions with country dummies in columns 2 and 4; robust standard errors clustered by country in parentheses for all columns. Time effects included in all regressions. Dependent variable is Polity Composite Index. Sample is all countries with at least 5 observations between 1840 and 1900 and independent by 1900. Columns 1 and 2 use 5-year data, and columns 3 and 4 use 10-year data. Panel A uses a long sample, 1840-1940, Panels B and C use 1840-2000. Panel C drops Venezuela. GDP per capita is from Maddison (2003). For detailed data definitions and sources see Table 1 and Appendix Table A1.

Table 9
Transitions toward Democracy, using Freedom House measure of Democracy

	Base sample, 1960-2000			Former European Colonies, 1960-2000			
	Pooled Cross- Section OLS (1)	Fixed Effects OLS (2)	Arellano-Bond GMM (3)	Pooled Cross- Section OLS (4)	Fixed Effects OLS (5)	Arellano-Bond GMM (6)	Pooled Cross- Section OLS (7)
<i>Dependent Variable is Transition toward Democracy</i>							
Democracy _{t-1}	0.819 (0.024)	0.652 (0.032)	0.761 (0.049)	0.809 (0.031)	0.606 (0.038)	0.711 (0.057)	0.751 (0.031)
Log GDP per Capita _{t-1}	0.022 (0.007)	0.019 (0.023)	-0.022 (0.046)	0.023 (0.012)	-0.029 (0.033)	-0.023 (0.091)	-0.009 (0.011)
Constraint on the Executive at Independence							0.084 (0.023)
Independence Year/100							-0.077 (0.013)
Log Population Density in 1500							-0.007 (0.005)
Time Effects F-test	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
Hansen J Test			[0.07]			[0.08]	
AR(2) Test			[0.08]			[0.06]	
Observations	945	945	838	541	541	520	541
Countries	150	150	127	80	80	78	80
R-squared	0.84	0.88		0.80	0.86		0.81

Pooled OLS regressions in columns 1, 4, and 7, fixed effects OLS, with country dummies, in columns 2 and 5. Robust standard errors clustered by country in parentheses. Columns 3 and 6 use GMM of Arellano and Bond (1991) with robust standard errors. Year dummies are included in all regressions. Dependent variable is $\max(d_{it}, d_{it-1})$, corresponding to transitions towards democracy, and using the augmented Freedom House Political Rights Index. Base sample is an unbalanced panel, 1960-2000, with data at 5-year intervals. Columns 4 through 7 use an unbalanced panel of former European colonies between 1960 and 2000; countries must be independent for at least 10 years; constraint on the executive and population density in 1500 for country to be included in this sample. For detailed data definitions and sources see Table 1 and Appendix Table A1.

Table 10
Transitions away from Democracy, using Freedom House measure of Democracy

	Base sample, 1960-2000			Former European Colonies, 1960-2000			
	Pooled Cross- Section OLS (1)	Fixed Effects OLS (2)	Arellano-Bond GMM (3)	Pooled Cross- Section OLS (4)	Fixed Effects OLS (5)	Arellano-Bond GMM (6)	Pooled Cross- Section OLS (7)
<i>Dependent Variable is Transition away from Democracy</i>							
Democracy _{t-1}	0.887 (0.021)	0.726 (0.035)	0.728 (0.056)	0.863 (0.029)	0.677 (0.044)	0.683 (0.079)	0.816 (0.031)
Log GDP per Capita _{t-1}	0.050 (0.007)	-0.009 (0.018)	-0.108 (0.054)	0.036 (0.010)	-0.059 (0.031)	-0.182 (0.096)	0.021 (0.009)
Constraint on the Executive at Independence							0.091 (0.023)
Independence Year/100							-0.021 (0.011)
Log Population Density in 1500							-0.007 (0.004)
Time Effects F-test	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
Hansen J Test			[0.36]			[0.24]	
AR(2) Test			[0.24]			[0.63]	
Observations	945	945	838	541	541	520	541
Countries	150	150	127	80	80	78	80
R-squared	0.90	0.93		0.85	0.89		0.86

Pooled OLS regressions in columns 1, 4, and 7, fixed effects OLS, with country dummies, in columns 2 and 5. Robust standard errors clustered by country in parentheses. Columns 3 and 6 use GMM of Arellano and Bond (1991) with robust standard errors. Year dummies are included in all regressions. Dependent variable is $\min(d_{it}, d_{it-1})$, corresponding to transitions away from democracy, and using the augmented Freedom House Political Rights Index. Base sample is an unbalanced panel, 1960-2000, with data at 5-year intervals. Columns 4 through 7 use an unbalanced panel of former European colonies between 1960 and 2000; countries must be independent for at least 10 years; constraint on the executive and population density in 1500 for country to be included in this sample. For detailed data definitions and sources see Table 1 and Appendix Table A1.

Table 11
Fixed Effects Results for Crises

	Base Sample, 1960-2000							
	Fixed Effects OLS		Arellano-Bond GMM		Fixed Effects OLS		Arellano-Bond GMM	
	(1)	(2)	(3)	(4)	(5)	(6)		
	Crisis threshold is -3%		Crisis threshold is -4%		Crisis threshold is -5%			
<i>Panel A</i>	<i>Dependent Variable is Democracy</i>							
Democracy _{t-1}	0.347 (0.056)	0.493 (0.090)	0.356 (0.056)	0.495 (0.089)	0.354 (0.055)	0.496 (0.089)		
Log GDP per Capita _{t-1}	-0.003 (0.047)	-0.141 (0.088)	0.003 (0.047)	-0.148 (0.091)	0.009 (0.047)	-0.155 (0.092)		
Crisis _{t-1}	0.044 (0.019)	0.057 (0.024)	0.059 (0.021)	0.063 (0.026)	0.074 (0.025)	0.096 (0.029)		
Hansen J Test		[0.41]		[0.46]		[0.58]		
AR(2) Test		[0.73]		[0.57]		[0.74]		
R-squared	0.79		0.79		0.79			
<i>Panel B</i>	<i>Dependent Variable is Transition toward Democracy</i>							
Democracy _{t-1}	0.602 (0.038)	0.697 (0.062)	0.609 (0.039)	0.698 (0.062)	0.607 (0.038)	0.699 (0.062)		
Log GDP per Capita _{t-1}	-0.004 (0.034)	-0.067 (0.062)	0.001 (0.034)	-0.071 (0.064)	0.005 (0.034)	-0.079 (0.066)		
Crisis _{t-1}	0.038 (0.014)	0.045 (0.016)	0.045 (0.016)	0.051 (0.018)	0.049 (0.020)	0.067 (0.021)		
Hansen J Test		[0.05]		[0.04]		[0.04]		
AR(2) Test		[0.23]		[0.35]		[0.18]		
R-squared	0.89		0.89		0.89			
<i>Panel C</i>	<i>Dependent Variable is Transition away from Democracy</i>							
Democracy _{t-1}	0.744 (0.038)	0.796 (0.054)	0.747 (0.038)	0.796 (0.054)	0.747 (0.038)	0.797 (0.053)		
Log GDP per Capita _{t-1}	0.001 (0.023)	-0.074 (0.057)	0.002 (0.022)	-0.076 (0.058)	0.003 (0.022)	-0.076 (0.058)		
Crisis _{t-1}	0.006 (0.011)	0.012 (0.014)	0.014 (0.011)	0.013 (0.015)	0.025 (0.011)	0.029 (0.017)		
Hansen J Test		[0.14]		[0.16]		[0.21]		
AR(2) Test		[0.51]		[0.47]		[0.48]		
R-squared	0.93		0.93		0.93			
Observations	739	659	739	659	739	659		
Countries	124	122	124	122	124	122		

Fixed effects OLS with country dummies and robust standard errors clustered by country in columns 1, 3, and 5; GMM of Arellano-Bond in columns 2, 4, and 6, with robust standard errors. The dependent variable in Panel A is the Freedom House Political Rights Index, in Panel B, Transition towards Democracy, calculated as $\max(d_{it-1})$ from this Index as in Table 9, and in Panel C Transition away from Democracy, calculated as $\min(d_{it-1})$ from this Index as in Table 10. For Arellano-Bond GMM we add a double lag of the crisis dummy as an instrument. Base sample is all countries in unbalanced panel, 1960-2000. Countries must be independent for 15 years before they enter the panel. Crisis is a dummy variable corresponding to a change in the growth rate by more than a threshold. In columns 1 and 2 the threshold is -3%, in columns 3 and 4 the threshold is -4%, and in columns 5 and 6 the threshold is -5%. See text for details. For detailed data definitions and sources see Table 1 and Appendix Table A1.

