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Daron Acemoglu
Simon Johnson
James A. Robinson
Pierre Yared

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ABSTRACT

We revisit one of the central empirical findings of the political economy literature that higher income per capita causes democracy. Existing studies establish a strong cross-country correlation between income and democracy, but do not typically control for factors that simultaneously affect both variables. 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 also present instrumental-variables using two different strategies. These estimates also show no causal effect of income on democracy. Furthermore, we reconcile the positive cross-country correlation between income and democracy with the absence of a causal effect of income on democracy by showing that the long-run evolution of income and democracy is related to historical factors. Consistent with this, the positive correlation between income and democracy disappears, even without fixed effects, when we control for the historical determinants of economic and political development in a sample of former European colonies.

Daron Acemoglu
Department of Economics
MIT, E52-380B
50 Memorial Drive
Cambridge, MA 02142-1347
and NBER
daron@mit.edu

Simon Johnson
Sloan School of Management
MIT, 50 Memorial Drive
Cambridge, MA 02139
and NBER
sjohnson@mit.edu

James A. Robinson
Department of Government
Harvard University
1875 Cambridge Street
Cambridge, MA 02138
jrobinson@gov.harvard.edu

Pierre Yared
Department of Economics
MIT
50 Memorial Drive
Cambridge, MA 02142-1347
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. This positive relationship is not only confined to a cross-country comparison. Most countries were nondemocratic before the modern growth process took off at the beginning of the 19th century. Democratization came together with growth. Barro (1999, S160), for example, summarizes the findings from his detailed study 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, which sees a direct causal link between economic growth and democracy. According to this theory, economic growth engenders "a culture of democracy" and provides the foundations for democratic political institutions. This thesis is clearly articulated in Lipset (1959), who argued that "only in a wealthy society in which relatively few citizens lived in real poverty could a situation exist in which the mass of the population could intelligently participate in politics and could develop the self-restraint necessary to avoid succumbing to the appeals of irresponsible demagogues" (p. 75). It is also reproduced in all the major works on democracy (e.g., Dahl, 1971, Huntington, 1991).

In this paper, we revisit the relationship between income per capita and democracy. Our starting point is that existing work, 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 relation-

[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 (2004).

[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

ship between income and democracy. Major sources of potential bias in a regression of democracy on income per capita are country-specific, historical factors influencing both political and economic development. If these omitted characteristics are, to a first approximation, time-invariant, the inclusion of fixed effects will remove them and this 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 effects, would suggest that there is a relationship between democracy and income. The idea of fixed effects is to move beyond this comparison and investigate the "within-country variation"; i.e., whether as Colombia becomes relatively richer, it also tends to become more democratic relative to the United States. In addition to improving inference on the causal effect of income on democracy, this approach is also more closely related to modernization theory as articulated by Lipset (1959), which claims that countries should become more democratic as they become richer, not simply that rich countries should be more democratic.

Our main finding from this strategy is that once fixed effects are introduced, the positive relationship between income per capita and various measures of democracy disappears. Figures 1 and 2 show this diagrammatically by plotting changes in our two measures of democracy, the Freedom House and Polity scores (see below for data details), for each country between 1970 and 1995 against the change in GDP per capita over the same period. There appears to be no relationship between changes in income per capita and democracy.

This basic finding holds with various indicators for democracy, with different econometric specifications and estimation techniques, in different subsamples, and is robust to the inclusion of additional covariates. Moreover, these results are not driven by large standard errors. 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 deter-

problems of measurement error worse because they remove a significant portion of the variation in the right-hand side variables. Consequently, fixed effects are certainly no substitute for using an instrumental-variables approach with a valid instrument.

[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.

minants of both democracy and income, it does not necessarily estimate the causal effect of income on democracy. An instrumental-variables (IV) strategy with a valid instrument would be a superior approach, but it is difficult to find valid instruments for income that could not affect democracy through other channels.[4] Our second strategy is to use IV regressions. 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, and argue that it should have no direct effect on democracy. Both IV strategies confirm our basic findings and show no evidence of a causal effect of income on democracy. We recognize that neither instrument is perfect, since there are some 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 concerns about the validity of the instruments, we show that the most likely sources of correlation between our instruments and the error term in the second stage are not present.

These results naturally raise the following important question: what is the source of the cross-sectional correlation between income and democracy? Why are rich countries democratic today? One possible explanation is that there is a causal effect of income on democracy, but it works at much longer horizons than the existing literature posited, that is, over 50 or even 100 years rather than 10 or 20 years. Another hypothesis, suggested by approaches that emphasize the importance of historical factors in long-run development,[5] is that the cross-sectional relationship reflects the persistent influence of these historical factors. Put differently, events during certain crucial junctures impact the economic and political "development path" of a society, leading to persistent, though not permanent, influences on economic and political outcomes. Both of these hypotheses suggest that the within-correlation between income and democracy should be stronger when we look at

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

[5] See, among others, North and Thomas (1973), North (1981), Jones (1981), Engerman and Sokoloff (1997), Acemoglu, Johnson and Robinson (2001, 2002).

longer horizons.

We investigate this possibility by looking at the relationship between income and democracy over the past 160 years, and over the past 500 years. We find little evidence for an effect of income on democracy in samples that span 100 or 160 years. In contrast, over the past 500 years there seems to be a very strong correlation. Our interpretation is that this pattern is consistent with the second hypothesis, because the 500 years in question spans the period of divergence of national development paths (e.g., the emergence of constitutional monarchies, the rise of the modern nation state, industrialization, and the colonial experience).

In addition, we also provide direct evidence consistent with the second hypothesis by looking at the sample of former European colonies. This sample is useful since it enables us to exploit the quasi-natural experiment provided by the colonization of many diverse societies by European powers after 1492, where differences in the colonization experience led to significant divergence in the economic and political development paths of these societies (see, e.g., Acemoglu, Johnson and Robinson, 2001, 2002, and Engerman and Sokoloff, 1997). We document that in this sample, the fixed effects in the democracy regressions are closely linked to the potential determinants of European colonization strategy (in particular, the density of the indigenous population at the time of colonization and potential mortality rates of European settlers), date of independence and measures of institutions in the early independence era. The positive correlation between income and democracy disappears, even without fixed effects, when we control for these historical determinants. This evidence further supports the second hypothesis.

Finally, we document that there are some income-related determinants of democracy in the postwar sample. In particular, contrary to the implications of modernization theory, we find that economic crises lead to democracy. We show that this result is driven entirely by the fact that dictatorships are more likely to collapse in the face of economic crises than democracies are likely to revert back to dictatorship.[6]

The paper proceeds as follows. In Section 2 we describe the data. In Section 3 we discuss our econometric approach and present the basic results. Section 4 presents our IV results. Section 5 discusses potential interpretations for these results. Motivated by these interpretations, Section 6 investigates the longer-run relationship between income and democracy, and Section 7 looks at the historical determinants of economic and political

[6]In passing, we also show that income per capita does not appear to have a causal effect when we look separately at transitions to and from democracy, contrary to the findings in Przeworski et al. (2000). Since we do not have space in this paper, we leave a more detailed investigation of transitions to future work.

development in the sample of former European colonies. Section 8 looks at the effect of crises on democracy. Section 9 concludes. The Appendix contains some additional results and further information on the construction of the instruments used in Section 4.

2 Data and Descriptive Statistics

We follow much of the existing research in this area in adopting a Schumpeterian definition based on a number of institutional conditions.[7] 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.[8] Following Barro (1999), we supplement this index with the related variable from Bollen (1990, 2001) for 1950, 1955, 1960, and 1965. As in Barro (1999), we transform both indices so that they lie between 0 and 1, with 1 corresponding to the most democratic set of institutions.

The Freedom House index, even when augmented with Bollen's data, only enables us to look at the postwar era. The Polity IV dataset, on the other hand, provides information for all countries since independence starting in 1800. Both 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 scoring countries according to competitiveness of political participation, the regulation of participation, the openness and competitiveness of executive recruitment and constraints on the chief executive. To facilitate compari-

[7] Schumpeter (1950, p. 250) argued that democracy was: "the institutional arrangement for arriving at political decisions in which individuals acquire the power to decide by means of a competitive struggle for the people's vote."

[8] 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

son with the Freedom House score, we also normalize the composite Polity index to lie between 0 and 1.

Using the Freedom House and the Polity data, we construct five-yearly, ten-yearly, and annual panels. For the five-year panels, we take the observation every fifth year. We prefer this procedure to averaging the five-yearly data, since averaging introduces additional serial correlation, making inference and estimation more difficult (see footnote 12). For the ten-yearly panels, we take the observation every tenth year for similar reasons. For the Freedom House data which begins in 1972, we follow Barro (1999) and assign the 1972 score to 1970 for the purpose of the five-year 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 key variables both for the whole world and for former European colonies, the sample we focus on for some of the historical regressions. It shows that there is significant variation in all the variables for both the entire sample and the former colonies sample. Countries in the former colonies sample are somewhat less democratic and substantially (about 30 percent) poorer than the average country in the whole sample.

3 Main Results

3.1 Basic Specifications and Interpretation

Our basic regression model is:

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

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 whether income has an effect 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, which 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. The sample period is 1960-2000 and time periods correspond to five-year intervals.

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 equilibrium democracy level.

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, 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 (where x^j_{it-1} refers to the jth component of the vector $\mathbf{x}_{it-1}$, and covariances refer the population covariances). 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 $T \to \infty$, see below). 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 will necessarily estimate the causal effect of income on democracy.

To illustrate this point and as a preparation for the discussion in Section 5, 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. Let us also add another error component, η^d_{it}, which admits a unit root, such that:

$$\begin{aligned} d_{it} &= \gamma y_{it} + \delta^d_i + \eta^d_{it} + u^d_{it}, \\ \text{where } \eta^d_{it} &= \eta^d_{it-1} + \upsilon^d_{it}. \end{aligned} \tag{2}$$

Moreover suppose that the statistical process for income per capita also admits a unit root,

$$\begin{aligned} y_{it} &= \delta^y_i + \eta^y_{it} + u^y_{it}, \\ \text{where } \eta^y_{it} &= \eta^y_{it-1} + \upsilon^y_{it}. \end{aligned} \tag{3}$$

While δ^d_i and δ^y_i correspond to fixed differences in levels of democracy and income across countries, η^d_{it} and η^y_{it} capture factors affecting the *evolution* of democracy and income across countries. As before, the parameter γ represents the causal effect of income on democracy. Denote the variance of υ^y_i by $\sigma^2_{\upsilon^y}$ and of u^y_i by $\sigma^2_{u^y}$. Assume that

$\text{Cov}\left(u_{it}^d, u_{it+k}^y\right)$ =$\text{Cov}\left(\upsilon_{it}^y, \upsilon_{it+k}^y\right)$ =$\text{Cov}\left(\upsilon_{it}^d, \upsilon_{it+k}^d\right)$ = 0 for all i and $k \neq 0$. Now imagine that we have data for two time periods. Then the probability limit of the fixed effects estimator $\hat{\gamma}^{FE}$ in a panel with only two periods is:

$$\begin{aligned} \text{plim}\hat{\gamma}^{FE} &= \gamma + \frac{\text{Cov}\left(\eta_{it}^d - \eta_{it-1}^d, \eta_{it}^y - \eta_{it-1}^y\right)}{\text{Var}\left(\eta_{it}^y - \eta_{it-1}^y + u_{it}^y - u_{it-1}^y\right)} \\ &= \gamma + \frac{\text{Cov}\left(\upsilon_{it}^d, \upsilon_{it}^y\right)}{\sigma_{\upsilon^y}^2 + 2\sigma_{u^y}^2}, \end{aligned}$$

where the second equality uses the assumptions on the u_i's and the υ_i's together with the definitions in (2) and (3).

This expression shows that the fixed effects estimator will lead to consistent estimates only if υ_{it}^d and υ_{it}^y are orthogonal, i.e., if there are no correlated shocks influencing the evolution of income and democracy. Nevertheless, if, plausibly, $\text{Cov}\left(\upsilon_{it}^d, \upsilon_{it}^y\right) > 0$ so that such shocks are *positively* correlated, the fixed effects estimator will be biased upwards and will provide an upper bound on the causal effect of income on democracy.

In addition to the conceptual issues, there is also an econometric problem involved in the estimation of (1). The regressor d_{it-1} is mechanically correlated with u_{is} for $s < t$, so the standard fixed effect estimator is not consistent (e.g., Wooldridge, 2002, chapter 11). However, it can be shown that the fixed effects OLS estimator becomes consistent as the number of time periods in the sample increases (i.e., as $T \to \infty$). We discuss and implement a number of strategies to deal with this problem below.

3.2 Results

Table 2 uses the Freedom House data and Table 3 uses the Polity data, in both cases for our entire (base) sample, over the period 1960-2000. All standard errors in the paper (unless indicated otherwise) are robust against arbitrary heteroscedasticity in the variance-covariance matrix, and allow for clustering at the country level.[9]

We start with a column showing the most parsimonious pooled OLS regression of the democracy score on its (five-year) lag and log income per capita. Lagged democracy is highly significant, and shows a considerable degree of persistence (mean reversion) in democracy. Log income per capita is also significant and illustrates the well-documented positive relationship between income and democracy. Though statistically significant, the

[9] Clustering is a simple strategy to correct the standard errors for potential correlation across observations both over time and within the same time period. See for example Moulton (1986) or Bertrand, Duflo and Mullainathan (2004). The heteroscedasticity correction takes care of fact that the democracy index takes discrete values.

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. Since lagged democracy has a coefficient of 0.706, the long-run effect of a 10% increase in GDP per capita would be 0.007/(1-0.706)≈0.024, which is still quantitatively small.

The remainder of Table 2 presents our basic results with fixed effects. Column 2 shows that the relationship between income and democracy disappears once fixed effects are included. Now 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 estimates have in fact the wrong (negative) sign, -0.006 (standard error=0.039).

One might be worried that the lack of relationship in the fixed effects regressions is a consequence of the imprecision of the estimates resulting from the inclusion of fixed effects. This does not seem to be the case. Although, as pointed out above, 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 and long-run effects greater than 0.013 on the democracy index (the implied long-run effect of 0.024 in the pooled cross-sectional regression is comfortably outside this interval because the coefficient on lagged democracy is smaller with fixed effects).

That these results are not driven by some econometric problems or some unusual feature of the data is further shown in Figures 1 and 2 above, which plot the change in the Freedom House and Polity score for each country between 1970 and 1995 against the change in GDP per capita over the same period. These scatterplots correspond to the estimation of the fixed effects equation (1) with contemporaneous income as the right-hand side regressor, without any covariates and using only two data points, 1970 and 1995.[10] They show clearly that there is no strong relationship between income growth and changes in democracy over this period.

[10] 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.

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

Our first strategy, adopted in column 3, is to use the methodology proposed by Anderson and Hsiao (1982), which is to time difference equation (1), to obtain

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

where the fixed country effects are removed by time differencing. Although equation (4) cannot be estimated consistently by OLS, in the absence of serial correlation in the original residual, u_{it} (i.e., no second order serial correlation in Δu_{it}), d_{it-2} is uncorrelated with Δu_{it}, so can be used as instrument for Δd_{it-1} to obtain consistent estimates and similarly, y_{it-2} is used as an instrument for Δy_{it-1}. 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).[11] With this estimate, the two standard error bands now 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.[12]

[11] 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.

[12] We also checked the results with five-year averaged data rather than our data set which uses only the democracy information every fifth year. The results 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.

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

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 corresponding OLS estimate is easily outside the two standard error bands (the OLS estimate without lagged democracy, which is not shown in the table, is 0.235 with a standard error of 0.012).

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. The table reports the p value of an F-test for the joint significance of these variables. The results show no evidence of a significant positive effect of income on democracy (while democracy is strongly predicted by its lags, as was the case in earlier columns).

