Income and Democracy*

Daron Acemoglu[†] **Simon Johnson**[‡] **James A. Robinson**[§]
Pierre Yared[¶]

First Version: May 2004.
This Version: October 2006.

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 both variables. In the post-war sample, we show that controlling for such factors by including country fixed effects removes the statistical association between income per capita and various measures of democracy. We present instrumental-variables estimates using two different strategies that also show no causal effect of income on democracy. Moreover, in a sample spanning the entire 20th century, the inclusion of country fixed effects again removes the statistical association between income and democracy. The cross-country correlation between income and democracy instead reflects longer-run changes, in particular, a positive correlation between changes in income and democracy over the past 500 years. We suggest a possible explanation for this pattern based on the idea that societies may have embarked on divergent political-economic development paths at certain critical junctures over the past 500 years. Consistent with this, the 500-year correlation between changes in income and democracy is significantly weakened or disappears when we control for potential determinants of these divergent development paths.

Keywords: democracy, economic growth, institutions, political development.
JEL Classification: P16, O10.

---

*We thank David Autor, Robert Barro, Sebastián Mazzuca, Robert Moffitt, Jason Seawright, four anonymous referees, and seminar participants at the Banco de la República de Colombia, Boston University, the Canadian Institute for Advanced Research, the CEPR annual conference on transition economics in Hanoi, MIT, and Harvard for comments.

[†]Department of Economics, Massachusetts Institute of Technology, 50 Memorial Drive, MA02139. e-mail: daron@mit.edu.

[‡]Sloan School of Management and International Monetary Fund, Massachusetts Institute of Technology, 50 Memorial Drive, MA02139. e-mail: sjohnson@mit.edu, sjohnson@imf.org.

[§]Department of Government, Harvard University, Littauer, 1875 Cambridge St., Cambridge MA02138; e-mail: jrobinson@gov.harvard.edu.

[¶]Department of Economics, Massachusetts Institute of Technology, 50 Memorial Drive, MA02139. e-mail: yared@mit.edu.

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, for example sub-Saharan Africa and Southeast Asia. The positive cross-country relationship between income and democracy in the 1990s is depicted in Figure 1. This 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. Barro (1999, S. 160), for example, summarizes this as: "increases in various measures of the standard of living forecast a gradual rise in democracy. In contrast, democracies that arise without prior economic development ... tend not to last."[1]

This statistical association between income and democracy is the cornerstone of the influential modernization theory. Lipset (1959) suggested that democracy was both created and consolidated by a broad process of 'modernization' which involved changes in "the factors of industrialization, urbanization, wealth, and education [which] are so closely interrelated as to form one common factor. And the factors subsumed under economic development carry with it the political correlate of democracy" (Lipset, 1959, p. 80). The central tenet of the modernization theory, that higher income per-capita causes a country to be democratic, is also reproduced in most major works on democracy (e.g., Dahl, 1971, Huntington, 1991, Rusechemeyer, Stephens and Stephens, 1992).

In this paper, we revisit the relationship between income per capita and democracy. Our starting point is that existing work, which is based on cross-country relationships, does not establish causation. 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.

We utilize two strategies to investigate the causal effect of income on democracy. Our first strategy is to control for country-specific factors affecting both income and democracy by including country fixed effects. While fixed effect regressions are not a panacea against omitted variable biases,[2] they are well-suited to the investigation of the

[1] Also see, among others, Lipset (1959), Londregan and Poole (1996), Przeworski and Limongi (1997), Barro (1997), Przeworski, Alvarez, Cheibub, and Limongi (2000), and Papaioannou and Siourounis (2006).

[2] Fixed 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

relationship between income and democracy, especially in the postwar era. The major source of potential bias in a regression of democracy on income per capita is 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 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 simple cross-country comparison, as well as the existing empirical strategies in the literature, which do not control for fixed country effects, would suggest that per capita income causes democracy. The idea of fixed effects is to move beyond this comparison and investigate the "within-country variation", that is to ask whether Colombia is more likely to *become* (relatively) democratic as it *becomes* (relatively) richer. In addition to improving inference on the causal effect of income on democracy, this approach is also more closely related to modernization theory as articulated by Lipset (1959), which emphasizes that individual countries should become more democratic if they are richer, not simply that rich countries should be democratic.

Our first result is that once fixed effects are introduced, the positive relationship between income per capita and various measures of democracy disappears. Figures 2 and 3 show this diagrammatically by plotting changes in our two measures of democracy, the Freedom House and Polity scores for each country between 1970 and 1995 against the change in GDP per capita over the same period (see Section 2 for data details). These figures confirm that there is no relationship between *changes* in income per capita and *changes* in democracy.

This basic finding is robust to using various different indicators for democracy, to different econometric specifications and estimation techniques, in different subsamples, and to the inclusion of additional covariates. The absence of a significant relationship between income and democracy is *not* driven by large standard errors. On the contrary, the relationship between income and democracy is estimated relatively precisely. In many cases, two-standard error bands include only very small effects of income on democracy and often exclude the OLS estimates. These results therefore shed considerable doubt on the claim that there is a strong causal effect of income on democracy.[3]

While the fixed effects estimation is useful in removing the influence of long-run de-

right-hand side variables. Consequently, fixed effects are certainly no substitute to instrumental-variables or structural estimation with valid exclusion restrictions.

[3]It remains true that over time there is a general tendency towards greater incomes and greater democracy 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 on democracy.

terminants of both democracy and income, it does not necessarily estimate the causal effect of income on democracy. Our second strategy is to use instrumental-variables (IV) regressions to estimate the impact of income on democracy.[4] We experiment with two potential instruments. The first is to use past savings rates, while the second is to use changes in the incomes of trading partners. The argument for the first instrument is that variations in past savings rates affect income per capita but should have no direct effect on democracy. The second instrument, which we believe is of independent interest, creates a matrix of trade shares and constructs predicted income for each country using a trade-share-weighted average income of other countries. We show that this predicted income has considerable explanatory power for income per capita. We also argue that it should have no direct effect on democracy. Our second major result is that both IV strategies show no evidence of a causal effect of income on democracy. We recognize that neither instrument is perfect, since there are reasonable scenarios in which our exclusion restrictions could be violated (e.g., saving rates might be correlated with future anticipated regime changes; or democracy scores of a country's trading partners, which are correlated with their income levels, might have a direct effect on its democracy). To alleviate these concerns, we show that the most likely sources of correlation between our instruments and the error term in the second stage are not present.

We also look at the relationship between income and democracy over the past 100 years using fixed effects regressions and again find no evidence of a positive impact of income on democracy. These results are depicted in Figure 4, which plots the change in Polity score for each country between 1900 and 2000 against the change in GDP per capita over the same period (see Section 6 for data details). This figure confirms that there is no relationship between income and democracy conditional on fixed effects.

These results naturally raise the following important question: why is there a cross-sectional correlation between income and democracy? Or in other words, why are rich countries democratic today? At a statistical level, the answer is clear; even though there is no relationship between changes in income and democracy in the postwar era or over the past hundred years or so, there is a positive association over the past 500 years. Most societies were nondemocratic 500 years ago and had broadly similar income levels. The positive cross-sectional relationship reflects the fact that those that have become more democratic over this time span are also those that have grown faster. One possible

[4] A recent creative attempt is by Miguel, Satyanath and Sergenti (2004), who use the weather conditions as an instrument for income in Africa to investigate the impact of income on civil wars. Unfortunately, weather conditions are only a good instrument for relatively short-run changes in income, thus not ideal to study the relationship between income and democracy.

explanation for the positive cross-sectional correlation is therefore that there is a causal effect of income on democracy, but it works at *much* longer horizons than the existing literature has posited. Although the lack of a relationship over 50 or 100 years sheds some doubt on this explanation, this is a logical possibility.

We favor another explanation for this pattern. Even in the absence of a simple causal link from income to democracy, political and economic development paths are interlinked and are jointly affected by various factors. Societies may embark on *divergent political-economic development paths*, some leading to relative prosperity and democracy, others to relative poverty and dictatorship. Our hypothesis is that the positive cross-sectional relationship and the 500-year correlation between changes in income and democracy are caused by the fact that countries have embarked on divergent development paths at some *critical junctures* during the past 500 years.[5]

We provide support for this hypothesis by documenting that the positive association between changes in income and democracy over the past 500 years are largely accounted for by a range of historical variables. In particular, for the whole world sample, the positive association is considerably weakened when we control for date of independence, early constraints on the executive and religion.[6] We then turn to the sample of former European colonies, where we have better proxies for factors that have influenced the development paths of nations. Acemoglu, Johnson and Robinson (2001, 2002) and Engerman and Sokoloff (1997) argue that differences in European colonization strategies have been a major determinant of the divergent development paths of colonial societies. This reasoning suggests that in this sample, the critical juncture for most societies corresponds to their experience under European colonization. Furthermore, Acemoglu, Johnson and Robinson (2002) show that the density of indigenous populations at the time of colonization has been a particularly important variable in shaping colonization strategies and provide estimates of population densities in 1500 (before the advent of colonization). When we use information on population density as well as on independence year and early constraints on the executive, the 500-year relationship between changes in income and democracy in the former colonies sample disappears. This pattern is consistent with the hypothesis that the positive cross-sectional relationship between income and democracy today is the result of societies embarking on divergent development paths at certain critical junctures during

[5] See, among others, North and Thomas (1973), North (1981), Jones (1981), Engerman and Sokoloff (1997), Acemoglu, Johnson and Robinson (2001, 2002) for theories that emphasize the impact of certain historical factors on development processes during critical junctures, such as the collapse of feudalism, the age of industrialization or the process of colonization.

[6] See Weber (1930), Huntington (1991), Fish (2002) for the hypothesis that religion might have an important effect on economic and political development.

the past 500 years (though other hypotheses might also account for these patterns).

A related question is whether income has a separate causal effect on transitions to and away from democracy. Space restrictions preclude us from investigating this question here, and the results of such an investigation are presented in our followup paper, Acemoglu, Johnson, Robinson and Yared (2006). Using both linear regression models and double-hazard models that simultaneously estimate the process of entry into and exit from democracy, we find no evidence that income has a causal effect on either the transitions to or from democracy. The IV strategies and the focus on the long run relationship are unique to the current paper.

The paper proceeds as follows. In Section 2 we describe the data. Section 3 presents our econometric model. Section 4 presents the fixed effects results for the post-war sample. Section 5 contains our IV results for the post-war sample, while the fixed effects results for the 100-year sample are presented in Section 6. Section 7 discusses the sources of the cross-country relationship between income and democracy we observe today. Section 8 concludes. The Appendix contains further information on the construction of the instruments used in Section 5.

2 Data and Descriptive Statistics

Our first and main measure of democracy is the Freedom House Political Rights Index. A country receives the highest score if political rights come closest to the ideals suggested by a checklist of questions, beginning with whether there are free and fair elections, whether those who are elected rule, whether there are competitive parties or other political groupings, whether the opposition plays an important role and has actual power, and whether minority groups have reasonable self-government or can participate in the government through informal consensus.[7] Following Barro (1999), we supplement this index with the related variable from Bollen (1990, 2001) for 1950, 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 infor-

[7] The 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 combined to form the index. See Freedom House (2004), http://www.freedomhouse.org/research/freeworld/2003/methodology.htm

mation for all independent countries starting in 1800. Both for pre-1950 events and as a check on our main measure, we also look at the other widely-used measure of democracy, the composite Polity index, which is the difference between Polity's Democracy and Autocracy indices (see Marshall and Jaggers, 2004). The Polity Democracy Index ranges from 0 to 10 and is derived from coding the competitiveness of political participation, the openness and competitiveness of executive recruitment and constraints on the chief executive. The Polity Autocracy Index also ranges from 0 to 10 and is constructed in a similar way to the democracy score based on 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 normalize the composite Polity index to lie between 0 and 1.

Using the Freedom House and the Polity data, we construct five-year, ten-year, twenty-year, and annual panels. For the five-year panels, we take the observation every fifth year. We prefer this procedure to averaging the five-year data, since averaging introduces additional serial correlation, making inference and estimation more difficult (see footnote 11). Similarly, for the ten-year and twenty-year panels, we use the observations from every tenth and twentieth year. For the Freedom House data, which begin in 1972, we follow Barro (1999) and assign the 1972 score to 1970 for the purpose of the five-year and ten-year regressions.

The GDP per capita (in PPP) and savings rate data for the postwar period are from Heston, Summers, and Atten (2002), and GDP per capita (in constant 1990 dollars) for the longer sample are from Maddison (2003). The trade-weighted world income instrument is built using data from International Monetary Fund Direction of Trade Statistics (2005). Other variables we use in the analysis are discussed later (see also Appendix Table A1 for detailed data definitions and sources).

Table 1 contains descriptive statistics for the main variables. The sample period is 1960-2000 and each observation corresponds to five-year intervals. The table shows these statistics for all countries and also for high- and low-income countries, split according to median income. The first panel refers to the baseline sample we use in Table 2, while the other panels are for samples used in other tables. In each case, we report means, standard deviations, and also the total number of countries for which we have data and the total number of observations. The comparison of high- and low-income countries in columns 2 and 3 confirms the pattern in Figure 1 that richer countries tend to be more democratic.

3 Econometric Model

Consider the following simple econometric model, which will be the basis of our work both for the post-war and in the 100-year samples:

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

where d_{it} is the democracy score of country i in period t. The lagged value of this variable on the right-hand side is included to capture persistence in democracy and also potentially mean-reverting dynamics (i.e., the tendency of the democracy score to return to some equilibrium value for the country). The main variable of interest is y_{it-1}, the lagged value of log income per capita. The parameter γ therefore measures the causal effect of income per capita on democracy. All other potential covariates are included in the vector $\mathbf{x}_{it-1}$. In addition, the δ_i's denote a full set of country dummies and the μ_t's denote a full set of time effects that capture common shocks to (common trends in) the democracy score of all countries. u_{it} is an error term, capturing all other omitted factors, with $E(u_{it}) = 0$ for all i and t.[8]

The standard regression in the literature, for example, Barro (1999), is pooled OLS, which is identical to (1) except for the omission of the fixed effects, δ_i's. In our framework, these country dummies capture any time-invariant country characteristic that affect the level of democracy. As is well known, when the true model is given by (1) and the δ_i's are correlated with y_{it-1} or $\mathbf{x}_{it-1}$, then pooled OLS estimates are biased and inconsistent. More specifically, let x^j_{it-1} denote the jth component of the vector $\mathbf{x}_{it-1}$ and let Cov denote population covariances. Then, if either $\text{Cov}(y_{it-1}, \delta_i + u_{it}) \neq 0$ or $\text{Cov}\left(x^j_{it-1}, \delta_i + u_{it}\right) \neq 0$ for some j, the OLS estimator will be inconsistent. In contrast, even when these covariances are nonzero, the fixed effects estimator will be consistent if $\text{Cov}(y_{it-1}, u_{it}) = \text{Cov}\left(x^j_{it-1}, u_{it}\right) = 0$ for all j (as $T \to \infty$). This structure of correlation is particularly relevant in the context of the relationship between income and democracy because of the possibility of 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 necessarily estimate the causal effect of income on democracy. First, the regressor d_{it-1} is mechanically correlated with u_{is} for $s < t$ so the standard fixed effect estimator is biased (e.g.,

[8] More generally, equation (1) can be combined with another equation that captures the effect of democracy on income. The simultaneous equation bias resulting from the endogeneity of democracy is addressed in Section 5. The estimation of the effect of democracy on income is beyond the scope of the current paper.