Table 12
Fixed Effects Results for Growth Accelerations

	Base Sample, 1960-2000					
	Fixed Effects OLS		Arellano-Bond GMM		Arellano-Bond GMM	
	(1)	(2)	(3)	(4)	(5)	(6)
	Acceleration threshold is 3%		Acceleration threshold is 4%		Acceleration threshold is 5%	
<i>Panel A</i>	<i>Dependent Variable is Democracy</i>					
Democracy _{t-1}	0.345 (0.056)	0.499 (0.089)	0.344 (0.056)	0.495 (0.092)	0.345 (0.057)	0.496 (0.091)
Log GDP per Capita _{t-1}	-0.001 (0.049)	-0.147 (0.088)	0.001 (0.048)	-0.166 (0.093)	0.006 (0.048)	-0.167 (0.092)
Growth Acceleration _{t-1}	-0.016 (0.019)	-0.045 (0.024)	-0.012 (0.023)	-0.046 (0.029)	0.012 (0.025)	-0.040 (0.034)
Hansen J Test		[0.46]		[0.48]		[0.48]
AR(2) Test		[0.74]		[0.78]		[0.65]
R-squared	0.79		0.79		0.79	
<i>Panel B</i>	<i>Dependent Variable is Transition toward Democracy</i>					
Democracy _{t-1}	0.601 (0.038)	0.699 (0.062)	0.600 (0.038)	0.698 (0.063)	0.600 (0.038)	0.701 (0.063)
Log GDP per Capita _{t-1}	-0.002 (0.035)	-0.077 (0.065)	-0.002 (0.035)	-0.090 (0.068)	0.001 (0.035)	-0.087 (0.068)
Growth Acceleration _{t-1}	-0.013 (0.013)	-0.024 (0.016)	-0.017 (0.015)	-0.038 (0.020)	-0.003 (0.017)	-0.033 (0.021)
Hansen J Test		[0.07]		[0.08]		[0.07]
AR(2) Test		[0.25]		[0.16]		[0.26]
R-squared	0.88		0.88		0.88	
<i>Panel C</i>	<i>Dependent Variable is Transition away from Democracy</i>					
Democracy _{t-1}	0.744 (0.038)	0.799 (0.053)	0.744 (0.039)	0.797 (0.054)	0.745 (0.039)	0.796 (0.054)
Log GDP per Capita _{t-1}	0.001 (0.023)	-0.070 (0.054)	0.003 (0.023)	-0.076 (0.055)	0.005 (0.022)	-0.080 (0.055)
Growth Acceleration _{t-1}	-0.003 (0.011)	-0.021 (0.013)	0.006 (0.013)	-0.008 (0.015)	0.015 (0.014)	-0.006 (0.021)
Hansen J Test		[0.12]		[0.15]		[0.10]
AR(2) Test		[0.63]		[0.50]		[0.46]
R-squared	0.93		0.93		0.93	
Observations	739	659	739	659	739	659
Countries	124	122	124	122	124	122

Fixed effects OLS with country dummies and robust standard errors clustered by country in columns 1, 3, and 5; GMM of Arellano-Bond in columns 2, 4, and 6, with robust standard errors. Dependent variable: in Panel A, the Freedom House Political Rights Index; in Panel B, Transition towards Democracy, calculated as $\max(d_{it}, d_{it-1})$ as in Table 9; and in Panel C, Transition away from Democracy, calculated as $\min(d_{it}, d_{it-1})$ as in Table 10. For Arellano-Bond GMM we add a double lag of the acceleration dummy as an instrument. Base sample is all countries in unbalanced panel, 1960-2000. Countries must be independent for 15 years before they enter the panel. Growth acceleration is a dummy variable corresponding to a change in the growth rate by more than a threshold. In columns 1 and 2 the threshold is 3%, in columns 3 and 4 the threshold is 4%, and in columns 5 and 6 the threshold is 5%. See text for details. For detailed definitions and sources, see Table 1 and Appendix Table A1.

Appendix Table A1

VARIABLE	DESCRIPTION	SOURCE
Freedom House Political Rights Index	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. 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 to non-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 Composite Democracy Index	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 to democracy are calculated as $\max(d_{it}, d_{it-1})$ and transitions to non-	http://www.cidcm.umd.edu/inscr/polity/
Boix/Rosato/Przeworski Democracy Index	Data for 1950-1995 in Boix and Rosato, original range 0,1. Transitions to democracy are calculated as $\max(d_{it}, d_{it-1})$ and transitions to non-democracy are calculated as $\min(d_{it}, d_{it-1})$.	Boix and Rosato (2001) "Complete Data Set of Political regimes, 1800-1999," Department of Political Science, Chicago.
GDP per Capita (Chain Weighted 1996 Prices)	Data for 1955-2000 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 1840-2000 measured as Log Real GDP per Capita (1990 Geary-Khamis dollars) from Maddison (2003).	http://www.eco.rug.nl/~Maddison/
Population	Total population in thousands.	World Bank (2002)
Education (average years of schooling in adult population)	Average total years of schooling in the population aged 25 and over. Data for 1960, 1965,..., 1995 from Barro-Lee.	Barro and Lee (2000) available at http://www.cid.harvard.edu/ciddata/ciddata.html
Male-Female Education Gap	Gap between male and female primary 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
Urbanization Rate	Percent of population living in urban areas, 0-1 scale.	World Bank (2002)
Significant Oil Producer	"1" if country is described by IMF as oil exporting. This is used as opposed to OPEC in attempting to replicate Barro's results.	following Barro (1999)
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 below 1800 coded as 1800. We coded Taiwan's independence year to 1948 and changed Zimbabwe's independence year to 1964. Identification of countries follows Polity and Boix/Rosato/Przeworski Democracy Indices.	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)
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)

Appendix Table A2
Pooled Cross-Section and Fixed Effects Results with Democracy on a Dichotomous Scale

	Base Sample, 1960-2000		Former European Colonies, 1960-2000		
	Pooled Cross-Section OLS (1)	Fixed Effects OLS (2)	Pooled Cross-Section OLS (3)	Fixed Effects OLS (4)	Pooled Cross-Section OLS, with Historical Institutions (5)
<i>Panel A</i>					
<i>Dependent Variable is Democracy</i>					
Democracy _{t-1}	0.686 (0.048)	0.293 (0.067)	0.692 (0.062)	0.261 (0.087)	0.640 (0.067)
Log GDP per Capita _{t-1}	0.095 (0.018)	0.063 (0.066)	0.074 (0.024)	-0.053 (0.117)	0.028 (0.024)
Time Effects F-test	[0.01]	[0.02]	[0.01]	[0.01]	[0.00]
Observations	797	797	467	467	467
Countries	123	123	78	78	78
R-squared	0.68	0.77	0.59	0.71	0.60
<i>Panel B</i>					
<i>Dependent Variable is Transition to Democracy</i>					
Democracy _{t-2}	-0.061 (0.028)	-0.156 (0.059)	-0.053 (0.034)	-0.210 (0.069)	-0.063 (0.044)
Log GDP per Capita _{t-1}	0.004 (0.012)	0.009 (0.048)	0.009 (0.016)	-0.049 (0.065)	-0.013 (0.017)
Time Effects F-test	[0.03]	[0.12]	[0.00]	[0.02]	[0.00]
Observations	740	740	467	467	467
Countries	119	119	78	78	78
R-squared	0.03	0.21	0.04	0.24	0.07
<i>Panel C</i>					
<i>Dependent Variable is Transition Away from Democracy</i>					
Democracy _{t-2}	-0.065 (0.019)	-0.069 (0.038)	-0.079 (0.025)	-0.080 (0.046)	-0.091 (0.023)
Log GDP per Capita _{t-1}	0.029 (0.009)	0.002 (0.031)	0.023 (0.013)	-0.010 (0.049)	0.020 (0.013)
Time Effects F-test	[0.02]	[0.06]	[0.02]	[0.10]	[0.02]
Observations	740	740	467	467	467
Countries	119	119	78	78	78
R-squared	0.04	0.20	0.07	0.22	0.08

Pooled OLS regressions in columns 1, 3, and 5, fixed effects OLS, with country dummies, in columns 2 and 4. Robust standard errors clustered by country in parentheses. Time effects are included in all columns. Dependent variable: Boix/Rosato/Przeworski Democracy Index in Panel A, Boix/Rosato/Przeworski Index Transition to Democracy in Panel B, and Boix/Rosato/Przeworski Index Transition to Non-Democracy in Panel C. Transition to democracy = 1 if $d_{it-1}=0$ and $d_{it}=1$, and = 0 otherwise. Transition to non-democracy is = 0 if $d_{it-1}=1$ and $d_{it}=0$, and = 1 otherwise. The base sample is an unbalanced panel, 1960-2000, with data at 5-year intervals. Columns 3, 4 and 5 use a balanced panel of former European colonies between 1960 and 2000; countries must have been independent for 10 years to enter. Lag value of democracy is d_{it-2} for Panels B and C for comparability with the non-linear results; countries must be independent for 10 years. Column 5 includes but does not report constraint on the executive at independence, independence year and log population density. Former European colonies sample includes only those colonies for which we have data on constraint on executive and population density in 1500. For detailed definitions and sources, see Table 1 and Appendix