Finally, columns 7 and 8 present regressions using a dataset consisting of ten-year observations. This is useful to investigate whether the relationship between income and democracy will be stronger with lower-frequency data. The results are similar to those with five-year observations and to the patterns in Figures 1 and 2, which show no evidence of a positive association between changes in income and democracy between 1970 and 1995.

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.

3.3 Robustness

Table 4 investigates the robustness of these results in alternative samples. To save space, we only report the robustness checks for the Freedom House data (the results with Polity are similar and are available upon request). Columns 1-3 show the regressions correspond-

ing to columns 2, 4 and 6 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. All three 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), and the two standard error bands now exclude the OLS estimate.

Columns 4-6 exclude sub-Saharan Africa, where many countries became democratic immediately after independence and later lapsed into nondemocracy. The results in this sample are also similar and show no evidence of a significant positive effect of income on democracy in any of the specifications. Columns 7-12 report regressions excluding Muslim countries and former socialist countries, again with very similar results.

Table 5 investigates the influence of various covariates on the relationship between income and democracy. To save space, we again report results only with the Freedom House data. We start with the pooled OLS regressions for comparison. Columns 1-3 includes log population and age structure, and columns 4-6 add education. Columns 7-9 include the full set of covariates from Barro's (1999) baseline specification.[13] In all cases, there is a positive and significant estimate of γ in the pooled cross section, which is smaller than the baseline estimate in column 1 of Table 2. The rest of the table shows that the presence of these covariates does 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 the other basic tenet of the modernization hypothesis, is therefore also not robust to controlling for country fixed effects. We investigate this issue in greater detail in Acemoglu, Johnson, Robinson, and Yared (2005).

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.[14]

[13] 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. In columns where we add covariates from Barro (1999), we 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). Additional covariates from Barro (1999)'s regression are urbanization rate, male-female education gap, and a dummy for major oil producer (used in the pooled cross-section only). For detailed definitions and sources see Appendix Table A1.

[14] The only subsample where we find a positive association between income per capita and democracy

4 Instrumental Variable Estimates

As discussed above, fixed effects estimators do not necessarily identify the causal effect of income on democracy. The estimation of such causal effects requires us to exploit a source of exogenous variation. While we do not have an ideal source of exogenous variation, there are two promising potential instruments and we now present IV results using these.

4.1 The 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

$$y_{it-1} = \pi^F s_{it-2} + \alpha^F d_{it-1} + \mathbf{x}'_{it-1}\boldsymbol{\beta}^F + \mu^F_{t-1} + \delta^F_i + u^F_{it-1}, \tag{5}$$

where all the variables are the same as defined above, 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 suggesting that 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 6 looks at the effect of GDP per capita on democracy in IV regressions using past savings rates as instruments and the Freedom House data (results using Polity data are in Appendix Table A2 and are similar). The savings rate is defined as nominal income minus consumption minus government expenditure divided

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.

by nominal income.[15]

We report a number of different specifications, with or without a lag of democracy, 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). Column 5 adds lagged democracy on the right hand side. The first stage is very similar, and now the estimate of γ is -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 relatively large and negative, and significant at 5%. These 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 shows a very similar estimate when sub-Saharan African countries are excluded. Column 8 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.[16] 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 9 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 10 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

[15] We calculate savings using nominal, not PPP, numbers from the Penn World Tables. The first stage is weaker and the second stage has a larger standard error if we use PPP data. The first and second results are similar if we use an "investment rate" which is this measure of savings minus net exports.

[16] 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.

restriction is accepted comfortably (the χ^2-statistic for a Hausman, 1978, test takes the value of 0.00, which is accepted at the p-value of 1.00).

4.2 The Trade-Weighted World Income Instrument

Our second instrument exploits the existence of trade relationships 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. Namely, ω_{ij} is the share of trade between country i and country j in the GDP of country i. In practice, we use two measures of Ω. The first is actual trade shares between 1980-1989 (which is chosen to maximize coverage). The second is a measure of predicted average trade shares from a standard gravity equation used in Frankel and Romer (1999). The Appendix provides details on data sources and construction.

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:

$$Y_{it-1} = \zeta \sum_{j=1, j\neq i}^{N} \omega_{ij} Y_{jt-1} + \varepsilon_{it-1}, \tag{6}$$

for all $i = 1, ..., N$, where Y_{it-1} denotes log 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 (6), the identification problem in the estimation of (1) can be restated as follows: the error term ε_{it-1} in (6) 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

$$\widehat{Y}_{it-1} = \zeta \sum_{j=1, j\neq i}^{N} \omega_{ij} Y_{jt-1}, \tag{7}$$

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 (7), we obtain our first-stage relationship:

$$y_{it-1} = \pi^F \sum_{j=1, j\neq i}^{N} \omega_{ij} Y_{jt-1} + \alpha^F d_{it-1} + \mathbf{x}'_{it-1}\boldsymbol{\beta}^F + \mu^F_{t-1} + \delta^F_i + u^F_{it-1}, \tag{8}$$

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 for $\widehat{Y}_{it-1}$ to be orthogonal to u_{it}. A sufficient condition for this is that Y_{jt-1} be orthogonal to u_{it} for all $j \neq i$.

There are two problems with this strategy, however. First, there may be "economic" 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. Although we have no way of ruling this out a priori, we test for this in our empirical specifications below by controlling for the direct effect of the democracy of trading partners, and find no evidence to support such a channel.

Second, there is an econometric problem, arising from the general equilibrium nature of equation (6).[17] 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}. To see this, let Y_{t-1} be the $N \times 1$ vector of log incomes, and let ε_{t-1} be the $N \times 1$ vector of errors in (6). Then

$$Y_{t-1} = \widehat{Y}_{t-1} + \varepsilon_{t-1} = \zeta\Omega Y_{t-1} + \varepsilon_{t-1} = \left[\left[(I - \zeta\Omega)^{-1}\right] - I\right]\varepsilon_{t-1} + \varepsilon_{t-1}.$$

Since $\left[\left[(I - \zeta\Omega)^{-1}\right] - I\right]_{ii}$, i.e., the diagonal elements of $\left[\left[(I - \zeta\Omega)^{-1}\right] - I\right]$, are not necessarily zero, ε_{it-1} will be mechanically correlated with $\widehat{Y}_{it-1}$. If we had a consistent estimator for ζ, $\hat{\zeta}$ (i.e., with $\text{plim}\hat{\zeta} = \zeta$), then by implication we would also have $\text{plim}\hat{\varepsilon}_{t-1} = \varepsilon_{t-1}$ (where the probability limit applies as $N \to \infty$). This would enable us to construct an adjusted instrument $\widehat{Y}^{ADJ}_{it-1}$, such that

$$\widehat{Y}^{ADJ}_{it-1} = \widehat{Y}_{it-1} - \left[\left[\left(I - \hat{\zeta}\Omega\right)^{-1}\right] - I\right]_{ii}\hat{\varepsilon}_{it-1}. \tag{9}$$

Using the Continuous Mapping Theorem (see, for example, van der Vaart, 1998, Theorem 2.3), $\widehat{Y}^{ADJ}_{it-1}$ would be uncorrelated with ε_{it-1}. In other words, this transformation would remove the indirect effect of ε_{it-1} on y_{it-1} working through the general equilibrium interactions across countries as well as the direct effect in (6). Obtaining a consistent estimate of ζ is not straightforward, however.[18] Here we take a number of approaches to deal with this problem. First, under some regularity conditions, the problem disappears as $N \to \infty$, so appealing to asymptotics on the number of countries, our first strategy is to ignore this

[17] We refer to this as "general equilibrium", since it would result from an equilibrium model of cross-country income determination as in Baxter (1995), Kraay and Ventura (2001), or Acemoglu and Ventura (2002).

[18] This problem is investigated in current work, Acemoglu, Kursteiner and Yared (2005).

problem and use (7). Our second strategy is to estimate ζ and perform the adjustment in (9) (more details on this are given in the Appendix). Our third strategy is to construct (7) with lagged values of Y_{jt-1}, which also removes the source of correlation between $\widehat{Y}_{jt-1}$ and u_{it} in equation (1) if ε_{it-1}'s are serially uncorrelated. All three strategies give very similar results.

The main results using the Freedom House data are presented in Table 7 (results using the Polity data are in Appendix Table A2). In the bottom panel we report the first stage. Similar to Table 6, the first three columns report OLS regressions with and without fixed effects, and the 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 (7) using the actual 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 shows a similar, though slightly less precise, estimate without sub-Saharan Africa. Column 8 investigates whether the democracy of trading partners might have an effect, influencing inference with this instrumental variable. We construct a world democracy index, $\tilde{d}_{it}$ using the same trade shares as in equation (7), 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 9 uses $\widehat{Y}_{it-2}$ instead of $\widehat{Y}_{it-1}$ on the right-hand side of (7) as an alternative strategy to remove the mechanical correlation between $\widehat{Y}_{it-1}$ and ε_{it-1}. Finally, column 10 performs an overidentification test similar to that in column 10 in Table 6 by including both $\widehat{Y}_{it-1}$ and $\widehat{Y}_{it-2}$. 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 easily accepted (the χ^2-statistic for a Hausman, 1978, test takes the value of 0.14, which is accepted at the p-value of 1.00).

Table 8 presents further robustness checks. Columns 1-3 exclude Singapore, which is an outlier in the first stage. The first-stage relationship is weaker but still significant, and the second-stage coefficient remains negative and insignificant. Columns 4-6 adjust $\widehat{Y}_{it-1}$ using (9), which has little effect on the estimates. Columns 7-9 present specifications using the gravity equation to construct Ω, which yield similar results to those in Table 7.

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.

5 Interpretation

The above results raise the following question: if there is no causal effect of income on democracy, what explains the strong cross-sectional relationship between the two variables? This strong cross-country relationship is shown in Figure 3 using the Freedom House data and is the source of the positive and significant estimates in the pooled cross-sectional regressions.

Two hypotheses naturally present themselves.

5.1 Hypothesis 1: Long Lags

The first possibility is that there is a causal effect of income on democracy, but it works with such long and variable lags that it is impossible to detect this causal channel in the postwar data. Although the issue of lags is not discussed explicitly in Lipset (1959), this is a natural extension of his thesis, since it may take a long time for a culture of democracy to emerge, or because political institutions change only slowly.

According to this hypothesis, we need to look at longer time spans. The key question is what the right horizon should be. Figures 1 and 2 show that there is no effect when we look at a 25-year period (1970-95) during the recent past. We believe that any reasonable version of this hypothesis should predict some effect when we look at a 50-year horizon or perhaps, at the longest, a 100-year horizon.

We next investigate the longer-run relationship between income and democracy. But before doing this, we discuss an alternative explanation for our results.

5.2 Hypothesis 2: Divergent Development Paths

The second possible explanation, which we find more appealing than hypothesis 1, is that the positive cross-sectional relationship reflects relatively time-invariant, historical factors that have influenced the economic and political development path of societies. There are reasons to expect that economic and political development processes should be related. For example, economic institutions encouraging economic growth are unlikely to develop and endure in societies where political power is in the hands of a small elite, who can exercise it to further their interests, even if this is at the cost of aggregate economic growth.

Moreover, events during certain critical junctures may have large and persistent effects on income and democracy because of their influence on the economic and political development paths of societies. Consequently, part of the variation in both prosperity and political institutions that we observe today may be the result of some societies having embarked on a path of development based on relatively democratic political and economic institutions encouraging growth, while other societies ended up with repressive regimes controlled by narrow elites, which engendered neither prosperity nor democracy.

The contrast between European colonies in Latin America, such as Peru and Bolivia, and the North American colonies illustrates the potential effects of key events during critical junctures. All of these societies were colonized by Europeans after 1492, but the exact colonization strategy differed a great deal. While smallholder societies, with the majority of the population enjoying access to land, secure property rights, and economic opportunities soon developed in the North American colonies, a highly coercive society, based on the exploitation and enslavement of the indigenous peoples, emerged in Peru and Bolivia (potential reasons for these differences in the colonization experience are discussed in Section 7). Moreover, these early differences in economic and political institutions have shown a considerable degree of persistence. While the United States and Canada have remained democratic and grown rapidly over the past 300 years, the political and economic record of Peru and Bolivia has been much more checkered. Hypothesis 2 implies that to understand the relationship between income and democracy, we need to look at the events and factors influencing institutional equilibria at critical junctures, in this case during the early phases of colonialism.

5.3 Implications and Discussion

Which of these two hypotheses is closer to the truth? We are unable to answer this question definitively at the moment, but two empirical exercises are informative. The first strategy is to look at the longer-run relationship between income and democracy, while the second strategy is to look directly at potential determinants of divergent historical development paths, which we turn to in Section 7.

Both hypotheses suggest that the longer-run relationship should be more positive, but for very different reasons, and potentially at very different horizons. In particular, according to hypothesis 2, the source of the correlation is the events during critical junctures, so the time interval should include dates spanning these junctures. In the context of the modern economic and political development experience, this means that the positive long-run relationship should emerge (or become more pronounced) in a data set covering

the era before the 19th century rather than a window of 50 or 100 years during the recent past.

To clarify the main issues, let us return to the statistical model in equations (2) and (3). In the context of these equations, 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} (the implicit assumption that these shocks happen at some common date $t = T^{*}$ for all countries is only for simplicity).

Now imagine we have data for two time periods again, $t = T - S$ and $t = T$. Time-differencing equations (2) and (3), we obtain:

$$d_{iT} - d_{iT-S} = \gamma \left(y_{iT} - y_{iT-S} \right) + \eta_{iT}^{d} - \eta_{iT-S}^{d} + u_{iT}^{d} - u_{iT-S}^{d}, \tag{10}$$

and

$$y_{iT} - y_{iT-S} = \eta_{iT}^{y} - \eta_{iT-S}^{y} + u_{iT}^{y} - u_{iT-S}^{y}.$$

Recall that the variances of υ_{i}^{y} and u_{i}^{y} are denoted by $\sigma_{\upsilon^{y}}^{2}$ and $\sigma_{u^{y}}^{2}$, and we have $\mathrm{Cov}\left(u_{it}^{d}, u_{it+k}^{y}\right) = 0$, $\mathrm{Cov}\left(\upsilon_{it}^{y}, \upsilon_{it+k}^{y}\right) = 0$, and $\mathrm{Cov}\left(\upsilon_{it}^{d}, \upsilon_{it+k}^{d}\right) = 0$ for all i and $k \neq 0$. Moreover, let $\mathrm{Cov}\left(\upsilon_{iT^{*}}^{d}, \upsilon_{iT^{*}}^{y}\right) = \sigma_{T^{*}}^{2}$ be positive and large, capturing the importance of a major event affecting both economic and political outcomes at this critical juncture. To contrast with this, suppose that $\mathrm{Cov}\left(\upsilon_{it}^{d}, \upsilon_{it}^{y}\right) = \sigma_{\sim T^{*}}^{2}$ for $t \neq T^{*}$, which we presume to be small and positive, and in particular much smaller than $\sigma_{T^{*}}^{2}$. Suppose also that $\mathrm{Cov}\left(\upsilon_{it}^{d}, \upsilon_{it+k}^{y}\right) = 0$ for all i and $k \neq 0$. Now consider the fixed effects estimator $\hat{\gamma}_{S}^{FE}$, where the time span is given by S. Standard arguments imply that the probability limit of this estimator using these two data points is:

$$\begin{aligned} \mathrm{plim}\hat{\gamma}_{S}^{FE} &= \gamma + \frac{\mathrm{Cov}\left(\sum_{t=T-S}^{T} \upsilon_{it}^{d}, \sum_{t=T-S}^{T} \upsilon_{it}^{y}\right)}{\mathrm{Var}\left(\sum_{t=T-S}^{T} \upsilon_{it}^{y}\right) + \mathrm{Var}\left(u_{iS}^{d} - u_{i0}^{d}\right)} \\ &= \begin{cases} \gamma + \frac{\sigma_{\sim T^{*}}^{2}}{\sigma_{\upsilon^{y}}^{2} + 2\sigma_{u^{y}}^{2}/S} & \text{if } T^{*} \notin [T-S, T] \\ \gamma + \frac{\left(\sigma_{T^{*}}^{2} - \sigma_{\sim T^{*}}^{2}\right)/S + \sigma_{\sim T^{*}}^{2}}{\sigma_{\upsilon^{y}}^{2} + 2\sigma_{u^{y}}^{2}/S} & \text{if } T^{*} \in [T-S, T] \end{cases}, \end{aligned} \tag{11}$$

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

Equation (11) has three important implications. First, as S increases, the bias of the fixed effects estimator and the predicted positive relationship between income and democracy increases (and as S becomes smaller, $\mathrm{plim}\hat{\gamma}_{S}^{FE}$ approaches γ). This is because, as S increases, the non-persistent shocks to income, which are uncorrelated with democracy, become less important relative to the component correlated with democracy, υ_{it}^{y}, and this

reduces the denominator through the term $2\sigma^2_{u^y}/S$. Second, and more important, when the time span from $t = T - S$ to $t = T$ includes the critical juncture T^*, we expect a stronger positive relationship between income and democracy, since $\sigma^2_{T^*} - \sigma^2_{\sim T^*} > 0$. This is relevant in interpreting why we see a positive relationship between these two variables during some horizons but not others. Finally, under hypothesis 2, the bias of (11) will be reduced if we are able to control variables that proxy for or are correlated with the common component in $\upsilon^d_{iT^*}$ and $\upsilon^y_{iT^*}$ (in practice, historical determinants of divergent development paths).