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 (i.e., as $T \to \infty$). We discuss and implement a number of strategies to deal with this problem in Section 4.

Second, even if we ignore this technical issue, it is possible that $\text{Cov}(y_{it-1}, u_{it}) \neq 0$ because of the reverse effect of democracy on income, because both changes in income and changes in democracy are caused by a third, time-varying factor, or because the correct model is one with fixed growth effects rather than fixed level effects (see the extended model in Section 7.1). In Section 5, we implement an instrumental variable strategy to account for these problems. It is worth noting, however, that almost all theories in political science, sociology and economics suggest that we should have $\text{Cov}(y_{it-1}, u_{it}) \geq 0$. Therefore, when it fails to be consistent, the fixed effects estimator of the relationship between income and democracy will be biased upwards. Our fixed effects results can thus be viewed as upper bounds on the causal effect of income on democracy. Consistent with this, instrumental-variables regressions in Section 5 lead to more negative estimates than the fixed effects results.

4 Fixed Effects Estimates

4.1 Main Results

We begin by estimating (1) in the post-war sample. Table 2 uses the Freedom House data and Table 3 uses the Polity data, in both cases for the period 1960-2000. All standard errors in the paper are fully robust against arbitrary heteroscedasticity and serial correlation at the county level (i.e., they are clustered at the country level, see Wooldridge, 2002).

The first columns of both Tables 2 and 3 replicate the standard pooled OLS regressions previously used in the literature using the five-year sample. These regressions include the (five-year) lag of democracy and log income per capita as the country variables, as well as a full set of time dummies. Lagged democracy is highly significant and indicates that there is a considerable degree of persistence in democracy. Log income per capita is also significant and illustrates the well-documented positive relationship between income and democracy. Though statistically significant, the effect of income is quantitatively small. For example, the coefficient of 0.072 (standard error = 0.010) in column 1 of Table 2 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). If this pooled cross-section regression identified the causal effect of income on democracy, then the long-run effect would be larger than this, because the lag of democracy on the right-hand side would be increasing over time, causing a further increase in the democracy score. The implied cumulative effect of log GDP per capita on democracy is shown in the fifth row. Since lagged democracy has a coefficient of 0.706, the cumulative effect of a 10% increase in GDP per capita is 0.007/(1-0.706)≈0.024, which is still quantitatively small.

The remaining columns of Tables 2 and 3 present our basic results with fixed effects. Column 2 shows that the relationship between income and democracy disappears once fixed effects are included. For example, in Table 2 with Freedom House data, the estimate of γ is 0.010 with a standard error of 0.035, which makes it highly insignificant. With the Polity data in Table 3, the estimate of γ has the "wrong" (negative) sign, -0.006 (standard error=0.039). The bottom rows in both tables again show the implied cumulative effect of income on democracy, which are small or negative.

A natural concern is that the lack of relationship in the fixed effects regressions may result from large standard errors. This does not seem to be the case. On the contrary, the relationship between income and democracy is estimated relatively precisely. Although the pooled OLS estimate of γ is quantitatively small, the two standard error bands of the fixed effects estimates almost exclude it. More specifically, with the Freedom House estimate, two standard error bands exclude short-run effects greater than 0.008.

That these results are not driven by some unusual feature of the data is further shown by Figures 2 and 3, 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 equation (9) with a start date at 1970 and end date at 1995 (and without lagged democracy on the right-hand side).[9] They show clearly that there is no strong relationship between income growth and changes in democracy over this period.

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 in Section 3.

Our first strategy, adopted in column 3, is to use the methodology proposed by An-

[9] These two dates are chosen to maximize sample size. The regression of the change in Freedom House score between 1970 and 1995 on change in log income per capita between 1970 and 1995 yields a coefficient of 0.032, with a standard error of 0.058, while the same regression with Polity data gives a coefficient estimate of -0.024, with a standard error of 0.063.

derson and Hsiao (1982), which is to time difference equation (1), to obtain

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

where the fixed country effects are removed by time differencing. Although equation (2) 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 an instrument for Δd_{it-1} to obtain consistent estimates and similarly, y_{it-2} is used as an instrument for Δy_{it-1}. 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 (GMM) estimator 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 4. The coefficients are now even more negative and more precisely estimated, for example -0.129 (standard error = 0.076) in Table 2.[10] In this case, the two standard error bands comfortably exclude the corresponding OLS estimate of γ (which, recall, was 0.072). 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 to 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.[11]

With the Polity data, both the Anderson and Hsiao and GMM 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.

[10] In addition, Arellano and Bover (1995) also use time-differenced instruments for the level equation, (1). Nevertheless, these instruments would only be valid if the time-differenced instruments are orthogonal to the fixed effect. Since this is not appealing in this context (e.g., five-year income growth is unlikely to be orthogonal to the democracy country fixed effect), we do not include these additional instruments.

[11] We also checked the results with five-year averaged data rather than our dataset which uses only the democracy information every fifth year. The estimates in all columns are very similar, but in this case, the AR(2) test shows evidence for additional serial correlation, which is not surprising given the serial correlation that averaging introduces. This motivates our reliance on the five-yearly or annual data sets.

Our analysis with annual data in column 6 of Tables 2 and 3 makes use of all of the available data.

Column 5 shows a simpler specification in which lagged democracy is dropped. With either the Freedom House or Polity measure of democracy there is again no evidence of a significant effect of income on democracy, and in this case, the two standard error bands comfortably exclude the corresponding OLS coefficient (the OLS estimate without lagged democracy, which is shown in the first column of Tables 5 and 6, is 0.233 with a standard error of 0.013).

Column 6 estimates (1) 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 predict changes in democracy). For this reason, we now include five lags of both democracy and log GDP per capita in these annual regressions. Column 6 in both tables reports the p-value of an F-test for the joint significance of these variables. There is no evidence of a significant positive effect of income on democracy either with the Freedom House or the Polity data (while democracy continues to be strongly predicted by its lags).

Columns 7 and 8 investigate the relationship between income and democracy at lower frequencies by estimating similar regressions using a dataset of ten-year observations. The results are similar to those with five-year observations and to the patterns in Figures 2 and 3, which show no evidence of a positive association between changes in income and democracy between 1970 and 1995. Finally, column 9 in both tables presents a fixed effect regression using a smaller dataset consisting of twenty-year observations. Once again, there is no evidence of a positive effect of income on democracy.

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

4.2 Robustness

Table 4 investigates the robustness of these results. 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 1-4 examine alternative samples. Columns 1 and 2 show the regressions corresponding to columns 2 and 4 of Table 2 for a balanced sample of countries from 1970 to 2000. This is useful to check whether entry and exit of countries from the base sample of Tables 2 and 3 might be affecting the results. Both columns

provide very similar results. For example, using the balanced sample of Freedom House data and the fixed effects OLS specification, the estimate of γ is -0.031 (standard error= 0.049). Columns 3 and 4 exclude former socialist countries, again with very similar results.

Columns 5-10 investigate the influence of various covariates on the relationship between income and democracy. Columns 5 and 6 include log population and age structure, and columns 7 and 8 add education. Columns 9 and 10 include the full set of covariates from Barro's (1999) baseline specification.[12] In each case, we present both fixed effects and GMM estimates. The results show that these covariates do not affect the (lack of) relationship between income and democracy when fixed effects are included. Age structure variables are significant in the specification that excludes education, but not when education is included. Education is itself insignificant with a negative coefficient. The causal effect of education on democracy, which is another basic tenet of the modernization hypothesis, is therefore also not robust to controlling for country fixed effects.

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. We also checked and found no evidence of an effect of the volatility in the growth rate of income per capita on democracy.[13]

5 Instrumental Variable Estimates

As discussed in Section 3, fixed effects estimators do not necessarily identify the causal effect of income on democracy. The estimation of causal effects requires exogenous sources of variation. While we do not have an ideal source of exogenous variation, there are two

[12] Age structure variables are from United Nations Population Division (2003) and include median age and variables corresponding to the fraction of the population in the following four age groups: 0-15, 15-30, 30-45, and 45-60. Total population is from World Bank (2002). In our regressions we measure education as total years of schooling in the population aged 25 and above. Columns 9 and 10 add covariates from Barro (1999), the urbanization rate and the male-female education gap. For consistency, these columns also follow Barro's strategy by measuring education as primary years of schooling in the population aged 25 and above. Both education variables are from Barro and Lee (2000). For detailed definitions and sources see Appendix Table A1.

[13] We also investigated the effect of growth accelerations using a definition similar to that in the recent paper by Hausmann, Pritchett and Rodrik (2005) and found no effect of growth accelerations on democracy. Interestingly, however, the incidence of crises are correlated with democracy once fixed effects are taken into account.

The 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 holds only 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.

promising potential instruments and we now present IV results using these.

5.1 Savings Rate Instrument

The first instrument is the savings rate in the previous five-year period, denoted by s_{it}. The corresponding first stage for log income per capita, y_{it-1}, in regression (1) is

$$y_{it-1} = \pi^F s_{it-2} + \alpha^F d_{it-1} + \mathbf{x}'_{it-1}\boldsymbol{\beta}^F + \mu^F_{t-1} + \delta^F_i + u^F_{it-1}, \tag{3}$$

where all the variables are defined in Section 3 and the only excluded instrument is s_{it-2}. The identification restriction is that $\text{Cov}(s_{it-2}, u_{it} \mid \mathbf{x}_{it-1}, \mu_t, \delta_i) = 0$, where u_{it} is the residual error term in the second-stage regression, (1).

We naturally expect the savings rate to influence income in the future. What about excludability? While we do not have a precise theory for why the savings rate should have no direct effect on democracy, it seems plausible to expect that changes in the savings rate over periods of 5-10 years should have no direct effect on the culture of democracy, the structure of political institutions or the nature of political conflict within society.

Nevertheless, there are a number of channels through which savings rates could be correlated with the error term in the second-stage equation, u_{it}. First, the savings rate itself might be influenced by the current political regime, for example, d_{it-2}, and could be correlated with u_{it} if all the necessary lags of democracy are not included in the system. Second, the savings rate could be correlated with changes in the distribution of income or composition of assets, which might have direct effects on political equilibria. Below, we provide evidence that these concerns are unlikely to be important in practice.

With these caveats in mind, Table 5 looks at the effect of GDP per capita on democracy in IV regressions using past savings rates as instruments and using the Freedom House data (results using Polity data are similar and available upon request). The savings rate is defined as nominal income minus consumption minus government expenditure divided by nominal income.

We report a number of different specifications, with or without lagged of democracy on the right-hand side, and with or without GMM. The first three columns show the OLS estimates in the pooled cross section, the fixed effects estimates without lagged democracy on the right-hand side, and the fixed effects estimates with lagged democracy on the right-hand side. Without fixed effects, there is a strong association between income per capita and democracy (the relationship in column 1 is stronger than before because it does not include lagged democracy on the right-hand side). With fixed effects, this relationship is

no longer present. The remaining columns look at IV specifications and the bottom panel shows the corresponding first stages.

Column 4 shows a strong first-stage relationship between income and the savings rate, with a t-statistic of almost 5. The 2SLS estimate of the effect of income per capita on democracy is -0.035 (standard error = 0.094). The two standard error bands comfortably exclude the OLS estimate from column 1. Column 5 adds lagged democracy to the right-hand side. The first stage is very similar and the estimate of γ is now -0.020 (standard error = 0.081). Column 6 uses the GMM procedure, again with the savings rate as the excluded instrument for income. Now the estimate of γ is again negative, relatively large and significant at 5%. These IV results, therefore, show no evidence of a positive causal effect of income on democracy.

The remaining columns investigate the robustness of this finding and the plausibility of our exclusion restriction. Column 7 adds labor share as an additional regressor, to check whether a potential correlation between the savings rate and inequality might be responsible for our results.[14] The first stage shows no significant effect of labor share on income per capita, and the 2SLS estimate of γ is similar to the estimate without the labor share. Column 8 includes further lags of democracy to check whether systematic differences in savings rates between democracies and dictatorships might have an effect on the results. The estimate of γ is similar to before and, if anything, a little more negative in this case. Finally, column 9 adds a further lag of the savings rate as an instrument. This is useful since it enables a test of the overidentifying restriction (namely, a test of whether the savings rate at t-3 is a valid instrument conditional on the savings rate at t-2 being a valid instrument). The 2SLS estimate of γ is again similar and the overidentification restriction that the instruments are valid is accepted comfortably (at the p-value of 1.00).

5.2 Trade-Weighted World Income Instrument

Our second instrument exploits trade linkages across countries. To develop this instrument, let $\Omega = [\omega_{ij}]_{i,j}$ denote the $N \times N$ matrix of (time-invariant) trade shares between countries in our sample, where N is the total number of countries. More precisely, ω_{ij} is the share of trade between country i and country j in the GDP of country i which measure using trade shares between 1980-1989 (which is chosen to maximize coverage).[15]

[14] This is the labor share of gross value added from Rodrik (1999). We use these data rather than the standard Gini indices, because they are available for a larger sample of countries. The results with Gini coefficients are very similar and are available upon request.

[15] We obtain similar results if we use predicted average trade shares from a standard gravity equation as in Frankel and Romer (1999). See the previous version of the paper for details.

The transmission of business cycles from one country to another through trade (e.g., Baxter, 1995, Kraay and Ventura, 2001) implies that we can think of a statistical model for income of a country as follows:

$$Y_{it-1} = \zeta \sum_{j=1, j\neq i}^{N} \omega_{ij} Y_{jt-1} + \varepsilon_{it-1}, \tag{4}$$

for all $i = 1, ..., N$, where Y_{it-1} denotes log total income, so $y_{it-1} = Y_{it-1} - P_{it-1}$ where P_{it-1} is the log population of i at $t-1$. The parameter ζ measures the effect of the trade-weighted world income on the income of each country.

Given equation (4), the identification problem in the estimation of (1) can be restated as follows: the error term ε_{it-1} in (4) is potentially correlated with u_{it} in equation (1), and if so, the estimates of the effect of income on democracy, γ, will be inconsistent. The idea of the approach in this section is to purge Y_{it-1}, and hence y_{it-1}, from ε_{it-1} to achieve consistent estimation of γ. For this purpose, we construct

$$\widehat{Y}_{it-1} = \sum_{j=1, j\neq i}^{N} \omega_{ij} Y_{jt-1}, \tag{5}$$

to use as an instrument for y_{it-1}. Here $\widehat{Y}_{it-1}$ is a weighted sum of world income for each country, with weights varying across countries depending on their trade pattern. Given $\widehat{Y}_{it-1}$, we can consider a model for income per capita of the form:
$y_{it-1} = \tilde{\pi}^F \widehat{Y}_{it-1} + \alpha^F d_{it-1} + \mathbf{x}'_{it-1}\boldsymbol{\beta}^F + \mu^F_{t-1} + \delta^F_i + u^F_{it-1}$. Substituting for (5), we obtain our first-stage relationship:

$$y_{it-1} = \pi^F \sum_{j=1, j\neq i}^{N} \omega_{ij} Y_{jt-1} + \alpha^F d_{it-1} + \mathbf{x}'_{it-1}\boldsymbol{\beta}^F + \mu^F_{t-1} + \delta^F_i + u^F_{it-1}, \tag{6}$$

where the parameter π^F corresponds to $\zeta\tilde{\pi}^F$ (we do not need separate estimates of ζ and $\tilde{\pi}^F$). The identification assumption for this strategy is that $\widehat{Y}_{it-1}$ is orthogonal to u_{it}. A sufficient condition for this is for Y_{jt-1} to be orthogonal to u_{it} for all $j \neq i$.

