

Income and Democracy

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Existing studies establish a strong cross-country correlation between income and democracy but do not 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 present instrumental-variables estimates that also show no causal effect of income on democracy. The cross-country correlation between income and democracy reflects a positive correlation between changes in income and democracy over the past 500 years. This pattern is consistent with the idea that societies embarked on divergent political-economic development paths at certain critical junctures. (JEL D72, E21)

One of the most notable empirical regularities in political economy is the relationship between income per capita and democracy. Today, all OECD countries are democratic, while many of the nondemocracies are in the poor parts of the world, for example sub-Saharan Africa and Southeast Asia. The positive cross-country relationship between income and democracy in the 1990s is depicted in Figure 1, which shows the association between the Freedom House measure of democracy and log income per capita in the 1990s.¹ This relationship is not confined solely to a cross-country comparison. Most countries were nondemocratic before the modern growth process took off at the beginning of the nineteenth century. Democratization came together with growth. Robert J. Barro (1999, 160), for example, summarizes this as follows: “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.”²

This statistical association between income and democracy is the cornerstone of the influential modernization theory. Lipset (1959) suggested that democracy was both created and consolidated by a broad process of “modernization” which involved changes in “the factors of industrialization, urbanization, wealth, and education [which] are so closely interrelated as to

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¹ Details on various measures of democracy and other variables are provided in Section I. All figures use the three-letter World Bank country codes to identify countries, which are provided in Appendix Table A2, except when multiple countries are clustered together. When such clustering happens, countries are grouped together, the averages for the group are plotted in the figure, and the countries in each group are identified in the footnote to the corresponding figure.

² See also, among others, Seymour Martin Lipset (1959), John B. Londregan and Keith T. Poole (1996), Adam Przeworski and Fernando Limongi (1997), Barro (1997), Przeworski et al. (2000), and Elias Papaioannou and Gregorios Siourounis (2006).

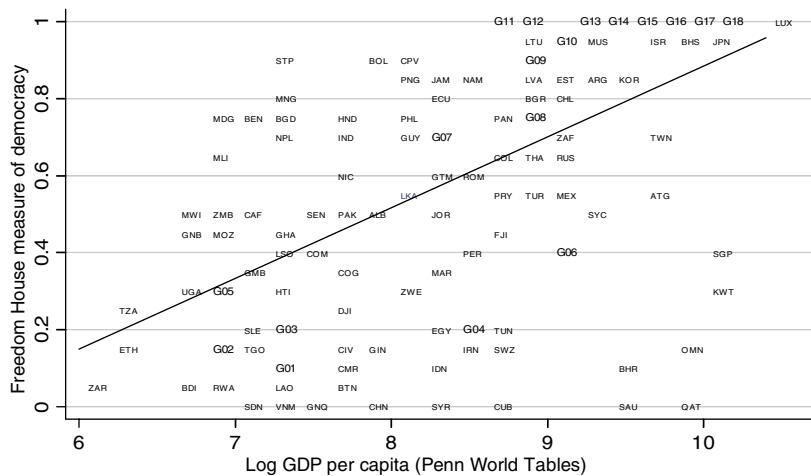


FIGURE 1. DEMOCRACY AND INCOME, 1990S

Notes: 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), $N = 147$, and $R^2 = 0.35$. The “G” prefix corresponds to the average for groups of countries. G01 is AGO and MRT; G02 is NGA and TCD; G03 is KEN and KHM; G04 is DZA and LBN; G05 is BFA, NER, and YEM; G06 is GAB and MYS; G07 is DOM and SLV; G08 is BRA and VEN; G09 is BWA, DMA, POL, and VCT; G10 is HUN and URY; G11 is CRI and GRD; G12 is BLZ and LCA; G13 is KNA and TTO; G14 is GRC and MLT; G15 is BRB, CYP, ESP, and PRT; G16 is FIN, GBR, IRL, and NZL; G17 is AUS, AUT, BEL, CAN, DEU, DNK, FRA, ISL, ITA, NLD, NOR, and SWE; and G18 is CHE and USA.

form one common factor. And the factors subsumed under economic development carry with it the political correlate of democracy” (80). The central tenet of the modernization theory, that higher income per capita causes a country to be democratic, is also reproduced in most major works on democracy (e.g., Robert A. Dahl 1971; Samuel P. Huntington 1991; Dietrich Rusechmeyer, John D. Stephens, and Evelyn H. Stephens 1992).

In this paper, we revisit the relationship between income per capita and democracy. Our starting point is that existing work, which is based on cross-country relationships, does not establish causation. First, there is the issue of reverse causality; perhaps democracy causes income rather than the other way round. Second, and more important, there is the potential for omitted variable bias. Some other factor may determine both the nature of the political regime and the potential for economic growth.

We utilize two strategies to investigate the causal effect of income on democracy. Our first strategy is to control for country-specific factors affecting both income and democracy by including country fixed effects. While fixed effect regressions are not a panacea for omitted variable biases,³ they are well suited to the investigation of the relationship between income and democracy,

³ 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, in fact, make problems of measurement error worse because they remove a significant portion of the variation in the right-hand-side variables. Consequently, fixed effects are certainly no substitute for instrumental-variables or structural estimation with valid exclusion restrictions.

especially in the postwar era. The major source of potential bias in a regression of democracy on income per capita is country-specific, historical factors influencing both political and economic development. If these omitted characteristics are, to a first approximation, time-invariant, the inclusion of fixed effects will remove them and this source of bias. Consider, for example, the comparison of the United States and Colombia. The United States is both richer and more democratic, so a simple cross-country comparison, as well as the existing empirical strategies in the literature, which do not control for fixed country effects, would suggest that higher per capita income causes democracy. The idea of fixed effects is to move beyond this comparison and investigate the “within-country variation,” that is, to ask whether Colombia is more likely to *become* (relatively) democratic as it *becomes* (relatively) richer. In addition to improving inference on the causal effect of income on democracy, this approach is more closely related to modernization theory as articulated by Lipset (1959), which emphasizes that individual countries should become more democratic if they are richer, not simply that rich countries should be democratic.

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

This basic finding is robust to using various different indicators for democracy, to different econometric specifications and estimation techniques, in different subsamples, and to the inclusion of additional covariates. The absence of a significant relationship between income and democracy is *not* driven by large standard errors. On the contrary, the relationship between income and democracy is estimated relatively precisely. In many cases, two-standard-error bands include only very small effects of income on democracy and often exclude the OLS estimates. These results, therefore, shed considerable doubt on the claim that there is a strong causal effect of income on democracy.⁴

While the fixed effects estimation is useful in removing the influence of long-run determinants of both democracy and income, it does not necessarily estimate the causal effect of income on democracy. Our second strategy is to use instrumental-variables (IV) regressions to estimate the impact of income on democracy.⁵ We experiment with two potential instruments. The first is to use past savings rates, and the second is to use changes in the incomes of trading partners. The argument for the first instrument is that variations in past savings rates affect income per capita but should have no direct effect on democracy. The second instrument, which we believe is of independent interest, creates a matrix of trade shares and constructs predicted income for each country using a trade-share-weighted average income of other countries. We show that this predicted income has considerable explanatory power for income per capita. We also argue that it should have no direct effect on democracy. Our second major result is that both IV strategies show no evidence of a causal effect of income on democracy. We recognize that neither instrument is perfect, since there are reasonable scenarios in which our exclusion restrictions could be violated (e.g., saving rates might be correlated with future anticipated regime changes; or democracy scores of a country’s trading partners, which are correlated with their income levels,

⁴ It remains true that over time there is a general tendency toward greater incomes and greater democracy throughout 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.

⁵ A recent creative attempt is by Edward Miguel, Shankar Satyanath, and Ernest Sergenti (2004), who use weather conditions as an instrument for income in Africa to investigate the impact of income on civil wars. Unfortunately, weather conditions are a good instrument only for relatively short-run changes in income, thus not ideal to study the relationship between income and democracy.

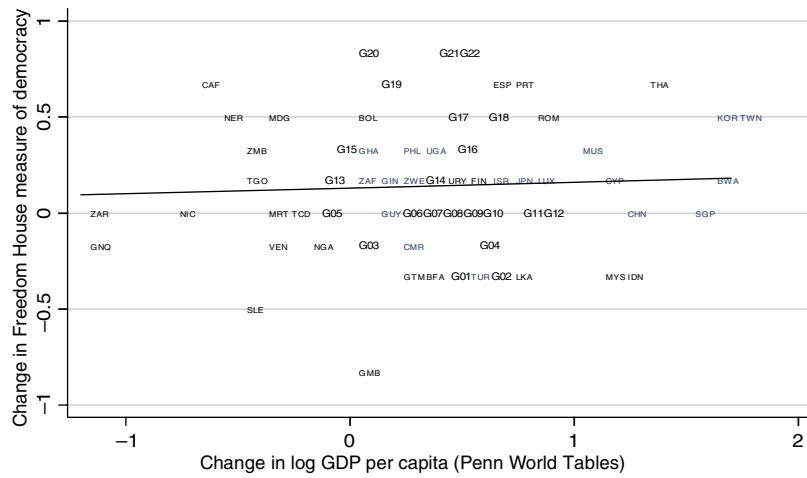


FIGURE 2. CHANGE IN DEMOCRACY AND INCOME, 1970–1995

Notes: 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), $N = 102$, $R^2 = 0.00$. The “G” prefix corresponds to the average for groups of countries. G01 is FJI and KEN; G02 is COL and IND; G03 is IRN, JAM, and SLV; G04 is CHL and DOM; G05 is CIV and RWA; G06 is CHE, CRI, and NZL; G07 is DZA and SWE; G08 is AUS, DNK, MAR, and NLD; G09 is BEL, CAN, FRA, and GBR; G10 is AUT, EGY, ISL, ITA, PRY, and USA; G11 is BRB, NOR, and TUN; G12 is IRL and SYR; G13 is BDI and TZD; G14 is GAB, MEX, and TTO; G15 is PER and SEN; G16 is HTI and JOR; G17 is LSO and NPL; G18 is BRA and COG; G19 is ARG and HND; G20 is BEN and MLI; G21 is GRC, MWI, and PAN; and G22 is ECU and HUN.