Appendix Table A3
Non-Linear Estimation Results on a Dichotomous Scale

	Base Sample, 1960-2000			Former European Colonies, 1960-2000		
	Probit (1)	Logit (2)	Conditional Logit (3)	Probit (4)	Conditional Logit (5)	Probit, with Historical Institutions (6)
<i>Panel A</i>						
<i>Dependent Variable is Democracy</i>						
Democracy _{t-1}	0.754 (0.043)	4.026 (0.402)	1.216 (0.363)	0.743 (0.060)	1.167 (0.440)	0.694 (0.068)
Log GDP per Capita _{t-1}	0.220 (0.034)	1.062 (0.170)	1.485 (0.862)	0.132 (0.037)	-0.274 (1.298)	0.037 (0.049)
Time Effects F-test	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
Observations	797	797	258	467	164	467
Countries	123	123	37	78	24	78
<i>Panel B</i>						
<i>Dependent Variable is Transition to Democracy</i>						
Democracy _{t-2}	-0.053 (0.023)	-1.145 (0.612)	-1.178 (0.431)	-0.045 (0.030)	-1.872 (0.593)	-0.044 (0.024)
Log GDP per Capita _{t-1}	0.001 (0.011)	0.060 (0.208)	0.047 (0.772)	0.008 (0.016)	-0.924 (1.401)	-0.014 (0.017)
Time Effects F-test	[0.05]	[0.08]	[0.28]	[0.02]	[0.00]	[0.05]
Observations	740	740	261	438	180	438
Countries	119	119	39	78	26	78
<i>Panel C</i>						
<i>Dependent Variable is Transition Away from Democracy</i>						
Democracy _{t-2}	-0.066 (0.021)	-1.794 (0.522)	-0.707 (0.527)	-0.087 (0.032)	-0.880 (0.631)	-0.096 (0.029)
Log GDP per Capita _{t-1}	0.024 (0.007)	0.815 (0.235)	0.624 (1.604)	0.017 (0.011)	0.571 (2.217)	0.020 (0.012)
Time Effects F-test	[0.29]	[0.47]	[0.57]	[0.15]	[0.44]	[0.29]
Observations	623	623	144	390	108	390
Countries	117	117	20	77	15	77

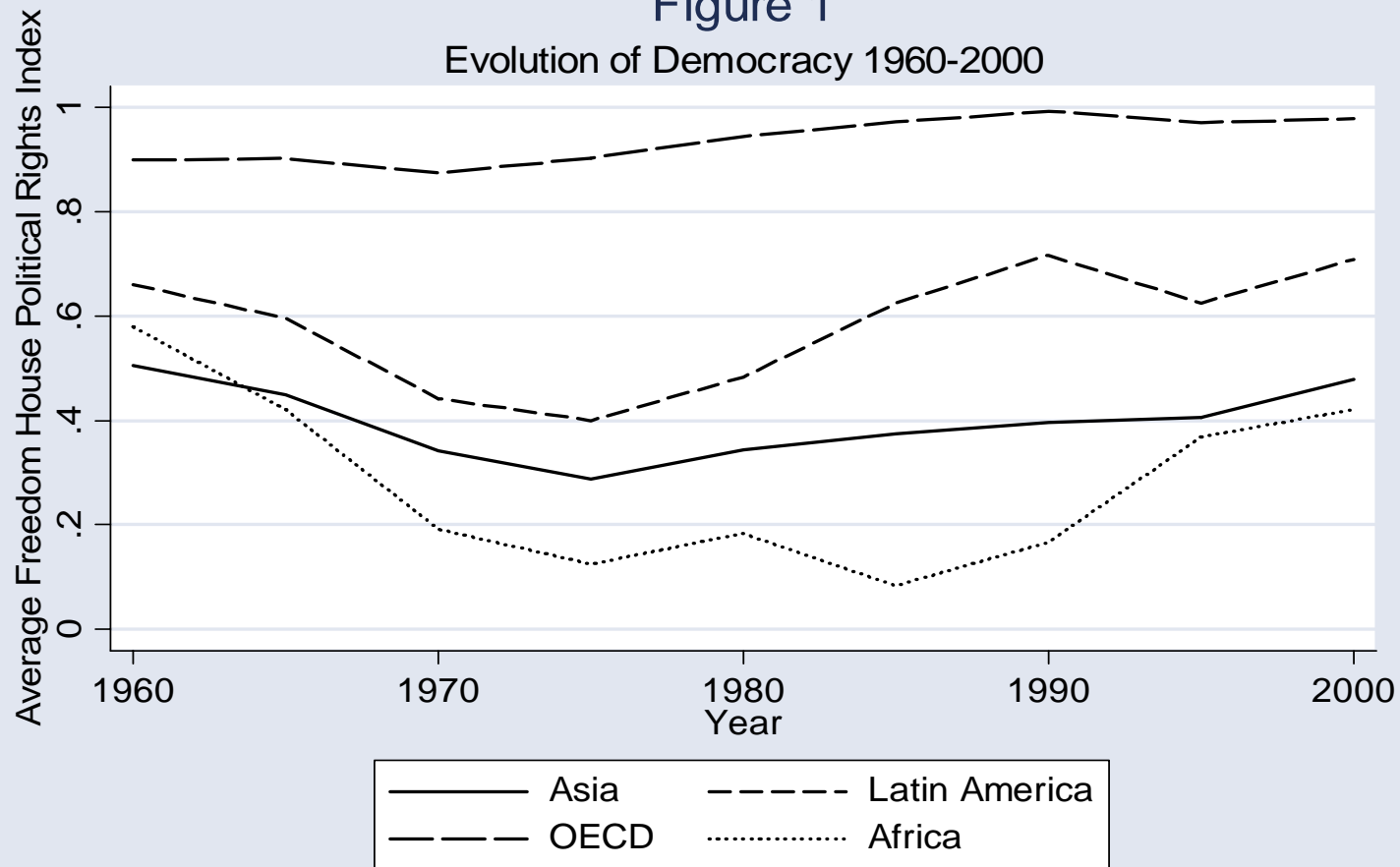
Standard errors clustered by country in parentheses, non-robust errors in columns 3 and 5. Time effects in all columns. Dependent variable: Boix/Rosato/Przeworski Democracy Index in Panel A, Boix/Rosato/Przeworski Index Transition to Democracy in Panel B, and Boix/Rosato/Przeworski Index Transition to Non-Democracy in Panel C. Transition to democracy = 1 if $d_{it-1}=0$ and $d_{it}=1$, and = 0 otherwise. Transition to non-democracy is = 0 if $d_{it-1}=1$ and $d_{it}=0$, and = 1 otherwise. The base sample is an unbalanced panel, 1960-2000, with data at 5-year intervals. When $t=1960$, $t-1=1955$. Columns 3, 4 and 5 use an unbalanced panel of European countries between 1960 and 2000; countries must have been independent for 10 years to enter. Columns 1, 4, and 6 report marginal effects at the mean of the continuous right-hand side variables and marginal effects at 0 for the discrete 0/1 right-hand side variables. Columns 2, 3 and 5 report coefficients for the linear equation of the latent dependent variable. Column 6 includes but does not report Constraint on the Executive at Independence, Independence Year/100, and Log Population Density 1500. Former European colonies sample includes only those colonies for which we have data on constraint on executive and population density in 1500. Lag value of democracy is at $t-2$ in Panel B and Panel C. For detailed definitions and sources, see Table 1 and Appendix Table A1.