6 Democracy and Income in the Long Run

6.1 Democracy and Income Over the past 160 Years

Although historical data are typically less reliable than postwar data, the Polity IV dataset extends back to the beginning of the 19th century (for countries that were then independent), and Maddison (2003) gives estimates of income for many countries from around 1820. Using these data, we construct a 5-yearly and a 10-yearly dataset between 1840 and 1940. Countries that gained and maintained independence before 1900 and have more than 5 observations in the 1840 to 1940 data period are included in this dataset. The result is an unbalanced panel with a country entering when there are observations from both Polity and Maddison.[19]

Table 9, Panel A reports some basic regressions with this dataset. In column 1, pooled cross-sectional OLS regressions again show the conventional result, with income per capita having a positive and significant sign. Column 2 adds fixed effects and similar to our results above, the coefficient estimate on income per capita becomes insignificant. Column 3 uses GMM and column 4 excludes lagged democracy, again with similarly insignificant results.

Columns 5 and 6 repeat the regressions from columns 2 and 3 using ten-year instead of five-year intervals. We use this strategy to check whether the lack of a significant effect of income on democracy is caused by measurement error or country noise in democracy over the five-year horizon, and also to investigate whether there could be an effect of income on democracy at lower frequencies than over 5 years. The basic results are identical to those in the first four columns of the table.

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

The conclusion from this investigation is that the pre-1940 evolution of countries in Europe and Latin America is similar to the results from the post-1960 sample. Once we control for fixed effects, there is no significant relationship between income per capita and democracy.

We further investigate these ideas in Panels B and C by taking the same sample of countries and extending the dataset to 2000 (thus constructing a 160-year panel with 28 countries). Without fixed effects, higher per capita income is again strongly associated with greater democracy. In this 160-year sample, there is also a positive coefficient on log GDP per capita when we control for fixed effects, though this relationship is not robust to estimation via GMM in the five-year sample. Figure 4 depicts the change in the Polity composite index versus the change in log GDP per capita between 1870 and 1995 (dates chosen to maximize sample size), and shows a positive but insignificant relationship between these dates.[20]

Further investigation suggests that the results in the 1840-2000 panel are partly driven by one country, Venezuela, especially the postwar correlation between income per capita and democracy in Venezuela.[21] Further investigation suggests that the correlation between the Venezuelan GDP and democracy over the relevant period is related to increases in oil revenues—not the type of income variation generally thought to promote democratization. In particular, there is a close association between oil income and democracy in Venezuela, but not between non-oil income and democracy (details are available upon request). Panel C therefore repeats the regressions in Panel B without Venezuela. In this case, though the estimate of the coefficient on income per capita remains positive, it is no longer statistically significant.

6.2 Democracy and Income Over the past 500 Years

Next we push the reasoning in equation (11) further. Our discussion of hypothesis 2 suggests that if we could construct a longer dataset, spanning the critical junctures for divergent development paths, we should obtain a stronger relationship. Although no existing dataset spans more than 160 years of political development, there exist rough estimates of income per capita for almost all areas of the world in 1500. Moreover, we

[20] The corresponding regression yields the coefficients of 0.076, with a standard error of 0.123. Parallel with our treatment of the very long run below, we also looked at the relationship over the period 1870-1995 assigning the lowest Polity score to countries without Polity data. We found only a small and marginally significant coefficient on income in the full sample, and a small and insignificant coefficient for former European colonies, which we discuss further below. Details are available upon request.

[21] In Tables 2-8, Venezuela did not have a disproportionate effect on the results because these regressions included a considerably larger set of countries than Table 9.

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 significant 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 (2004b) provide a coding of constraint on the executive for European countries (based on the Polity definition) going back to 1500 from various sources. It also appears reasonable to assume that constraint on the executive for non-European countries and the other components of the Polity index (competitiveness of executive recruitment, openness of executive recruitment, and competitiveness of political participation) both for European and non-European countries should take the lowest score in 1500. Based on this information, we can construct the Polity Composite index for 1500 (details available upon request). Combining these data with estimates of income per capita, we can get a glimpse of the relationship between income and democracy over this 500-year interval.

This is done in Figure 5 using Maddison's (2003) estimates of income per capita in 1500.[22] The figure shows a strong positive relationship between changes in democracy and changes in income for 143 countries (1995 is used as the end date in the plot and in Table 10 to maximize sample size). This plot corresponds to a fixed effect regression from 1500 to 1995 with only two time periods, of the form:

$$\Delta d_{i,1995-1500} = \gamma_0 + \gamma \Delta y_{i,1995-1500} + \Delta u_i.$$

This regression is reported in column 1 of Table 10, and yields a large and statistically significant estimate of γ, 0.139 (standard error = 0 .033). Column 2 estimates the same relationship this time assigning the lowest democracy score to all countries in 1500, and shows a very similar estimate. The rest of Table 10 will be discussed below.

Overall, the very long-run evidence shows an association between democracy and income, but this correlation is not present or quite weak when we look at samples of 100 years or so. We interpret this evidence as consistent with hypothesis 2. Hypothesis 1 would have suggested a strong effect even in the 100- or 140-year data sets. In contrast,

[22] For this exercise, when Maddison (2003) provides estimates for individual countries, we use these estimates. When he provides estimates for broad geographic areas, we assign this estimate to all countries now occupying these territories without country-level data. We take into account the cross-correlation in the right hand side variable by clustering the standard errors at the data aggregation level for each country.

hypothesis 2 would suggest the stronger relationship should emerge once we consider a time interval sufficiently long to span the critical junctures leading to the divergent development paths. In this case, the fact that a strong relationship emerges when we look at a 500-year period, but not when we only go back to the mid-19th century, is consistent with this view.

7 Understanding the Fixed Effects

We now directly investigate the potential determinants of divergent development paths. If we can pinpoint these potential determinants and their influence on the relationship between income and democracy, this would be evidence supporting hypothesis 2. Moreover, as suggested in Section 5, if we can directly control for these historical factors or their proxies, the positive correlation between income and democracy should weaken.

7.1 Divergent Development Paths Among the Colonies

Acemoglu, Johnson and Robinson (2001, 2002) document that factors affecting the profitability of different institutional structures for European colonizers had a major impact on early institutions, and on subsequent political and economic development. They emphasize that two factors affecting colonial strategies, and therefore the subsequent development paths, were the mortality rate faced by potential European settlers and the population density of indigenous peoples before colonization (in practice around 1500). Higher mortality rates discouraged Europeans from settling, and made an extractive strategy, associated with coercive and non-participatory institutions, more likely. More densely-settled areas also discouraged European settlements, and even conditional on settlements, encouraged the establishment of coercive institutions designed to control the indigenous population and to transfer resources from them.

We next use these ideas in the sample of former European colonies, where European intervention created a potentially exogenous source of divergence in political and economic development paths. We examine the effects of both the density of the indigenous population in 1500 (population density in 1500, for short) and of settler mortality.[23] We expect countries with high rates of settler mortality and higher indigenous population density in 1500 to have experienced greater extraction of resources and repression by Europeans,

[23]Population density in 1500 is calculated by dividing the historical measures of population from McEvedy and Jones (1975) by the area of arable land (see Acemoglu, Johnson and Robinson, 2002). Finally, data on settler mortality are from Acemoglu, Johnson and Robinson (2001), who constructed it based on research by Philip Curtin and other historians (e.g., Curtin, 1989, 1998, and Gutierrez, 1986).

and consequently to be less democratic today. However, both population density in 1500 and European settler mortality rates are subject to a large amount of measurement error, and are only some of the influences on the ultimate development path. For example, for various reasons, Europeans opted for extractive institutions in many areas, such as Brazil, with low population density. Therefore, a direct measure of institutions immediately after the end of the colonial period is also useful to gauge the effect of the historical conditions on current outcomes. For this reason, we look at the measure of constraint on the executive from the Polity IV dataset right at (soon after) independence for each former colony, measured as the average score during the first ten years after independence.[24] This is the closest variable we have to a measure of institutions during colonialism. We normalize this score to a 0 to 1 scale like democracy, with 1 representing the highest constraint on the executive.

Finally, we also control for the date of independence. This is useful because constraint on the executive at different dates of independence may mean different things, so it is important to control for the date of independence. In addition and potentially more important, countries where Europeans settled and developed secure property rights and more democratic institutions typically gained their independence earlier than colonies with extractive institutions. Another important effect of the date of independence on political and economic development might be that former colonies undergo a relatively lengthy period of instability after independence, adversely affecting both growth prospects and democracy.

7.2 Historical Variables and Fixed Effects

Figures 6-9 show that the fixed effects in the democracy regressions are closely linked to the historical determinant and proxies for divergent development paths among the former European colonies.[25] In particular, they show a strong association between the fixed effects, and respectively, settler mortality rates, the density of the population in 1500, constraint on the executive at (shortly after) independence, and year of independence. These figures suggest that the fixed effects are indeed related to the conditions which contributed to the divergent development paths of these countries.

[24] 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. In our former colonies sample only one country, the United States became independent before 1800. The United States broke with Britain in 1776 and was recognized as a new nation following the Treaty of Paris in 1783. We code the U.S. date of independence as 1800.

[25] These fixed effects are from the regression for the former colonies sample in column 2 of Table 11.

Tables 11 further documents this point using regression evidence. The first two columns show the pooled OLS and fixed effects relationship between income and democracy in the sample of former European colonies.[26] As in the other samples, there is a strong cross sectional relationship between income and democracy, but no evidence of a causal effect with fixed effects in the sample of former European colonies.

The remaining columns of Tables 11 show that when the four proxies for the evolution of the development path are included in regressions *without* fixed effects, the influence of income per capita on democracy is substantially weakened and in many specifications, it disappears. For example, when all four of settler mortality, population density in 1500, constraint on executive at independence and independence year are included in column 6, or when only the last three variables are included, there is no longer a significant effect of income per capita on democracy.[27] Table A3 in the Appendix shows similar results using the Polity data set.

These results suggest that the fixed effects, which account for the cross-sectional relationship between income and democracy, are closely related to colonial history in the sample of former European colonies. As such, they lend support to hypothesis 2, which emphasizes the importance of divergent economic and political development paths.

7.3 Revisiting the Very Long Run

In the light of these findings, we now return to the positive relationship in the 500-year sample shown in Table 10. We have so far argued that this relationship was consistent with hypothesis 2, since it was precisely during this period that countries embarked upon divergent development paths. A further check on hypothesis 2 would be that when we include the historical variables emphasized in the previous section, the 500-year relationship between income and democracy in the former European colonies sample should again

[26] We adopt the definition of former European colonies used in Acemoglu, Johnson and Robinson (2001, 2002), which excludes the Middle Eastern countries that were briefly colonized by European powers during the 20th century. This definition is motivated by our interest in former colonies as a sample where the process of institutional development, in particular during the 19th century and earlier, was shaped by European intervention (see Acemoglu, Johnson and Robinson, 2002). The results are robust to including the Middle Eastern countries.

[27] We also investigated whether other time-invariant characteristics that have been emphasized in the previous literature, in particular, identity of the colonizer, legal origin, religion or latitude, have explanatory power for democracy once the fixed effects are removed in the sample of former colonies. We found that none of them have any effect on democracy when the historical determinants emphasized in this section are included in the regressions. Furthermore, we added more lags of democracy on the right hand side, and constraint on the executive at independence remained significant, suggesting that this variable does not capture some slow dynamics in democracy, but is instead related to the political and economic development path of the former colonies. Results are available upon request.

weaken or even disappear. This is investigated in the rest of Table 10.

Column 3 shows a similar relationship between change in income and change in democracy to that shown in Figure 5 and in column 1 among the former European colonies. The remaining columns show that the positive relationship disappears when we include the historical variables emphasized in the previous section. For example, in column 7 when all four of population density in 1500, settler mortality, constraint on executive at independence and independence year are included, the coefficient on $\Delta y_{1500,1995}$ becomes essentially zero (-0.006, standard error = 0.054) instead of 0.126 in column 3. This is illustrated in Figure 10 which depicts the relationship between changes in democracy and changes in income in former colonies conditional on these four historical factors. This evidence also supports hypothesis 2.

8 Economic Crises and Democracy

Our analysis so far shows that income seems to have little predictive power for the over-time variation (at the five- or ten-year frequencies) in democracy in the postwar sample. In this section, we document that there are some predictable movements in changes in democracy.

A number of theories, including Haggard and Kaufman (1995) and Acemoglu and Robinson (2004), emphasize economic crises as events destabilizing both democratic and nondemocratic regimes, and leading to regime transitions. The overall effect on the level of democracy is ambiguous, and depends on whether the destabilizing effect on democracies or nondemocracies is greater.[28]

We investigate this issue in Table 12. We define an economic crisis as a sudden and sharp decline in growth relative to five years ago. More specifically, there is an economic crisis at time $t-1$ if the change in the five-yearly average growth rate of GDP per capita in any year in the five-year period between $t-1$ and $t-2$ is less than a certain threshold $-\overline{\Delta y}$.[29] We choose the threshold $\overline{\Delta y}$ as 3 percent (columns 1 and 2), 4 percent (columns 3 and 4) or 5 percent (columns 5 and 6). These thresholds are motivated by the fact that

[28] Various empirical papers in the political science literature, for instance Gasiorowski (1995) and Przeworski et al. (2000) have investigated the impact of crises on regime transitions. However, this research has not attempted to control for omitted variables or for the endogeneity of income.

[29] Recall that the time period, t, refers to five year intervals. The years s between $t-2$ and $t-1$ are $s \in [5t-10, 5t-6]$. We denote the forward average five-year growth rate in year s by $\Delta\tilde{y}_s = \sum_{j=1}^{5} \Delta y_{s+j}/5$, and define a crisis during year s as occuring if $\Delta\tilde{y}_s - \Delta\tilde{y}_{s-5} < -\overline{\Delta y}$. This means that we need 15 years of data on income per capita preceding the democracy observation, and our sample in Table 12 starts in 1965.

one standard deviation of this variable is equal to 4 percent.