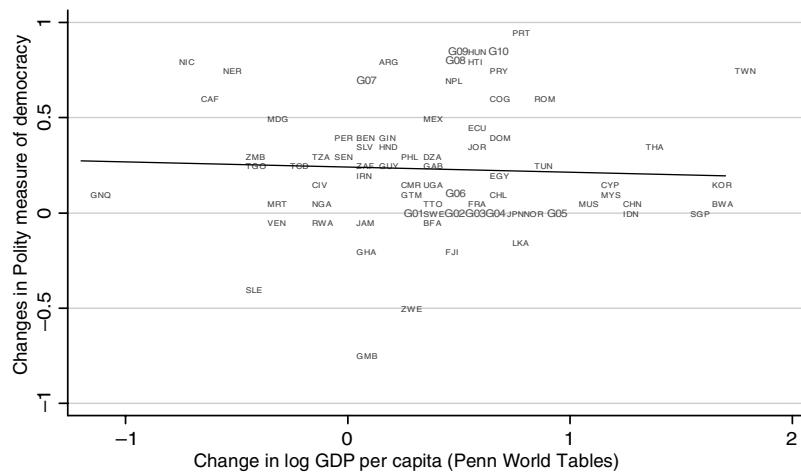


FIGURE 3. CHANGE IN DEMOCRACY AND INCOME, 1970–1995

Notes: See notes to Figure 2. The regression represented by the fitted line yields a coefficient of -0.024 (standard error = 0.063), $N = 98$, $R^2 = 0.00$. G01 is CHE, CRI, and NZL; G02 is AUS, DNK, and NLD; G03 is BEL, CAN, FIN, GBR, and TUR; G04 is AUT, COL, IND, ISL, ISR, ITA, and USA; G05 is IRL and SYR; G06 is KEN, MAR, and URY; G07 is BOL and MLI; G08 is MWI and PAN; G09 is GRC and LSO; and G10 is BRA and ESP.

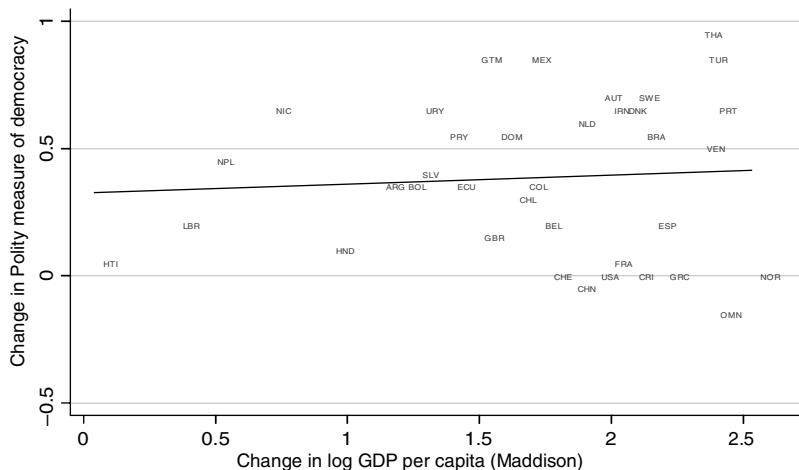


FIGURE 4. CHANGE IN DEMOCRACY AND INCOME, 1900–2000

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

might have a direct effect on its democracy). To alleviate these concerns, we show that the most likely sources of correlation between our instruments and the error term in the second stage are not present.

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

These results naturally raise the following important question: why is there a cross-sectional correlation between income and democracy? In other words, why are rich countries democratic today? At a statistical level, the answer is clear: even though there is no relationship between changes in income and democracy in the postwar era or over the past 100 years or so, there is a positive association over the past 500 years. Most societies were nondemocratic 500 years ago and had broadly similar income levels. The positive cross-sectional relationship reflects the fact that those that have become more democratic over this time span are also those that have grown faster. One possible explanation for the positive cross-sectional correlation is, therefore, that there is a causal effect of income on democracy, but it works at *much* longer horizons than the existing literature has posited. Although the lack of a relationship over 50 or 100 years sheds some doubt on this explanation, this is a logical possibility.

We favor another explanation for this pattern. Even in the absence of a simple causal link from income to democracy, political and economic development paths are interlinked and are jointly affected by various factors. Societies may embark on *divergent political-economic development paths*, some leading to relative prosperity and democracy, others to relative poverty and dictatorship. Our hypothesis is that the positive cross-sectional relationship and the 500-year correlation

between changes in income and democracy are caused by the fact that countries have embarked on divergent development paths at some *critical junctures* during the past 500 years.⁶

We provide support for this hypothesis by documenting that the positive association between changes in income and democracy over the past 500 years is largely accounted for by a range of historical variables. In particular, for the whole world sample, the positive association is considerably weakened when we control for date of independence, early constraints on the executive, and religion.⁷ We then turn to the sample of former European colonies, where we have better proxies for factors that have influenced the development paths of nations. Acemoglu, Johnson, and Robinson (2001, 2002) and Engerman and Sokoloff (1997) argue that differences in European colonization strategies have been a major determinant of the divergent development paths of colonial societies. This reasoning suggests that in this sample, the critical juncture for most societies corresponds to their experience under European colonization. Furthermore, Acemoglu, Johnson, and Robinson (2002) show that the density of indigenous populations at the time of colonization has been a particularly important variable in shaping colonization strategies, and provide estimates of population densities in the year 1500 (before the advent of colonization). When we use information on population density, as well as on independence year and early constraints on the executive, the 500-year relationship between changes in income and democracy in the former colonies sample disappears. This pattern is consistent with the hypothesis that the positive cross-sectional relationship between income and democracy today is the result of societies embarking on divergent development paths at certain critical junctures during the past 500 years (although other hypotheses might account for these patterns).

A related question is whether income has a separate causal effect on transitions to, and away from, democracy. Space restrictions preclude us from investigating this question here; the results of such an investigation are presented in our follow-up paper, Acemoglu et al. (2007). Using both linear regression models and double-hazard models that simultaneously estimate the process of entry into, and exit from, democracy, we find no evidence that income has a causal effect on the transitions either to or from democracy. The IV strategies and the focus on the long-run relationship are unique to the current paper.

The paper proceeds as follows. In Section I we describe the data. Section II presents our econometric model. Section III presents the fixed effects results for the postwar sample. Section IV contains our IV results for the postwar sample, while the fixed effects results for the 100-year sample are presented in Section V. Section VI discusses the sources of the cross-country relationship between income and democracy we observe today. Section VII concludes. The Appendix contains further information on the construction of the instruments used in Section IV.

I. Data and Descriptive Statistics

Our first and main measure of democracy is the Freedom House Political Rights Index. A country receives the highest score if political rights come closest to the ideals suggested by a checklist of questions, beginning with whether there are free and fair elections, whether those who are elected rule, whether there are competitive parties or other political groupings, whether the opposition plays an important role and has actual power, and whether minority groups have reasonable

⁶ See, among others, Douglass C. North and Robert P. Thomas (1973), North (1981), Eric L. Jones (1981), Stanley L. Engerman and Kenneth L. Sokoloff (1997), and Acemoglu, Johnson, and Robinson (2001, 2002) for theories that emphasize the impact of certain historical factors on development processes during critical junctures, such as the collapse of feudalism, the age of industrialization, or the process of colonization.

⁷ See Max Weber (1930), Huntington (1991), and Steven M. Fish (2002) for the hypothesis that religion might have an important effect on economic and political development.

self-government or can participate in the government through informal consensus.⁸ Following Barro (1999), we supplement this index with the related variable from Kenneth A. Bollen (1990, 2001) for 1950, 1955, 1960, and 1965. As in Barro (1999), we transform both indices so that they lie between zero and one, with one corresponding to the most democratic set of institutions.

The Freedom House index, even when augmented with Bollen's data, enables us to look only at the postwar era. The Polity IV dataset, on the other hand, provides information for all independent countries starting in 1800. Both for pre-1950 events and as a check on our main measure, we also look at the other widely used measure of democracy, the composite Polity index, which is the difference between Polity's Democracy and Autocracy indices (see Monty G. Marshall and Keith Jagers 2004). The Polity Democracy Index ranges from zero to ten 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 zero to ten and is constructed in a similar way to the democracy score based on competitiveness of political participation, the regulation of participation, the openness and competitiveness of executive recruitment, and constraints on the chief executive. To facilitate comparison with the Freedom House score, we normalize the composite Polity index to lie between zero and one.

Using the Freedom House and the Polity data, we construct five-year, ten-year, twenty-year, and annual panels. For the five-year panels, we take the observation every fifth year. We prefer this procedure to averaging the five-year data, since averaging introduces additional serial correlation, making inference and estimation more difficult (see footnote 12). Similarly, for the ten-year and twenty-year panels, we use the observations from every tenth and twentieth year. For the Freedom House data, which begin in 1972, we follow Barro (1999) and assign the 1972 score to 1970 for the purpose of the five-year and ten-year regressions.

The GDP per capita (in PPP) and savings rate data for the postwar period are from Alan Heston, Robert Summers, and Bettina 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 the International Monetary Fund Direction of Trade Statistics (2005). Other variables we use in the analysis are discussed later (see also Appendix Table A1 for detailed data definitions and sources).

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

II. Econometric Model

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

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

⁸ The main checklist includes three questions on the electoral process, four questions on the extent of political pluralism and participation, and three questions on the functioning of government. For each checklist question, zero to four points are added, depending on the comparative rights and liberties present (zero represents the least, four 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>.