Appendix Table A4
Fixed Effects Results: Investigating Londregan and Poole (1996)

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

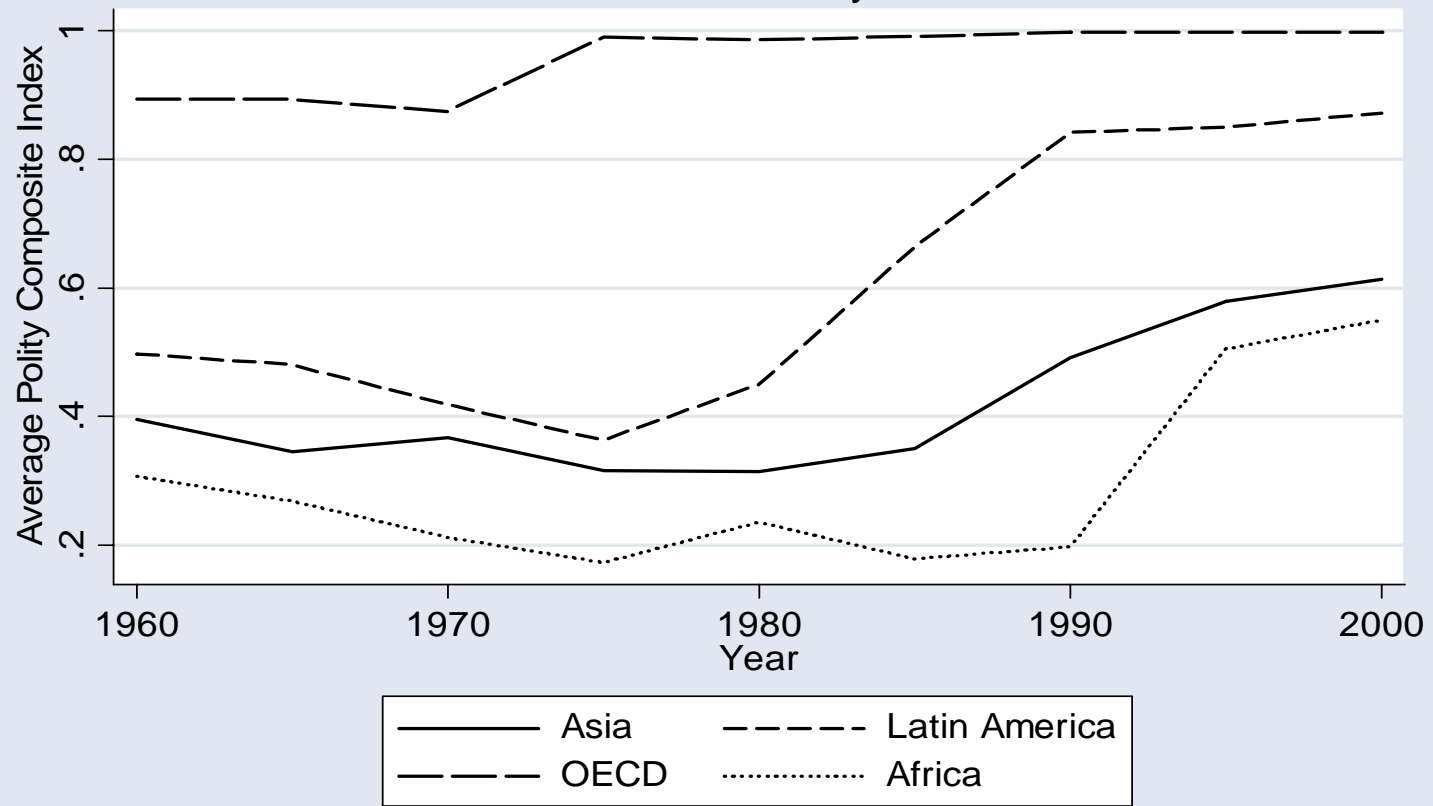
In both columns 1 and 2, non-robust standard errors are in parentheses. Country dummies are included in all columns. Columns 3 and 4 include time effects and cluster the standard errors (included in parentheses) to account for potential residual serial correlation. Column 5 includes lags of democracy 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 3a and 3b, columns 4 and 8. 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 $\ln(\text{Polity}+10.5)-\ln(10.5-\text{Polity})$ 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 2-4 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.

Figure 1
Evolution of Democracy 1960-2000



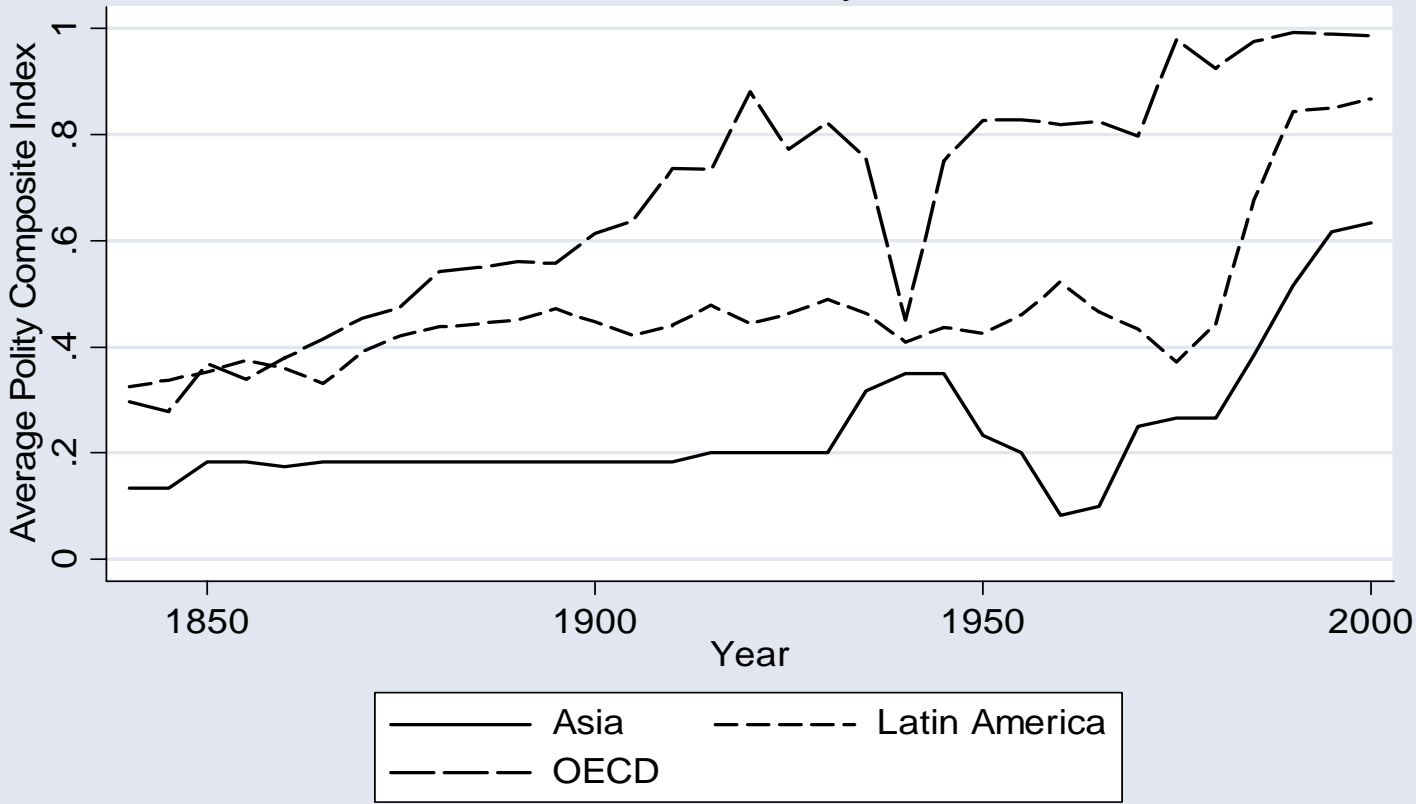
See Appendix Table A1 for data definitions and sources. Countries are included only if they were independent by 1960 and have data available for 1960. OECD does not include countries that joined since 1990. Asia excludes Japan (as it is included in OECD countries) and also excludes former Soviet countries and the Middle East. Africa refers to sub-Saharan Africa only. Latin America includes Mexico, Central America, and the Caribbean.