The dependent variable in Panel A is the Freedom House democracy score. The results in Panel A show that, again contrary to the predictions of modernization theory, economic crises make democracy *more likely*. This result at first looks paradoxical. How could crises make democracy more likely?

Panels B and C show why this is so: economic crises have a small, and typically not statistically significant, effect on transitions away from democracy, but a large effect on transitions away from nondemocracy to democracy. In other words, economic crises cause dictatorships to fall, thus increasing the likelihood of democracy. To document this, we take a simple approach and create two variables measuring movements towards and away from democracy. In particular, equation (1) is modified as follows:

$$d_{it}^{+} = \alpha d_{it-1} + \gamma y_{it-1} + \mathbf{x}_{it-1}'\boldsymbol{\beta} + \mu_t + \delta_i + u_{it} \tag{12}$$

for transitions to democracy and

$$d_{it}^{-} = \alpha d_{it-1} + \gamma y_{it-1} + \mathbf{x}_{it-1}'\boldsymbol{\beta} + \mu_t + \delta_i + u_{it} \tag{13}$$

for transitions from democracy, where $d_{it}^{+} = \max\{d_{it}, d_{it-1}\}$ and $d_{it}^{-} = \min\{d_{it}, d_{it-1}\}$.[30] This procedure implies that for d_{it}^{+}, we only consider upward movements in the democracy score and ignore declines in democracy, whereas d_{it}^{-} only considers deterioration in the democracy score. A full analysis of democratic transitions is beyond the scope of this paper. For our purposes here, it is sufficient to note that estimation of these equations with fixed effects (OLS or GMM) finds no evidence that income per capita has a causal effect on either transitions to or transitions away from democracy. However, these estimates suggest that economic crises which enter into $\mathbf{x}_{it-1}'$ do have an effect on transitions to democracy.[31]

Using these definitions, in Panels B and C, we investigate the relationship between economic crises and transitions to and away from democracy. The estimates show that economic crises make dictatorships more likely to transition to democracy, but have a much smaller effect on transitions away from democracy. These two results combined together explain why economic crises are found to lead to greater democracy in Panel A.[32]

[30] Although (12) and (13) are non-linear equations, the fixed effects, the δ_i's, enter additively and can be differenced out to achieve consistent estimation.

[31] We did not find any robust results using this definition of crises on the 160 year sample discussed in Section 6.

[32] A natural question is whether growth accelerations have a similar effect on regime change. A recent

9 Conclusion

The conventional wisdom in the political economy literature 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. These results shed considerable doubt on the conventional wisdom both in the academic literature and in the popular press that income causes democracy, and a general increase in income per capita will bring improvements in institutions.

This result immediately leads to an important question: why are richer countries more democratic 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, in the sample of former European colonies where we have good measures of the historical sources of variation in development paths, we showed that the fixed effects indeed capture the impact of these historical differences.

In emphasizing the importance of historical development paths, we do not want to suggest that there is a historical determinism in political institutions. The fixed effects in the regressions, and the development paths in theory, create a tendency, but many other factors influence equilibrium political institutions. In the last section, we showed how severe economic crises lead to the collapse of dictatorships, making democracy more likely. 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.

paper by Hausmann, Pritchett and Rodrik (2004) emphasizes the importance of growth accelerations. We also investigated the effect of growth accelerations in a symmetric way to economic crises by creating a dummy for acceleration if the change in the five-yearly average growth rate of GDP per capita in any year between $t-2$ and $t-1$ is above a certain threshold $\overline{\Delta y}$ (again 3 percent, 4 percent or 5 percent). None of these specifications showed any effect of growth accelerations on democracy.

10 Appendix

This Appendix describes the construction of the trade-weighted world income instrument used in Section 4. First, we describe the instrument constructed from actual trade shares. Second, we describe the instrument derived from gravity-predicted trade shares. Finally, we describe our method of adjusting for the potential bias caused by the general equilibrium nature of equation (6).

First, we 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 (7) as follows. Define $I_{jt-1} = \{0, 1\}$ as an indicator for Y_{jt-1} being available in the dataset. Then

$$\widehat{Y}_{it-1} = \zeta \left(\sum_{j=1, j \neq i}^{N} \omega_{ij} I_{jt-1} Y_{jt-1} \right) \left(\frac{\sum_{j=1, j \neq i}^{N} \omega_{ij}}{\sum_{j=1, j \neq i}^{N} I_{jt-1} \omega_{ij}} \right), \tag{14}$$

where Y_{jt-1} is log income as before. The third term in (14) 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 (14), 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}.

Second, we generate an alternate measure of Ω, Ω^G, from the following gravity equation

as in Frankel and Romer (1999):

$$\log(\omega_{ij}) = \beta_0 + \beta_1 I_{ij}^B + \beta_2 Z_{ij} + \beta_3 I_{ij}^B Z_{ij} + \epsilon_{ij}, \tag{15}$$

where Z_{ij} is a vector of country-pair level covariates, I_{ij}^B is a dummy variable which equals 1 if the two countries share a land border, and ϵ_{ij} is an error term where $E(\epsilon_{ij}) = 0$. Z_{ij} includes the following covariates: dummy if i is landlocked, dummy if j is landlocked, log distance between i and j, log area of i, log area of j, log population of i, and log population of j.[33] We estimate (15) using OLS. With our estimates of $\widehat{\beta}_0$, $\widehat{\beta}_1$, $\widehat{\beta}_2$, and $\widehat{\beta}_3$, we construct $\Omega^G = \left[\omega_{ij}^G\right]_{i,j}$ where:

$$\omega_{ij}^G = \exp\left(\widehat{\beta}_0 + \widehat{\beta}_1 I_{ij}^B + \widehat{\beta}_2 Z_{ij} + \widehat{\beta_3} I_{ij}^B Z_{ij}\right),$$

We can use ω_{ij}^G in an analogous fashion to build an alternate instrument $\widehat{Y}_{it-1}^G$ now using gravity estimated weights:

$$\widehat{Y}_{it-1}^G = \zeta \left(\sum_{j=1,j\neq i}^{N} \omega_{ij}^G Y_{jt-1}\right)\left(\frac{\sum_{j=1,j\neq i}^{N} \omega_{ij}}{\sum_{j=1,j\neq i}^{N} I_{jt-1}\omega_{ij}^G}\right), \tag{16}$$

where second and third terms of (16) represent a gravity-weighted average of world income where the sum of the weights is actual trade openness $\sum_{j=1,j\neq i}^{N} \omega_{ij}$.[34]

Finally, for the adjustment in (9), we estimate (6) using actual trade shares in a specification which includes a full set of time and country dummies and using the same sample in the second stage in Table 7 column 4:

$$Y_{it-1} = \zeta \sum_{j=1,j\neq i}^{N} \omega_{ij} Y_{jt-1} + \varepsilon_{it-1}, \text{ where } \varepsilon_{it-1} = \mu_{t-1}^0 + \delta_i^0 + u_{it-1}^0.$$

This provides us with an estimate of $\hat{\zeta}$ and $\hat{\varepsilon}_{it-1}$. We then use (9) to create an adjusted instrument. In inverting the matrix $\left(I - \hat{\zeta}\Omega\right)$, we drop the ith row and ith column of Ω whenever ω_{ij} is unavailable for any j.

[33] As in Frankel and Romer (1999), we constrain the coefficients on the two landlocked dummies to be the same. For this exercise, area is from CIA (2004). Population is the average log population from 1980 to 1989 from WDI (2002). Landlocked, contiguity, and distance are from Glick and Rose (2002). Detailed results are available upon request.

[34] Results are similar if the sum of the weights is predicted openness. Results are available upon request.

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Table 1
Descriptive Statistics

	All countries	Former Colonies	High Income Countries	Low Income Countries
	(1)	(2)	(3)	(4)
Freedom House Measure of Democracy$_t$	0.57	0.47	0.78	0.36
	(0.36)	(0.34)	(0.30)	(0.30)
Polity Measure of Democracy$_t$	0.57	0.50	0.80	0.36
	(0.38)	(0.35)	(0.30)	(0.31)
Log GDP per Capita $_{t-1}$	8.16	7.85	9.02	7.30
(Chain Weighted 1996 Prices)	(1.02)	(0.89)	(0.56)	(0.53)
Log GDP per Capita $_{t-1}$	7.92	7.62	8.84	7.08
(1990 dollars)	(1.05)	(0.93)	(0.61)	(0.55)
Savings Rate $_{t-2}$	0.17	0.16	0.22	0.11
	(0.13)	(0.14)	(0.10)	(0.14)
Trade-Weighted World Log GDP $_{t-1}$	11.61	11.09	13.02	10.28
	(8.43)	(9.30)	(9.83)	(6.60)
Constraint on the Executive at Independence		0.38		
		(0.35)		
Independence Year		1911		
		(65)		
Log Population Density in 1500		0.41		
		(1.60)		
Log Settler Mortality		4.75		
		(1.20)		
Observations	945	541	473	472

Values are averages during sample period, with standard deviations in parentheses. All countries are those for which democracy, lag democracy, and lag income in five-year intervals are available at least once during 1960-2000, for the Freedom House measure of democracy. A country must be independent for at least 5 years. Column 1 refers to the sample in the regression in Table 2, column 1. Column 2 refers to the sample in Table 11 column 1. Columns 3 and 4 split the sample in column 1 by the median income (from Penn World Tables 6.1) in the sample of column 1. Freedom House Measure of Democracy is the Political Rights Index, augmented following Barro (1999). Polity Measure of Democracy is Democracy Index minus Autocracy Index from Polity IV. GDP per capita in 1996 prices with PPP adjustment is from the Penn World Tables 6.1; GDP per capita in 1990 Geary-Khamis dollars is from Maddison (2003). 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 World log GDP is constructed as in equation (7) using data from IMF Direction of Trade Statistics (2005) and Penn World Tables 6.1. Constraint on the Executive at Independence is from Polity. Year of independence is from the CIA World Factbook. Log Population Density in 1500 is from Acemoglu, Johnson, and Robinson (2002). Log Settler Mortality is from Acemoglu, Johnson, and Robinson (2001). Former colonies is the subsample colonized by European powers before 1900. For detailed definitions and sources, see Appendix Table A1.

Table 2

Fixed Effects Results using Freedom House Measure of Democracy

	Base Sample, 1960-2000							
	5-year data					Annual data	10-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
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Dependent Variable is Democracy							
Democracy $_{t-1}$	0.706	0.379	0.469	0.489		[0.00]	-0.025	0.226
	(0.035)	(0.051)	(0.100)	(0.085)			(0.088)	(0.123)
Log GDP per Capita $_{t-1}$	0.072	0.010	-0.104	-0.129	0.054	[0.33]	0.053	-0.318
	(0.010)	(0.035)	(0.107)	(0.076)	(0.046)		(0.066)	(0.180)
Hansen J Test				[0.26]				[0.07]
AR(2) Test				[0.45]				[0.96]
Observations	945	945	838	838	958	2895	457	338
Countries	150	150	127	127	150	148	127	118
R-squared	0.73	0.80			0.76	0.93	0.77	

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, and 7, with country dummies and robust standard errors clustered by country in parentheses. 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 augmented Freedom House Political Rights Index. 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. 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	
	Pooled OLS	Fixed Effects OLS	Anderson-Hsiao IV	Arellano-Bond GMM	Fixed Effects OLS	Fixed Effects OLS	Fixed Effects OLS	Arellano-Bond GMM
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Dependent Variable is Democracy							
Democracy $_{t-1}$	0.749	0.449	0.582	0.590		[0.00]	0.060	0.309
	(0.034)	(0.063)	(0.127)	(0.106)			(0.091)	(0.134)
Log GDP per Capita $_{t-1}$	0.053	-0.006	-0.413	-0.351	-0.011	[0.53]	0.007	-0.368
	(0.010)	(0.039)	(0.163)	(0.127)	(0.055)		(0.070)	(0.190)
Hansen J Test				[0.03]				[0.01]
AR(2) Test				[0.39]				[0.38]
Observations	854	854	747	747	880	3701	419	302
Countries	136	136	114	114	136	134	114	107
R-squared	0.77	0.82			0.77	0.96	0.77	

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, and 7, with country dummies and robust standard errors clustered by country in parentheses. 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 Composite Democracy Index. 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. In column 6, each right hand side variables has five annual lags; we report the p-value from an F-test for the joint significance of all 5 lags. For detailed data definitions and sources see Table 1 and Appendix Table A1.

Table 4

Fixed Effects Results using Freedom House Measure of Democracy: Alternate Samples

	Balanced Panel, 1970-2000			Base Sample, 1960-2000, without Sub-Saharan African Countries			Base Sample, 1960-2000, without Muslim Countries			Base Sample, 1960-2000, without Former Socialist Countries		
	5-year data		Annual data	5-year data		Annual data	5-year data		Annual data	5-year data		Annual data
	Fixed Effects OLS	Arellano-Bond GMM	Fixed Effects OLS	Fixed Effects OLS	Arellano-Bond GMM	Fixed Effects OLS	Fixed Effects OLS	Arellano-Bond GMM	Fixed Effects OLS	Fixed Effects OLS	Arellano-Bond GMM	Fixed Effects OLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Dependent Variable is Democracy											
Democracy $_{t-1}$	0.283	0.472	[0.00]	0.410	0.520	[0.00]	0.397	0.563	[0.00]	0.362	0.436	[0.00]
	(0.058)	(0.092)		(0.060)	(0.096)		(0.051)	(0.080)		(0.052)	(0.085)	
Log GDP per Capita $_{t-1}$	-0.031	-0.262	[0.52]	0.069	0.044	[0.80]	0.030	-0.070	[0.54]	0.005	-0.151	[0.25]
	(0.049)	(0.128)		(0.043)	(0.084)		(0.035)	(0.074)		(0.035)	(0.078)	
Hansen J Test		[0.40]			[0.31]			[0.25]			[0.34]	
AR(2) Test		[0.73]			[0.83]			[0.91]			[0.49]	
Observations	630	567	2328	660	591	1943	774	693	2358	908	823	2766
Countries	90	81	97	107	85	105	122	104	122	128	124	127
R-squared	0.80		0.93	0.78		0.93	0.81		0.94	0.79		0.93

Fixed effects OLS regressions in columns 1, 3, 4, 6, 7, 9, 10, and 12 with country dummies and robust standard errors clustered by country in parentheses. Columns 2, 5, 8, and 11 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. Dependent variable is augmented Freedom House Political Rights Index. 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); columns 3, 6, 9, and 12 use annual data from the same sample; a country must be independent for 5 years before it enters panel. In columns 3, 6, 9, and 12 each right hand side variables has five annual lags; values in brackets are the p-value from an F-test of the joint significance of all 5 lags. Columns 1-3 use a balanced panel from 1970 to 2000. Columns 4-6 exclude sub-Saharan African countries. Columns 7-9 exclude countries where the percent of the population which is Muslim in 1980 exceeds 40 percent. Columns 10-12 exclude Soviet bloc countries. For detailed data definitions and sources see Table 1 and Appendix Table A1.