TABLE 1—DESCRIPTIVE STATISTICS

| | All countries (1) | High-income countries (2) | Low-income countries (3) |
|--|----------------------|---------------------------------|--------------------------------|
| <i>Panel A</i> | | | |
| Freedom House measure of democracy | 0.57 (0.36) | 0.78 (0.30) | 0.36 (0.30) |
| Log GDP per capita $t-1$ (chain weighted 1996 prices) | 8.16 (1.02) | 9.02 (0.56) | 7.30 (0.53) |
| Observations | 945 | 473 | 472 |
| Countries | 150 | 93 | 98 |
| <i>Panel B</i> | | | |
| Polity measure of democracy _t | 0.57 (0.38) | 0.79 (0.31) | 0.36 (0.31) |
| Observations | 854 | 427 | 427 |
| Countries | 136 | 81 | 88 |
| <i>Panel C</i> | | | |
| Log population $t-1$ | 9.10 (1.54) | 9.13 (1.56) | 9.07 (1.52) |
| Education $t-1$ | 4.57 (2.86) | 6.62 (2.36) | 2.52 (1.53) |
| Observations | 676 | 338 | 338 |
| Countries | 95 | 57 | 65 |
| <i>Panel D</i> | | | |
| Savings rate $t-2$ | 0.17 (0.13) | 0.22 (0.10) | 0.11 (0.14) |
| Observations | 891 | 446 | 445 |
| Countries | 134 | 82 | 84 |
| <i>Panel E</i> | | | |
| Trade-weighted log GDP $t-1$ | 11.61 (8.43) | 12.98 (9.74) | 10.24 (6.62) |
| Observations | 895 | 448 | 447 |
| Countries | 124 | 75 | 85 |

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

where d_{it} is the democracy score of country i in period t . The lagged value of this variable on the right-hand side is included to capture persistence in democracy and also potentially mean-reverting dynamics (i.e., the tendency of the democracy score to return to some equilibrium value for the country). The main variable of interest is y_{it-1} , the lagged value of log income per capita. The parameter γ therefore measures the causal effect of income per capita on democracy. All

TABLE 2—FIXED EFFECTS RESULTS USING FREEDOM HOUSE MEASURE OF DEMOCRACY

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

Notes: Pooled cross-sectional OLS regression in column 1, with robust standard errors clustered by country in parentheses. Fixed effects OLS regressions in columns 2, 5, 6, 7, and 9, with country dummies and robust standard errors clustered by country in parentheses. Implied cumulative effect of income represents the coefficient estimate of log GDP per capita_{t-1}/(1–democracy_{t-1}), and the *p*-value from a nonlinear test of the significance of this coefficient is in brackets. Column 3 uses the instrumental variables method of Theodore W. Anderson and Cheng Hsiao (1982), with clustered standard errors, and columns 4 and 8 use the GMM of Manuel Arellano and Stephen R. Bond (1991), with robust standard errors; in both methods we instrument for income using a double lag. Year dummies are included in all regressions. Dependent variable is Freedom House measure of democracy. Base sample is an unbalanced panel, 1960–2000, with data at five-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 five years before it enters the panel. Columns 7 and 8 use ten-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 ten years before it enters the panel. Column 9 uses twenty-year data from the same sample, where, as before, the start date of the panel refers to the dependent variable (i.e., $t = 1980$, so $t - 1 = 1960$); a country must be independent for twenty 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 five lags. For detailed data definitions and sources, see Table 1 and Appendix Table A1.

other potential covariates are included in the vector \mathbf{x}_{it-1} . In addition, the δ_i 's denote a full set of country dummies and the μ_t 's denote a full set of time effects that capture common shocks to (common trends in) the democracy score of all countries; u_{it} is an error term, capturing all other omitted factors, with $E(u_{it}) = 0$ for all i and t .⁹

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 characteristics that affect the level of democracy. As is well known, when the true model is given by (1) and the δ_i 's are correlated with y_{it-1} or \mathbf{x}_{it-1} , then pooled OLS estimates are biased and inconsistent. More specifically, let x_{it-1}^j denote the j th component of the vector \mathbf{x}_{it-1} and let Cov denote population covariances. Then, if either $\text{Cov}(y_{it-1}, \delta_i + u_{it}) \neq 0$ or $\text{Cov}(x_{it-1}^j, \delta_i + u_{it}) \neq 0$ for some j , the OLS estimator will be inconsistent. In contrast, even when these covariances are nonzero, the fixed effects estimator will be consistent if $\text{Cov}(y_{it-1}, u_{it}) = \text{Cov}(x_{it-1}^j, u_{it}) = 0$ for all j (as $T \rightarrow \infty$). This structure of correlation

⁹ More generally, equation (1) can be combined with another equation that captures the effect of democracy on income. The simultaneous equation bias resulting from the endogeneity of democracy is addressed in Section IV. The estimation of the effect of democracy on income is beyond the scope of the current paper.

is particularly relevant in the context of the relationship between income and democracy because of the possibility of underlying political and social forces shaping both equilibrium political institutions and the potential for economic growth.

Nevertheless, there should be no presumption that fixed effects regressions necessarily estimate the causal effect of income on democracy. First, the regressor d_{it-1} is mechanically correlated with u_{is} for $s < t$ so the standard fixed effect estimator is biased (e.g., Jeffrey M. Wooldridge 2002, chap. 11). It can be shown, however, that the fixed effects OLS estimator becomes consistent as the number of time periods in the sample increases (i.e., as $T \rightarrow \infty$). We discuss and implement a number of strategies to deal with this problem in Section III.

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

III. Fixed Effects Estimates

A. Main Results

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

The first columns of Table 2 and Table 3 replicate the standard pooled OLS regressions previously used in the literature using the five-year sample. These regressions include the (five-year) lag of democracy and log income per capita as the country variables, as well as a full set of time dummies. Lagged democracy is highly significant and indicates that there is a considerable degree of persistence in democracy. Log income per capita is also significant and illustrates the well-documented positive relationship between income and democracy. Though statistically significant, the effect of income is quantitatively small. For example, the coefficient of 0.072 (standard error = 0.010) in column 1 of Table 2 implies that a 10 percent increase in GDP per capita is associated with an increase in the Freedom House score of less than 0.007, which is very small (for comparison, the gap between the United States and Colombia today is 0.5). If this pooled cross-section regression identified the causal effect of income on democracy, then the long-run effect would be larger than this, because the lag of democracy on the right-hand side would be increasing over time, causing a further increase in the democracy score. The implied cumulative effect of log GDP per capita on democracy is shown in the fifth row. Since lagged democracy has a coefficient of 0.706, the cumulative effect of a 10 percent increase in GDP per capita is $0.007/(1-0.706) \approx 0.024$, which is still quantitatively small.

The remaining columns of Table 2 and Table 3 present our basic results with fixed effects. Column 2 shows that the relationship between income and democracy disappears once fixed effects are included. For example, in Table 2 with Freedom House data, the estimate of γ is 0.010 with a standard error of 0.035, which makes it highly insignificant. With the Polity data in

TABLE 3—FIXED EFFECTS RESULTS USING POLITY MEASURE OF DEMOCRACY

| | Base sample, 1960–2000 | | | | | | | | |
|-------------------------------------|------------------------|-------------------|-------------------|-------------------|-------------------|-------------------|-------------------|-------------------|--------------------|
| | Five-year data | | | Annual data | | | Ten-year data | | Twenty-year data |
| | Pooled OLS | Fixed effects OLS | Anderson-Hsiao IV | Arellano-Bond GMM | Fixed effects OLS | Fixed effects OLS | Fixed effects OLS | Arellano-Bond GMM | Fixed effects OLS |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Dependent variable is democracy | | | | | | | | | |
| Democracy _{t-1} | 0.749 (0.034) | 0.449 (0.063) | 0.582 (0.127) | 0.590 (0.106) | | [0.00] | 0.060 (0.091) | 0.309 (0.134) | -0.516 (0.165) |
| Log GDP per capita _{t-1} | 0.053 (0.010) | -0.006 (0.039) | -0.413 (0.127) | -0.351 (0.127) | -0.011 (0.055) | [0.53] | 0.007 (0.070) | -0.368 (0.190) | -0.1260 (0.164) |
| Hansen J test | | | [0.03] | [0.03] | | | | | [0.01] |
| AR(2) test | | | [0.39] | [0.39] | | | | | [0.38] |
| Implied cumulative effect of income | 0.211 [0.00] | -0.011 [0.89] | -0.856 [0.00] | -0.856 [0.00] | | | 0.007 [0.92] | -0.533 [0.04] | -0.083 [0.45] |
| Observations | 854 | 854 | 747 | 747 | 880 | 3701 | 419 | 302 | 168 |
| Countries | 136 | 136 | 114 | 114 | 136 | 134 | 114 | 107 | 100 |
| R-squared | 0.77 | 0.82 | | | 0.77 | 0.96 | 0.77 | | 0.87 |

Notes: Pooled cross-sectional OLS regression in column 1, with robust standard errors clustered by country in parentheses. Fixed effects OLS regressions in columns 2, 5, 6, 7, and 9, with country dummies and robust standard errors clustered by country in parentheses. Implied cumulative effect of income represents the coefficient estimate of log GDP per capita_{t-1}/(1–democracy_{t-1}) and the *p*-value from a nonlinear test of the significance of this coefficient is in brackets. Column 3 uses the instrumental variables method of Anderson and Hsiao (1982), with clustered standard errors, and columns 4 and 8 use the GMM of Arellano and Bond (1991), with robust standard errors; in both methods we instrument for income using a double lag. Year dummies are included in all regressions. Dependent variable is Polity measure of democracy. Base sample is an unbalanced panel, 1960–2000, with data at five-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 five years before it enters the panel. Columns 7 and 8 use ten-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 ten years before it enters the panel. Column 9 uses twenty-year data from the same sample, where, as before, the start date of the panel refers to the dependent variable (i.e., $t = 1980$, so $t - 1 = 1960$); a country must be independent for twenty 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 five lags. For detailed data definitions and sources, see Table 1 and Appendix Table A1.

Table 3, the estimate of γ has the “wrong” (negative) sign, -0.006 (standard error = 0.039). The bottom rows in both tables again show the implied cumulative effects of income on democracy, which are small or negative.

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

That these results are not driven by some unusual feature of the data is further shown by Figures 2 and 3, which plot the change in the Freedom House and Polity scores for each country between 1970 and 1995 against the change in GDP per capita over the same period.¹⁰ They show

¹⁰ These scatterplots correspond to the estimation of equation (long-run relationship specification) in Section VIA with a start date at 1970 and end date at 1995. 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.

clearly that there is no strong relationship between income growth and changes in democracy over this period.