Figure 2
Evolution of Democracy 1960-2000



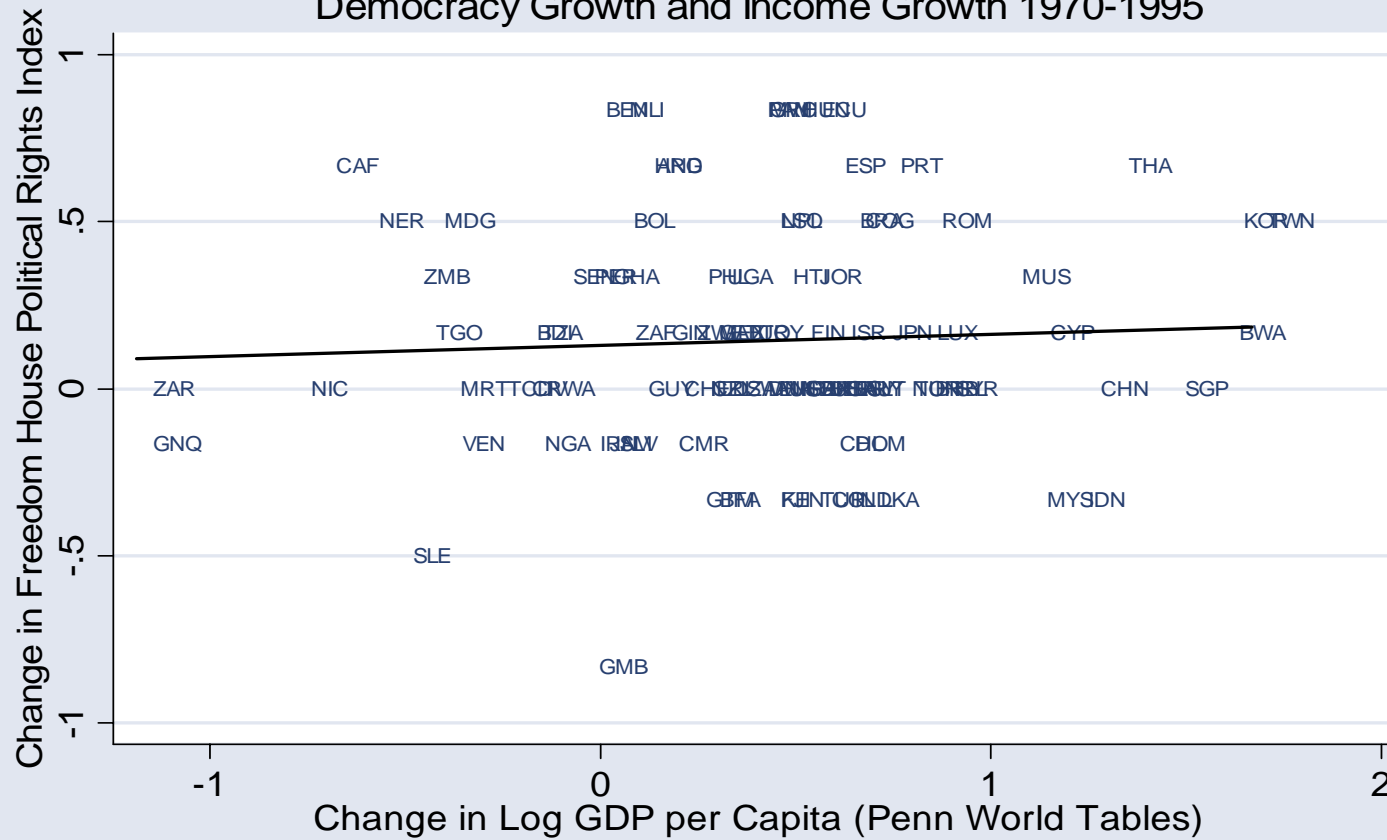
See notes to Figure 1.

Figure 3
Evolution of Democracy 1840-2000



See Appendix Table A1 for data definitions and sources. Countries are included only if they were independent by 1840, have data available for 1840, and are in existence today. See notes to Figure 1 for group classification.

Figure 4
Democracy Growth and Income Growth 1970-1995



See Appendix Table A1 for data definitions and sources. Changes are total difference between 1970 and 1995. Countries are included if they were independent by 1970. 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=102, $R^2=0.00$.

Figure 5
Democracy Growth and Income Growth 1970-1995

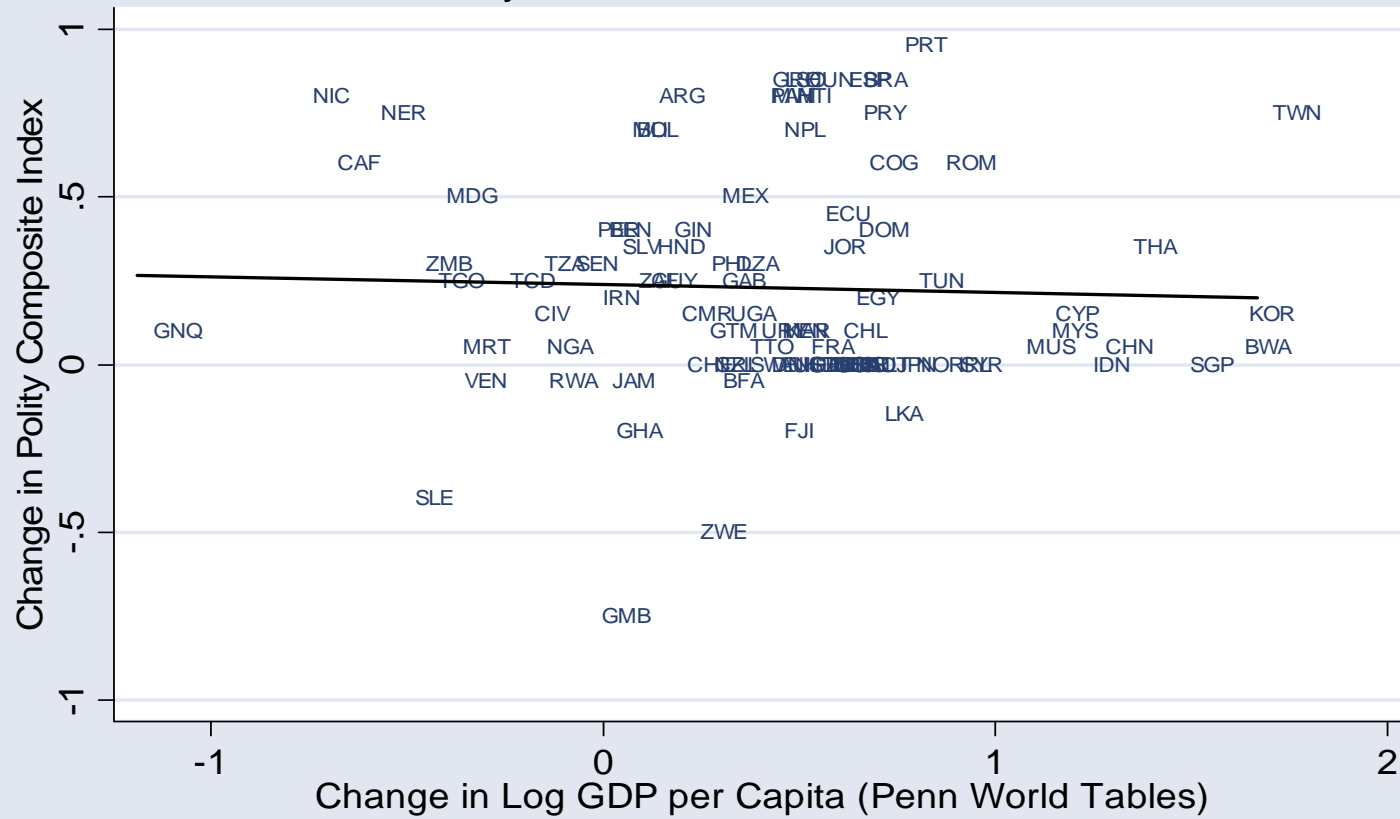
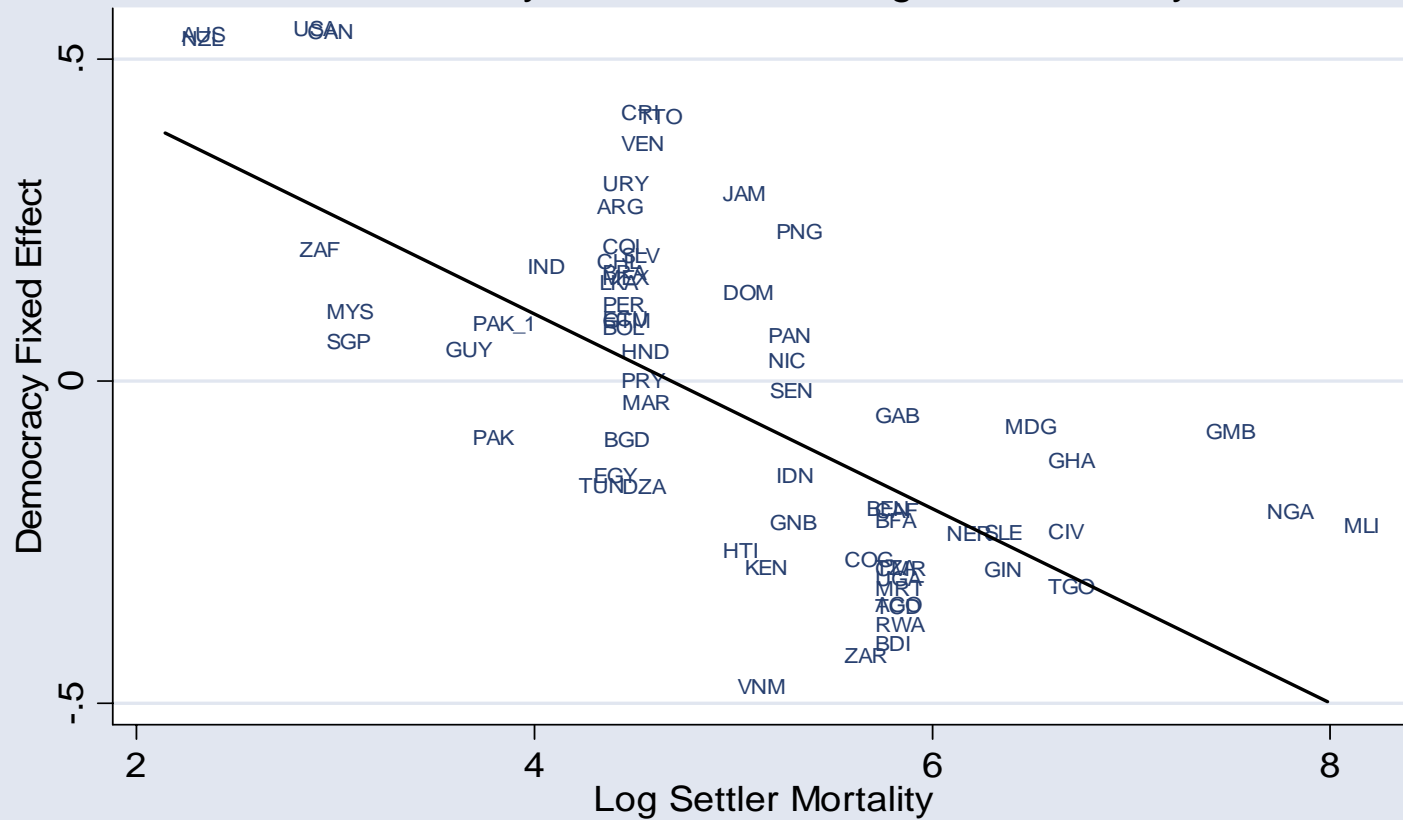
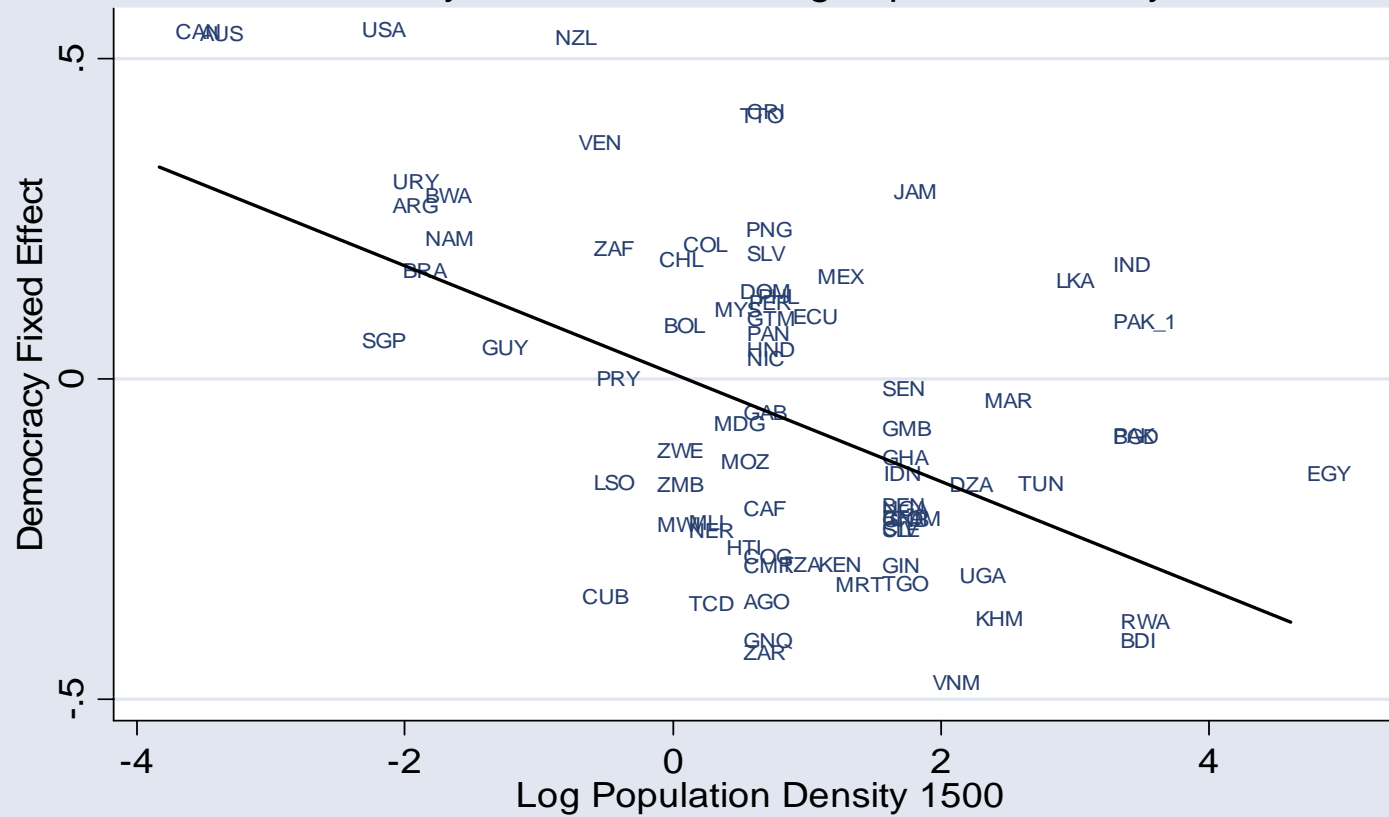