There may be reasons for this identification assumption to be violated. For example, Y_{jt-1} may be correlated with democracy in country j at time t, d_{jt}, which may influence d_{it} through other, political, social or cultural channels.[16] Although we have no way of ruling out these channels of influence a priori, below we control for the direct effect of the

[16] Because ω_{ij} is time-invariant, it does not capture *changes* in trade patterns and in trade agreements, which could possibly have a direct effect on democracy.

democracy of trading partners and find no evidence to support such a channel.[17]

The main results using the Freedom House data are presented in Table 6 (results using Polity data are similar and available upon request). In the bottom panel we report the first-stage relationships. The first three columns again report OLS regressions with and without fixed effects; the basic patterns are similar to those presented before. Column 4 shows our basic 2SLS estimate with the trade-weighted instrument. The instrument is constructed as in (5) using the average trade shares between 1980 and 1989. The bottom panel shows a strong first-stage relationship with a t-statistic of almost 5. The 2SLS estimate of γ is -0.213 (standard error= 0.150). When we add lag democracy in column 5, the estimate is slightly less negative and more precise, -0.120 (standard error = 0.105), and becomes a little more precise with GMM in column 6, -0.133 (standard error = 0.077).

Column 7 investigates whether the democracy of trading partners of country j might have a direct effect on d_{jt}. We construct a world democracy index, $\tilde{d}_{it}$ using the same trade shares as in equation (5) and include this both in the first and second stages. This democracy index, $\tilde{d}_{it}$, also varies across countries because of the differences in weights. We find that $\tilde{d}_{it}$ has no effect either in the first or the second stages, consistent with our identification assumption that $\widehat{Y}_{it-1}$ should have no effect on democracy in country i except through its influence on y_{it-1}. Column 8 uses Y_{jt-2} instead of Y_{jt-1} on the right-hand side of (5) as an alternative strategy. Finally, column 9 performs an overidentification test similar to that in column 9 in Table 5 by including both the instrument constructed using Y_{jt-2} and the instrument constructed using Y_{jt-1}. The estimate of γ is similar to the baseline estimate in column 4 and the overidentifying restriction that the twice-lagged instrument is valid conditional on the first instrument being valid is again accepted comfortably (at the p-value of 1.00).

Overall, our two IV strategies give results consistent with the fixed effects estimates and indicate that there is no evidence for a strong causal effect of income on democracy.[18]

[17] There is an econometric problem arising from the general equilibrium nature of equation (4). Since this equation also applies for country j, the disturbance term ε_{it-1}, which determines Y_{it-1}, will be correlated with Y_{jt-1}, inducing a correlation between Y_{jt-1} and ε_{it-1}, and thus between $\widehat{Y}_{it-1}$ and ε_{it-1}. However, under some regularity conditions, the problem disappears as $N \to \infty$. In exercises included in the previous version of our paper, we have estimated ζ adjusting for potential bias and found no change in our results. Details available upon request.

[18] We also performed overidentification tests using the savings rate as the base instrument and trade-weighted income as the additional instrument and the χ^2-statistic for a Hausman test takes the value of 1.63 which is accepted at the p-value of 1.00. The reverse procedure with trade-weighted income as the base instrument yields a χ^2-statistic for a Hausman test takes the value of 1.97 which is accepted at the p-value of 0.99.

6 Fixed Effects Estimates Over 100 Years

We have so far followed much of the existing literature in focusing on the post-war period, where the democracy and income data are of higher quality. Nevertheless, it is important to investigate whether there may be an effect of income on democracy at longer horizons.

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 per-capita for many countries during this period. To investigate longer-term relationship between income and democracy, we construct a 25-year dataset starting in 1875.[19] This dataset contains a balanced panel of 25 countries for which democracy, lagged democracy (calculated 25 years earlier), and lagged income (calculated 25 years earlier) are available for every 25th year between 1875 and 2000.[20] We also construct a larger dataset with 50-year observations that starts in 1900. This dataset contains a larger sample of 37 independent countries.[21]

Table 7 presents the basic fixed effects results with these two samples. The specifications of columns 1-4 in Table 7 are identical to the specifications of columns 1, 2, 4, and 5 of Table 2, but it uses the 25-year valid sample over 1875-2000 with the Polity index as the dependent variable. These results are very similar to those from the post-war panel presented in Tables 2-4. For example, without fixed effects, the coefficient on income per capita is positive and significant at 0.116 (standard error=0.034), and with fixed effects the coefficient has the wrong sign and is insignificant at -0.020 (0.093). Column 5 reports the baseline regression on a smaller sample excluding all countries with imputed income estimates (see footnote 19). The results are very similar to column 1. Columns 6-10 repeat the same regressions using the data in 50-year intervals from 1900 to 2000, again with similar results. Once fixed effect are included, the coefficient on income is small and insignificant. Figure 4 depicts these results graphically and shows that there is little

[19] Since Maddison reports income estimates for 1820, 1870 and 1929, we assign income per capita from 1820 to 1850, income per capita in 1870 to 1875, and income per capita in 1929 to 1925. All of our results are robust to dropping the 1875 observation so as to not use the 1850 estimate of income per capita as the value of lag income. If income per capita is not available for a particular country-year pair, it is estimated at the lowest aggregation level at which Maddison's data are available (e.g., Costa Rica, Guatemala, and Honduras are assigned the same income per capita in 1850) and the standard errors are computed by clustering at the highest aggregation level assigned to a particular country.

[20] The countries included in this dataset are Argentina, Austria, Belgium, Brazil, Chile, China, Colombia, Costa Rica, Denmark, El Salvador, Greece, Guatemala, Honduras, Mexico, Netherlands, Nicaragua, Norway, Sweden, Switzerland, Thailand, Turkey, United Kingdom, United States, Uruguay, and Venezuela.

[21] In addition to the countries in the 25 year sample, this sample includes Bolivia, Dominican Republic, Ecuador, France, Haiti, Iran, Liberia, Nepal, Oman, Paraguay, Portugal, and Spain.

relationship between changes in democracy and income in this 100-year sample.

As emphasized in Section 3, these results do not necessarily correspond to the causal effect of income on democracy, since there may be omitted time-varying covariates.[22] Nevertheless, most plausible omitted variables (as well as potential reverse causality) would bias these estimates upwards, so it is safe to conclude that there is no evidence of causal effect of income on democracy over the past 100 years.

7 Sources of Income-Democracy Correlations

The results presented so far show no evidence of a causal effect of income on democracy. Nevertheless, there is a strong positive association between income and democracy today as shown in Figure 1. Since 500 years ago most (or all) societies were nondemocratic and exhibited relatively small differences in income, this current-day correlation suggests that over the past 500 years societies that have grown faster have also become democratic. We now investigate why this may have been and how to reconcile this 500-year pattern with our econometric results. We start with a variation on the econometric model presented in Section 3 to motivate our theoretical approach and empirical work.

7.1 Divergent Development Paths

We first extend the econometric model introduced in Section 3 and use it to clarify the notions of *divergent development paths* and *critical junctures*. Consider a simplified version of (1), without the lagged dependent variable and the other covariates and with contemporaneous income per capita on the right-hand side:

$$d_{it} = \gamma y_{it} + \delta_i^d + u_{it}^d \tag{7}$$

Moreover suppose that the statistical process for income per capita is

$$y_{it} = \delta_i^y + u_{it}^y. \tag{8}$$

The parameter γ again represents the causal effect of income on democracy, while δ_i^d and δ_i^y correspond to fixed differences in levels of democracy and income across countries. These fixed differences have so far been taken out by country fixed effects.[23]

[22] We also looked at IV regressions on this sample using a version of trade-weighted income constructed as in Section 5. However, in this smaller sample of countries, the first-stage relationship was not strong enough to allow the estimation of meaningful second stage regressions.

[23] Allowing democracy to influence income in equation (8) does not change the conclusions as long as the effect is nonnegative.

Imagine we have data for two time periods, $t = T - S$ and $t = T$. Time-differencing equations (7) and (8), we obtain:

$$d_{iT} - d_{iT-S} = \gamma \left(y_{iT} - y_{iT-S} \right) + u^d_{iT} - u^d_{iT-S}, \tag{9}$$

and

$$y_{iT} - y_{iT-S} = u^y_{iT} - u^y_{iT-S}.$$

Consider the fixed effects estimator $\hat{\gamma}^{FE}_S$ using only these two data points, where the time span is given by S. Standard arguments imply that the probability limit of this estimator using these two data points is:

$$\text{plim}\hat{\gamma}^{FE}_S = \gamma + \frac{\text{Cov}\left(u^d_{iT} - u^d_{iT-S}, u^y_{iT} - u^y_{iT-S} \right)}{\text{Var}\left(u^y_{iT} - u^y_{iT-S} \right)}. \tag{10}$$

Therefore, estimation of (9) would yield a consistent estimate of the effect of income on democracy only if $\text{Cov}\big(u^d_{iT} - u^d_{iT-S}, u^y_{iT} - u^y_{iT-S} \big) = 0$, that is, only if changes in income over the relevant time horizon are not correlated with changes in democracy through a third common factor.

The condition $\text{Cov}\big(u^d_{iT} - u^d_{iT-S}, u^y_{iT} - u^y_{iT-S} \big) = 0$ is restrictive, especially over long horizons. The presence of divergent political-economic development paths across countries implies that this covariance is likely to be positive. Intuitively, divergent development paths refer to processes of development whereby political and economic outcomes evolve jointly. This joint evolution implies that u^d_{it} and u^y_{it} are not orthogonal and that $\text{Cov}\big(u^d_{iT} - u^d_{iT-S}, u^y_{iT} - u^y_{iT-S} \big) \neq 0$.

As an example, let us contrast the development experience of the United States with that of Peru and Bolivia. The United States grew rapidly during the late 18th and 19th centuries and became gradually more democratic, while these Andean societies stagnated and did not show a tendency to become democratic. Nondemocracy and stagnation in the Andes cannot be separated; the hacienda system, based on labor repression and the control of the indigenous Indian communities, was not conducive to industrialization and rapid growth during the 19th century. This system and its continuation, even after the abolition of formal systems of Indian tribute and forced labor, were not consistent with democratic institutions and a relatively equal distribution of political power within the society. This contrasts with the small-holder society in the United States, which resulted from the process of European colonization based on settlements in relatively empty and healthy lands. This social structure dominated by small-holders was much more consistent

with democratic representation.[24] Democratic representation was in turn conducive to an environment where new industries and new entrepreneurs could flourish with relatively little resistance from established interests.[25] This description suggests that beyond the impact of income on democracy or the impact of democracy on income, we may want to think of political and economic development taking place jointly.[26] These ideas in general and the contrast between Northeast United States and the Andes in particular are captured by our notion of divergent development paths.

This description naturally leads to the question of what determines whether a country embarks upon a specific development path and brings us to the notion of *critical juncture*. The colonization strategies brought about by the Europeans, ranging from the settler societies of Northeast United States to the repressive economies of the Andes, were clearly important for the kind of development paths these societies embarked upon. In this sense, we can think of the early stages of the colonization process as the critical juncture for these development paths.

In summary, the simple conceptual framework proposed here is one in which income and democracy evolvejointly. The development path a society embarks upon is partly influenced by its experience during certain critical junctures, which might include the early stages of colonization for former colonies, the aftermath of independence or the founding of a nation, the epoch of the collapse of feudalism for Western European nations, the age of industrialization, i.e., the 19th century, and the periods of significant ideological shocks such as the Reformation, the Enlightenment or the rise of Islam.

These ideas can be incorporated into the econometric model above in a simple way. Suppose that the critical juncture (for example, the early dates of European colonization) is denoted by T^*, and for notational simplicity, suppose that this is a single common date for all countries. Suppose moreover that each of the stochastic terms in (7) and (8), u_{it}^d

[24] See Galenson (1996) and Keyssar (2000) on the development of Northeastern United States as a settler colony, with a relatively democratic and open institutions. See Lockhart (1968) and Jacobsen (1993) for the creation and persistence of colonial practices in Peru, and see Klein (1992) on Bolivia. For a contrast of these development paths, see, among others, Engerman and Sokoloff (1997) and Acemoglu, Johnson and Robinson (2001,2002).

[25] Sokoloff and Kahn (1990) and Kahn and Sokoloff (1993) show that many of the major U.S. inventors in the 19th century were not members of the already-established economic elite, but newcomers with diverse backgrounds.

[26] Examples of models in which democracy and economic outcomes are jointly determined include Acemoglu and Robinson (2000), Acemoglu (2003), Cervellati, Fortunato, and Sunde (2005), and Llavador and Oxoby (2005).

and u_{it}^{y}, admits a unit-root representation:

$$\begin{aligned} u_{it}^{d} &= \eta_{it}^{d} + \xi_{it}^{d} \text{ and } u_{it}^{y} = \eta_{it}^{y} + \xi_{it}^{y} \\ \text{where } \eta_{it}^{d} &= \eta_{it-1}^{d} + \upsilon_{it}^{d} \text{ and } \eta_{it}^{y} = \eta_{it-1}^{y} + \upsilon_{it}^{y}, \end{aligned}$$

with $E\left(\xi_{it}^{d}\right) = E\left(\xi_{it}^{y}\right) = E\left(\upsilon_{it}^{d}\right) = E\left(\upsilon_{it}^{y}\right) = 0$. Let the variances of υ_{i}^{y} and ξ_{i}^{y} be denoted by $\sigma_{\upsilon^{y}}^{2}$ and $\sigma_{\xi^{y}}^{2}$, and assume that the ξ's are independent of the υ's. Moreover, let $\mathrm{Cov}\left(\xi_{it}^{d}, \xi_{it+k}^{y}\right) = 0$, $\mathrm{Cov}\left(\upsilon_{it}^{y}, \upsilon_{it+k}^{y}\right) = 0$, and $\mathrm{Cov}\left(\upsilon_{it}^{d}, \upsilon_{it+k}^{d}\right) = 0$ for all i and $k \neq 0$. Given this formulation, our emphasis on political and economic development paths diverging at some critical juncture corresponds to large and correlated shocks υ_{it}^{d} and υ_{it}^{y} at some $t = T^{*}$, which will then have a persistent effect on democracy and prosperity because of the unit root in η_{it}^{d} and η_{it}^{y}. To capture this, let $\mathrm{Cov}\left(\upsilon_{iT^{*}}^{d}, \upsilon_{iT^{*}}^{y}\right) = \sigma_{T^{*}}^{2}$ be positive and large (i.e., $\sigma_{T^{*}}^{2} >> 0$), corresponding to the importance of a major event affecting both economic and political outcomes at this critical juncture. In contrast with the pattern during critical junctures, we have that $\mathrm{Cov}\left(\upsilon_{it}^{d}, \upsilon_{it}^{y}\right) = \sigma_{\sim T^{*}}^{2}$ for $t \neq T^{*}$, which we presume to be positive but small (i.e., $\sigma_{\sim T^{*}}^{2} \geq 0$ but $\sigma_{\sim T^{*}}^{2} \simeq 0$). Suppose also that $\mathrm{Cov}\left(\upsilon_{it}^{d}, \upsilon_{it+k}^{y}\right) = 0$ for all i and $k \neq 0$. With this additional structure, equation (10) implies the following probability limit for the fixed effect estimator $\hat{\gamma}_{S}^{FE}$,

$$\mathrm{plim}\hat{\gamma}_{S}^{FE} = \begin{cases} \gamma + \frac{\sigma_{\sim T^{*}}^{2}}{\sigma_{\upsilon^{y}}^{2} + 2\sigma_{u^{y}}^{2}/S} & \text{if } T^{*} \notin [T-S, T] \\ \gamma + \frac{\left(\sigma_{T^{*}}^{2} - \sigma_{\sim T^{*}}^{2}\right)/S + \sigma_{\sim T^{*}}^{2}}{\sigma_{\upsilon^{y}}^{2} + 2\sigma_{u^{y}}^{2}/S} & \text{if } T^{*} \in [T-S, T] \end{cases}, \tag{11}$$

where the second equality exploits the fact that υ_{i}'s and u_{i}'s are serially uncorrelated.