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

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

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

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

Although the instrumental variable estimator of Anderson and Hsiao (1982) leads to consistent estimates, it is not efficient, since, under the assumption of no further serial correlation in u_{it} , not only d_{it-2} , but all further lags of d_{it} are uncorrelated with Δu_{it} , and can also be used as additional instruments. Arellano and Bond (1991) develop a generalized method of moments (GMM) estimator using all of these moment conditions. When these conditions are valid, this GMM estimator is more efficient than the Anderson and Hsiao (1982) estimator. We use this GMM estimator in column 4. The coefficients are now even more negative and more precisely estimated, for example -0.129 (standard error = 0.076) in Table 2.¹¹ In this case, the two standard error bands comfortably exclude the corresponding OLS estimate of γ (which, recall, was 0.072). In addition, the presence of multiple instruments in the GMM procedure allows us to investigate whether the assumption of no serial correlation in u_{it} can be rejected, and also allows us 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

With the Polity data, both the Anderson and Hsiao and GMM procedures lead to more negative (and statistically significant) estimates. In this case, however, although 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 two standard error bands comfortably

¹¹ In addition, Arellano and Olympia Bover (1995) also use time-differenced instruments for the level equation, (1). Nevertheless, these instruments would be valid only 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 with our dataset, which uses only the democracy information every fifth year. The estimates in all columns are very similar, but in this case, the AR(2) test shows evidence for additional serial correlation, which is not surprising, given the serial correlation that averaging introduces. This motivates our reliance on the five-year or annual datasets. Our analysis with annual data in column 6 of Tables 2 and 3 makes use of all of the available data.

exclude the corresponding OLS coefficient (the OLS estimate without lagged democracy, which is shown in the first column of Table 5 and Table 6, is 0.233 with a standard error of 0.013).

Column 6 estimates (1) with OLS using annual observations. This is useful since the fixed effect OLS estimator becomes consistent as the number of observations becomes large. With annual observations, we have a reasonably large time dimension. Estimating the same model on annual data with a single lag would, however, induce significant serial correlation (since our results so far indicate that five-year lags of democracy predict changes in democracy). For this reason, we now include five lags of both democracy and log GDP per capita in these annual regressions. Column 6 in both tables reports the *p*-value of an F-test for the joint significance of these variables. There is no evidence of a significant positive effect of income on democracy either with the Freedom House or the Polity data (while democracy continues to be strongly predicted by its lags).

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

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

B. Robustness

Table 4 investigates the robustness of these results. To save space, we report the robustness checks for the Freedom House data only (the results with Polity are similar and are available upon request). Columns 1–4 examine alternative samples. Columns 1 and 2 show the regressions corresponding to columns 2 and 4 of Table 2 for a balanced sample of countries from 1970 to 2000. This is useful to check whether entry and exit of countries from the base sample of Tables 2 and 3 might be affecting the results. Both columns provide very similar results. For example, using the balanced sample of Freedom House data and the fixed effects OLS specification, the estimate of γ is -0.031 (standard error = 0.049). Columns 3 and 4 exclude former socialist countries, again with very similar results.

Columns 5–8 investigate the influence of various covariates on the relationship between income and democracy. Columns 5 and 6 include log population and age structure, and columns 7 and 8 add education.¹³ In each case, we present both fixed effects and GMM estimates. The results show that these covariates do not affect the (lack of) relationship between income and democracy when fixed effects are included. Age structure variables are significant in the specification that excludes education, but not when education is included. Education is itself insignificant with a negative coefficient. The causal effect of education on democracy, which is another basic tenet of the modernization hypothesis, is therefore also not robust to controlling for country fixed effects. This finding is consistent with the results reported in Acemoglu et al.

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 data are from the World Bank (2002). We measure education as total years of schooling in the population age 25 and above. For detailed definitions and sources, see Appendix Table A1.

TABLE 4—FIXED EFFECTS RESULTS USING FREEDOM HOUSE MEASURE OF DEMOCRACY: ROBUSTNESS CHECKS

| | Five-year data | | | | | | | |
|-------------------------------------|--|------------------------------|-----------------------------|------------------------------|-----------------------------|-----------------------------|-----------------------------|------------------------------|
| | Base sample, 1960–2000, without former socialist countries | | | | Base sample, 1960–2000 | | | |
| Balanced panel, 1970–2000 | Fixed effects OLS (1) | Arellano- Bond GMM (2) | Fixed effects OLS (3) | Arellano- Bond GMM (4) | Fixed effects OLS (5) | Arellano-Bond GMM (6) | Fixed effects OLS (7) | Arellano- Bond GMM (8) |
| Dependent variable is democracy | | | | | | | | |
| Democracy t_{-1} | 0.283 (0.058) | 0.472 (0.092) | 0.362 (0.052) | 0.436 (0.085) | 0.353 (0.053) | 0.480 (0.087) | 0.351 (0.055) | 0.499 (0.097) |
| Log GDP per capita t_{-1} | -0.031 (0.049) | -0.262 (0.128) | 0.005 (0.035) | -0.151 (0.078) | 0.015 (0.041) | -0.008 (0.139) | -0.001 (0.049) | -0.121 (0.182) |
| Log population t_{-1} | | | | | -0.109 (0.100) | -0.001 (0.113) | -0.042 (0.108) | 0.049 (0.143) |
| Education t_{-1} | | | | | | | -0.007 (0.020) | -0.020 (0.026) |
| Age structure t_{-1} | | | | | [0.05] | [0.63] | [0.19] | [0.27] |
| Hansen J test | | [0.40] | | [0.34] | | [0.08] | | [0.15] |
| AR(2) test | | [0.73] | | [0.49] | | [0.43] | | [0.88] |
| Implied cumulative effect of income | -0.043 [0.53] | -0.496 [0.03] | 0.008 [0.89] | -0.268 [0.05] | 0.023 [0.72] | -0.015 [0.96] | -0.002 [0.98] | -0.242 [0.50] |
| Observations | 630 | 567 | 908 | 823 | 863 | 731 | 676 | 589 |
| Countries | 90 | 81 | 128 | 124 | 142 | 120 | 95 | 92 |
| R-squared | 0.80 | | 0.79 | | 0.80 | | 0.77 | |

Notes: Fixed effects OLS regressions in columns 1, 3, 5, and 7 with country dummies and robust standard errors clustered by country in parentheses. In columns 2, 4, 6, and 8, we use the GMM of Arellano and Bond (1991), with robust standard errors; in this method we instrument for income using a double lag. Year dummies are included in all regressions. Implied cumulative effect of income represents the coefficient estimate of log GDP per capita $_{t-1}$ /(1—democracy $_{t-1}$), and the p-value from a nonlinear test of the significance of this coefficient is in brackets. Dependent variable is the Freedom House measure of democracy. Base sample is an unbalanced panel, 1960–2000, with data at five-year intervals in levels where the start date of the panel refers to the dependent variable (i.e., $t = 1960$, so $t - 1 = 1955$); a country must be independent for five years before it enters the panel. Columns 1 and 2 use a balanced panel from 1970 to 2000. Columns 3 and 4 exclude Soviet bloc countries. Education is average years of total schooling in the population. Columns 5–8 include but do not display the median age of the population at $t - 1$ and four 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 gives the p-value for the joint significance of these variables. For detailed data definitions and sources, see Table 1 and Appendix Table A1.

(2005). In regressions not included here, we find that our results remain unchanged if we include the full set of covariates from Barro's (1999) baseline specification.¹⁴

In addition, in regressions not reported here, we checked for nonlinear and nonmonotonic effects of income on democracy and for potential nonlinear interactions between income and other variables, and found no evidence of such relationships. We also checked and found no evidence of an effect of the volatility in the growth rate of income per capita on democracy.¹⁵

¹⁴ These results are included in the working paper version and are very similar to the results in columns 7 and 8 of Table 4.

¹⁵ We also investigated the effect of growth accelerations using a definition similar to that in the recent paper by Ricardo Hausmann, Lant Pritchett, and Dani Rodrik (2005) and found no effect of growth accelerations on democracy. Interestingly, however, the incidence of crises is correlated with democracy once fixed effects are taken into account.

The only subsample where we find a positive association between income per capita and democracy conditional on fixed effects is the postwar sample with 18 West European countries. This relationship holds only with the Freedom

IV. Instrumental Variable Estimates

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

A. 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

$$(3) \quad y_{it-1} = \pi^F s_{it-2} + \alpha^F d_{it-1} + \mathbf{x}'_{it-1} \boldsymbol{\beta}^F + \mu_{it-1}^F + \delta_i^F + u_{it-1}^F,$$

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

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

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

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

We report a number of different specifications, with or without lagged democracy on the right-hand side, and with or without GMM. The first three columns show the OLS estimates in the pooled cross section, the fixed effects estimates without lagged democracy on the right-hand side, and the fixed effects estimates with lagged democracy on the right-hand side. Without fixed effects, there is a strong association between income per capita and democracy (the relationship in column 1 is stronger than before because it does not include lagged democracy on the right-hand side). With fixed effects, this relationship is no longer present. The remaining columns look at IV specifications and the bottom panel shows the corresponding first stages.

Column 4 shows a strong first-stage relationship between income and the savings rate, with a t-statistic of almost 5. The 2SLS estimate of the effect of income per capita on democracy is -0.035 (standard error = 0.094). The two standard error bands comfortably exclude the OLS estimate from column 1. Column 5 adds lagged democracy to the right-hand side. The first stage

House data, however, and not with the Polity data, and also disappears when we look at a longer sample than the post-war period alone. Details are available upon request.