Figure 6
Democracy Fixed Effect and Log Settler Mortality



See Appendix Table A1 for data definitions and sources. Country fixed effects are those estimated in the specification of Table 5a, column 2.

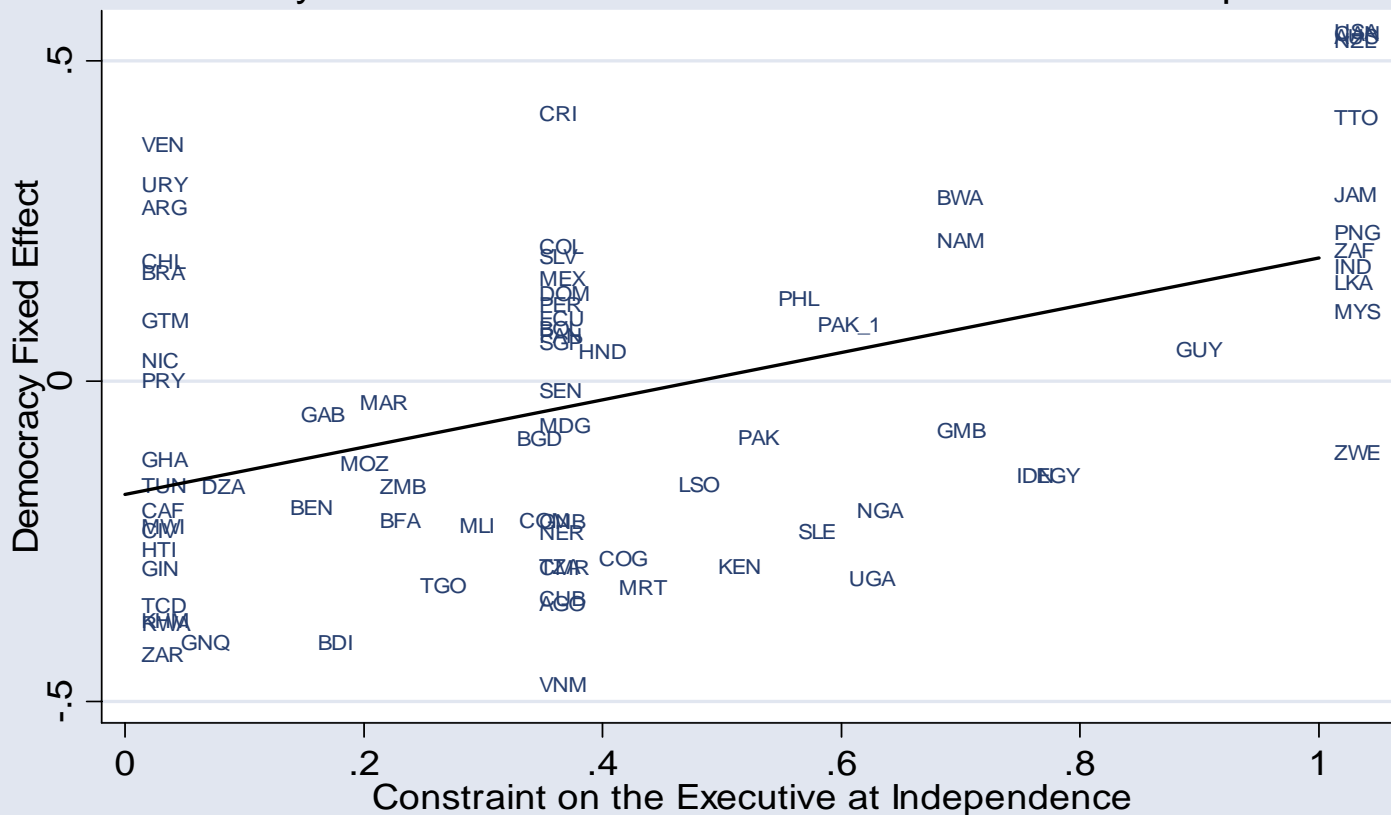
Figure 7
Democracy Fixed Effect and Log Population Density 1500



See notes to Figure 6.