Equation (11) emphasizes that the bias of $\hat{\gamma}_{S}^{FE}$ will crucially depend on whether or not the critical juncture T^{*} takes place between the dates $T-S$ and T. If it does not do so, the first-term applies and to the extent that $\sigma_{\sim T^{*}}^{2} \simeq 0$, the estimator will be "approximately" consistent. Note, however, that as S increases, the denominator falls, so the potential bias in this estimator may increase when $\sigma_{\sim T^{*}}^{2} > 0$. Nevertheless, by and large, the fixed effects estimator will be approximately consistent when the critical juncture does not take place during the sample period. This is the reason why we have some confidence in the results obtained using the fixed effects regressions in the postwar and 20th century samples.

However, as the second line in (11) illustrates, when the critical juncture T^{*} is in our sample, the estimate of γ will be more severely inconsistent, since $\sigma_{T^{*}}^{2} >> 0$. This observation may be relevant in interpreting why we may see a positive relationship between these two variables during the past 500 years, where many major events affecting the ultimate development path of various societies have taken place, but not during the postwar era or the entire 20th century.

Equation (11) also suggests an empirical methodology for checking whether events during the critical junctures might indeed be responsible for the cross-country correlation between income and democracy that we observe today. If we can control for variables correlated with the common component in $\upsilon^d_{iT^*}$ and $\upsilon^y_{iT^*}$ (in practice, historical determinants of divergent development paths) while estimating (9), the positive association between changes in income and democracy should weaken significantly or disappear. We investigate this issue in the next subsection.

7.2 Income and Democracy over the Past 500 Years

As discussed above, the current cross-country correlation between income and democracy likely reflects the changes in income and democracy over the past 500 years. We now investigate this relationship and interpret it in the light of the econometric framework introduced in the previous subsection. The major hurdle against an analysis of the relationship between income and democracy over long horizons is the availability of data. Nevertheless, there exist rough estimates of income per capita for almost all areas of the world in 1500. Moreover, we also have information about the variation in political institutions around the turn of the 16th century. While no country was fully "democratic" according to current definitions, there were certain notable differences in the political institutions of countries around the world even at this date. In particular, most countries outside Europe were ruled by absolutist regimes while some European countries had developed certain constraints on the behavior of their monarchs.

Acemoglu, Johnson and Robinson (2005) provide a coding of constraint on the executive for European countries going back to 1500 (based on the Polity definition). Constraint on the executive is a key input to the Polity democracy score for European countries. In addition, it seems reasonable that constraint on the executive for non-European countries and the other components of the Polity index (competitiveness of executive recruitment, openness 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 construct estimates for the Polity Composite index for 1500 (details available upon request). We combine these data with Maddison's (2003) estimates of income per capita in 1500 and 2000 and Polity's democracy score for 2000.[27]

We first check whether the current income-democracy correlation is indeed caused by changes over the past 500 years by estimating (9) over this sample period. Table 8

[27] Countries that have become independent in the 1990s are excluded from the sample. If the Polity score for 2000 is missing we assign the 1995 score to the observation.

column 1 provides estimates for our entire world sample and column 5 focuses on the sample of former European colonies, which will allow us to better control for potential determinants of divergent development paths. As conjectured above, in both samples, the coefficient on income is large and significant. For example, in this 500-year sample, the coefficient on change in income in the entire world sample is 0.134 (standard error=0.021) and for the former colonies sample, it is 0.136 (standard error=0.019). Figure 5 depicts the association between the changes in democracy and changes in income over the past 500 years for the entire world sample. These results suggest that the current cross-country correlation between income and democracy is indeed accounted for by the developments over the past 500 years.

We next investigate how the inclusion of proxies for the divergent development paths affects this relationship. For the entire world sample, we use two sets of proxies. The first set of variables include a measure of early political institutions, constraint on the executive at independence from Polity IV, and the independence year. Since the date of independence is a possible critical juncture for most countries, a direct measure of institutions immediately after the end of the colonial period (for former colonies) or at the date of national independence (for non-colonies) is a useful proxy for the nature of the development paths that these societies have embarked upon. This variable is constructed as the average score of constraint on the executive from Polity IV during the first ten years after independence. We again normalize this score to a 0 to 1 scale like democracy, with 1 representing the highest constraint on the executive. It is useful to control for date of independence as well, since this is also related to the development paths that societies may have embarked upon (with early independence more indicative of a pro-growth and pro-democracy development path). Moreover, constraint on the executive at the date of independence would not be comparable across countries if we did not control for date of independence, since the meaning of this constraint likely varies over time.[28]

Column 2 of Table 8 includes constraint on the executive at independence and independence year in the regression for our entire world sample. The coefficient on income is reduced from 0.134 (standard error=0.021) to 0.061 (standard error=0.023), and higher values of constraint on the executive at independence and earlier dates of independence significantly predict greater changes in democracy over the past 500 years (conditional on the change in income). The coefficient on the change in income is still significant in

[28] Data on date of independence are from the CIA World Factbook (2004). For detailed data definitions and sources see Appendix Table A1. The data on constraint on the executive from Polity begins in 1800 or at the date of independence. Countries independent prior to 1800 are coded as being independent in 1800.

this regression, perhaps in part because constraint on the executive at independence and independence year are very crude proxies for the divergent development paths of nations.

For this reason, we look for additional potential determinants of development paths. An important candidate suggested in the literature is religion. Citing the experience of England as the primary example, Weber (1930) argued that the Protestant ethic was responsible for the development of an institutional structure conducive to the rise of democracy and capitalism. Other arguments pointing to religion as an important determinant of political and economic development have been articulated by Huntington (1991) and Fish (2002), who emphasize the importance of Islam as an institutional barrier to the economic and political development.

In column 3 we present estimates including the fractions of different religions (in particular, fractions of Protestants, Catholics, and Muslims in the population).[29] The coefficient on income is again reduced, now to 0.088 (standard error=0.020), and religious fractions are individually significant at the 10% level with the fraction Muslim being most significant and negative at -0.233 (standard error=0.083). Column 4 combines the religion variables with the proxies of early institutions and date of independence. Now there is a more substantial drop in the estimate of the effect of change in income on the change in democracy, to 0.047 (standard error=0.023), which is just significant at 5%. Figure 6 illustrates the significant weakening in the relationship between changes in income and democracy once we control for historical factors affecting divergent development paths. It depicts the residual plot of the regression in Table 8, column 4. It shows that the inclusion of historical factors significantly reduces the upward sloping relationship apparent in Figue 5. Recall also that this estimate is likely to be an upper bound on the effect of changes in income on changes in democracy over the past 500 years, since our historical measures are only crude proxies for the determinants of divergent development paths.

Although the change in income continues to be significant in this regression, the magnitude of the effect is very small. If the coefficient of 0.047 represented the causal effect of income on democracy, it would imply that an "average" dictatorship in 1500 with income per capita of $566 (average of world income in 1500 in 1990 Geary-Khamis dollars) would need to reach a per capita income of $984 billion to become democratic![30]

[29] Data from La Porta, Lopez-de-Silanes, Shleifer, and Vishny (1999)

[30] This follows since, given the estimates in column 4, a change from a score of democracy of 0 to 1 would require an increase in log GDP per capita of 1/0.047. This translates into a exp(1/0.047)-fold (i.e., $\simeq$ 1.7.billion-fold!) increase in GDP per capita starting from $566, which leads to an income per capita of $984 billion. In contrast, the coefficient of 0.134 in column 1 implies that a substantially smaller (though still large) 1742-fold increase in income per capita is necessary for a society to move from a democracy score of 0 to 1.

The rest of Table 8 turns to the former colonies sample. The advantage of this sample is that we have a better understanding of the factors that have shaped the divergent development path during critical junctures. In particular, Acemoglu, Johnson and Robinson (2002) document that former colonies with high rates of indigenous population density in 1500 have experienced greater extraction of resources and repression by Europeans, and consequently have been more likely to embark on a development path leading to relative stagnation and nondemocracy. They also provide estimates for population density of the indigenous population in 1500.[31] Motivated by this reasoning, we use the estimates of the size of the indigenous population in 1500 (population density in 1500, for short) as an additional proxy for factors determining the divergent development paths of nations.

Columns 5-8 are similar to columns 2-4, but refer to the former colonies. They show that the inclusion of constraints on the executive, independence year and religion variables weakens the 500-year correlation between changes in democracy and income, but a significant relationship still remains (and is in fact stronger than was the case for the entire world sample). Column 9 turns to the effect of population density in 1500 by including the log of the population density of the indigenous population. This variable is significant and has the expected sign. Moreover, its inclusion reduces the coefficient on the change in income per capita substantially. Column 10 shows that the inclusion of this variable together with constraint on the executive and date of independence is sufficient to remove the significant association between changes in income and democracy over the past 500 years entirely. Now the coefficient on the change in income per capita, which was originally equal to 0.136 (standard error=0.019), is reduced to 0.025 (standard error=0.024), which is highly insignificant. Moreover, constraint on the executive, independence year, and population density in 1500 are each individually significant. For example, the coefficient on population density in 1500 is -0.059 (0.021). Therefore, in this sample, there is no evidence that changes in income causes changes in democracy once we condition on certain proxies for divergent development paths of former colonies. Finally, column 11 includes the religion variables as well and again, the coefficient on income is low and insignificant at 0.029 (standard error=0.026)

Overall, these results are encouraging for our hypothesis, since they indicate that once reasonable proxies for the divergent development paths are included, the 500-year correlation between changes in income and democracy disappears and the cross-country correlation between income and democracy can be largely accounted by these divergent

[31] 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).

development paths.

8 Conclusion

The conventional wisdom in the political economy literature is that income per capita has a causal effect on democracy. In this paper, we argue that, though income and democracy are positively correlated, there is no evidence of a causal effect. 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. Consequently, regressions that include country fixed effects and/or instrumental variable regressions show no evidence of a causal effect of income on democracy over the postwar era or the past 100 years. These results shed considerable doubt on the conventional wisdom both in the academic literature and in the popular press that income per capita is a key determinant of democracy and that a general increase in income per capita will bring improvements in institutions.

These results raise the question of why there is a positive cross-country correlation between income and democracy today We provided evidence that this is likely to be because the political and economic development paths are interwoven. Some countries appear to have embarked upon a development path associated with democracy and economic growth, while others pursued a path based on dictatorship, repression and more limited growth. Consistent with this, we have showed that historical sources of variation in development paths are responsible for much of the statistical association between long-run economic and political changes.

In emphasizing the importance of historical development paths, we do not want to suggest that there is a historical determinism in political for economic institutions. The fixed effects in the regressions and the presence of divergent development paths create a tendency, but many other factors influence equilibrium political institutions.[32] The most important area for future research is a further investigation of the effect of these time-varying and human factors on the evolution of equilibrium political institutions.

[32] Many current factors could and in fact appear to influence democracy. In the previous versions of our paper, we showed how severe economic crises lead to the collapse of dictatorships, making democracy more likely. Jones and Olken (2006) show how deaths of autocratic leaders make subsequent democracy more likely.

9 Appendix

This Appendix addresses the construction of trade-weighted instrument used in Section 5. We first measure the matrix $\Omega = [\omega_{ij}]_{i,j}$ using actual trade shares between 1980 and 1989. These dates are chosen to maximize coverage. Bilateral trade data are from from the International Monetary Fund Direction of Trade Statistics (DoT) (2005) CD-ROM. Let X_{ijs} denote the total trade flow between i and j in year s, meaning the sum of exports from i to j and exports from j to i in year s. We calculate X_{ijs} for all country pairs in year s for which both flows from i to j and from j to i are available. These flows can be measured using either FOB exports from i to j or CIF imports by j from i. When both are available, we take the average, and otherwise we use whichever measure is available. All trade data are deflated into 1983 US dollars using the US CPI from International Financial Statistics (2004).

Let Y^*_{is} denote the total GDP of country i in year s in 1983 US dollars obtained from Heston, Summers, and Aten (2002), and $\mathcal{I}_{ij}$ be the number of years between 1980 and 1989 for which bilateral data between i and j are available. Our main measure of $\Omega = [\omega_{ij}]_{i,j}$ is:

$$\omega_{ij} = \frac{1}{\mathcal{I}_{ij}} \sum_{s=1980}^{1989} \left(\frac{X_{ijs}}{Y^*_{is}} \right),$$

where $X_{iis} = 0$ by definition.

Since we have an unbalanced panel, we construct our instrument defined in (5) as follows. Define $I_{jt-1} = \{0, 1\}$ as an indicator for Y_{jt-1} being available in the dataset. Then

$$\widehat{Y}_{it-1} = \zeta \left(\sum_{j=1, j \neq i}^{N} \omega_{ij} I_{jt-1} Y_{jt-1} \right) \left(\frac{\sum_{j=1, j \neq i}^{N} \omega_{ij}}{\sum_{j=1, j \neq i}^{N} I_{jt-1} \omega_{ij}} \right), \tag{12}$$

where Y_{jt-1} is log income as before. The third term in (12) ensures that the sum of the weights ω_{ij} are the same across time for a given country i, and this adjustment term is equal to 1 in a balanced panel. We measure trade-weighted democracy $\tilde{d}_{it}$ in an analogous fashion using (12), where we substitute d_{jt} for Y_{jt-1} and let I_{jt-1} now represent an indicator referring to the availability of the variable d_{jt}.

10 Bibliography

Acemoglu, Daron (2003) "The Form of Property Rights : Oligarchic Versus Democratic Societies" NBER Working Paper No. 10037.

Acemoglu, Daron and James A. Robinson (2000) "Why did the West Extend the Franchise? Democracy, Inequality and Growth in Historical Perspective," *Quarterly Journal of Economics*, CXV, 1167-1199.