TABLE 5—FIXED EFFECTS RESULTS USING FREEDOM HOUSE MEASURE OF DEMOCRACY: TWO-STAGE LEAST SQUARES WITH SAVINGS RATE INSTRUMENT

| Base sample, 1960–2000 | | | | | | | | | |
|-------------------------------------|------------------|---|-----------------------|------------------------|------------------------|-----------------------|------------------------|------------------------|------------------------|
| | All countries | | | | | | | | |
| | Pooled OLS (1) | Fixed effects OLS (2) | Fixed effects OLS (3) | Fixed effects 2SLS (4) | Fixed effects 2SLS (5) | Arellano-Bond GMM (6) | Fixed effects 2SLS (7) | Fixed effects 2SLS (8) | Fixed effects 2SLS (9) |
| <i>Panel A</i> | | <i>Dependent variable is democracy</i> | | | | | | | |
| Democracy t_{-1} | | 0.359 (0.054) | | 0.363 (0.056) | 0.427 (0.100) | | | [0.00] | |
| Log GDP per capita t_{-1} | 0.233 (0.013) | 0.044 (0.051) | 0.009 (0.038) | -0.035 (0.094) | -0.020 (0.081) | -0.228 (0.102) | -0.036 (0.191) | -0.074 (0.113) | 0.016 (0.095) |
| Labor share t_{-1} | | | | | | 0.250 (0.199) | | | |
| <i>Panel B</i> | | <i>First stage for log GDP per capita t_{-1}</i> | | | | | | | |
| Democracy t_{-1} | | | | 0.144 (0.066) | | | | [0.24] | |
| Labor share t_{-1} | | | | | | 0.329 (0.187) | | | |
| Savings rate t_{-2} | | | 1.356 (0.277) | 1.343 (0.270) | | 1.202 (0.315) | 1.173 (0.254) | 1.022 (0.218) | |
| Savings rate t_{-3} | | | | | | | | 0.720 (0.182) | |
| Hansen J test | | | | | | [0.34] | | | |
| AR(2) test | | | | | | [0.72] | | | |
| Implied cumulative effect of income | | 0.014 [0.82] | | -0.031 [0.80] | | -0.398 [0.01] | | | |
| Observations | 900 | 900 | 891 | 900 | 891 | 764 | 471 | 733 | 796 |
| Countries | 134 | 134 | 134 | 134 | 134 | 124 | 98 | 124 | 125 |
| R-squared in first stage | | | 0.96 | 0.96 | | 0.98 | 0.97 | 0.97 | |

Notes: Pooled cross-sectional OLS regression in column 1, with robust standard errors clustered by country in parentheses. Fixed effects OLS regressions in columns 2 and 3 with country dummies and robust standard errors clustered by country in parentheses. Fixed effects 2SLS regressions in columns 4, 5, 7, 8, and 9 with country dummies and robust standard errors clustered by country in parentheses; first-stage regressions are displayed in panel B and include all second-stage covariates (apart from income) on the right-hand side with robust standard errors clustered by country in parentheses. GMM of Arellano-Bond in column 6 with robust standard errors; in this method we instrument for income in the first differenced equation with the first difference of the instrument. Year dummies are included in all regressions. Implied cumulative effect of income represents the coefficient estimate of $\log \text{GDP per capita}_{t-1}/(1-\text{democracy}_{t-1})$, and the *p*-value from a nonlinear test of the significance of this coefficient is in brackets. Dependent variable is Freedom House measure of democracy. Base sample is an unbalanced panel, 1960–2000, with data at five-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 five years before it enters the panel. Columns 4–9 include instrument for $\log \text{GDP per capita}_{t-1}$ with savings rate t_{-2} . Column 9 includes savings rate t_{-3} as an additional instrument. Column 8 includes but does not display democracy t_{-1} , democracy t_{-2} , and democracy t_{-3} ; we report the *p*-value from an *F*-test for the joint significance of all three lags. For detailed data definitions and sources, see Table 1 and Appendix Table A1.

is very similar and the estimate of γ is now -0.020 (standard error = 0.081). Column 6 uses the GMM procedure, again with the savings rate as the excluded instrument for income. Now the estimate of γ is again negative, relatively large, and significant at 5 percent. These IV results, therefore, show no evidence of a positive causal effect of income on democracy.

The remaining columns investigate the robustness of this finding and the plausibility of our exclusion restriction. Column 7 adds labor share as an additional regressor, to check whether

a potential correlation between the savings rate and inequality might be responsible for our results.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 8 includes further lags of democracy to check whether systematic differences in savings rates between democracies and dictatorships might have an effect on the results. The estimate of γ is similar to before and, if anything, a little more negative in this case. Finally, column 9 adds a further lag of the savings rate as an instrument. This is useful since it enables a test of the overidentifying restriction (namely, a test of whether the savings rate at $t - 3$ is a valid instrument conditional on the savings rate at $t - 2$ being a valid instrument). The 2SLS estimate of γ is again similar and the overidentification restriction that the instruments are valid is accepted comfortably (at the p -value of 1.00).

B. Trade-Weighted World Income Instrument

Our second instrument exploits trade linkages across countries. To develop this instrument, let $\Omega = [\omega_{ij}]_{i,j}$ denote the $N \times N$ matrix of (time-invariant) trade shares between countries in our sample, where N is the total number of countries. More precisely, ω_{ij} is the share of trade between country i and country j in the GDP of country i which measure using trade shares between 1980 and 1989 (which is chosen to maximize coverage).¹⁷

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

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

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

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

$$(5) \quad \hat{Y}_{it-1} = \sum_{j=1, j \neq i}^N \omega_{ij} Y_{jt-1},$$

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

$$(6) \quad y_{it-1} = \tilde{\pi}^F \sum_{j=1, j \neq i}^N \omega_{ij} Y_{jt-1} + \alpha^F d_{it-1} + \mathbf{x}'_{it-1} \boldsymbol{\beta}^F + \mu_{t-1}^F + \delta_i^F + u_{it-1}^F,$$

¹⁶This 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.

¹⁷We obtain similar results if we use predicted average trade shares from a standard gravity equation, as in Jeffrey A. Frankel and David Romer (1999). See the previous version of the paper for details.

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

There may be reasons for this identification assumption to be violated. For example, Y_{jt-1} may be correlated with democracy in country j at time t , d_{jt} , which may influence d_{it} through other political, social, or cultural channels.¹⁸ Although we have no way of ruling out these channels of influence a priori, below we control for the direct effect of the democracy of trading partners and find no evidence to support such a channel.¹⁹

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

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

Overall, our two IV strategies give results consistent with the fixed effects estimates and indicate that there is no evidence for a strong causal effect of income on democracy.²⁰

V. Fixed Effects Estimates over 100 Years

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

Although historical data are typically less reliable, the Polity IV dataset extends back to the beginning of the nineteenth century for all independent countries, and Maddison (2003) gives

¹⁸ Because ω_{ij} is time-invariant, it does not capture *changes* in trade patterns and in trade agreements, which could possibly have a direct effect on democracy.

¹⁹ There is an econometric problem arising from the general equilibrium nature of equation (4). Since this equation also applies for country j , the disturbance term ε_{it-1} , which determines Y_{it-1} , will be correlated with Y_{jt-1} , inducing a correlation between Y_{jt-1} and ε_{it-1} , and thus between \hat{Y}_{it-1} and ε_{it-1} . However, under some regularity conditions, the problem disappears as $N \rightarrow \infty$. In exercises included in the previous version of our paper, we have estimated ζ adjusting for potential bias and found no change in our results. Details available upon request.

²⁰ We also tested the overidentifying restriction that the savings rate instrument is valid conditional on the trade-weighted income instrument being valid, and vice versa. Both hypotheses are accepted comfortably (at the p -values of 0.99 and 1.00, respectively).

TABLE 6—FIXED EFFECTS RESULTS USING FREEDOM HOUSE MEASURE OF DEMOCRACY: TWO-STAGE LEAST SQUARES WITH TRADE-WEIGHTED WORLD INCOME INSTRUMENT

| Base sample, 1960–2000 | | | | | | | | | |
|---|-------------------|---|--------------------|--------------------|-------------------|--------------------|--------------------|--------------------|--------------------|
| <i>All countries</i> | | | | | | | | | |
| 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) | |
| <i>Panel A</i> | | <i>Dependent variable is democracy</i> | | | | | | | |
| Democracy _{t-1} | | 0.376 (0.051) | | 0.393 (0.057) | 0.478 (0.094) | | | | |
| Log GDP per capita _{t-1} | 0.233 (0.013) | 0.038 (0.045) | 0.001 (0.034) | -0.213 (0.150) | -0.120 (0.105) | -0.133 (0.077) | -0.202 (0.130) | -0.198 (0.160) | -0.217 (0.149) |
| Trade-weighted democracy _t | | | | | | -0.137 (0.635) | | | |
| <i>Panel B</i> | | <i>First stage for log GDP per capita_{t-1}</i> | | | | | | | |
| Democracy _{t-1} | | | 0.169 (0.063) | | | | | | |
| Trade-weighted democracy _{t-1} | | | | | | -1.195 (0.959) | | | |
| Trade-weighted log GDP _{t-1} | | | 0.402 (0.083) | 0.421 (0.082) | | 0.441 (0.070) | | | 0.529 (0.180) |
| Trade-weighted log GDP _{t-2} | | | | | | | 0.341 (0.090) | -0.127 (0.206) | |
| Hansen J test | | | | | [0.19] | | | | |
| AR(2) test | | | | | [0.50] | | | | |
| Implied cumulative effect of income | | 0.002 [0.98] | | -0.198 [0.28] | -0.255 [0.07] | | | | |
| Observations | 906 | 906 | 895 | 906 | 895 | 812 | 906 | 906 | 906 |
| Countries | 124 | 124 | 124 | 124 | 124 | 122 | 124 | 124 | 124 |
| R-squared in first stage | | | 0.95 | 0.96 | | 0.95 | 0.95 | 0.95 | 0.95 |

Notes: 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. GMM of Arellano-Bond in column 6 with robust standard errors; in this method we instrument for income in the first differenced equation with the first difference of the instrument. Year dummies are included in all regressions. Implied cumulative effect of income represents the coefficient estimate of $\log \text{GDP per capita}_{t-1}/(1-\text{democracy}_{t-1})$, and the *p*-value from a nonlinear test of the significance of this coefficient is in brackets. Dependent variable is Freedom House measure of democracy. Base sample is an unbalanced panel, 1960–2000, with data at five-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 five years before it enters the panel. Columns 4–8 instrument for $\log \text{GDP per capita}_{t-1}$ with trade-weighted world $\log \text{GDP}_{t-1}$. Column 9 uses trade-weighted world $\log \text{GDP}_{t-2}$ as an additional instrument. For detailed data definitions and sources, see Table 1 and Appendix Table A1.

estimates of income per capita for many countries during this period. To investigate longer-term relationships between income and democracy, we construct a 25-year dataset starting in 1875.²¹ This dataset contains a balanced panel of 25 countries for which democracy, lagged democracy

²¹ Since Maddison reports income estimates for 1820, 1870, and 1929, we assign income per capita from 1820 to 1850, income per capita from 1870 to 1875, and income per capita from 1929 to 1925. All of our results are robust to dropping the 1875 observation so as not to use the 1850 estimate of income per capita as the value of log income. If income per capita is not available for a particular country-year pair, it is estimated at the lowest aggregation level at which Maddison's data are available (e.g., Costa Rica, Guatemala, and Honduras are assigned the same income per capita in 1850) and the standard errors are computed by clustering at the highest aggregation level assigned to a particular country.