Table 5
Fixed Effects Results using Freedom House Measure of Democracy: Alternate Covariates

	Base Sample, 1960-2000								
	5-year data								
	Pooled OLS (1)	Fixed Effects OLS (2)	Arellano-Bond GMM (3)	Pooled OLS (4)	Fixed Effects OLS (5)	Arellano-Bond GMM (6)	Pooled OLS (7)	Fixed Effects OLS (8)	Arellano-Bond GMM (9)
	Dependent Variable is Democracy								
Democracy $_{t-1}$	0.687	0.353	0.480	0.625	0.351	0.499	0.619	0.352	0.475
	(0.039)	(0.053)	(0.087)	(0.044)	(0.055)	(0.097)	(0.042)	(0.050)	(0.088)
Log GDP per Capita $_{t-1}$	0.066	0.015	-0.008	0.047	-0.001	-0.121	0.061	-0.042	-0.126
	(0.014)	(0.041)	(0.139)	(0.020)	(0.049)	(0.182)	(0.017)	(0.045)	(0.130)
Log Population $_{t-1}$	0.001	-0.109	-0.001	-0.001	-0.042	0.049	0.005	-0.070	-0.016
	(0.005)	(0.100)	(0.113)	(0.006)	(0.108)	(0.143)	(0.006)	(0.112)	(0.163)
Education $_{t-1}$				0.012	-0.007	-0.020	0.022	-0.006	-0.011
				(0.005)	(0.020)	(0.026)	(0.007)	(0.038)	(0.052)
Age Structure $_{t-1}$	[0.00]	[0.05]	[0.63]	[0.06]	[0.19]	[0.27]			
Barro (1999) Covariates	NO	NO	NO	NO	NO	NO	YES	YES	YES
Hansen J Test			[0.08]			[0.15]			[0.25]
AR(2) Test			[0.43]			[0.88]			[0.75]
Observations	863	863	731	676	676	589	676	676	588
Countries	142	142	120	95	95	92	96	96	92
R-squared	0.72	0.80		0.70	0.77		0.70	0.76	

Pooled cross-sectional OLS regressions in columns 1, 4, and 7, with robust standard errors clustered by country in parentheses. Fixed effects OLS regressions in columns 2, 6, and 8, with country dummies and robust standard errors clustered by country in parentheses. GMM of Arellano-Bond in columns 3, 6, and 9, with robust standard errors; in this method we instrument for income using a double lag. Year dummies are included in all regressions. Dependent variable is augmented Freedom House Political Rights Index. 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. Education is average years of total schooling in the population in columns 4-6. Education is average years of primary schooling in the population in columns 7-9. Columns 1-6 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 7-9 include but do not display additional covariates used by Barro (1999): male-female education gap and urbanization rate, and significant oil producer dummy (the last is in column 7 only). 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 Savings Rate Instrument

	Base Sample, 1960-2000									
	All Countries						Without Sub-Saharan Africa	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	Fixed Effects 2SLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
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	-0.045	-0.036	-0.074	0.016
	(0.013)	(0.051)	(0.038)	(0.094)	(0.081)	(0.102)	(0.130)	(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		1.483	1.202	1.173	1.022
				(0.277)	(0.270)		(0.320)	(0.315)	(0.254)	(0.218)
Savings Rate $_{t-3}$										0.720
										(0.182)
Hansen J Test						[0.34]				
AR(2) Test						[0.72]				
Observations	900	900	891	900	891	764	627	471	733	796
Countries	134	134	134	134	134	124	92	98	124	125
R-squared in First Stage				0.96	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. Dependent variable is augmented Freedom House Political Rights Index. 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. Column 7 excludes sub-Saharan African countries. Columns 4-10 instrument for Log GDP per Capita$_{t-1}$ with Savings Rate $_{t-2}$. Column 10 includes Savings Rate $_{t-3}$ as an additional instrument. Column 9 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 7

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						Without Sub-Saharan Africa	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	Fixed Effects 2SLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
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	-0.219	-0.202	-0.198	-0.217
	(0.013)	(0.045)	(0.034)	(0.150)	(0.105)	(0.077)	(0.178)	(0.130)	(0.160)	(0.149)
Trade-Weighted World 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 World Democracy $_{t}$								-1.195		
								(0.959)		
Trade-Weighted World Log GDP $_{t-1}$				0.402	0.421		0.320	0.441		0.529
				(0.083)	(0.082)		(0.063)	(0.070)		(0.180)
Trade-Weighted World Log GDP $_{t-2}$									0.341	-0.127
									(0.090)	(0.206)
Hansen J Test						[0.19]				
AR(2) Test						[0.50]				
Observations	906	906	895	906	895	812	616	906	906	906
Countries	124	124	124	124	124	122	81	124	124	124
R-squared in First Stage				0.95	0.96		0.95	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, 9 and 10 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 difference equation with the first difference of the instrument. Year dummies are included in all regressions. Dependent variable is augmented Freedom House Political Rights Index. 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. Column 7 excludes sub-Saharan African countries. Columns 5-8 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 alternative instrument. Column 10 includes both Trade-Weighted World Log GDP$_{t-1}$ and Trade-Weighted World Log GDP $_{t-2}$ as 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 8

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

	Base Sample, 1960-2000 without Singapore			Base Sample, 1960-2000					
	Fixed Effects 2SLS (1)	Fixed Effects 2SLS (2)	Arellano-Bond GMM (3)	Fixed Effects 2SLS (4)	Fixed Effects 2SLS (5)	Arellano-Bond GMM (6)	Fixed Effects 2SLS (7)	Fixed Effects 2SLS (8)	Arellano-Bond GMM (9)
Panel A	*Dependent Variable is Democracy*								
Democracy $_{t-1}$		0.410	0.482		0.393	0.478		0.394	0.478
		(0.069)	(0.093)		(0.057)	(0.094)		(0.057)	(0.095)
Log GDP per Capita $_{t-1}$	-0.348	-0.215	-0.124	-0.213	-0.119	-0.133	-0.237	-0.127	-0.134
	(0.326)	(0.217)	(0.079)	(0.151)	(0.105)	(0.078)	(0.168)	(0.107)	(0.082)
Panel B	*First Stage for Log GDP per Capita $_{t-1}$*								
Democracy $_{t-1}$		0.170			0.169			0.173	
		(0.063)			(0.063)			(0.063)	
Trade-Weighted World Log GDP $_{t-1}$	0.394	0.422							
	(0.182)	(0.182)							
Trade-Weighted World Log GDP $_{t-1}$ with 2 Step Adjustment for Bias				0.403	0.421				
				(0.085)	(0.083)				
Trade-Weighted World Log GDP $_{t-1}$ with Gravity Estimated Weights							0.396	0.418	
							(0.101)	(0.099)	
Hansen J Test			[0.30]			[0.19]			[0.22]
AR(2) Test			[0.50]			[0.50]			[0.50]
Observations	899	888	805	906	895	812	906	895	812
Countries	123	123	121	124	124	122	124	124	122
R-squared in First Stage	0.95	0.95		0.95	0.96		0.95	0.96	

Fixed effects 2SLS regressions in columns 1, 2, 4, 5, 7 and 8 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 columns 3, 6, and 9 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. Dependent variable is augmented Freedom House Political Rights Index. 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. Columns 1-3 exclude Singapore. Columns 1-3 instrument for Log GDP per Capita $_{t-1}$ with Trade-Weighted World Log GDP $_{t-1}$. Columns 4-6 instrument for Log GDP per Capita $_{t-1}$ with Trade-Weighted World Log GDP $_{t-1}$ with a 2 Step Adjustment for Bias; this is constructed using the same sample as Table 7, column 4 and following a procedure described in Section 4.2 of the text and in the Appendix. Columns 7-9 instrument for Log GDP per Capita $_{t-1}$ with Trade-Weighted World Log GDP $_{t-1}$ with Gravity Estimated Weights (using Frankel and Romer, 1999 specification). For detailed data definitions and sources see Table 1 and Appendix Table A1. See Appendix for details on the construction of the instruments.

Table 9
Polity Measure of Democracy in the Long Run

	5-year data				10-year data	
	Pooled OLS	Fixed Effects OLS	Arellano-Bond GMM	Fixed Effects OLS	Fixed Effects OLS	Arellano-Bond GMM
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All Countries	*Dependent Variable is Democracy, 1840-1940*					
Democracy $_{t-1}$	0.841	0.652	0.433		0.455	0.230
	(0.039)	(0.089)	(0.121)		(0.136)	(0.238)
Log GDP per Capita $_{t-1}$	0.072	-0.076	-0.183	-0.109	-0.035	-0.092
	(0.017)	(0.064)	(0.090)	(0.107)	(0.135)	(0.287)
Hansen J Test			[1.00]			[1.00]
AR(2) Test			[0.40]			[0.05]
Observations	358	358	297	370	188	142
Countries	28	28	27	28	28	28
R-squared	0.78	0.81		0.69	0.75	
Panel B: All Countries	*Dependent Variable is Democracy, 1840-2000*					
Democracy $_{t-1}$	0.772	0.682	0.443		0.407	0.287
	(0.043)	(0.070)	(0.096)		(0.082)	(0.104)
Log GDP per Capita $_{t-1}$	0.081	0.066	-0.041	0.135	0.134	0.176
	(0.015)	(0.032)	(0.033)	(0.072)	(0.070)	(0.112)
Hansen J Test			[1.00]			[1.00]
AR(2) Test			[0.83]			[0.93]
Observations	656	656	578	682	342	285
Countries	28	28	28	28	28	28
R-squared	0.79	0.80		0.63	0.70	
Panel C: Without Venezuela	*Dependent Variable is Democracy, 1840-2000*					
Democracy $_{t-1}$	0.771	0.673	0.427		0.400	0.244
	(0.045)	(0.074)	(0.099)		(0.085)	(0.105)
Log GDP per Capita $_{t-1}$	0.078	0.041	-0.058	0.104	0.076	0.099
	(0.015)	(0.030)	(0.034)	(0.079)	(0.068)	(0.147)
Hansen J Test			[1.00]			[1.00]
AR(2) Test			[0.91]			[0.96]
Observations	637	637	562	662	331	276
Countries	27	27	27	27	27	27
R-squared	0.78	0.80		0.63	0.69	

Pooled cross-sectional OLS regression in column 1, with robust standard errors clustered by country in parentheses. Fixed effects OLS regressions in columns 2, 4, and 5, with country dummies and robust standard errors clustered by country in parentheses. Column 3 and 6 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 Composite Democracy Index. Base sample is all countries with at least 5 observations in the 5-year data between 1840 and 1900 and which are independent by 1900. Columns 1-4 use 5-year data where the start date of the panel refers to the dependent variable (i.e., t=1840, so t-1=1835); a country must be independent for 5 years before it enters the panel. Columns 5 and 6 use 10-year data where the start date of the panel refers to the dependent variable (i.e., t=1840, so t-1=1830); a country must be independent for 10 years before it enters the panel. Panel A uses a long sample, 1840-1940, Panels B and C use 1840-2000. Panel C drops Venezuela. GDP per capita is from Maddison (2003). For detailed data definitions and sources see Table 1 and Appendix Table A1.

Table 10
Polity Measure of Democracy in the Very Long Run

	Base Sample, 1500-1995		Former European Colonies, 1500-1995					
	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Dependent Variable is Change in Democracy Over Sample Period							
		All countries in 1500 assumed to have the lowest possible democracy score						
Change in Log GDP per Capita Over Sample Period	0.139	0.148	0.126	0.097	0.065	0.048	-0.006	-0.002
	(0.033)	(0.035)	(0.025)	(0.019)	(0.042)	(0.024)	(0.054)	(0.047)
Constraint on the Executive at Independence						0.131	0.139	0.154
						(0.075)	(0.109)	(0.077)
Independence Year/100						-0.222	-0.211	-0.204
						(0.020)	(0.028)	(0.013)
Log Population Density in 1500					-0.066		-0.047	-0.057
					(0.042)		(0.043)	(0.044)
Log Settler Mortality				-0.053			-0.030	
				(0.016)			(0.030)	
Observations	143	143	78	65	78	78	65	78
R-squared	0.17	0.18	0.15	0.23	0.22	0.28	0.41	0.33

Cross-section OLS regression in all columns, with robust standard errors clustered by aggregation level of the region for GDP per Capita in 1500. In columns 3-8 data represents all available values in the former colonies sample for which Constraint on the Executive at Independence, Independence Year, and Log Population Density in 1500 are available. Changes are total differences between 1500 and 1995. GDP per capita is from Maddison (2003), and democracy is calculated using the Polity Composite Democracy Index, which comprises in part constraint on the executive. Column 1 assumes some democracy in 1500 in a few European countries, following Acemoglu et al (2004b). Democracy in 1500 is assigned to lowest possible score for all countries in columns 2-8. For detailed data definitions and sources see Table 1 and Appendix Table A1.

Table 11

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

	Former European Colonies							
	Unbalanced Panel, 1960-2000							Balanced Panel 1970-2000
	Pooled OLS	Fixed Effects	Pooled OLS					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Dependent Variable is Democracy							
Democracy $_{t-1}$	0.672	0.283	0.636	0.668	0.573	0.538	0.567	0.471
	(0.048)	(0.062)	(0.050)	(0.050)	(0.047)	(0.049)	(0.047)	(0.064)
Log GDP per Capita $_{t-1}$	0.059	-0.088	0.039	0.046	0.025	0.016	0.012	0.008
	(0.014)	(0.050)	(0.017)	(0.015)	(0.013)	(0.018)	(0.014)	(0.020)
Constraint on the Executive at Independence					0.170	0.178	0.175	0.212
					(0.030)	(0.030)	(0.028)	(0.036)
Independence Year/100					-0.101	-0.101	-0.098	-0.135
					(0.016)	(0.018)	(0.016)	(0.022)
Log Population Density in 1500				-0.013		-0.011	-0.013	-0.019
				(0.007)		(0.007)	(0.006)	(0.010)
Log Settler Mortality			-0.033			-0.009		
			(0.011)			(0.010)		
Observations	541	541	486	541	541	486	541	336
Countries	80	80	68	80	80	68	80	48
R-squared	0.62	0.72	0.63	0.62	0.65	0.66	0.65	0.63

Pooled cross-sectional OLS in columns 1 and 3-8, with robust standard errors clustered by country in parentheses. Fixed effects OLS regressions in column 2, with country dummies and robust standard errors clustered by country in parentheses. Year dummies are included in all regressions. Dependent variable is augmented Freedom House Political Rights Index. Data are at 5-year intervals, for former European colonies. Columns 1-7 are an unbalanced panel 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 10 years before it enters the panel. Column 8 is a balanced panel from 1970 to 2000; a country must be independent by 1960 to enter the panel and first lagged value is 1965. In all columns, data represents all available values in the former colonies sample for which Constraint on the Executive at Independence, Independence Year, and Log Population Density in 1500 are available. For detailed data definitions and sources see Table 1 and Appendix Table A1.