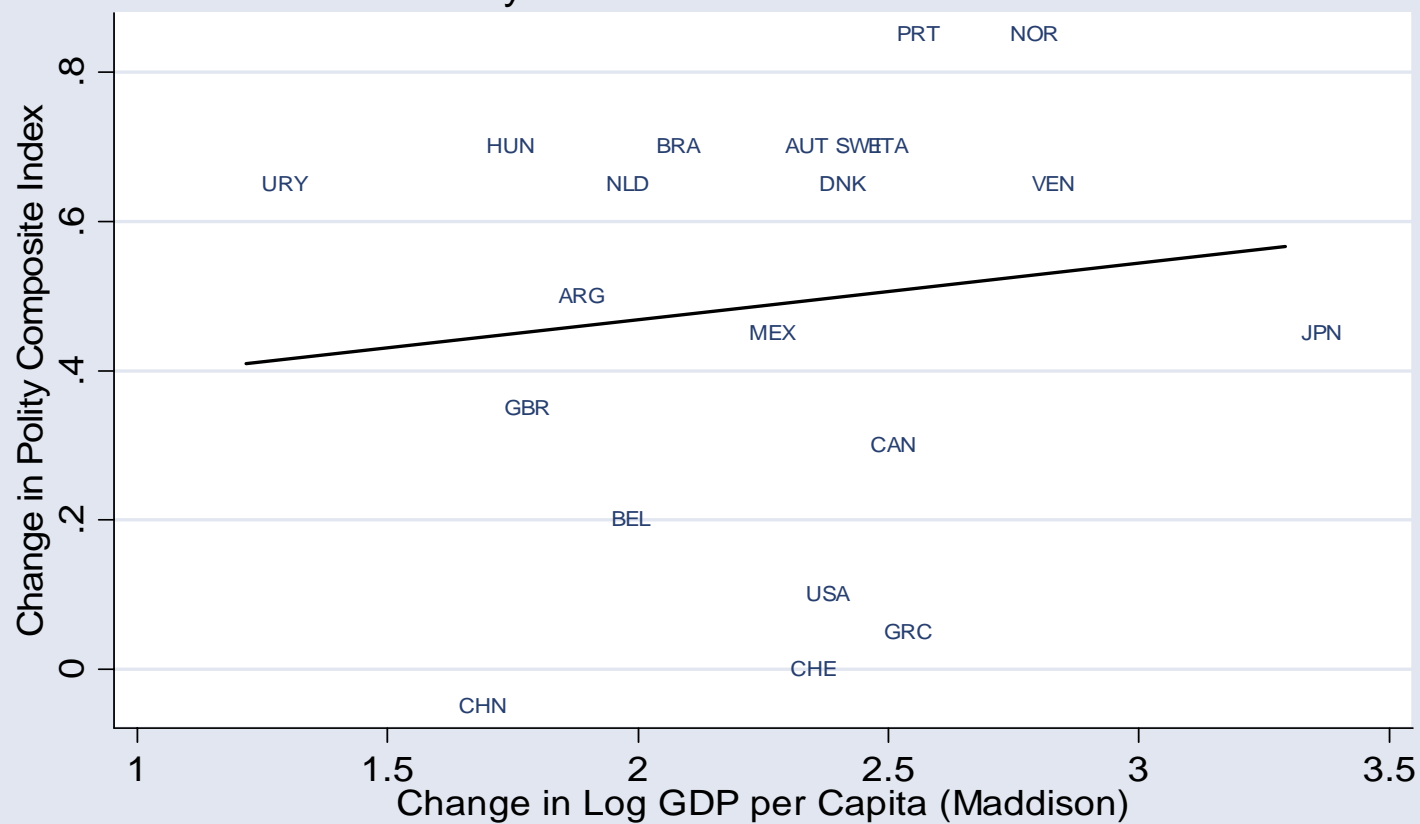
Figure 8

Democracy Fixed Effect and Constraint on the Executive at Independence



See notes to Figure 6.

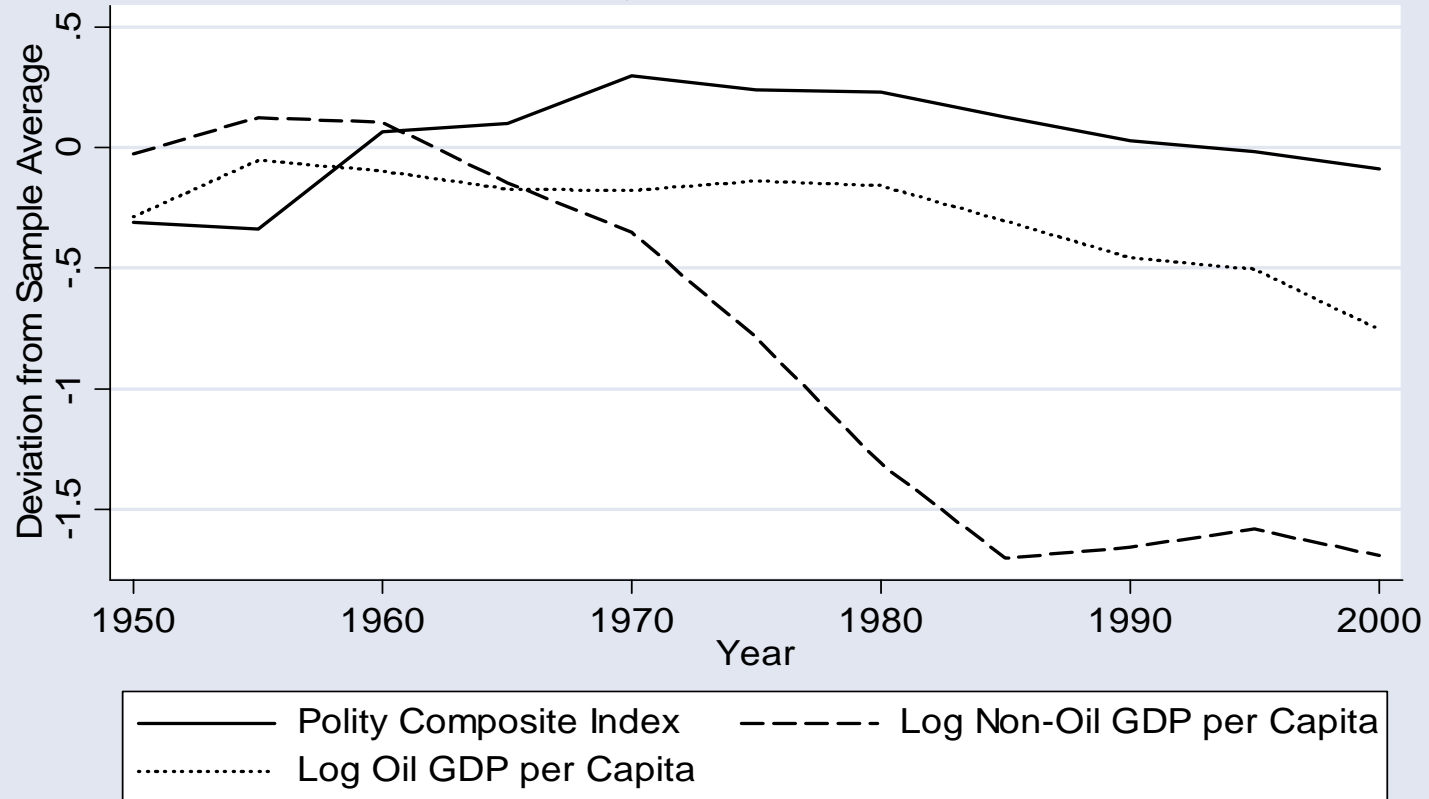
Figure 10
Democracy Growth and Income Growth 1870-1995



Log GDP per Capita is from Maddison (2003). See Appendix Table A1 for data definitions and sources. Changes are total difference between 1870 and 1995. Countries are included if they are in the long sample discussed in Section 6 of the text and were independent by 1870. 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 0.076 (standard error=0.123), $N=21$, $R^2=0.02$.