(calculated 25 years earlier), and lagged income (calculated 25 years earlier) are available for every twenty-fifth year between 1875 and 2000.²² We also construct a larger dataset with 50-year observations that starts in 1900. This dataset contains a larger sample of 37 independent countries.²³

Table 7 presents the basic fixed effects results with these two samples. The specifications of columns 1–4 of the top panel in Table 7 are identical to the specifications of columns 1, 2, 4, and 5 of Table 2, but we use the 25-year valid sample over 1875–2000 with the Polity index as the dependent variable. These results are very similar to those from the postwar panel presented in Tables 2–4. For example, without fixed effects, the coefficient on income per capita is positive and significant at 0.116 (standard error = 0.034), and with fixed effects the coefficient has the wrong sign and is insignificant at –0.020 (0.093). Column 5 reports the baseline regression on a smaller sample, excluding all countries with imputed income estimates (see footnote 21). The results are very similar to those of column 2. The bottom panel repeats the same regressions using the data in 50-year intervals from 1900 to 2000, again with similar results. Once fixed effects are included, the coefficient on income is small and insignificant. Figure 4 depicts these results graphically and shows that there is little relationship between changes in democracy and income in this 100-year sample.

As emphasized in Section II, these results do not necessarily correspond to the causal effect of income on democracy, since there may be omitted time-varying covariates.²⁴ Nevertheless, most plausible omitted variables (as well as potential reverse causality) would bias these estimates upward, so it is safe to conclude that there is no evidence of causal effect of income on democracy over the past 100 years.

VI. Sources of Income-Democracy Correlations

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

A. Divergent Development Paths

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

$$(7) \quad d_{it} = \gamma y_{it} + \delta_i^d + u_{it}^d.$$

²² The countries included in this dataset are Argentina, Austria, Belgium, Brazil, Chile, China, Colombia, Costa Rica, Denmark, El Salvador, Greece, Guatemala, Honduras, Mexico, Netherlands, Nicaragua, Norway, Sweden, Switzerland, Thailand, Turkey, United Kingdom, United States, Uruguay, and Venezuela.

²³ In addition to the countries in the 25-year sample, this sample includes Bolivia, Dominican Republic, Ecuador, France, Haiti, Iran, Liberia, Nepal, Oman, Paraguay, Portugal, and Spain.

²⁴ We also looked at IV regressions on this sample using a version of trade-weighted income constructed as in Section IV. In this smaller sample of countries, however, the first-stage relationship was not strong enough to allow the estimation of meaningful second-stage regressions.

TABLE 7—FIXED EFFECTS RESULTS USING POLITY MEASURE OF DEMOCRACY IN THE LONG RUN

| | <i>Balanced panel, 1875–2000</i> | | | | |
|-------------------------------------|--|-----------------------------|-----------------------------|-----------------------------|-----------------------------|
| | Pooled OLS (1) | Fixed Effects OLS (2) | Arellano-Bond GMM (3) | Fixed Effects OLS (4) | Fixed Effects OLS (5) |
| <i>Panel A</i> | <i>Dependent variable is democracy</i> | | | | |
| Democracy _{t-1} | 0.487 (0.085) | 0.192 (0.119) | 0.439 (0.143) | | 0.212 (0.140) |
| Log GDP per capita _{t-1} | 0.116 (0.034) | -0.020 (0.093) | -0.495 (0.266) | 0.003 (0.092) | 0.074 (0.118) |
| Hansen J test | | | [0.27] [0.42] | | |
| AR(2) test | | | | | |
| Implied cumulative effect of income | 0.226 [0.00] | -0.025 [0.84] | -0.882 [0.02] | | 0.094 [0.53] |
| Observations | 150 | 150 | 125 | 150 | 78 |
| Countries | 25 | 25 | 25 | 25 | 13 |
| R-squared | 0.55 | 0.65 | | 0.63 | 0.72 |
| <i>Panel B</i> | <i>Dependent variable is democracy</i> | | | | |
| Democracy _{t-1} | 0.233 (0.070) | -0.248 (0.124) | 0.319 (0.148) | | -0.266 (0.188) |
| Log GDP per capita _{t-1} | 0.191 (0.043) | 0.039 (0.110) | -0.411 (0.194) | -0.004 (0.092) | 0.028 (0.222) |
| Hansen J test | | | [0.87] | | |
| Implied cumulative effect of income | 0.249 [0.00] | 0.031 [0.72] | -0.604 [0.04] | | 0.031 [0.90] |
| Observations | 111 | 111 | 74 | 111 | 48 |
| Countries | 37 | 37 | 37 | 37 | 16 |
| R-squared | 0.49 | 0.72 | | 0.70 | 0.69 |

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

Moreover, suppose that the statistical process for income per capita is

$$(8) \quad y_{it} = \delta_i^d + u_{it}^y.$$

The parameter γ again represents the causal effect of income on democracy, while δ_i^d and δ_i^y correspond to fixed differences in levels of democracy and income across countries. These fixed differences have so far been taken out by country fixed effects.²⁵

²⁵ Allowing democracy to influence income in equation (8) does not change the conclusions, as long as the effect is nonnegative.

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

$$(9) \quad d_{iT} - d_{iT-S} = \gamma(y_{iT} - y_{iT-S}) = u_{iT}^d - u_{iT-S}^d,$$

and

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

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

$$(10) \quad \text{plim } \hat{\gamma}_S^{FE} = \gamma + \frac{\text{Cov}(u_{iT}^d - u_{iT-S}^d, u_{iT}^y - u_{iT-S}^y)}{\text{Var}(u_{iT}^y - u_{iT-S}^y)}.$$

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

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

As an example, let us contrast the development experience of the United States with those of Peru and Bolivia. The United States grew rapidly during the late eighteenth and nineteenth centuries and became gradually more democratic, while these Andean societies stagnated and did not show a tendency to become democratic. Nondemocracy and stagnation in the Andes cannot be separated; the hacienda system, based on labor repression and the control of the indigenous Indian communities, was not conducive to industrialization and rapid growth during the nineteenth century. This system and its continuation, even after the abolition of formal systems of Indian tribute and forced labor, were not consistent with democratic institutions and a relatively equal distribution of political power within the society. This contrasts with the small-holder society in the United States, which resulted from the process of European colonization based on settlements in relatively empty and healthy lands. This social structure dominated by small-holders was much more consistent with democratic representation,²⁶ which in turn was conducive to an environment where new industries and new entrepreneurs could flourish with relatively little resistance from established interests.²⁷ This description suggests that beyond the impact of income on democracy or the impact of democracy on income, we may want to think of political and economic development taking place jointly.²⁸ These ideas in general, and the contrast

²⁶ See David W. Galenson (1996) and Alexander Keysser (2000) on the development of the northeastern United States as a settler colony, with relatively democratic and open institutions. See James Lockhart (1968) and Nils Jacobsen (1993) for the creation and persistence of colonial practices in Peru, and see Herbert S. Klein (1992) on Bolivia. For a contrast of these development paths, see, among others, Engerman and Sokoloff (1997) and Acemoglu, Johnson, and Robinson (2001, 2002).

²⁷ Sokoloff and B. Zorina Khan (1990) and Khan and Sokoloff (1993) show that many of the major US inventors in the nineteenth century were not members of the already established economic elite, but newcomers with diverse backgrounds.

²⁸ Examples of models in which democracy and economic outcomes are jointly determined include Acemoglu and Robinson (2000, 2007), Acemoglu (2007), Matteo Cervellati, Piergiuseppe Fortunato, and Uwe Sunde (2005), and Humberto Llavorad and Robert J. Oxoby (2005).

between the northeast United States and the Andes in particular, are captured by our notion of divergent development paths.

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

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

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

$$u_{it}^d = \eta_{it}^d + \xi_{it}^d \text{ and } u_{it}^y = \eta_{it}^y + \xi_{it}^y, \quad \text{where} \quad \eta_{it}^d = \eta_{it-1}^d + v_{it}^d \text{ and } \eta_{it}^y = \eta_{it-1}^y + v_{it}^y,$$

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

$$(11) \quad \text{plim } \hat{\gamma}_S^{FE} = \begin{cases} \gamma + \frac{\sigma_{-T^*}^2}{\sigma_{vy}^2 + 2\sigma_{wy}^2/S} & \text{if } T^* \notin [T - S, T] \\ \gamma + \frac{(\sigma_{T^*}^2 - \sigma_{-T^*}^2)/S + \sigma_{-T^*}^2}{\sigma_{vy}^2 + 2\sigma_{wy}^2/S} & \text{if } T^* \in [T - S, T] \end{cases},$$

where the second equality exploits the fact that v_i 's and u_i 's are serially uncorrelated.

Equation (11) emphasizes that the bias of $\hat{\gamma}_S^{FE}$ will crucially depend on whether the critical juncture T^* takes place between the dates $T - S$ and T . If it does not do so, the first term applies, and to the extent that $\sigma_{-T^*}^2 \approx 0$, the estimator will be “approximately” consistent. Note, however,

that as S increases, the denominator falls, so the potential bias in this estimator may increase when $\sigma_{T^*}^2 > 0$. Nevertheless, by and large, the fixed effects estimator will be approximately consistent when the critical juncture does not take place during the sample period. This is the reason we have some confidence in the results obtained using the fixed effects regressions in the postwar and twentieth-century samples.

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

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

B. Income and Democracy over the Past 500 Years

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

Acemoglu et al. (2005) provide a coding of constraint on the executive for European countries going back to 1500 (based on the Polity definition). Constraint on the executive is a key input to the Polity democracy score for European countries. In addition, it seems reasonable that constraint on the executive for non-European countries and the other components of the Polity index (competitiveness of executive recruitment, openness of executive recruitment, and competitiveness of political participation) both for European and non-European countries should take the lowest score in 1500. Based on this information, we construct estimates for the Polity composite index for 1500 (details available upon request). We combine these data with Maddison’s (2003) estimates of income per capita in 1500 and 2000 and Polity’s democracy score for 2000.²⁹

We first check whether the current income-democracy correlation is indeed caused by changes over the past 500 years by estimating (9) over this sample period. Table 8A, column 1, provides estimates for our entire world sample, and Table 8B, column 1, focuses on

²⁹ Countries that have become independent in the 1990s are excluded from the sample. If the Polity score for 2000 is missing, we assign the 1995 score to the observation.