Table 12
Fixed Effects Results for Crises, using Freedom House Measure of Democracy

	Base Sample, 1965-2000					
	Fixed Effects OLS	Arellano-Bond GMM	Fixed Effects OLS	Arellano-Bond GMM	Fixed Effects OLS	Arellano-Bond GMM
	(1)	(2)	(3)	(4)	(5)	(6)
	Crisis threshold is -3%		Crisis threshold is -4%		Crisis threshold is -5%	
Panel A	*Dependent Variable is Democracy*					
Democracy $_{t-1}$	0.347	0.493	0.356	0.495	0.354	0.496
	(0.056)	(0.090)	(0.056)	(0.089)	(0.055)	(0.089)
Log GDP per Capita $_{t-1}$	-0.003	-0.141	0.003	-0.148	0.009	-0.155
	(0.047)	(0.088)	(0.047)	(0.091)	(0.047)	(0.092)
Crisis $_{t-1}$	0.044	0.057	0.059	0.063	0.074	0.096
	(0.019)	(0.024)	(0.021)	(0.026)	(0.025)	(0.029)
Hansen J Test		[0.41]		[0.46]		[0.58]
AR(2) Test		[0.73]		[0.57]		[0.74]
R-squared	0.79		0.79		0.79	
Panel B	*Dependent Variable is Transition toward Democracy*					
Democracy $_{t-1}$	0.602	0.697	0.609	0.698	0.607	0.699
	(0.038)	(0.062)	(0.039)	(0.062)	(0.038)	(0.062)
Log GDP per Capita $_{t-1}$	-0.004	-0.067	0.001	-0.071	0.005	-0.079
	(0.034)	(0.062)	(0.034)	(0.064)	(0.034)	(0.066)
Crisis $_{t-1}$	0.038	0.045	0.045	0.051	0.049	0.067
	(0.014)	(0.016)	(0.016)	(0.018)	(0.020)	(0.021)
Hansen J Test		[0.05]		[0.04]		[0.04]
AR(2) Test		[0.23]		[0.35]		[0.18]
R-squared	0.89		0.89		0.89	
Panel C	*Dependent Variable is Transition away from Democracy*					
Democracy $_{t-1}$	0.744	0.796	0.747	0.796	0.747	0.797
	(0.038)	(0.054)	(0.038)	(0.054)	(0.038)	(0.053)
Log GDP per Capita $_{t-1}$	0.001	-0.074	0.002	-0.076	0.003	-0.076
	(0.023)	(0.057)	(0.022)	(0.058)	(0.022)	(0.058)
Crisis $_{t-1}$	0.006	0.012	0.014	0.013	0.025	0.029
	(0.011)	(0.014)	(0.011)	(0.015)	(0.011)	(0.017)
Hansen J Test		[0.14]		[0.16]		[0.21]
AR(2) Test		[0.51]		[0.47]		[0.48]
R-squared	0.93		0.93		0.93	
Observations	739	659	739	659	739	659
Countries	124	122	124	122	124	122

Fixed effects OLS with country dummies and robust standard errors clustered by country in columns 1, 3, and 5; GMM of Arellano-Bond in columns 2, 4, and 6, with robust standard errors; in this method we instrument for income and for crises using a double lag. Year dummies are included in all regressions. The dependent variable in Panel A is the augmented Freedom House Political Rights Index, in Panel B, Transition towards Democracy, calculated as max(d_{it},d_{it-1}) from this Index, and in Panel C Transition away from Democracy, calculated as min(d_{it},d_{it-1}) from this Index. Base sample is an unbalanced panel, 1965-2000, with data at 5-year intervals in levels where the start date of the panel refers to the dependent variable (i.e., t=1965, so t-1=1960). Countries must be independent for 15 years before they enter the panel. Crisis is a dummy variable corresponding to a change in the growth rate by more than a threshold. In columns 1 and 2 the threshold is –3%, in columns 3 and 4 the threshold is –4%, and in columns 5 and 6 the threshold is –5%. See text for details. For detailed data definitions and sources see Table 1 and Appendix Table A1.

Appendix Table A1

VARIABLE	DESCRIPTION	SOURCE
Freedom House Political Rights Index, also refered 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. Transitions to democracy are calculated as max((d_{it},d_{it-1}) and transitions to non-democracy are calculated as min(d_{it},d_{it-1}).	http://www.freedomhouse.org/ratings/, and Bollen (2001) "Cross National Indicators of Liberal Democracy 1950-1990" available on ICPSR
Polity Composite Democracy Index, also refered to here as the Polity Measure of Democracy	Data for 1840-2000 in Polity IV. The composite index is the democracy score minus the autocracy score. Original range -10,-9,...10, normalized 0-1.	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 (2004) 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 1800-2000 measured as Log Real GDP per Capita (1990 Geary-Khamis dollars) from Maddison (2003).	http://www.eco.rug.nl/~Maddison/
Population	Total population in thousands.	World Bank (2002)
Education	Average 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)
Significant Oil Producer	"1" if country is described by IMF as oil exporting. This is used as opposed to OPEC in line with Barro (1999).	following Barro (1999)
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 World 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 World 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
Trade-Weighted World Log GDP with Gravity Estimated Weights	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. Additional data for estimation of the gravity equation from CIA World Factbook (2004), World Development Indicators (2002), and Glick and Rose (2002). Using the same specification as in Frankel and Romer (1999). See Appendix for more details.	http://pwt.econ.upenn.edu/, IMF DoTS CD-ROM (2005), http://www.cia.gov/cia/publications/factbook/, World Bank (2002), and http://faculty.haas.berkeley.edu/arose/RecRes.htm#Software
Constraint on the Executive at Independence	Data in Polity IV, original range 1,2,3...7, normalized 0-1. Calculated as the average of constraint on the executive in a country during the first 10 years after its independence (ignoring missing data). If data for the first 10 years after independence is missing, we find the first year these data are available in Polity, then average over the following ten years (ignoring missing data).	http://www.cidcm.umd.edu/inscr/polity/
Independence year	Year when country became independent, with any year before 1800 coded as 1800. We coded Taiwan's independence year to 1948 and changed Zimbabwe's independence year to 1964. Classification of countries follows Polity.	CIA World Factbook (2004) available at http://www.cia.gov/cia/publications/factbook/
Population Density in 1500	Indigenous population divided by arable land in 1500.	Acemoglu et al (2002)
Settler mortality	Historical mortality rates of potential European settlers.	Acemoglu et al (2001)
Religion	Percent of population in 1980 which is (1) Catholic, (2) Protestant, or (3) Muslim.	La Porta et al (1999)

Appendix Table A2

Fixed Effects Results using Polity Measure of Democracy: Two Stage Least Squares

	Base Sample, 1960-2000					
	Pooled OLS	Fixed Effects OLS	Fixed Effects OLS	Fixed Effects 2SLS	Fixed Effects 2SLS	Arellano-Bond GMM
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Savings Rate Instrument	*Dependent Variable is Democracy*					
Democracy $_{t-1}$			0.452		0.451	0.638
			(0.064)		(0.064)	(0.110)
Log GDP per Capita $_{t-1}$	0.225	-0.009	-0.003	-0.029	-0.020	-0.290
	(0.014)	(0.059)	(0.042)	(0.084)	(0.075)	(0.127)
Hansen J Test						[0.02]
AR(2) Test						[0.38]
Observations	827	827	804	827	804	673
Countries	121	121	121	121	121	113
Panel B: Trade-Weighted World Income Instrument	*Dependent Variable is Democracy*					
Democracy $_{t-1}$			0.440		0.432	0.589
			(0.063)		(0.070)	(0.108)
Log GDP per Capita $_{t-1}$	0.227	-0.035	-0.022	-0.417	-0.264	-0.260
	(0.014)	(0.052)	(0.037)	(0.198)	(0.128)	(0.117)
Hansen J Test						[0.03]
AR(2) Test						[0.39]
Observations	839	839	815	839	815	730
Countries	113	113	113	113	113	112

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 and 5 with country dummies and robust standard errors clustered by country in parentheses; first stage regressions are not displayed but are similar in Panel A to Table 6, Panel B, columns 4 and 5, and are similar in Panel B to Table 7, Panel B, columns 4 and 5. 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. Dependent variable is Polity Composite Democracy Index. 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. Panel A, columns 4-6 instrument Log GDP per Capita $_{t-1}$ with Savings Rate $_{t-2}$. Panel B, columns 4-6 instrument Log GDP per Capita $_{t-1}$ with Trade-Weighted World Log GDP $_{t-1}$. For detailed data definitions and sources see Table 1 and Appendix Table A1. See Appendix for details on the construction of the instruments.

Appendix Table A3

Effect of Historical Factors on Democracy: Former Colonies, using Polity Measure of Democracy

	Former European Colonies							
	Unbalanced Panel, 1960-2000							Balanced Panel 1970-2000
	Pooled OLS	Fixed Effects	Pooled OLS					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Dependent Variable is Democracy							
Democracy $_{t-1}$	0.723	0.354	0.711	0.720	0.643	0.622	0.637	0.549
	(0.047)	(0.078)	(0.050)	(0.048)	(0.054)	(0.057)	(0.053)	(0.083)
Log GDP per Capita $_{t-1}$	0.043	-0.056	0.032	0.032	0.015	0.009	0.004	-0.010
	(0.014)	(0.060)	(0.017)	(0.014)	(0.012)	(0.018)	(0.013)	(0.020)
Constraint on the Executive at Independence					0.131	0.153	0.137	0.169
					(0.037)	(0.037)	(0.036)	(0.050)
Independence Year/100					-0.083	-0.087	-0.081	-0.119
					(0.016)	(0.018)	(0.016)	(0.029)
Log Population Density in 1500				-0.010		-0.009	-0.011	-0.019
				(0.006)		(0.008)	(0.007)	(0.010)
Log Settler Mortality			-0.019			-0.001		
			(0.010)			(0.010)		
Observations	514	514	462	514	514	462	514	273
Countries	80	80	68	80	80	68	80	39
R-squared	0.68	0.76	0.70	0.69	0.70	0.71	0.70	0.71

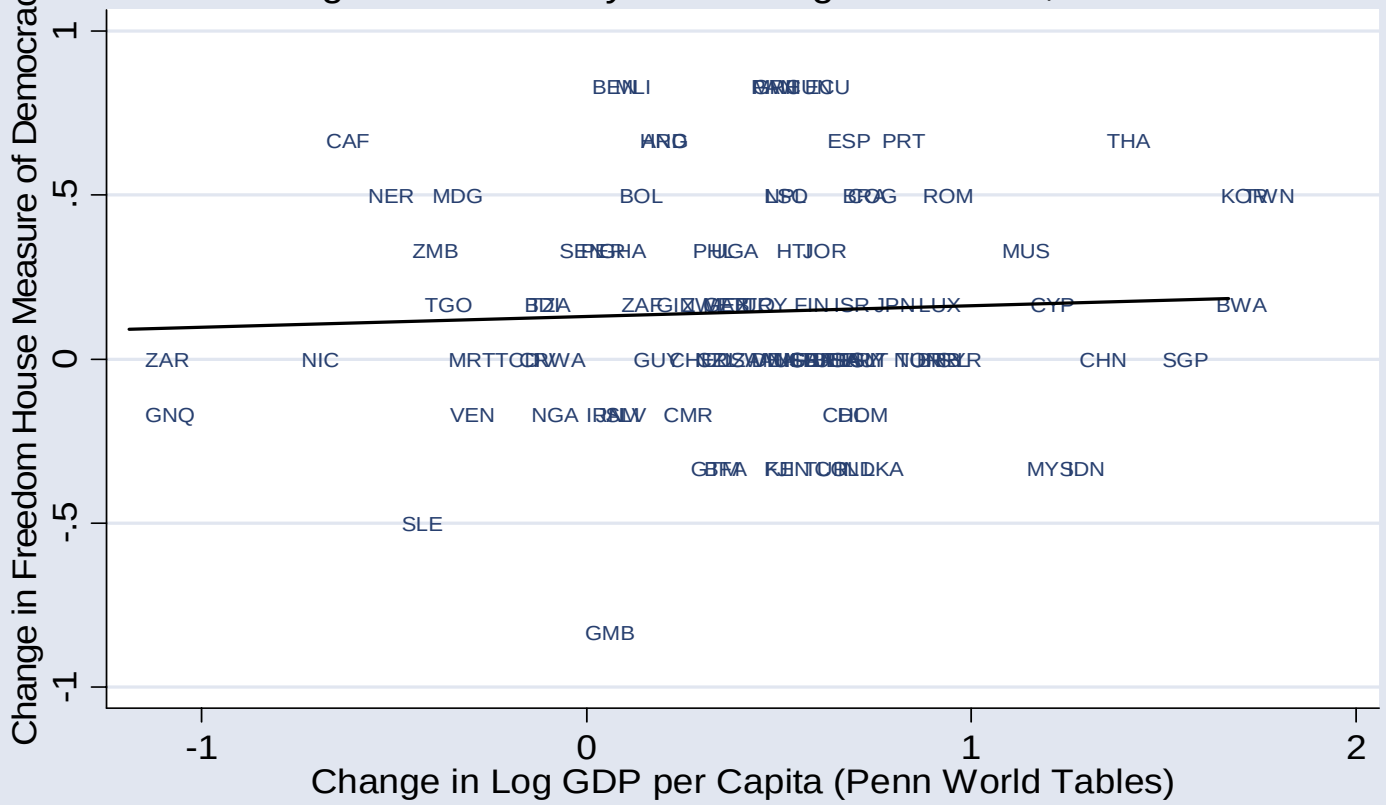
Pooled cross-sectional OLS in columns 1 and 3-8, with robust standard errors clustered by country in parentheses. Fixed effects OLS regressions in column 2, with country dummies and robust standard errors clustered by country in parentheses. Year dummies are included in all regressions. Dependent variable is Polity Composite Democracy Index. Data are at 5-year intervals, for former European colonies. Columns 1-7 are an unbalanced panel 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 10 years before it enters the panel. Column 8 is a balanced panel from 1970 to 2000; a country must be independent by 1960 to enter the panel and first lagged value is 1965. In all columns, data represents all available values in the former colonies sample for which Constraint on the Executive at Independence, Independence Year, and Log Population Density in 1500 are available. For detailed data definitions and sources see Table 1 and Appendix Table A1.

Appendix Table A4
Codes Used to Represent Countries in Figures

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

Figure 1

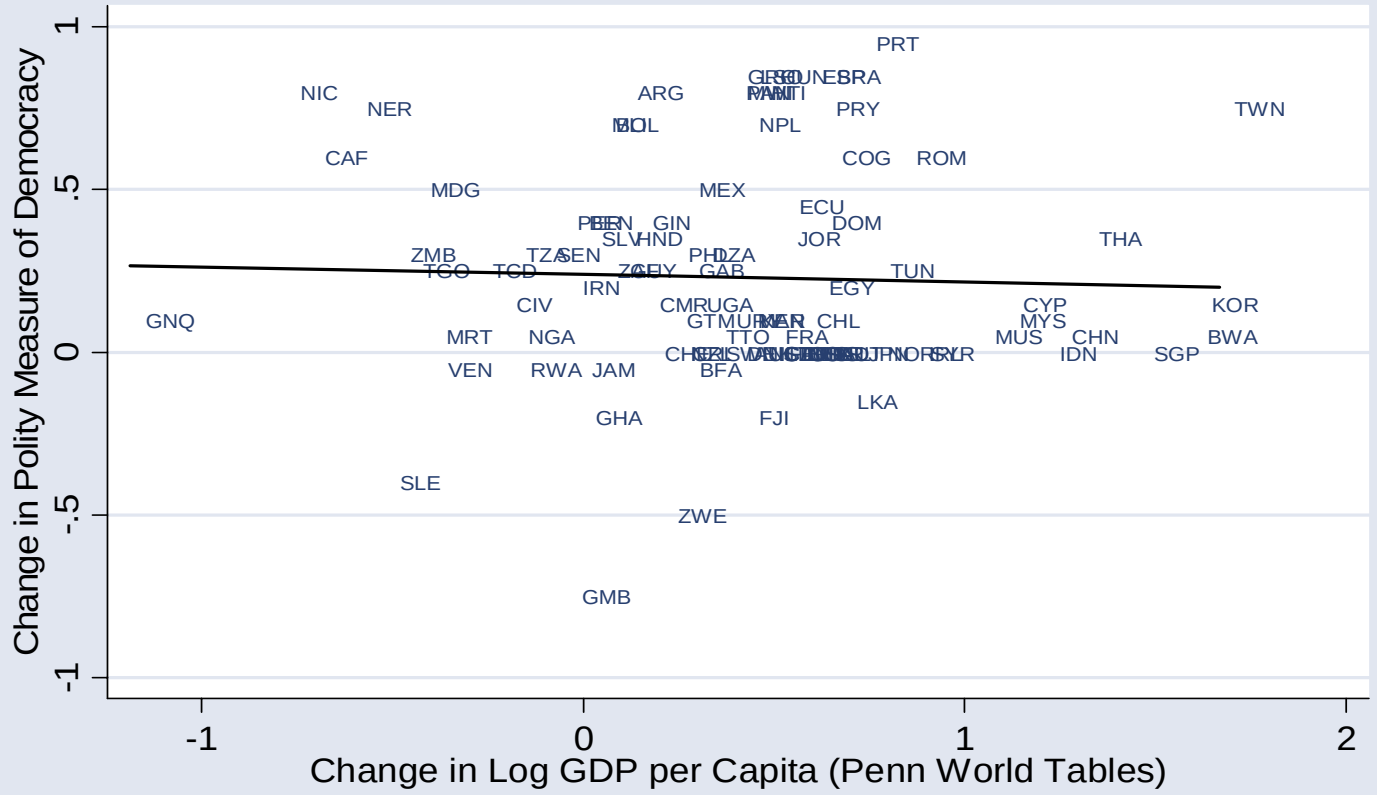
Change in Democracy and Change in Income, 1970-1995