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

Acemoglu, Daron, Simon Johnson and James A. Robinson (2001) "The Colonial Origins of Comparative Development: An Empirical Investigation," *American Economic Review*, December, 91, 1369-1401.

Acemoglu, Daron, Simon Johnson and James A. Robinson (2002) "Reversal of Fortune: Geography and Institutions in the Making of the Modern World Income Distribution," *Quarterly Journal of Economics*, 118, 1231-1294.

Acemoglu, Daron, Simon Johnson and James A. Robinson (2005) "Rise of Europe: Atlantic Trade, Institutional Change and Economic Growth" *American Economic Review*, 95, 546-579. .

Acemoglu, Daron, Simon Johnson, James A. Robinson, and Pierre Yared (2006) "Reevaluating the Modernization Hypothesis," Working Paper.

Anderson, Theodore W. and Cheng Hsiao (1982) "Formulation and Estimation of Dynamic Models using Panel Data," *Journal of Econometrics*, 18, 67-82.

Arellano, Manuel and Stephen R. Bond (1991) "Some Specification tests for Panel Data: Monte Carlo Evidence and an Application to Employment Equations," *Review of Economic Studies*, 58, 277-298.

Arellano, Manuel and Olympia Bover (1995) "Another Look at the Instrumental Variables Estimation of Error-Component Models," *Journal of Econometrics*, 68, 29-51.

Barro, Robert J. (1997) *The Determinants of Economic Growth: A Cross-Country Empirical Study*, Cambridge; MIT Press.

Barro, Robert J. (1999) "The Determinants of Democracy," *Journal of Political Economy*, 107, S158-S183.

Barro, Robert J. and Jong-Wha Lee (2000) "International Data on Educational Attainment: Updates and Implications," CID Working Paper #42.

Baxter, Marianne (1995) "International Trade and Business Cycles," in Gene M. Grossman and Kenneth Rogoff eds. *Handbook of International Economics*, Amsterdam;

North-Holland.

Bollen, Kenneth A. (1990) "Political Democracy: Conceptual and Measurement Traps," *Studies in Comparative International Development*, 25, 7-24.

Bollen, Kenneth A. (2001) "Cross-National Indicators of Liberal Democracy, 1950-1990," [Computer file]. 2nd ICPSR version. Chapel Hill, NC: University of North Carolina [producer], 1998. Inter-university Consortium for Political and Social Research [distributor].

Central Intelligence Agency (2004) *CIA World Factbook,* Website and Book. Washington, DC.

Cervellati, Matteo, Piergiuseppe Fortunato, and Uwe Sunde (2005) "Consensual and Conflictual Democratization," mimeo.

Dahl, Robert A. (1971) *Polyarchy: Participation and Opposition*, New Haven; Yale University Press.

Engerman, Stanley L. and Kenneth L. Sokoloff (1997) "Factor Endowments, Institutions, and Differential Paths of Growth among New World Economies," in Stephen H. Haber ed. *How Latin America Fell Behind*, Stanford; Stanford University Press.

Fish, Steven M. (2002) "Islam and Authoritarianism," *World Politics*, 55, 4-37.

Frankel, Jeffrey A. and David Romer (1999) "Does Trade Cause Growth?," *American Economic Review*, 89, 379-399.

Freedom House (2004) *Freedom in the World,* Website and Book. Washington, DC.

Galenson, David W. (1996) "The Settlement and Growth of the Colonies: Population, Labor and Economic Development," in Stanley L. Engerman and Robert E. Gallman eds. *The Cambridge Economic History of the United States,* Volume I, The Colonial Era, Cambridge University Press, New York.

Heston, Alan, Robert Summers, and Bettina Aten (2002) *Penn World Tables Version 6.1.* Center for International Comparisons at the University of Pennsylvania (CICUP).

Hausman, Jerry (1978) "Specification Tests in Econometrics," *Econometrica*, 46, 1251-1271.

Hausmann, Ricardo, Lant Pritchett and Dani Rodrik (2005) "Growth Accelerations," *Journal of Economic Growth*, 10, 303-329.

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

International Monetary Fund (2005) *Direction of Trade Statistics*, Database and

Browser CD-ROM. Washington, D.C.

International Monetary Fund (2004) *International Financial Statistics*, Database and Browser CD-ROM. Washington, D.C.

Jacobsen, Nils (1993) *Mirages of transition: the Peruvian altiplano, 1780-1930*, University of California Press; Berkeley.

Jones, Benjamin and Benjamin Olken (2006) "Do Leaders Matter?" *Quarterly Journal of Economics,* 120, 835-864.

Jones, Eric L. (1981) *The European Miracle: Environments, Economies, and Geopolitics in the History of Europe and Asia*, Cambridge University Press, New York.

Kahn, Zorina and Kenneth Sokoloff (1993) "Schemes of Practical Utility: Entrepreneurship and Innovation Among Great Inventors in the United States, 1790-1865" *Journal of Economic History*, 53, 289-307.

Keysser, Alexander (2000) *The Right to Vote: The Contested History of Democracy in the United States*, Basic Books; New York.

Klein, Herbert S. (1992) *Bolivia: the evolution of a multi-ethnic society*, 2nd ed. Oxford University Press; New York.

Kraay, Aart and Jaume Ventura (2001) "Comparative Advantage and the Cross-Section of Business Cycles," NBER Working Paper #8104.

La Porta, Rafael, Florencio Lopez-de-Silanes, Andrei Shleifer and Robert W. Vishny (1999) "The Quality of Government," *Journal of Law Economics and Organization*, 15, 222-279.

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

Llavador, Humberto and Robert J. Oxoby (2005) "Partisan Competition, Growth, and the Franchise," *Quarterly Journal of Economics*, 120, 1155-1189.

Lockhart, James (1968) *Spanish Peru, 1532-1560; a colonial society*, University of Wisconsin Press; Madison.

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

Maddison, Angus (2003) *The World Economy: Historical Statistics*, Development Centre of the Organisation for Economic Cooperation and Development, Paris, France.

Marshall, Monty G. and Keith Jaggers (2004) "Political Regime Characteristics and Transitions, 1800-2002," Polity IV Project. University of Maryland.

McEvedy, Colin and Richard Jones (1975) *Atlas of World Population History*, Facts on File, New York.

Miguel, Edward, Shanker Satyanath, and Ernest Sergenti (2004) "Economic Shocks and Civil Conflict: An Instrumental Variables Approach," *Journal of Political Economy*, 112, 725-753.

North, Douglass C. (1981) *Structure and Change in Economic History*, New York; W.W. Norton & Co.

North, Douglass C. and Robert P. Thomas (1973) *The Rise of the Western World: A New Economic History*, Cambridge University Press, Cambridge UK.

Papaioannou, Elias and Gregorios Siourounis (2006) "Economic and Social Factors Driving the Third Wave of Democratization" mimeo, London Business School.

Przeworski, Adam, Michael Alvarez, José A. Cheibub and Fernando Limongi (2000) *Democracy and Development: Political Institutions and material well-being in the world, 1950-1990*, Cambridge University Press, New York NY.

Przeworski, Adam and Fernando Limongi (1997) "Modernization: Theory and Facts," *World Politics*, 49, 155-183.

Rodrik, Dani (1999) "Democracies Pay Higher Wages," *Quarterly Journal of Economics*, 114, 707-738.

Rusechemeyer, Dietrich, John D. Stephens and Evelyn H. Stephens (1992) *Capitalist Development and Democracy*, University of Chicago Press; Chicago.

Sokoloff, Kenneth and Zorina Kahn (1990) "The Democratization of Invention during Early Industrialization: Evidence from the United States, 1790-1846" *Journal Economic History*, 50, 363-378.

United Nations Population Division (2003) *World Population Prospects: The 2002 Revision,* Disk. New York.

Weber, Max (1930) *The Protestant Ethic and the Spirit of Capitalism.* Allen and Unwin; London.

Wooldridge, Jeffery M. (2002) *Econometric Analysis of Cross Section and Panel Data*, MIT Press; Cambridge.

World Bank (2002) *World Development Indicators*, CD-ROM and Book. Washington, DC.

Table 1
Descriptive Statistics

	All countries	High Income Countries	Low Income Countries
	(1)	(2)	(3)
Panel A			
Freedom House Measure of Democracy$_t$	0.57	0.78	0.36
	(0.36)	(0.30)	(0.30)
Log GDP per Capita $_{t-1}$ (Chain Weighted 1996 Prices)	8.16	9.02	7.30
	(1.02)	(0.56)	(0.53)
Observations	945	473	472
Countries	150	93	98
Panel B			
Polity Measure of Democracy$_t$	0.57	0.79	0.36
	(0.38)	(0.31)	(0.31)
Observations	854	427	427
Countries	136	81	88
Panel C			
Log Population $_{t-1}$	9.10	9.13	9.07
	(1.54)	(1.56)	(1.52)
Education $_{t-1}$	4.57	6.62	2.52
	(2.86)	(2.36)	(1.53)
Observations	676	338	338
Countries	95	57	65
Panel D			
Savings Rate $_{t-2}$	0.17	0.22	0.11
	(0.13)	(0.10)	(0.14)
Observations	891	446	445
Countries	134	82	84
Panel E			
Trade-Weighted Log GDP $_{t-1}$	11.61	12.98	10.24
	(8.43)	(9.74)	(6.62)
Observations	895	448	447
Countries	124	75	85

Values are averages during sample period, with standard deviations in parentheses. Panel A refers to the sample in Table 1, column 1; Panel B refers to the sample in Table 2, column 1; Panel C refers to the sample in Table 4 column 7; Panel D refers to the sample in Table 5, column 3; Panel E refers to the sample in Table 6, column 3. Column 1 in each panel refers to the full sample and columns 2 and 3 split the sample in column 1 by the median income (from Penn World Tables 6.1) in the sample of column 1. The number of observations refers to the total number of observations in the unbalanced panel. The number of countries refers to the number of countries for which we use observations. Freedom House Measure of Democracy is the Political Rights Index, augmented following Barro (1999). Polity Measure of Democracy is Democracy Index minus Autocracy Index from Polity IV. GDP per capita in 1996 prices with PPP adjustment is from the Penn World Tables 6.1. Population is from the World Bank (2002). Education is average total years of schooling in the population aged 25 and over and is from Barro and Lee (2000). Nominal Savings Rate is from Penn World Tables 6.1 and is defined as nominal income minus consumption minus government expenditure divided by nominal income (not PPP). Trade-Weighted log GDP is constructed as in equation (5) using data from IMF Direction of Trade Statistics (2005) and Penn World Tables 6.1. For detailed definitions and sources, see Appendix Table A1.

Table 2
Fixed Effects Results using Freedom House Measure of Democracy

	Base Sample, 1960-2000								
	5-year data					Annual data	10-year data		20-year data
	Pooled OLS (1)	Fixed Effects OLS (2)	Anderson-Hsiao IV (3)	Arellano-Bond GMM (4)	Fixed Effects OLS (5)	Fixed Effects OLS (6)	Fixed Effects OLS (7)	Arellano-Bond GMM (8)	Fixed Effects OLS (9)
	Dependent Variable is Democracy								
Democracy $_{t-1}$	0.706	0.379	0.469	0.489		[0.00]	-0.025	0.226	-0.581
	(0.035)	(0.051)	(0.100)	(0.085)			(0.088)	(0.123)	(0.198)
Log GDP per Capita $_{t-1}$	0.072	0.010	-0.104	-0.129	0.054	[0.33]	0.053	-0.318	-0.030
	(0.010)	(0.035)	(0.107)	(0.076)	(0.046)		(0.066)	(0.180)	(0.156)
Hansen J Test				[0.26]				[0.07]	
AR(2) Test				[0.45]				[0.96]	
Implied Cumulative Effect of Income	0.245	0.016	-0.196	-0.252				-0.411	-0.019
	[0.00]	[0.76]	[0.33]	[0.09]				[0.09]	[0.85]
Observations	945	945	838	838	958	2895	457	338	192
Countries	150	150	127	127	150	148	127	118	118
R-squared	0.73	0.80			0.76	0.93	0.77		0.89

Pooled cross-sectional OLS regression in column 1, with robust standard errors clustered by country in parentheses. Fixed effects OLS regressions in columns 2, 5, 6, 7, and 9, with country dummies and robust standard errors clustered by country in parentheses. Implied cumulative effect of income represents the coefficient estimate of log GDP per Capita $_{t-1}$/(1-Democracy$_{t-1}$) and the p-value from a non-linear test of the significance of this coefficient is in brackets. Column 3 uses instrumental variables method of Anderson and Hsiao (1982), with clustered standard errors, and columns 4 and 8 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. Dependent variable is Freedom House Measure of Democracy. Base sample is an unbalanced panel, 1960-2000, with data at 5-year intervals, where the start date of the panel refers to the dependent variable (i.e., t=1960, so t-1=1955); column 6 uses annual data from the same sample; a country must be independent for 5 years before it enters the panel. Columns 7 and 8 use 10-year data from the same sample, where as before the start date of the panel refers to the dependent variable (i.e., t=1960, so t-1=1950); a country must be independent for 10 years before it enters the panel. Column 9 uses 20-year data from the same sample, where as before the start date of the panel refers to the dependent variable (i.e., t=1980, so t-1=1960); a country must be independent for 20 years before it enters the panel. In column 6, each right hand side variable has five annual lags; we report the p-value from an F-test for the joint significance of all 5 lags. For detailed data definitions and sources see Table 1 and Appendix Table A1.

Table 3

Fixed Effects Results using Polity Measure of Democracy

	Base Sample, 1960-2000								
	5-year data					Annual data	10-year data		20-year data
	Pooled OLS	Fixed Effects OLS	Anderson-Hsiao IV	Arellano-Bond GMM	Fixed Effects OLS	Fixed Effects OLS	Fixed Effects OLS	Arellano-Bond GMM	Fixed Effects OLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Dependent Variable is Democracy								
Democracy $_{t-1}$	0.749	0.449	0.582	0.590		[0.00]	0.060	0.309	-0.516
	(0.034)	(0.063)	(0.127)	(0.106)			(0.091)	(0.134)	(0.165)
Log GDP per Capita $_{t-1}$	0.053	-0.006	-0.413	-0.351	-0.011	[0.53]	0.007	-0.368	-0.126
	(0.010)	(0.039)	(0.163)	(0.127)	(0.055)		(0.070)	(0.190)	(0.164)
Hansen J Test				[0.03]				[0.01]	
AR(2) Test				[0.39]				[0.38]	
Implied Cumulative Effect of Income	0.211	-0.011	-0.988	-0.856			0.007	-0.533	-0.083
	[0.00]	[0.89]	[0.01]	[0.00]			[0.92]	[0.04]	[0.45]
Observations	854	854	747	747	880	3701	419	302	168
Countries	136	136	114	114	136	134	114	107	100
R-squared	0.77	0.82			0.77	0.96	0.77		0.87

Pooled cross-sectional OLS regression in column 1, with robust standard errors clustered by country in parentheses. Fixed effects OLS regressions in columns 2, 5, 6, 7, and 9, with country dummies and robust standard errors clustered by country in parentheses. Implied cumulative effect of income represents the coefficient estimate of log GDP per Capita $_{t-1}$/(1-Democracy$_{t-1}$) and the p-value from a non-linear test of the significance of this coefficient is in brackets. Column 3 uses instrumental variables method of Anderson and Hsiao (1982), with clustered standard errors, and columns 4 and 8 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. Dependent variable is Polity Measure of Democracy. Base sample is an unbalanced panel, 1960-2000, with data at 5-year intervals, where the start date of the panel refers to the dependent variable (i.e., t=1960, so t-1=1955); column 6 uses annual data from the same sample; a country must be independent for 5 years before it enters the panel. Columns 7 and 8 use 10-year data from the same sample, where as before the start date of the panel refers to the dependent variable (i.e., t=1960, so t-1=1950); a country must be independent for 10 years before it enters the panel. Column 9 uses 20-year data from the same sample, where as before the start date of the panel refers to the dependent variable (i.e., t=1980, so t-1=1960); a country must be independent for 20 years before it enters the panel. In column 6, each right hand side variable has five annual lags; we report the p-value from an F-test for the joint significance of all 5 lags. For detailed data definitions and sources see Table 1 and Appendix Table A1.