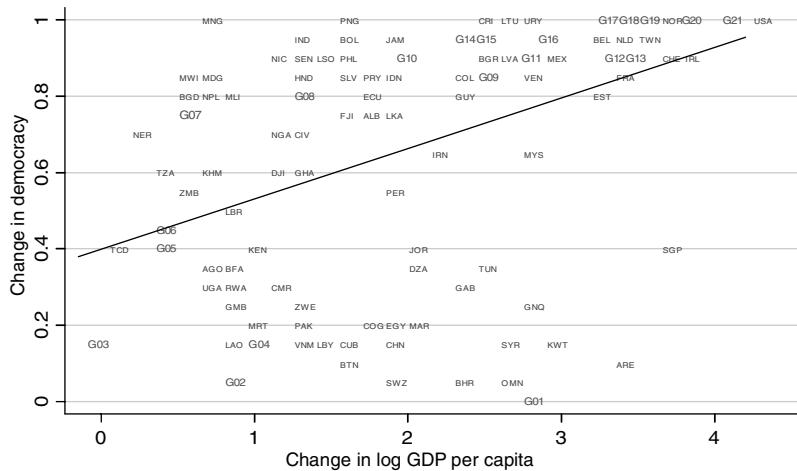


FIGURE 5. CHANGE IN DEMOCRACY AND CHANGE IN INCOME, 1500–2000

Notes: See Appendix Table A1 for data definitions and sources. Changes are total differences between 1500 and 2000. GDP per capita is from Maddison (2003). Democracy is calculated using the Polity measure of democracy, which comprises in part constraint on the executive; data for 1500 from Acemoglu et al. (2005). The regression represented by the fitted line yields a coefficient of 0.134 (standard error = 0.021), $N = 135$, $R^2 = 0.20$, and corresponds to the specification of Table 8A, column 1. The “G” prefix corresponds to the average for groups of countries. G01 is QAT and SAU; G02 is IRQ and PRK; G03 is AFG and SLE; G04 is MMR and SDN; G05 is HTI and TGO; G06 is BDI, COM, and GIN; G07 is CAF and GNB; G08 is BEN and MOZ; G09 is RUS and TUR; G10 is DOM, GTM, and ROM; G11 is ARG, BRA, HUN, and POL; G12 is GBR and KOR; G13 is DNK and SWE; G14 is PAN and ZAF; G15 is BWA and THA; G16 is CHL and ITA; G17 is ESP, PRT, and TTO; G18 is AUT, DEU, GRC, ISR, and MUS; G19 is CYP and ISL; G20 is FIN, JPN, and NZL; and G21 is AUS and CAN.

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

We next investigate how the inclusion of proxies for the divergent development paths affects this relationship. For the entire world sample, we use two sets of proxies. The first set of variables includes a measure of early political institutions, constraint on the executive at independence from Polity IV, and the independence year. Since the date of independence is a possible critical juncture for most countries, a direct measure of institutions immediately after the end of the colonial period (for former colonies) or at the date of national independence (for noncolonies) is a useful proxy for the nature of the development paths that these societies have embarked upon. This variable is constructed as the average score of constraint on the executive from Polity IV during the first ten years after independence. We again normalize this score to a zero-to-one scale like democracy, with one representing the highest constraint on the executive. It is useful to control for date of independence as well, since this is also related to the development paths that societies may have embarked upon (with early independence more indicative of a pro-growth and pro-democracy development path). Moreover, constraint on the executive at the

TABLE 8A—DEMOCRACY IN THE VERY LONG RUN

| | Base sample, 1500–2000 | | | |
|--|------------------------|-------------------|-------------------|-------------------|
| | OLS (1) | OLS (2) | OLS (3) | OLS (4) |
| Dependent variable is change in democracy over sample period | | | | |
| Change in log GDP per capita over sample period | 0.134 (0.021) | 0.061 (0.023) | 0.088 (0.020) | 0.047 (0.023) |
| Constraint on the executive at independence | | 0.260 (0.120) | | 0.164 (0.064) |
| Independence year/100 | | -0.206 (0.063) | | -0.133 (0.036) |
| Fraction Muslim | | | -0.299 (0.097) | -0.233 (0.083) |
| Fraction Protestant | | | 0.191 (0.112) | 0.180 (0.091) |
| Fraction Catholic | | | 0.155 (0.073) | 0.117 (0.069) |
| Historical factors F-test | | [0.02] | [0.00] | [0.01] |
| Observations | 135 | 135 | 131 | 131 |
| R-squared | 0.20 | 0.34 | 0.40 | 0.45 |

Notes: Cross-section OLS regression in all columns, with robust standard errors clustered by level of aggregation for 1500 income data in parentheses. Countries are included if independent prior to 1990, as determined by *CIA World Factbook* (2004). Changes are total differences between 1500 and 2000. GDP per capita is from Maddison (2003), and democracy is calculated using the Polity measure of democracy, which comprises in part constraint on the executive. All columns assume some values of democracy in 1500 in a few European countries, following Acemoglu et al. (2005) and assign the lowest value of democracy for all other countries. The historical factors *F*-test reports the *p*-value for all variables other than change in income. For detailed data definitions and sources see Table 1 and Appendix Table A1.

date of independence would not be comparable across countries if we did not control for date of independence, since the meaning of this constraint likely varies over time.³⁰

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

For this reason, we look for additional potential determinants of development paths. An important candidate suggested in the literature is religion. Citing the example of England as the primary example, Weber (1930) argued that the Protestant ethic was responsible for the development of an institutional structure conducive to the rise of democracy and capitalism. Other arguments pointing to religion as an important determinant of political and economic development

³⁰ 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 begin in 1800 or at the date of independence. Countries independent prior to 1800 are coded as being independent in 1800.

TABLE 8B—DEMOCRACY IN THE VERY LONG RUN (CONTINUED)

| | Former colonies sample, 1500–2000 | | | | | | |
|--|-----------------------------------|------------------|-------------------|------------------|-------------------|-------------------------------------|-------------------------------------|
| | OLS (1) | OLS (2) | OLS (3) | OLS (4) | OLS (5) | OLS (6) | OLS (7) |
| Dependent variable is change in democracy over sample period | | | | | | | |
| Change in log GDP per capita over sample period | 0.136 (0.019) | 0.067 (0.012) | 0.099 (0.012) | 0.057 (0.013) | 0.081 (0.027) | 0.025 (0.024) | 0.029 (0.026) |
| Constraint on the executive at independence | | 0.189 (0.072) | | 0.189 (0.075) | | 0.166 (0.089) | 0.167 (0.087) |
| Independence year/100 | | | -0.190 (0.032) | | -0.105 (0.075) | | -0.179 (0.023) -0.128 (0.074) |
| Fraction Muslim | | | | 0.023 (0.101) | 0.059 (0.105) | | 0.038 (0.088) |
| Fraction Protestant | | | | 0.508 (0.258) | 0.491 (0.154) | | 0.411 (0.196) |
| Fraction Catholic | | | | 0.306 (0.130) | 0.277 (0.229) | | 0.200 (0.221) |
| Log population density, 1500 | | | | | | -0.059 (0.021) -0.049 (0.022) | -0.031 (0.027) |
| Historical factors <i>F</i> -test | | [0.05] | [0.00] | [0.00] | [0.02] | [0.00] | [0.00] |
| Observations | 87 | 87 | 87 | 87 | 83 | 83 | 83 |
| <i>R</i> -squared | 0.20 | 0.30 | 0.33 | 0.37 | 0.25 | 0.34 | 0.38 |

Notes: Cross-section OLS regression in all columns, with robust standard errors clustered by level of aggregation for 1500 income data in parentheses. Countries are included if they are independent prior to 1990, as determined by CIA (2006), and if they are former European colonies. Changes are total differences between 1500 and 2000. GDP per capita is from Maddison (2003), and democracy is calculated using the Polity measure of democracy, which comprises, in part, constraint on the executive. All columns assign the lowest value of democracy for all countries in 1500. The historical factors *F*-test reports the *p*-value for all variables other than change in income. For detailed data definitions and sources, see Table 1 and Appendix Table A1.

have been articulated by Huntington (1991) and Fish (2002), who emphasize the importance of Islam as an institutional barrier to economic and political development.

In column 3 of Table 8A we present estimates that include the fractions of different religions (in particular, fractions of Protestants, Catholics, and Muslims in the population).³¹ The coefficient on income is again reduced, now to 0.088 (standard error = 0.020), and religious fractions are individually significant at the 10 percent level with the fraction Muslim being most significant and negative at -0.299 (standard error = 0.097). Column 4 combines the religion variables with the proxies of early institutions and date of independence. Now there is a more substantial drop in the estimate of the effect of change in income on the change in democracy, to 0.047 (standard error = 0.023), which is just significant at 5 percent. Figure 6 illustrates the significant weakening in the relationship between changes in income and democracy once we control for historical factors affecting divergent development paths. It depicts the residual plot of the regression in Table 8A, column 4. It shows that the inclusion of historical factors significantly reduces the upward sloping relationship apparent in Figure 5. Recall also that this estimate is likely to be an upper bound on the effect of changes in income on changes in democracy over

³¹ Data from Rafael La Porta et al. (1999).