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

Figure 2

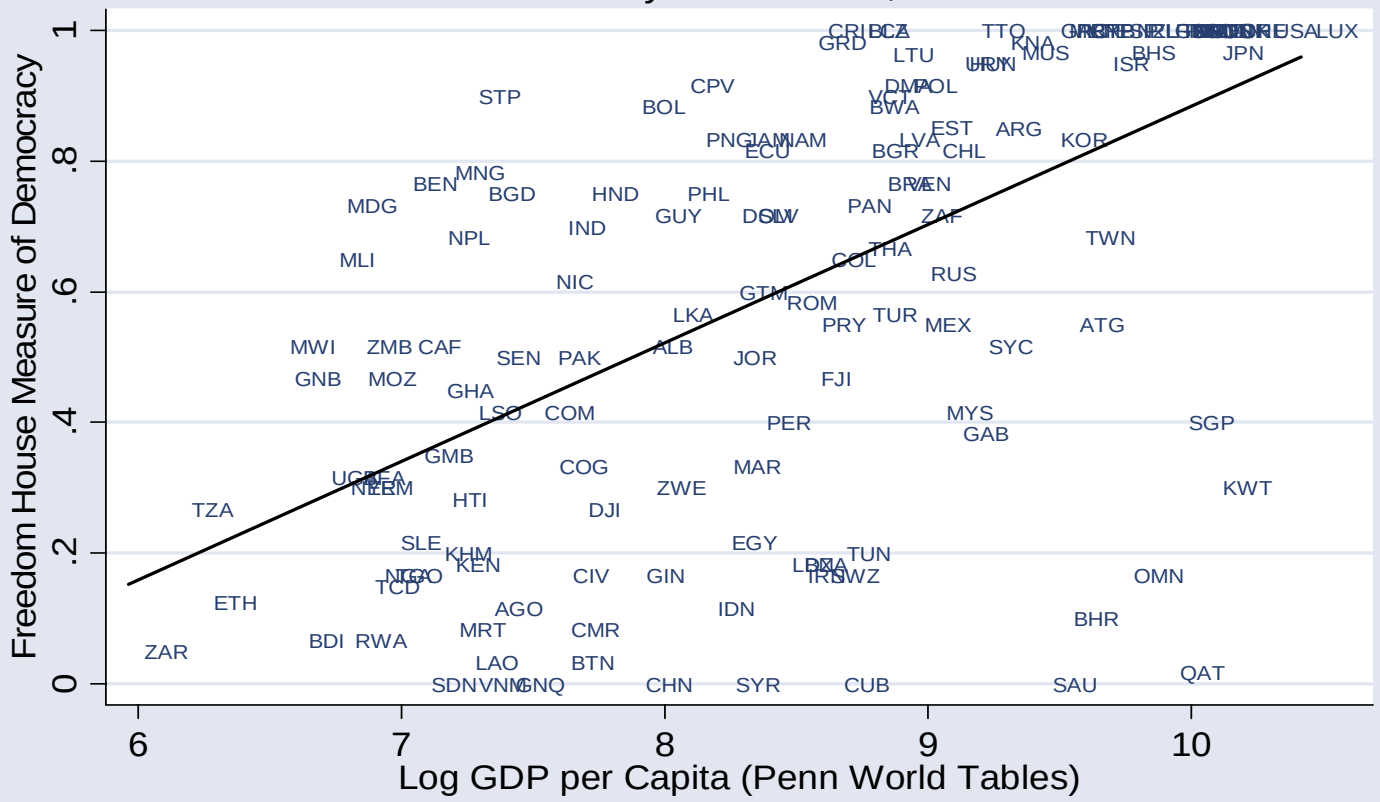
Change in Democracy and Change in Income, 1970-1995



See notes to Figure 1. The regression represented by the fitted line yields a coefficient of -0.024 (standard error=0.063), N=98, R^2=0.00.

Figure 3

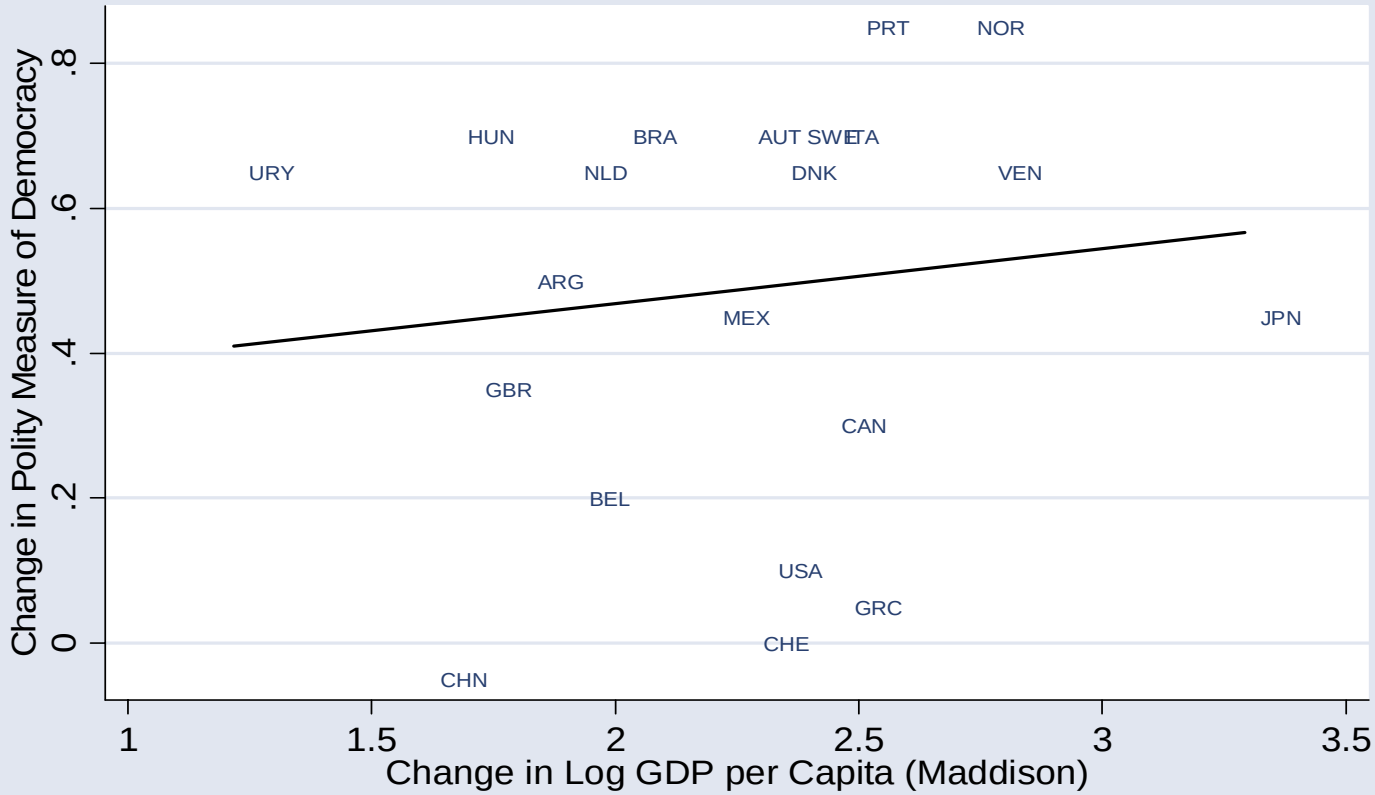
Democracy and Income, 1990s



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.

Figure 4

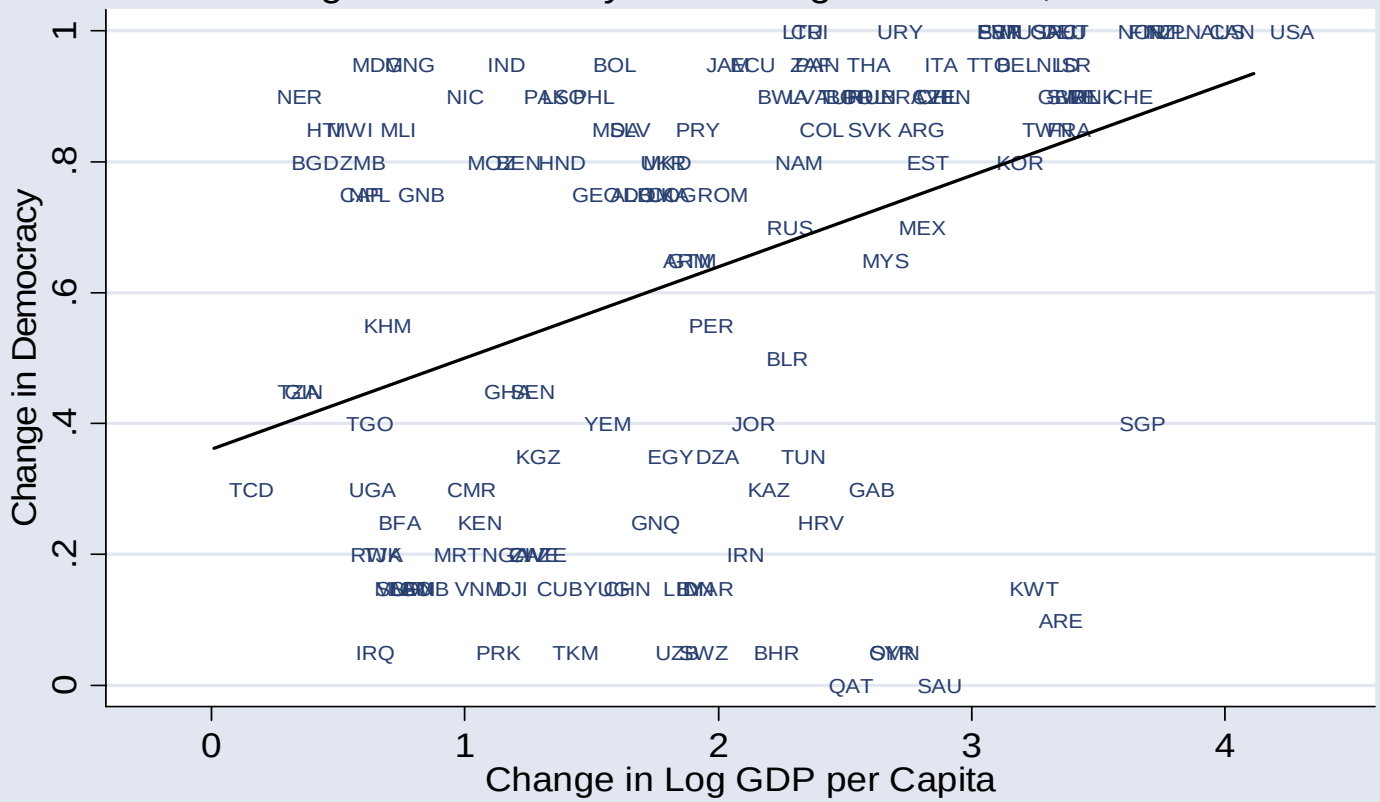
Change in Democracy and Change in Income, 1870-1995



Log GDP per Capita is from Maddison (2003). See Appendix Table A1 for data definitions and sources. Changes are total difference between 1870 and 1995. Countries are included if they are in the 200-year sample discussed in Section 6 of the text and were independent by 1870. Start and end dates are chosen to maximize the number of countries in the cross-section. The regression represented by the fitted line yields a coefficient of 0.076 (standard error=0.123), N=21, R^2=0.02.

Figure 5

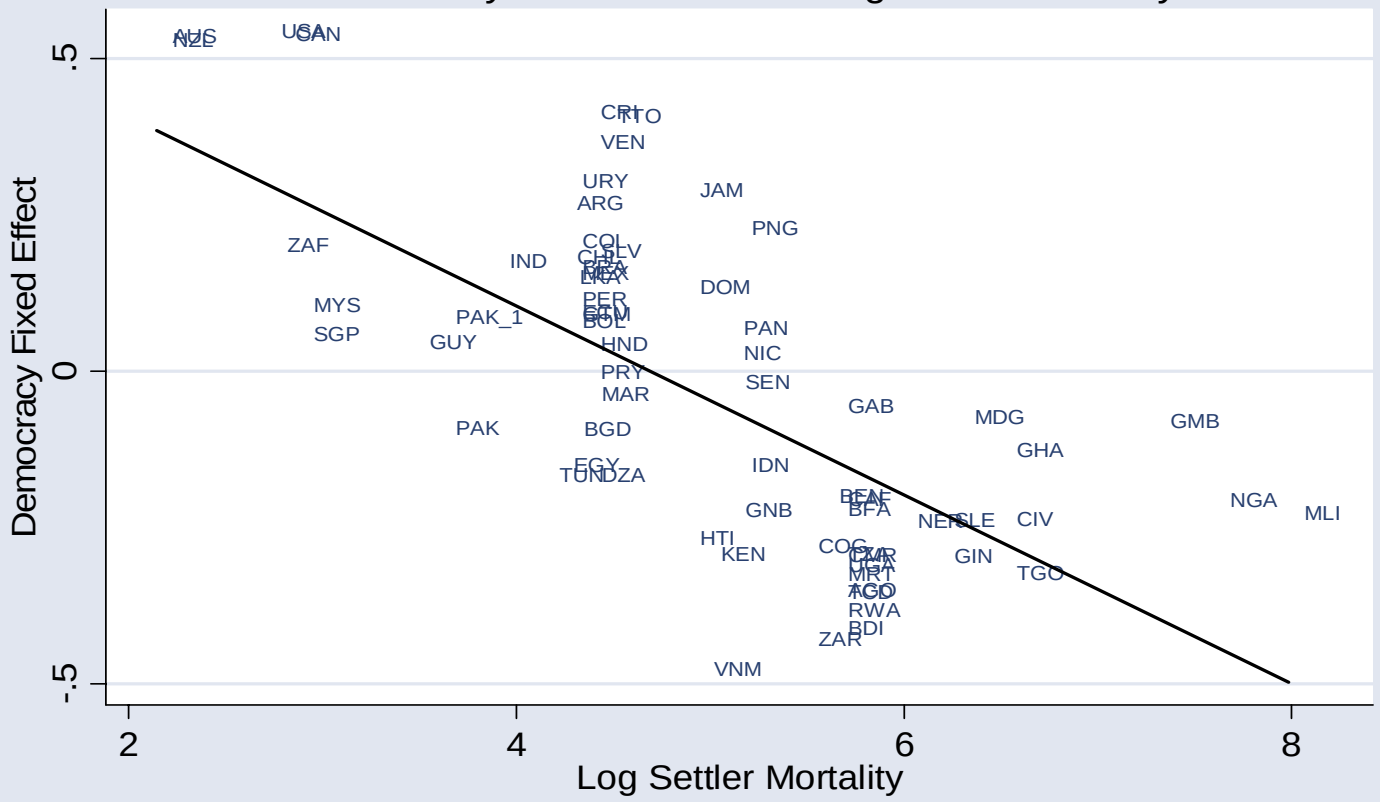
Change in Democracy and Change in Income, 1500-1995



See Appendix Table A1 for data definitions and sources. Changes are total differences between 1500 and 1995. GDP per capita is from Maddison. Democracy is calculated using the Polity measure of democracy, which comprises in part constraint on the executive; data for 1500 from Acemoglu et al (2004b). The end date is chosen to maximize the number of countries in the cross-section. The regression represented by the fitted line yields a coefficient of 0.139 (standard error=0.033), N=143, R^2=0.17, and corresponds to the specification of Table 10, column 1.