Table 4
Fixed Effects Results using Freedom House Measure of Democracy: Robustness Checks

	Balanced Panel, 1970-2000		Base Sample, 1960-2000, without Former Socialist Countries		Base Sample, 1960-2000					
	5-year data									
	Fixed Effects OLS (1)	Arellano-Bond GMM (2)	Fixed Effects OLS (3)	Arellano-Bond GMM (4)	Fixed Effects OLS (5)	Arellano-Bond GMM (6)	Fixed Effects OLS (7)	Arellano-Bond GMM (8)	Fixed Effects OLS (9)	Arellano-Bond GMM (10)
	Dependent Variable is Democracy									
Democracy $_{t-1}$	0.283	0.472	0.362	0.436	0.353	0.480	0.351	0.499	0.352	0.475
	(0.058)	(0.092)	(0.052)	(0.085)	(0.053)	(0.087)	(0.055)	(0.097)	(0.050)	(0.088)
Log GDP per Capita $_{t-1}$	-0.031	-0.262	0.005	-0.151	0.015	-0.008	-0.001	-0.121	-0.042	-0.126
	(0.049)	(0.128)	(0.035)	(0.078)	(0.041)	(0.139)	(0.049)	(0.182)	(0.045)	(0.130)
Log Population $_{t-1}$					-0.109	-0.001	-0.042	0.049	-0.070	-0.016
					(0.100)	(0.113)	(0.108)	(0.143)	(0.112)	(0.163)
Education $_{t-1}$							-0.007	-0.020	-0.006	-0.011
							(0.020)	(0.026)	(0.038)	(0.052)
Age Structure $_{t-1}$					[0.05]	[0.63]	[0.19]	[0.27]		
Barro (1999) Covariates	NO	NO	NO	NO	NO	NO	NO	NO	YES	YES
Hansen J Test		[0.40]		[0.34]		[0.08]		[0.15]		[0.25]
AR(2) Test		[0.73]		[0.49]		[0.43]		[0.88]		[0.75]
Implied Cumulative Effect of Income	-0.043	-0.496	0.008	-0.268	0.023	-0.015	-0.002	-0.242	-0.065	-0.240
	[0.53]	[0.03]	[0.89]	[0.05]	[0.72]	[0.96]	[0.98]	[0.50]	[0.35]	[0.33]
Observations	630	567	908	823	863	731	676	589	676	588
Countries	90	81	128	124	142	120	95	92	96	92
R-squared	0.80		0.79		0.80		0.77		0.76	

Fixed effects OLS regressions in columns 1, 3, 5, 7, and 9 with country dummies and robust standard errors clustered by country in parentheses. Columns 2, 4, 6, 8, and 10 use GMM of Arellano and Bond (1991), with robust standard errors; in this method we instrument for income using a double lag. Year dummies are included in all regressions. Implied cumulative effect of income represents the coefficient estimate of log GDP per Capita$_{t-1}$/(1-Democracy$_{t-1}$) and the p-value from a non-linear test of the significance of this coefficient is in brackets. Dependent variable is Freedom House Measure of Democracy. Base sample is an unbalanced panel, 1960-2000, with data at 5-year intervals in levels where the start date of the panel refers to the dependent variable (i.e., t=1960, so t-1=1955); a country must be independent for 5 years before it enters panel. Columns 1 and 2 use a balanced panel from 1970 to 2000. Columns 3 and 4 exclude Soviet bloc countries. Education is average years of total schooling in the population in columns 7 and 8. Education is average years of primary schooling in the population in columns 9 and 10. Columns 7-8 include but do not display the median age of the population at t-1 and 4 covariates corresponding to the percent of the population at t-1 in the following age groups: 0-15, 15-30, 30-45, and 45-60. The age structure F-test is gives the p-value for the joint significance of these variables. Columns 9 and 10 include but do not display additional covariates used by Barro (1999): male-female education gap and urbanization rate. For detailed data definitions and sources see Table 1 and Appendix Table A1.

Table 5

Fixed Effects Results using Freedom House Measure of Democracy: Two Stage Least Squares with Savings Rate Instrument

	Base Sample, 1960-2000								
	All Countries								
	Pooled OLS	Fixed Effects OLS	Fixed Effects OLS	Fixed Effects 2SLS	Fixed Effects 2SLS	Arellano-Bond GMM	Fixed Effects 2SLS	Fixed Effects 2SLS	Fixed Effects 2SLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A	*Dependent Variable is Democracy*								
Democracy $_{t-1}$			0.359		0.363	0.427		[0.00]	
			(0.054)		(0.056)	(0.100)			
Log GDP per Capita $_{t-1}$	0.233	0.044	0.009	-0.035	-0.020	-0.228	-0.036	-0.074	0.016
	(0.013)	(0.051)	(0.038)	(0.094)	(0.081)	(0.102)	(0.191)	(0.113)	(0.095)
Labor Share $_{t-1}$							0.250		
							(0.199)		
Panel B	*First Stage for Log GDP per Capita $_{t-1}$*								
Democracy $_{t-1}$					0.144			[0.24]	
					(0.066)				
Labor Share $_{t-1}$							0.329		
							(0.187)		
Savings Rate $_{t-2}$				1.356	1.343		1.202	1.173	1.022
				(0.277)	(0.270)		(0.315)	(0.254)	(0.218)
Savings Rate $_{t-3}$									0.720
									(0.182)
Hansen J Test						[0.34]			
AR(2) Test						[0.72]			
Implied Cumulative Effect of Income			0.014		-0.031	-0.398			
			[0.82]		[0.80]	[0.01]			
Observations	900	900	891	900	891	764	471	733	796
Countries	134	134	134	134	134	124	98	124	125
R-squared in First Stage				0.96	0.96		0.98	0.97	0.97

Pooled cross-sectional OLS regression in column 1, with robust standard errors clustered by country in parentheses. Fixed effects OLS regressions in columns 2 and 3 with country dummies and robust standard errors clustered by country in parentheses. Fixed effects 2SLS regressions in columns 4, 5, 7, 8, and 9 with country dummies and robust standard errors clustered by country in parentheses; first stage regressions are displayed in Panel B and include all second stage covariates (apart from income) on the right hand side with robust standard errors clustered by country in parentheses. GMM of Arellano-Bond in column 6 with robust standard errors; in this method we instrument for income in the first differenced equation with the first difference of the instrument. Year dummies are included in all regressions. Implied cumulative effect of income represents the coefficient estimate of log GDP per Capita$_{t-1}$/(1-Democracy$_{t-1}$) and the p-value from a non-linear test of the significance of this coefficient is in brackets. Dependent variable is Freedom House Measure of Democracy. Base sample 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); a country must be independent for 5 years before it enters the panel. In columns 4-9 instrument for Log GDP per Capita$_{t-1}$ with Savings Rate $_{t-2}$. Column 9 includes Savings Rate $_{t-3}$ as an additional instrument. Column 8 includes but does not display Democracy$_{t-1}$, Democracy$_{t-2}$, and Democracy$_{t-3}$; we report the p-value from an F-test for the joint significance of all 3 lags. For detailed data definitions and sources see Table 1 and Appendix Table A1.

Table 6

Fixed Effects Results using Freedom House Measure of Democracy: Two Stage Least Squares with Trade-Weighted World Income Instrument

	Base Sample, 1960-2000								
	All Countries								
	Pooled OLS	Fixed Effects OLS	Fixed Effects OLS	Fixed Effects 2SLS	Fixed Effects 2SLS	Arellano-Bond GMM	Fixed Effects 2SLS	Fixed Effects 2SLS	Fixed Effects 2SLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A	*Dependent Variable is Democracy*								
Democracy $_{t-1}$			0.376		0.393	0.478			
			(0.051)		(0.057)	(0.094)			
Log GDP per Capita $_{t-1}$	0.233	0.038	0.001	-0.213	-0.120	-0.133	-0.202	-0.198	-0.217
	(0.013)	(0.045)	(0.034)	(0.150)	(0.105)	(0.077)	(0.130)	(0.160)	(0.149)
Trade-Weighted Democracy $_{t}$							-0.137		
							(0.635)		
Panel B	*First Stage for Log GDP per Capita $_{t-1}$*								
Democracy $_{t-1}$					0.169				
					(0.063)				
Trade-Weighted Democracy $_{t}$							-1.195		
							(0.959)		
Trade-Weighted Log GDP $_{t-1}$				0.402	0.421		0.441		0.529
				(0.083)	(0.082)		(0.070)		(0.180)
Trade-Weighted Log GDP $_{t-2}$								0.341	-0.127
								(0.090)	(0.206)
Hansen J Test						[0.19]			
AR(2) Test						[0.50]			
Implied Cumulative Effect of Income			0.002		-0.198	-0.255			
			[0.98]		[0.28]	[0.07]			
Observations	906	906	895	906	895	812	906	906	906
Countries	124	124	124	124	124	122	124	124	124
R-squared in First Stage				0.95	0.96		0.95	0.95	0.95

Pooled cross-sectional OLS regression in column 1, with robust standard errors clustered by country in parentheses. Fixed effects OLS regressions in columns 2 and 3 with country dummies and robust standard errors clustered by country in parentheses. Fixed effects 2SLS regressions in columns 4, 5, 7, 8, and 9 with country dummies and robust standard errors clustered by country in parentheses; first stage regressions are displayed in Panel B and include all second stage covariates excluding income on the right hand side with robust standard errors clustered by country in parentheses. GMM of Arellano-Bond in column 6 with robust standard errors; in this method we instrument for income in the first differenced equation with the first difference of the instrument. Year dummies are included in all regressions. Implied cumulative effect of income represents the coefficient estimate of log GDP per Capita$_{t-1}$/(1-Democracy$_{t-1}$) and the p-value from a non-linear test of the significance of this coefficient is in brackets. Dependent variable is Freedom House Measure of Democracy. Bas sample 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); a country must be independent for 5 years before it enters the panel. Columns 4-8 instrument for Log GDP per Capita$_{t-1}$ with Trade-Weighted World Log GDP$_{t-1}$. Column 9 uses Trade-Weighted World Log GDP$_{t-2}$ as an additional instrument. For detailed data definitions and sources see Table 1 and Appendix Table A1. See Appendix for details on the construction of the instruments.

Table 7
Fixed Effects Results using Polity Measure of Democracy in the Long Run

	Balanced Panel, 1875-2000									
	25-year data					50-year data				
	Pooled OLS	Fixed Effects OLS	Arellano-Bond GMM	Fixed Effects OLS	Fixed Effects OLS	Pooled OLS	Fixed Effects OLS	Arellano-Bond GMM	Fixed Effects OLS	Fixed Effects OLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Dependent Variable is Democracy									
Democracy $_{t-1}$	0.487	0.192	0.439		0.212	0.233	-0.248	0.319		-0.266
	(0.085)	(0.119)	(0.143)		(0.140)	(0.070)	(0.124)	(0.148)		(0.188)
Log GDP per Capita $_{t-1}$	0.116	-0.020	-0.495	0.003	0.074	0.191	0.039	-0.411	-0.004	0.028
	(0.034)	(0.093)	(0.266)	(0.092)	(0.118)	(0.043)	(0.110)	(0.194)	(0.092)	(0.222)
Hansen J Test			[0.27]					[0.87]		
AR(2) Test			[0.42]					N.A.		
Implied Cumulative Effect of Income	0.226	-0.025	-0.882		0.094	0.249	0.031	-0.604		0.031
	[0.00]	[0.84]	[0.02]		[0.53]	[0.00]	[0.72]	[0.04]		[0.90]
Observations	150	150	125	150	78	111	111	74	111	48
Countries	25	25	25	25	13	37	37	37	37	16
R-squared	0.55	0.65		0.63	0.72	0.49	0.72		0.70	0.69

Pooled cross-sectional OLS regression in columns 1 and 6, with robust standard errors clustered by highest level of aggregation for income data in parentheses. Fixed effects OLS regressions in columns 2, 4, 5, 7, 9, and 10, with country dummies and robust standard errors clustered by highest level of aggregation for income data in parentheses. Column 3 and 8 use GMM of Arellano and Bond (1991), with robust standard errors; we instrument for income using a double lag. Year dummies are included in all regressions. Implied cumulative effect of income represents the coefficient estimate of log GDP per Capita$_{t-1}$/(1-Democracy$_{t-1}$) and the p-value from a non-linear test of the significance of this coefficient is in brackets. Dependent variable is Polity Measure of Democracy. Base sample is a balanced panel 1875-2000. Columns 1-5 use 25-year data where the start date of the panel refers to the dependent variable (i.e., t=1875, so t-1=1850), and columns 6-10 use 50-year data where the start date of the panel refers to the dependent variable (i.e., t=1900, so t-1=1850), where the sample begins in 1900. In columns 5 and 10, we drop countries for which the level of aggregation for income data changes across the sample period. The AR (2) test is not possible in column 7 since there are two observation years. GDP per capita is from Maddison (2003). For detailed data definitions and sources see Table 1 and Appendix Table A1.

Table 8

Democracy in the Very Long Run

	Base Sample, 1500-2000				Former Colonies Sample, 1500-2000						
	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Dependent Variable is Change in Democracy Over Sample Period										
Change in Log GDP per Capita Over Sample Period	0.134	0.061	0.088	0.047	0.136	0.067	0.099	0.057	0.081	0.025	0.029
	(0.021)	(0.023)	(0.020)	(0.023)	(0.019)	(0.012)	(0.012)	(0.013)	(0.027)	(0.024)	(0.026)
Constraint on the Executive at Independence		0.260		0.164		0.189		0.189		0.166	0.167
		(0.120)		(0.064)		(0.072)		(0.075)		(0.089)	(0.087)
Independence Year/100		-0.206		-0.133		-0.190		-0.105		-0.179	-0.128
		(0.063)		(0.036)		(0.032)		(0.075)		(0.023)	(0.074)
Fraction Muslim			-0.299	-0.233			0.023	0.059			0.038
			(0.097)	(0.083)			(0.101)	(0.105)			(0.088)
Fraction Protestant			0.191	0.180			0.508	0.491			0.411
			(0.112)	(0.091)			(0.258)	(0.154)			(0.196)
Fraction Catholic			0.155	0.117			0.306	0.277			0.200
			(0.073)	(0.069)			(0.130)	(0.229)			(0.221)
Log Population Density, 1500									-0.059	-0.049	-0.031
									(0.021)	(0.022)	(0.027)
Historical Factors F-test		[0.02]	[0.00]	[0.01]		[0.05]	[0.00]	[0.00]	[0.02]	[0.00]	[0.00]
Observations	135	135	131	131	87	87	87	87	83	83	83
R-squared	0.20	0.34	0.40	0.45	0.20	0.30	0.33	0.37	0.25	0.34	0.38

Cross-section OLS regression in all columns, with robust standard errors clustered by level of aggregation for 1500 income data in parentheses. Countries are included if independent prior to 1990, as determined by CIA (2006). Sample limited to former European colonies in columns 5-11. Changes are total differences between 1500 and 2000. GDP per capita is from Maddison (2003), and democracy is calculated using the Polity Measure of Democracy, which comprises in part constraint on the executive. All columns assume some values of democracy in 1500 in a few European countries, following Acemoglu et al (2004b) and assigns the lowest value of democracy for all other countries. The historical factors F-test reports the p-value for all variables other than change in income. For detailed data definitions and sources see text, Table 1, and Appendix Table A1.