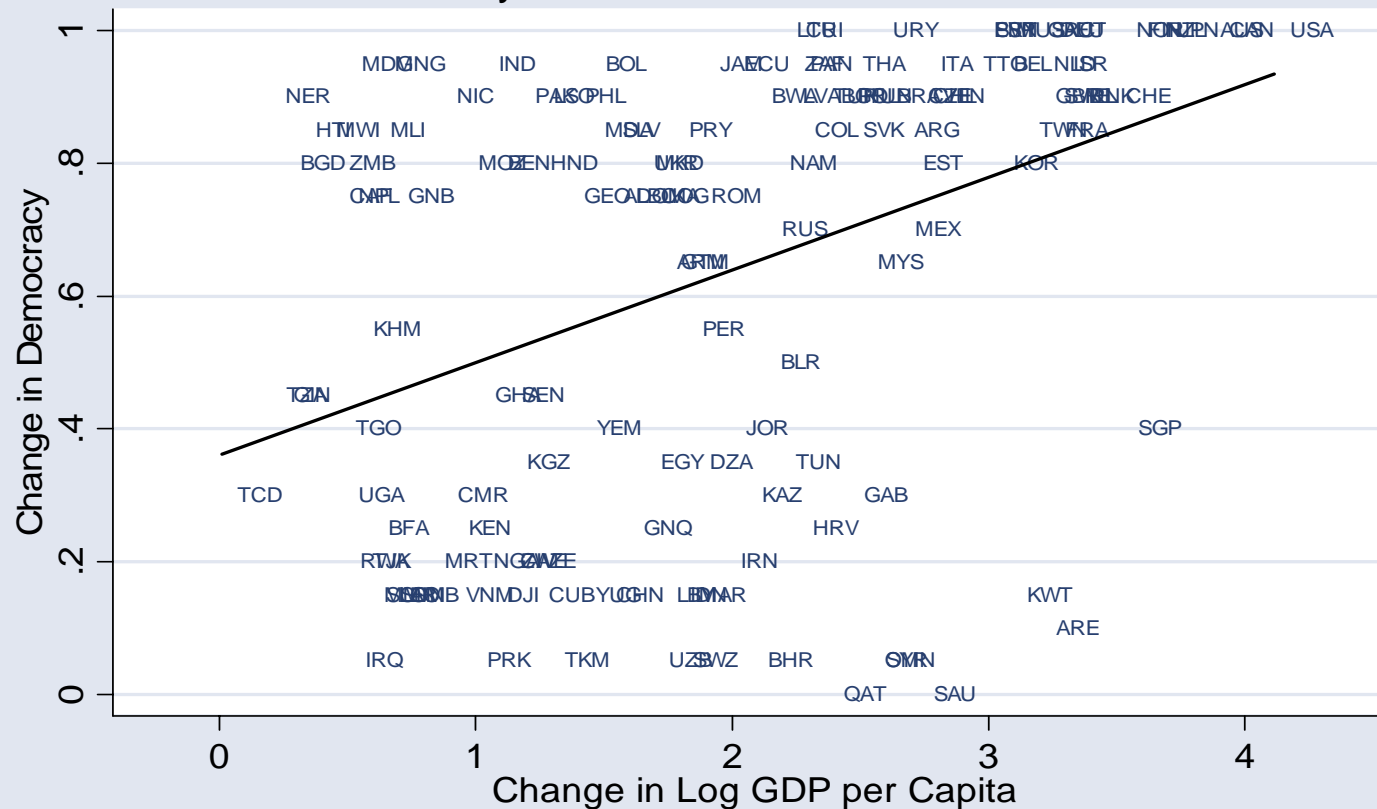
Figure 11

Evolution of Democracy and Income in Venezuela 1950-2000



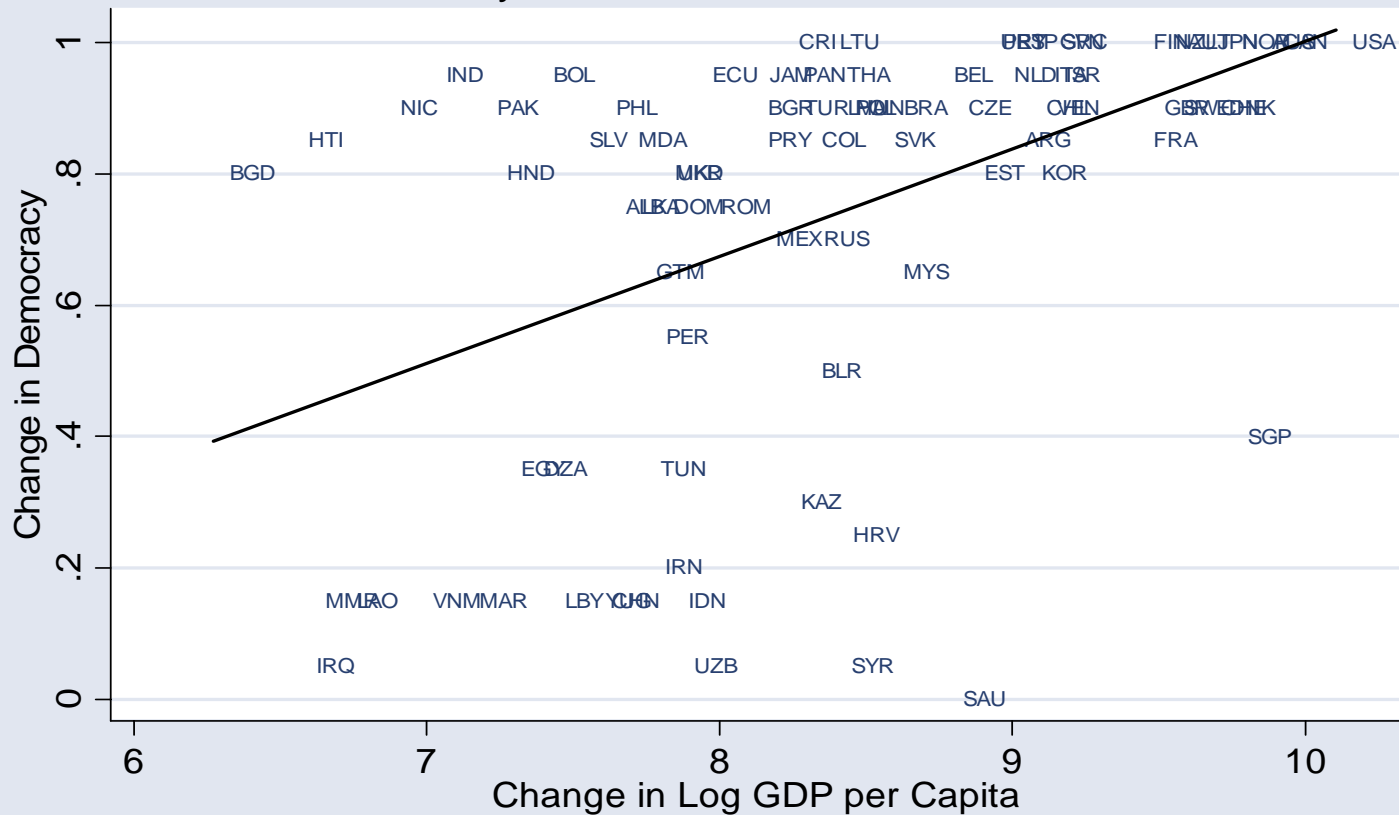
Reference sample is long sample discussed in Section 6 of the text. Breakdown of GDP is from Venezuela national statistics available only after 1950. In calculating the deviation from sample average, we average GDP and democracy scores in each period for 28 countries.

Figure 12
Democracy Growth and Income Growth 1500-1995



See Appendix Table A1 for data definitions and sources. Changes are total differences between 1500 and 1995. GDP per capita is from Maddison. Democracy is calculated using the Polity Composite Index, which comprises in part constraint on the executive; data for 1500 from Acemoglu et al (2004b). The end date is chosen to maximize the number of countries in the cross-section. The regression represented by the fitted line yields a coefficient of 0.139 (standard error=0.033), $N=143$, $R^2=0.17$.

Figure 13
Democracy Growth and Income Growth 1500-1995



See Appendix Table A1 for data definitions and sources. Changes are total differences between 1500 and 1995. GDP per capita in 1500 is estimated using the method of Acemoglu et al (2002), based on urbanization. Democracy is calculated using the Polity Composite Index, which comprises in part constraint on the executive; data for 1500 from Acemoglu et al (2004b). The end date is chosen to maximize the number of countries in the cross-section. The regression represented by the fitted line yields a coefficient of 0.163 (standard error=0.034), $N=84$, $R^2=0.23$.