Figure 6

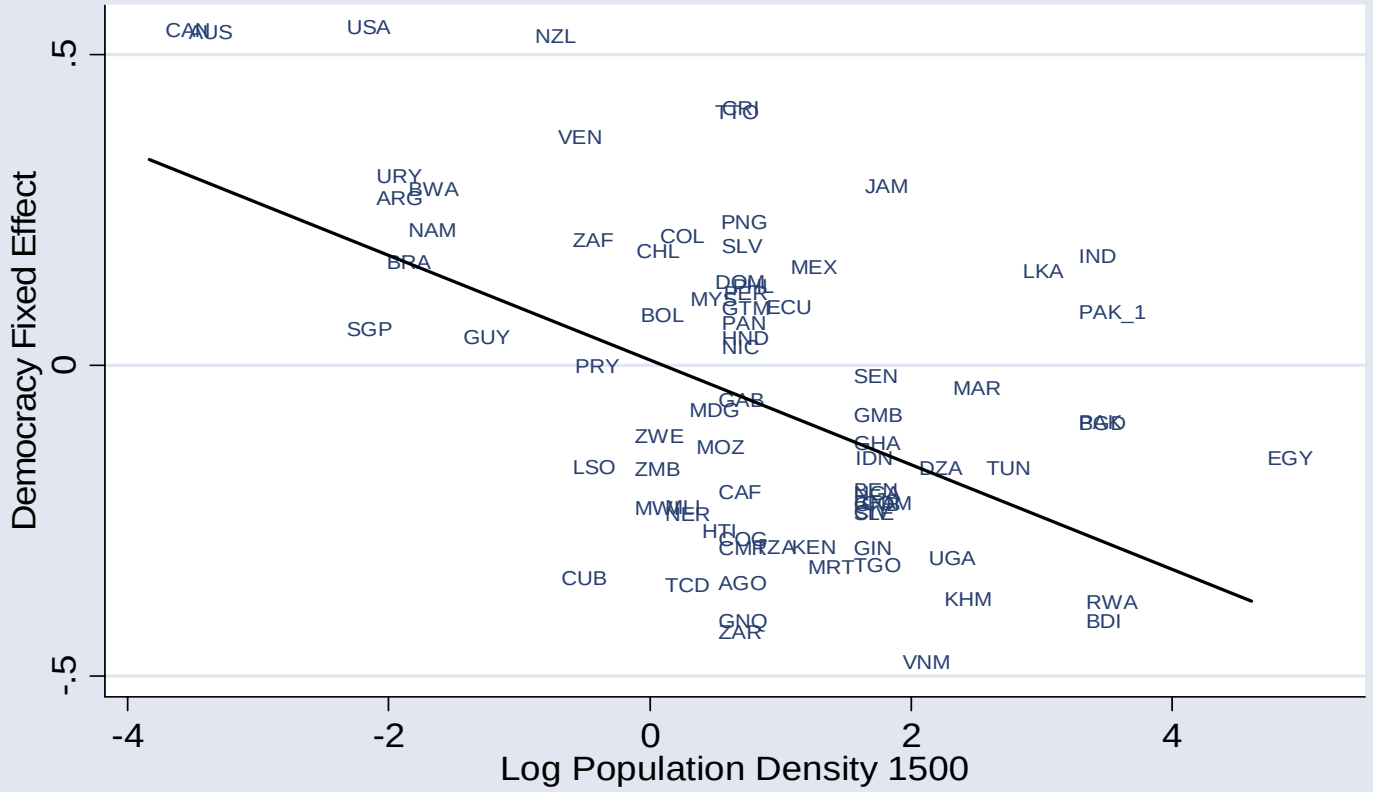
Democracy Fixed Effect and Log Settler Mortality



See Appendix Table A1 for data definitions and sources. Country fixed effects are those estimated in the specification of Table 11, column 2 using Freedom House measure of democracy. PAK_1 refers to Pakistan pre-1972. The regression represented by the fitted line yields a coefficient of -0.151 (standard error=0.020), N=68, R^2=0.47.

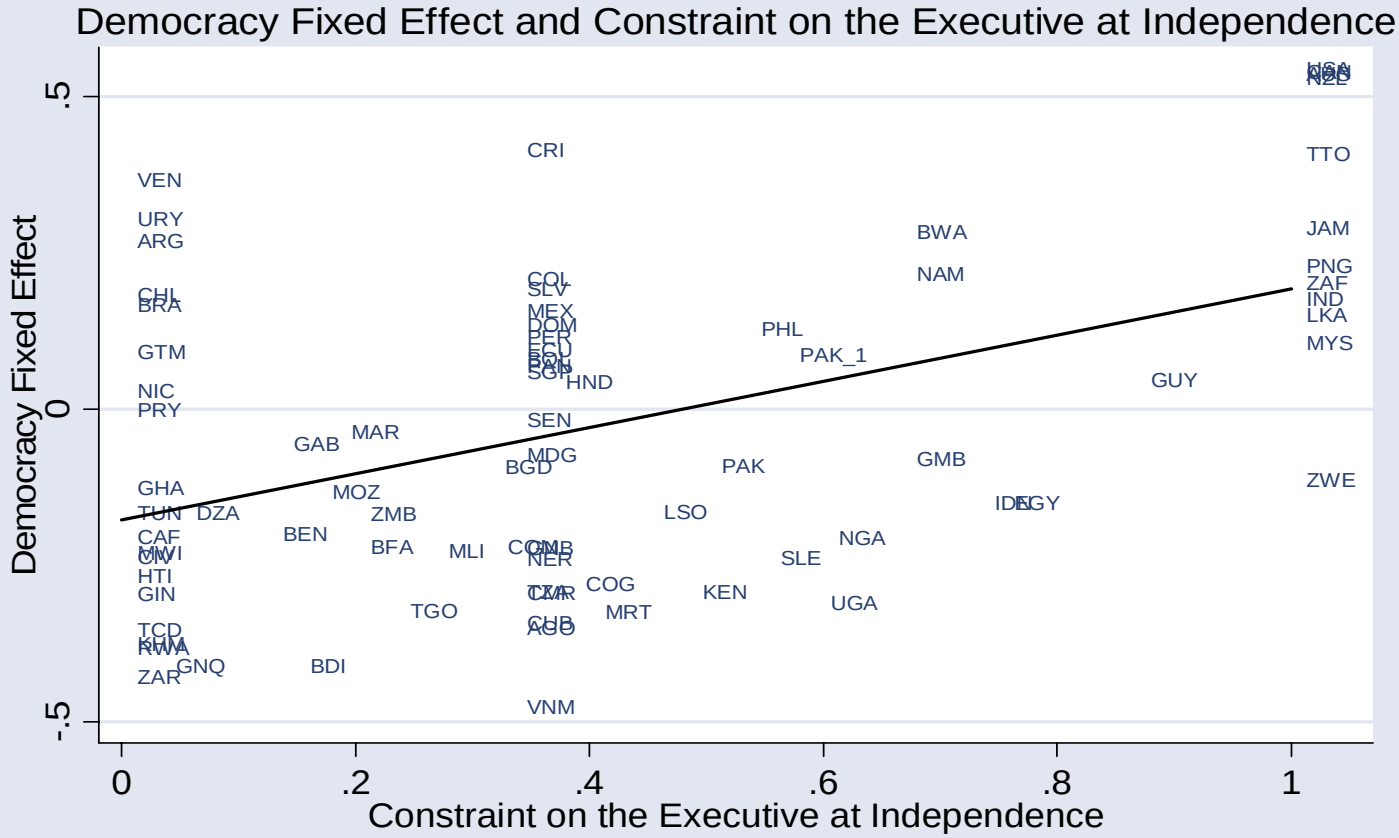
Figure 7

Democracy Fixed Effect and Log Population Density in 1500



See notes to Figure 6. The regression represented by the fitted line yields a coefficient of -0.084 (standard error=0.016), N=80, R^2=0.27.

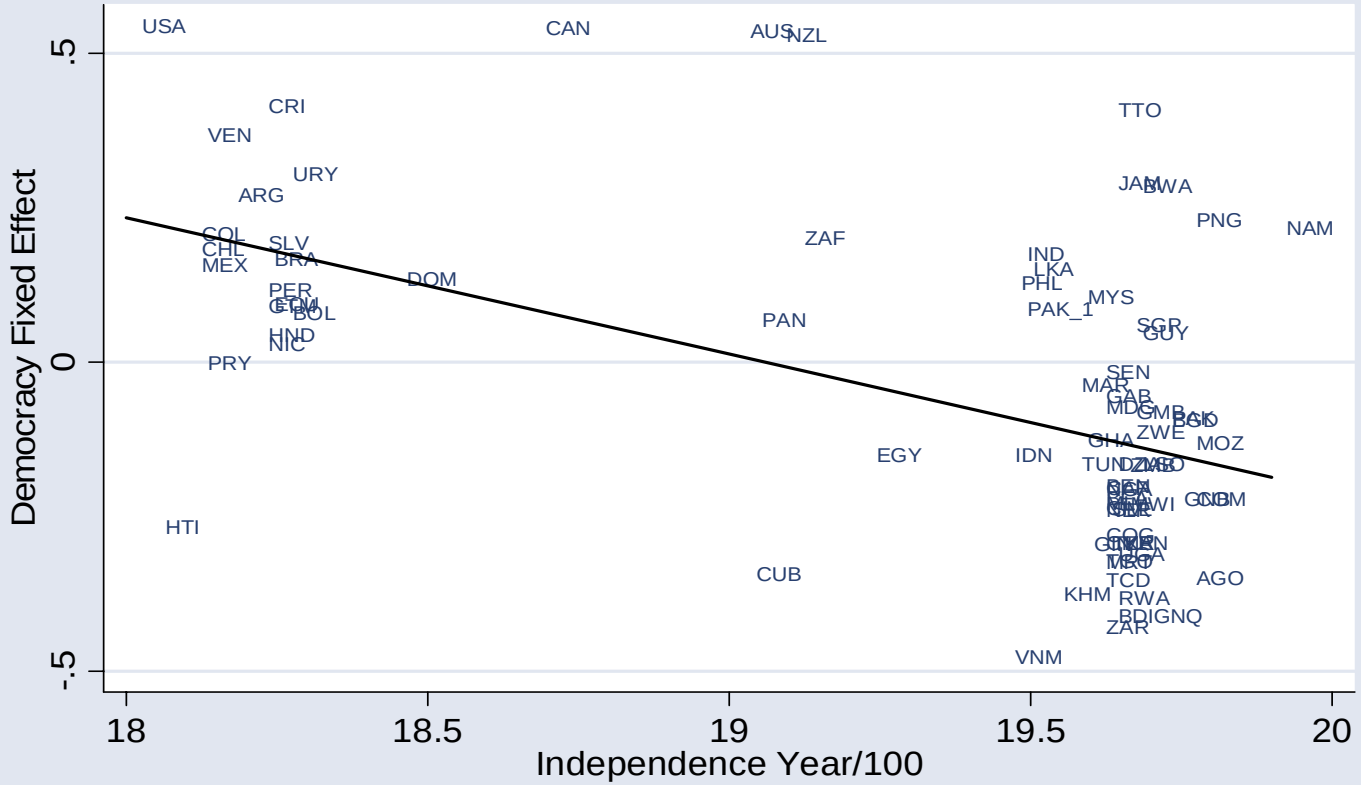
Figure 8



See notes to Figure 6. The regression represented by the fitted line yields a coefficient of 0.370 (standard error=0.079), N=80, R^2=0.23.

Figure 9

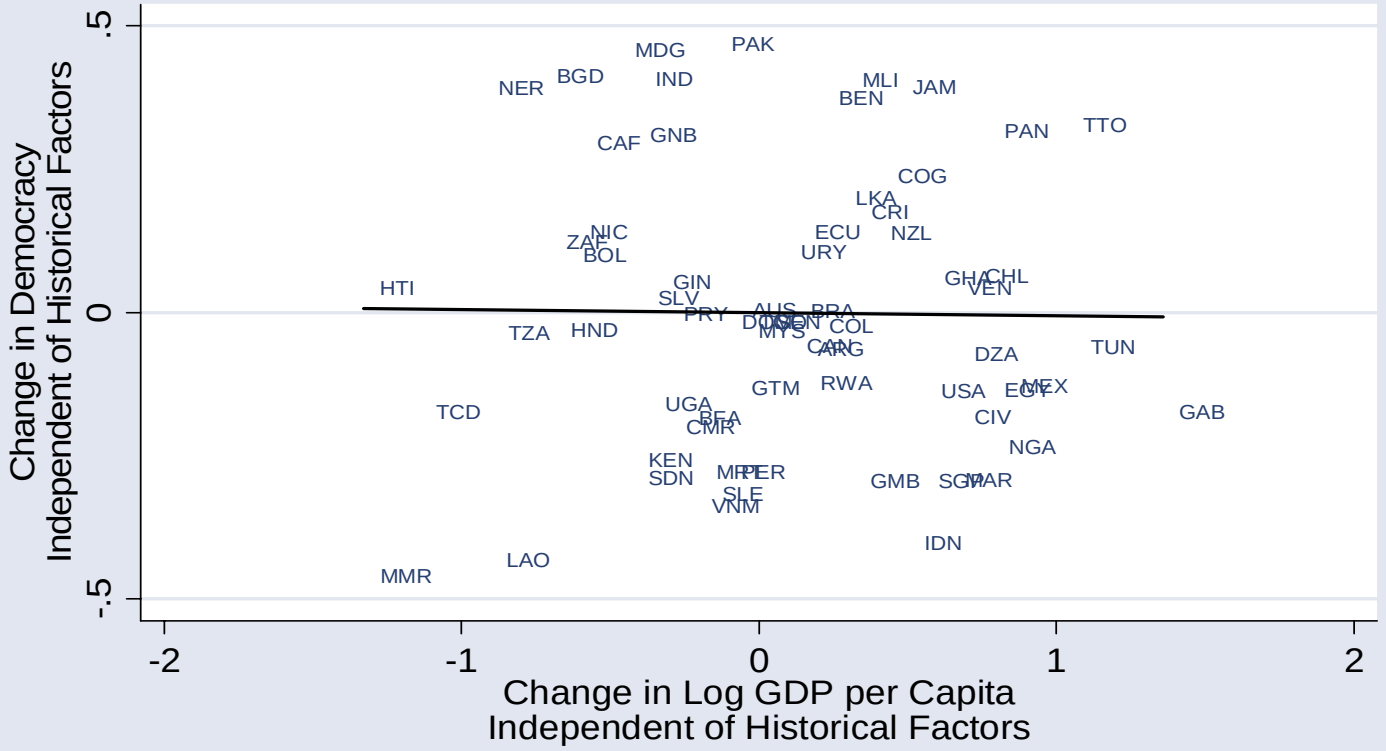
Democracy Fixed Effect and Independence Year



See notes to Figure 6. We divide independence year by 100 in order that the coefficient estimates in Table 11 be visible. The regression represented by the fitted line yields a coefficient of -0.221 (standard error=0.037), N=80, R^2=0.28.

Figure 10

Change in Democracy and Change in Income, 1500-1995
Conditional on Historical Factors



See Appendix Table A1 for data definitions and sources. Changes are total differences between 1500 and 1995 (see Figure 5 for the construction of these differences) in the sample of former European colonies which are not predicted in a linear regression by historical determinants of development path: Log Settler Mortality, Log Population Density in 1500, Constraint on the Executive at Independence, and Independence Year. This corresponds to the residual plot of the regression in Table 10, column 7 and it yields a coefficient of -0.006 (standard error=0.054), N=65, R^2=0.41.