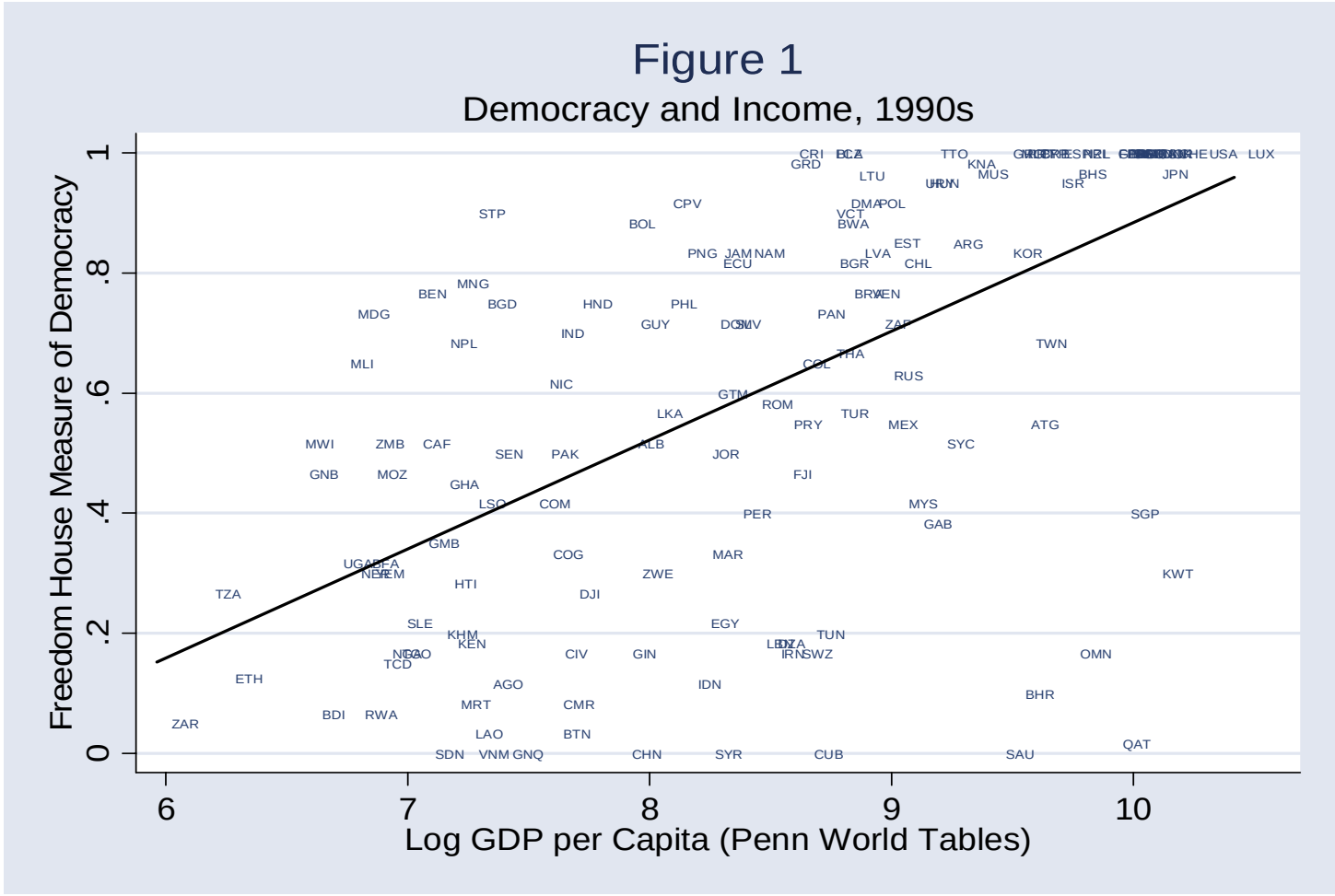
Appendix Table A1

VARIABLE	DESCRIPTION	SOURCE
Freedom House Political Rights Index, also referred to here as Freedom House Measure of Democracy	Data for 1972-2000 in Freedom House Political Rights Index, original range 1,2,3,…,7 normalized 0-1. Data for 1972 used for 1970. Data for 1950, 1955, 1960 and 1965, in Bollen, original range 0.00,0.01,…0.99,1.	http://www.freedomhouse.org/ratings/, and Bollen (2001) "Cross National Indicators of Liberal Democracy 1950-1990" available on ICPSR
Polity Composite Democracy Index, also referred to here as the Polity Measure of Democracy	Data for 1850-2000 in Polity IV. The composite index is the democracy score minus the autocracy score. Original range -10,-9,...10, normalized 0-1. For the purposes of the historical regressions, countries for which data is not available in 2000 are assigned the data for 1995.	http://www.cidcm.umd.edu/inscr/polity/
Polity Composite Democracy Index in 1500	Constructed using constraint on the executive score from Acemoglu, Johnson, and Robinson (2004b) for the sample of European countries. Components of the index other than constraint on the executive are assigned a value of zero for all countries.	Acemoglu, Johnson, and Robinson (2004)
GDP per Capita (Chain Weighted 1996 Prices)	Data for 1950-2000 measured as Log Real GDP per Capita (Chain Method in 1996 prices) from Penn World Tables 6.1.	http://pwt.econ.upenn.edu/
GDP per Capita (1990 dollars)	Data for 1500-2000 measured as Log Real GDP per Capita (1990 Geary-Khamis dollars) from Maddison (2003). Countries are assigned values at the lowest possible aggregation. Data in 1820 is used for 1850. Data in 1870 is used for 1875. Data in 1929 is used for 1925.	http://www.eco.rug.nl/~Maddison/
Population	Total population in thousands.	World Bank (2002)
Education	Average total years of schooling in the population aged 25 and over. Data for 1960, 1965,…, 1995 from Barro and Lee. We include average years of primary schooling in the population aged 25 and over in specifications which include the same covariates as Barro (1999).	Barro and Lee (2000) available at http://www.cid.harvard.edu/ciddata/ciddata.html
Age Structure	Data for 1950, 1955,…, 2000 from United Nations Population Division (2002). These variables are median age of the population and fraction of the population 5 different age ranges: 0 to 15, 15 to 30, 30 to 45, 45 to 60, and 60 and above.	United Nations Population Division (2003)
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)
Savings Rate	Data for 1950-2000 measured as (Y-G-C)/Y from Penn World Tables 6.1 where Y is nominal income, C is nominal consumption, and G is nominal government spending.	http://pwt.econ.upenn.edu/
Labor Share	Labor share of value added from Rodrik (1999). 0-1 scale.	Rodrik (1999)

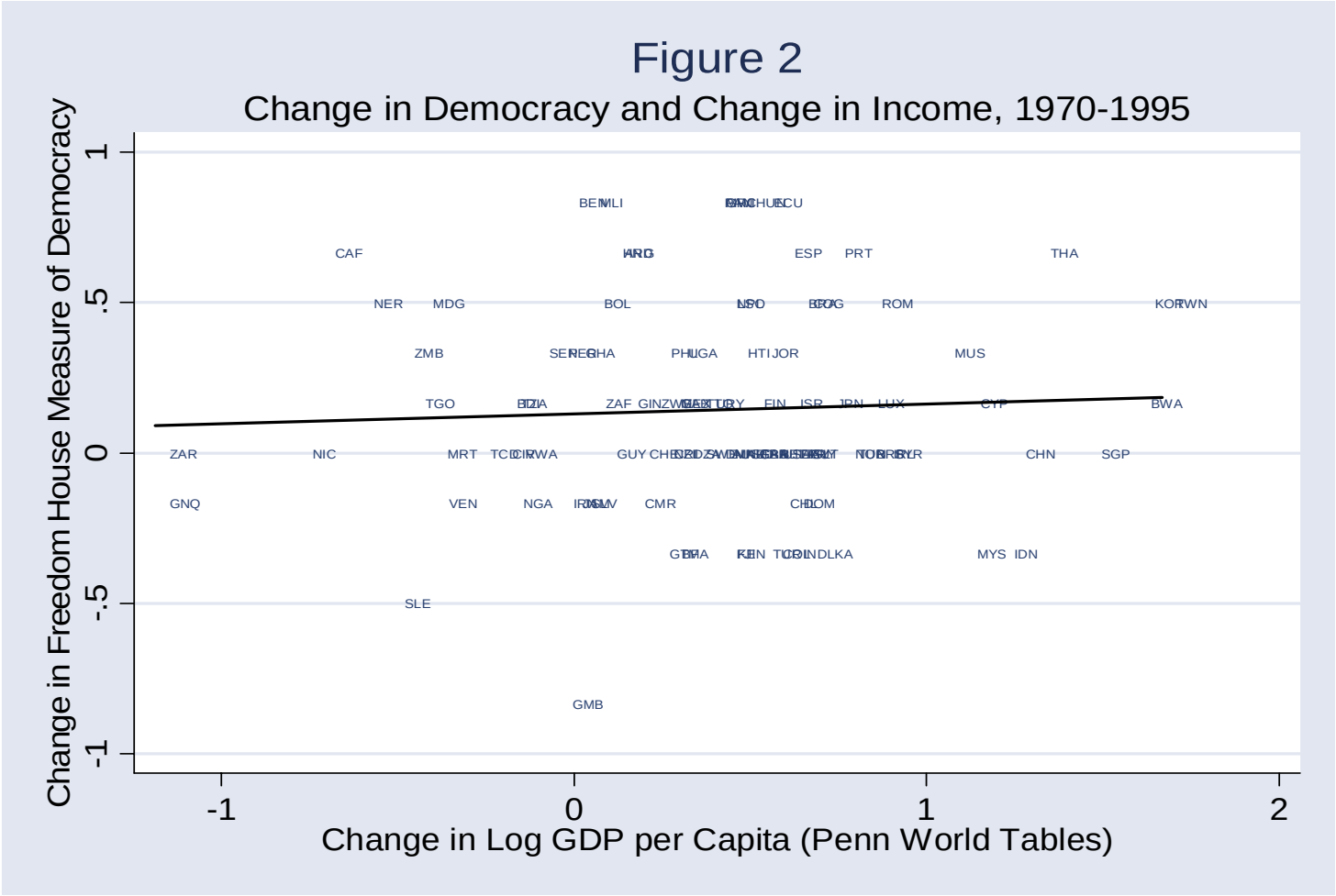
VARIABLE	DESCRIPTION	SOURCE
Trade-Weighted Log GDP	Constructed using GDP per Capita from Penn World Tables 6.1 and average trade shares between 1980 and 1989 from International Monetary Fund Direction of Trade Statistics (2005) according to procedures described in Appendix.	http://pwt.econ.upenn.edu/ and IMF DoTS CD-ROM (2005)
Trade-Weighted Democracy	Constructed using Freedom House Political Rights Index, GDP per Capita from Penn World Tables 6.1, and average trade shares between 1980 and 1989 from International Monetary Fund Direction of Trade Statistics (2005) according to procedures described in Appendix.	http://pwt.econ.upenn.edu/, IMF DoTS CD-ROM (2005), and http://www.freedomhouse.org/ratings/, and Bollen (2001) "Cross National Indicators of Liberal Democracy 1950-1990" available on ICPSR
Constraint on the Executive at Independence	Data in Polity IV, original range 1,2,3...7, normalized 0-1. Calculated as the average of constraint on the executive in a country during the first 10 years after its independence (ignoring missing data). If data for the first 10 years after independence is missing, we find the first year these data are available in Polity, then average over the following ten years (ignoring missing data).	http://www.cidcm.umd.edu/inscr/polity/
Independence year	Year when country became independent, with any year before 1800 coded as 1800. We coded Taiwan's independence year to 1948 and changed Zimbabwe's independence year to 1964. Classification of countries follows Polity.	CIA World Factbook (2004) available at http://www.cia.gov/cia/publications/factbook/
Population Density in 1500	Indigenous population divided by arable land in 1500.	Acemoglu et al (2002)
Religion	Percent of population in 1980 which is (1) Catholic, (2) Protestant, or (3) Muslim.	La Porta et al (1999)