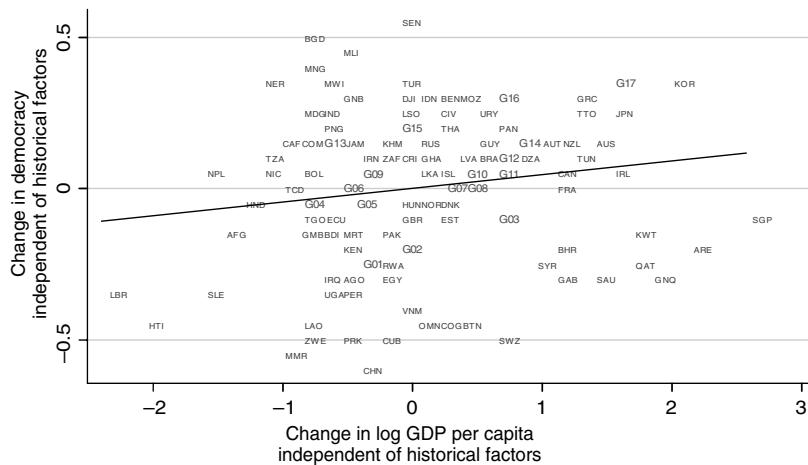


FIGURE 6. CHANGE IN DEMOCRACY AND CHANGE IN INCOME, 1500–2000, CONDITIONAL ON HISTORICAL FACTORS

Notes: See Appendix Table A1 for data definitions and sources. Changes are total differences between 1500 and 2000 (see Figure 5 for the construction of these differences) which are not predicted in a linear regression by historical factors: fraction Muslim, fraction Protestant, fraction Catholic, constraint on the executive at independence, and independence year. This corresponds to the residual plot of the regression in Table 8A, column 4, and it yields a coefficient of 0.047 (standard error = 0.023), $N = 131$, $R^2 = 0.45$. The “G” prefix corresponds to the average for groups of countries. G01 is CMR and SDN; G02 is CHE and LBY; G03 is MAR and USA; G04 is SLV and ZMB; G05 is BFA and COL; G06 is FJI and PRY; G07 is MEX and SWE; G08 is BEL and VEN; G09 is DOM and GTM; G10 is ARG, JOR, and POL; G11 is FIN and NLD; G12 is CHL and MYS; G13 is GIN and NGA; G14 is DEU, ESP, ITA, and PRT; G15 is PHL and ROM; G16 is BGR and BWA; and G17 is ISR and MUS.

the past 500 years, since our historical measures are only crude proxies for the determinants of divergent development paths.

Although the change in income continues to be significant in this regression, the magnitude of the effect is very small. If the coefficient of 0.047 represented the causal effect of income on democracy, it would imply that an “average” dictatorship in 1500 with income per capita of \$566 (average of world income in 1500 in 1990 dollars) would need to reach a per capita income of \$984 billion to become democratic!³²

Table 8B turns to the former colonies sample. The advantage of this sample is that we have a better understanding of the factors that have shaped the divergent development path during critical junctures. In particular, Acemoglu et al. (2002) document that former colonies with high rates of indigenous population density in 1500 have experienced greater extraction of resources and repression by Europeans, and consequently have been more likely to embark on a development path leading to relative stagnation and nondemocracy. They also provide estimates for population density of the indigenous population in 1500.³³ Motivated by this reasoning, we use the estimates of the size of the indigenous population in 1500 (population density

³² This follows since, given the estimates in column 4, a change from a score of democracy of 0 to 1 would require an increase in log GDP per capita of $1/0.047$. This translates into an $\exp(1/0.047)$ -fold (i.e., ≈ 1.7 billion-fold!) increase in GDP per capita starting from \$566, which leads to an income per capita of \$984 billion. In contrast, the coefficient of 0.134 in column 1 implies that a substantially smaller (though still large) 1,742-fold increase in income per capita is necessary for a society to move from a democracy score of 0 to 1.

³³ Population density in 1500 is calculated by dividing the historical measures of population from Colin McEvedy and Richard Jones (1975) by the area of arable land (see Acemoglu, Johnson, and Robinson 2002).

in 1500, for short) as an additional proxy for factors determining the divergent development paths of nations.

Columns 2–4 of Table 8B are similar to columns 2–4 of Table 8A, but refer to the former colonies. They show that the inclusion of constraint on the executive, independence year, and religion variables weakens the 500-year correlation between changes in democracy and income, but a significant relationship still remains (and is in fact stronger than was the case for the entire-world sample). Column 5 turns to the effect of population density in 1500 by including the log of the population density of the indigenous population. This variable is significant and has the expected sign. Moreover, its inclusion reduces the coefficient on the change in income per capita substantially. Column 6 shows that the inclusion of this variable, together with constraint on the executive and date of independence, is sufficient to remove the significant association between changes in income and democracy over the past 500 years entirely. Now the coefficient on the change in income per capita, which was originally equal to 0.136 (standard error = 0.019), is reduced to 0.025 (standard error = 0.024), which is highly insignificant. Moreover, constraint on the executive, independence year, and population density in 1500 are each individually significant. For example, the coefficient on population density in 1500 is -0.049 (0.022). Therefore, in this sample, there is no evidence that changes in income cause changes in democracy once we condition on certain proxies for divergent development paths of former colonies. Finally, column 7 includes the religion variables as well and, again, the coefficient on income is low and insignificant at 0.029 (standard error = 0.026).

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

VII. Conclusion

The conventional wisdom in the political economy literature is that income per capita has a causal effect on democracy. In this paper, we argue that, although income and democracy are positively correlated, there is no evidence of a causal effect. Instead, omitted—most probably historical—factors appear to have shaped the divergent political and economic development paths of various societies, leading to the positive association between democracy and economic performance. Consequently, regressions that include country fixed effects and/or instrumental variable regressions show no evidence of a causal effect of income on democracy over the post-war era or the past 100 years. These results shed considerable doubt on the conventional wisdom both in the academic literature and in the popular press that income per capita is a key determinant of democracy and that a general increase in income per capita will bring improvements in institutions.

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

Nevertheless, some caution is necessary in interpreting our results. First, even though our results do not provide evidence for a causal effect of income on democracy, such an effect might be present but working at much lower frequencies (for example, over horizons of 100 years or longer), or this causal effect might be conditional on some other characteristics

(though we have not found any evidence for this type of interaction effects using the available cross-country data). Second, our results do not imply that democracy has no effect on economic growth (see, e.g., Torsten Persson and Guido Tabellini 2007). Finally, while we have emphasized the importance of historical development paths, we do not want to suggest that there is a historical determinism in political or economic institutions; the fixed effects in the regressions and the presence of divergent development paths create a tendency, but many other factors influence equilibrium political institutions.³⁴ The potential effects of democracy and political institutions on economic growth, the possible conditional relationship between income and democracy, and the impact of various time-varying and human factors on the evolution of equilibrium political institutions appear to be important areas for future theoretical and empirical research.

APPENDIX

This Appendix addresses the construction of trade-weighted instrument used in Section IV. We first measure the matrix $\Omega = [\omega_{ij}]_{i,j}$ using actual trade shares between 1980 and 1989. These dates are chosen to maximize coverage. Bilateral trade data are from the International Monetary Fund Direction of Trade Statistics (DOTS) (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 (free on board) exports from i to j or CIF (cost, insurance, and freight) 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 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

$$\omega_{ij} = \frac{1}{I_{ij}} \sum_{s=1980}^{1989} \left(\frac{X_{ijs}}{Y_{is}^*} \right),$$

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

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

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

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

³⁴ Various current factors could, and in fact do, appear to influence democracy. In the previous versions of our paper, we showed how severe economic crises lead to the collapse of dictatorships, making democracy more likely. Benjamin Jones and Benjamin Olken (2006) show how deaths of autocratic leaders make subsequent democracy more likely.

APPENDIX TABLE A1—DATA AND SOURCES

| Variable | Description | Source |
|---|--|--|
| Freedom House Political Rights Index, also referred to here as Freedom House measure of democracy | Data for 1972–2000 in Freedom House Political Rights Index, original range 1,2,3,..., 7 normalized 0–1. Data for 1972 used for 1970. Data for 1950, 1955, 1960, and 1965, in Bollen, original range 0.00,0.01,...0.99,1. | http://www.freedomhouse.org/ratings/ , and Bollen (2001), “Cross National Indicators of Liberal Democracy 1950–1990” available on ICPSR |
| Polity Composite Democracy Index, also referred to here as the Polity measure of democracy | Data for 1850–2000 in Polity IV. The composite index is the democracy score minus the autocracy score. Original range –10,–9,...10, normalized 0–1. For the purposes of the historical regressions, countries for which data are not available in 2000 are assigned the data for 1995. | http://www.cidcm.umd.edu/inscr/polity/ |
| Polity Composite Democracy Index in 1500 | Constructed using constraint on the executive score from Acemoglu, Johnson, and Robinson (2004b) for the sample of European countries. Components of the index other than constraint on the executive are assigned a value of zero for all countries. | Acemoglu, Johnson, and Robinson (2004) |
| GDP per capita (chain weighted 1996 prices) | Data for 1950–2000 measured as log real GDP per capita (chain method in 1996 prices) from Penn World Tables 6.1. | http://pwt.econ.upenn.edu/ |
| GDP per capita (1990 dollars) | Data for 1500–2000 measured as log real GDP per capita (1990 Geary-Khamis dollars) from Maddison (2003). Countries are assigned values at the lowest possible aggregation. Data in 1820 are used for 1850. Data in 1870 are used for 1875. Data in 1929 are used for 1925. | http://www.eco.rug.nl/~Maddison/ |
| Population | Total population in thousands. | World Bank (2002) |
| Education | Average total years of schooling in the population age 25 and over. Data for 1960, 1965,..., 1995 from Barro and Lee. We include average years of primary schooling in the population age 25 and over in specifications that 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 in five different age ranges: 0 to 15, 15 to 30, 30 to 45, 45 to 60, and 60 and above. | United Nations Population Division (2003) |
| 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) |
| Trade-weighted log GDP | Constructed using GDP per capita from Penn World Tables 6.1 and average trade shares between 1980 and 1989 from International Monetary Fund Direction of Trade Statistics (2005), according to procedures described in Appendix. | http://pwt.econ.upenn.edu/ and IMF DoTS CD-ROM (2005) |
| Trade-weighted democracy | Constructed using Freedom House Political Rights Index, GDP per capita from Penn World Tables 6.1, and average trade shares between 1980 and 1989 from International Monetary Fund Direction of Trade Statistics (2005), according to procedures described in Appendix. | http://pwt.econ.upenn.edu/ , IMF DoTS CD-ROM (2005), and http://www.freedomhouse.org/ratings/ , and Bollen (2001), “Cross National Indicators of Liberal Democracy 1950–1990” available on ICPSR |

APPENDIX TABLE A1 (CONTINUED)

| Variable | Description | Source |
|---|---|---|
| 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 ten years after its independence (ignoring missing data). If data for the first ten years after independence are missing, we find the first year these data are available in Polity, then average over the following ten years (ignoring missing data). | http://www.cidem.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 as 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, Johnson, and Robison (2002) |
| Religion | Percent of population in 1980 which is (1) Catholic, (2) Protestant, or (3) Muslim. | La Porta et al. (1999) |

APPENDIX TABLE A2—CODES USED TO REPRESENT COUNTRIES

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

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