Appendix Table A2
Codes Used to Represent Countries in Figures

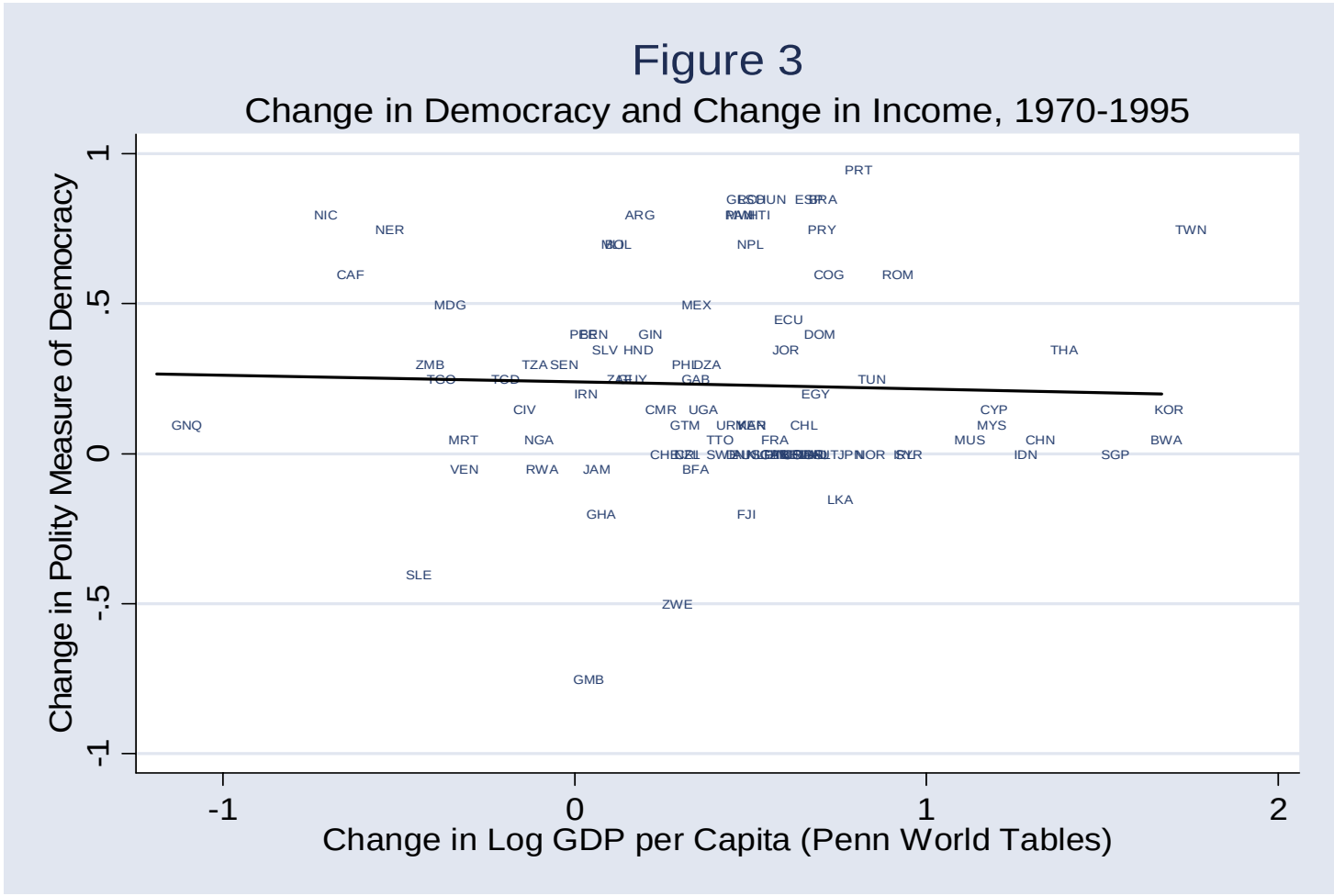
Country	Code	Country	Code	Country	Code
Andorra	ADO	Ghana	GHA	Netherlands	NLD
Afghanistan	AFG	Guinea	GIN	Norway	NOR
Angola	AGO	Gambia, The	GMB	Nepal	NPL
Albania	ALB	Guinea-Bissau	GNB	New Zealand	NZL
United Arab Emirates	ARE	Equatorial Guinea	GNQ	Oman	OMN
Argentina	ARG	Greece	GRC	Pakistan	PAK
Armenia	ARM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



See Appendix Table A1 for data definitions and sources. Values are averaged by country from 1990 to 1999. GDP per Capita is in PPP terms. The regression represented by the fitted line yields a coefficient of 0.181 (standard error=0.019), N=147, R^2=0.35.



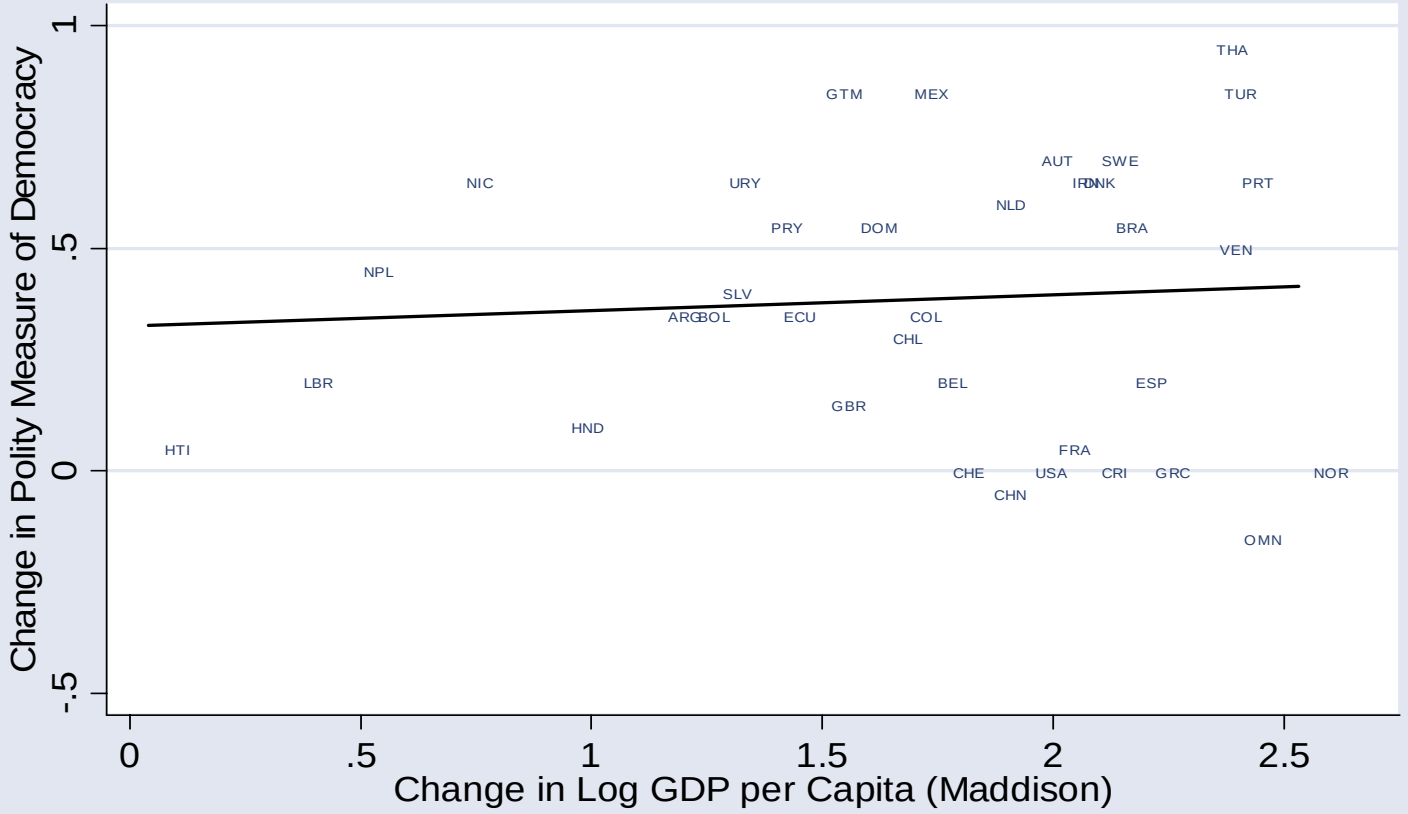
See Appendix Table A1 for data definitions and sources. Changes are total difference between 1970 and 1995. Countries are included if they were independent by 1970. Start and end dates are chosen to maximize the number of countries in the cross-section. The regression represented by the fitted line yields a coefficient of 0.032 (standard error=0.058), N=102, R^2=0.00.



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

Figure 4

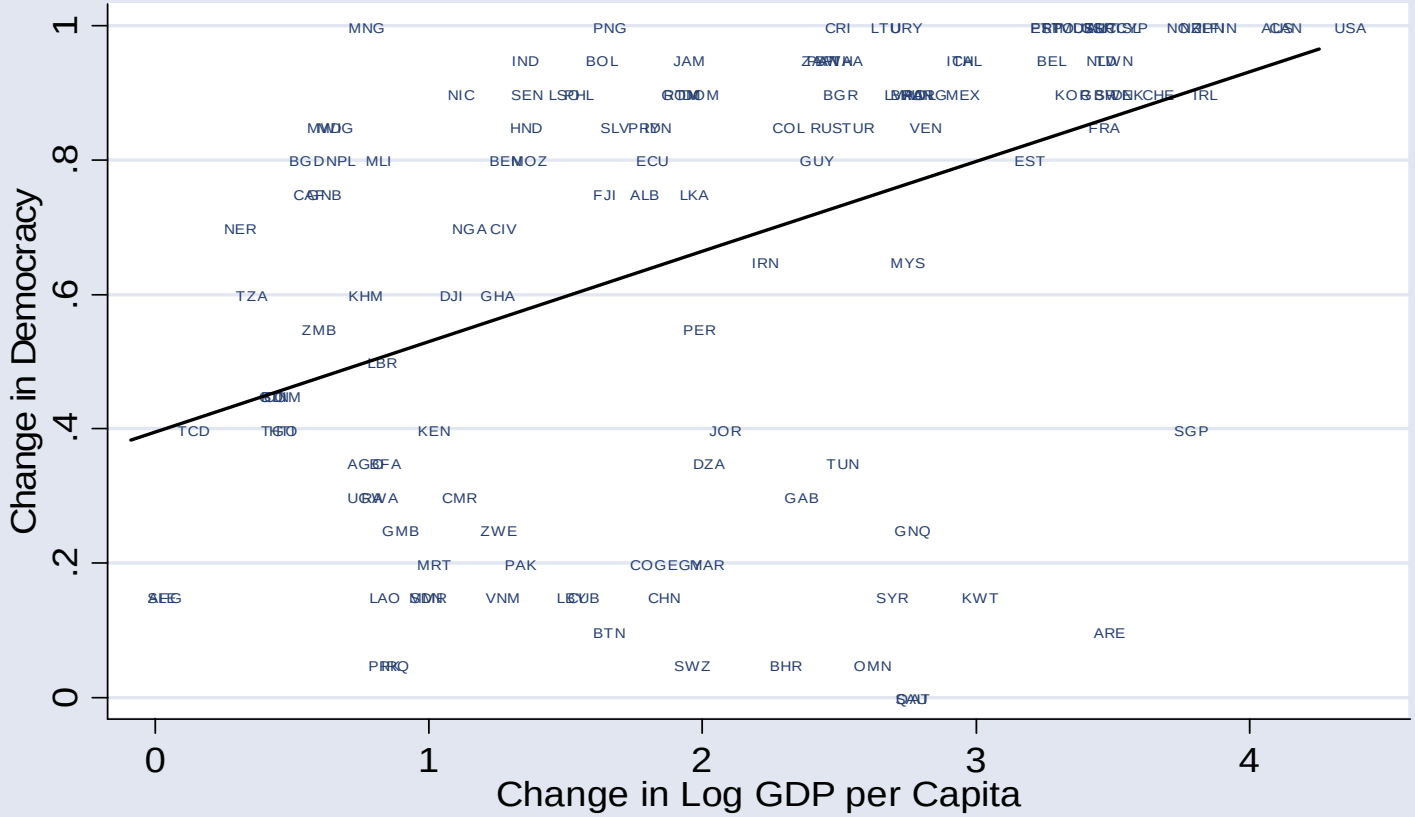
Change in Democracy and Change in Income, 1900-2000



Log GDP per Capita is from Maddison (2003). See Appendix Table A1 for data definitions and sources. Changes are total difference between 1900 and 2000. Countries are included if they are in the 1900-2000 balanced 50 year panel discussed in Section 6 of the text. The regression represented by the fitted line yields a coefficient of 0.035 (standard error=0.049), N=37, R^2=0.00.

Figure 5

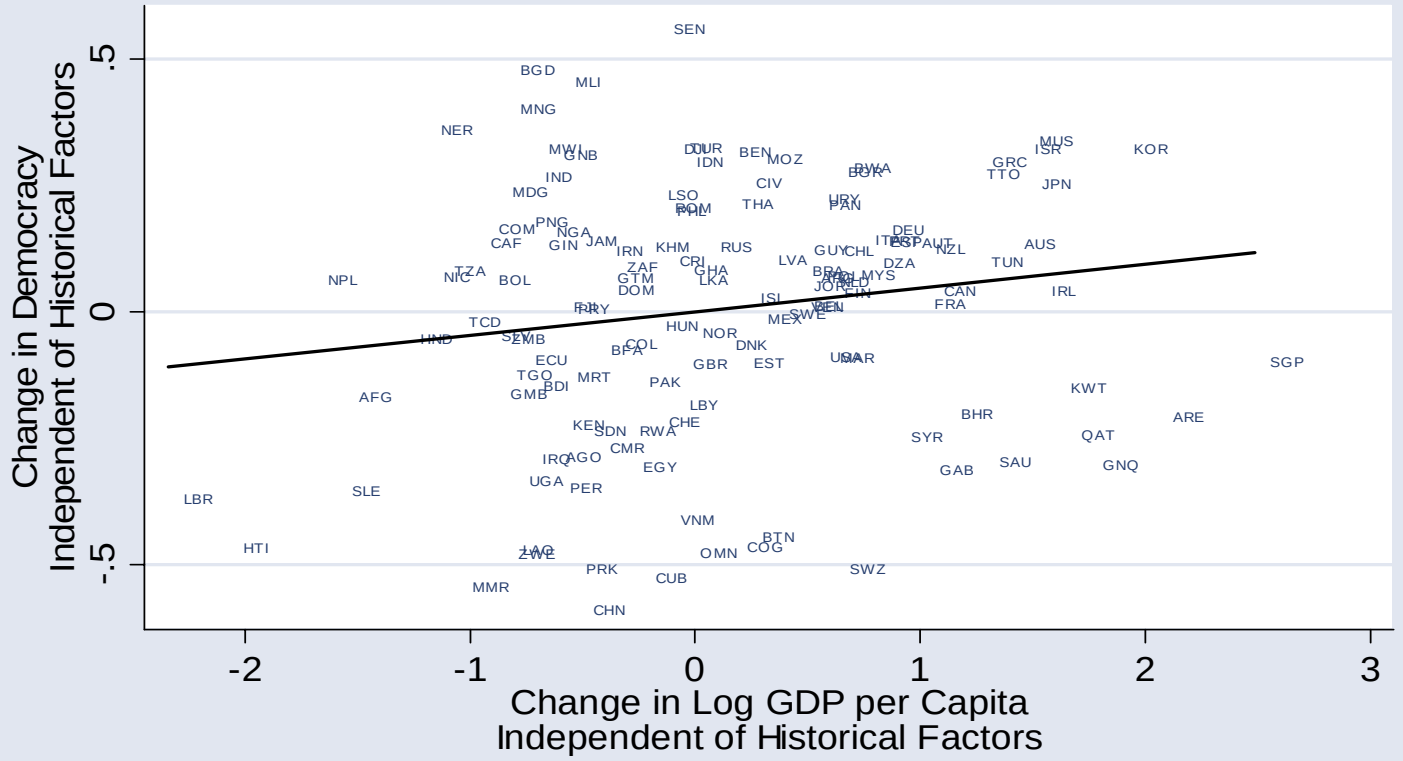
Change in Democracy and Change in Income, 1500-2000



See Appendix Table A1 for data definitions and sources. Changes are total differences between 1500 and 2000. GDP per capita is from Maddison. Democracy is calculated using the Polity measure of democracy, which comprises in part constraint on the executive; data for 1500 from Acemoglu et al (2004b). The regression represented by the fitted line yields a coefficient of 0.134 (standard error=0.021), N=135, R^2=0.20, and corresponds to the specification of Table 8, column 1.

Figure 6

Change in Democracy and Change in Income, 1500-2000 Conditional on Historical Factors



See Appendix Table A1 for data definitions and sources. Changes are total differences between 1500 and 2000 (see Figure 5 for the construction of these differences) which are not predicted in a linear regression by historical factors: Fraction Muslim, Fraction Protestant, Fraction Catholic, Constraint on the Executive at Independence, and Independence Year. This corresponds to the residual plot of the regression in Table 8, column 4 and it yields a coefficient of 0.047 (standard error=0.023), N=131, R^2=0.45.