



Should the Modernization Hypothesis Survive Acemoglu, Johnson, Robinson, and Yared? Some More Evidence

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[LINK TO ABSTRACT](#)

Seymour Martin Lipset (1959, 75) wrote, “The more well-to-do a nation, the greater the chances that it will sustain democracy.” Lipset (1959; 1960) is frequently interpreted as having advanced “the modernization hypothesis,” a claim that income and education are, in a statistical sense, predictors of democracy.⁴ This claim is supported by a vast empirical literature spanning several decades in the areas of comparative politics and political economy,⁵ although some literature has

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4. The modernization hypothesis refers to changes in social and political institutions induced by development. These changes include the demographic transition, political development, secularization, increased urbanization, diminished share of agriculture in total GDP, and greater levels of education and life expectancy, among others (see McCleary and Barro 2006a; 2006b). Paldam and Gundlach (2012; 2013; also Gundlach and Paldam 2009) encompass these cultural, social, political, economic, and demographic transformations under the rubric of the Grand Transition. Accordingly, we are studying a subset of the several transitions associated with development, namely the transition from autocracy to democracy. Precursors of the modernization literature include Aristotle (in his *Politics*) on political development or democratization, and Hume (1757) and Marx (1859) on secularization.

5. See Barro (1999); Boix and Stokes (2003); Bollen (1980); Burkhart and Lewis-Beck (1994); Glaeser, Ponzetto, and Shleifer (2007); Inglehart (1997); Jackman (1973); Londregan and Poole (1996); McCrea and Cnudde (1967); and Muller (1988), among others.

challenged the claim that higher levels of income induce democratic transitions.⁶

The modernization hypothesis, understood as political development, is investigated in several papers by Daron Acemoglu, Simon Johnson, James Robinson, and Pierre Yared (2005; 2008; 2009), a team of authors hereafter referred to as AJRY. Using mainly panels of countries spanning the period 1960–2000, they find no correlation between income and democracy after controlling for country specific factors and world trends, that is, after allowing for country and time effects, and likewise for education and democracy. AJRY (2009) interpret their country fixed effects results as being consistent with the critical junctures hypothesis.⁷ The fixed effects, AJRY say, are “capturing the impact of time-invariant, historical variables simultaneously affecting the evolution of income and democracy” (AJRY 2009, 1057). Put differently, fixed effects proxy for country-specific differences in institutional quality that ultimately account for the observed correlation between income and democracy.

Econometric specifications used in the 2008 and 2009 papers by AJRY always include among the independent variables income and a proxy for democracy, both lagged, and in the 2005 paper they always include education and democracy, also both lagged. The democracy variable measures quality of *political* institutions, but AJRY do not control for *economic* institutions. More specifically, AJRY do not include a variable to control for the level of inclusiveness of economic institutions.

Income is at least to some extent a result of the interplay between economic and political institutions. In our view, economies tend to grow if political and economic institutions induce a stable environment where private property of the vast majority of the population is protected, creating incentives to work, innovate, invest, and allocate resources efficiently.⁸ We strive to incorporate such mechanisms in comparative political development research by including, in addition to democracy and income, an index of economic freedom as a proxy for

6. Przeworski and Limongi (1997) and Przeworski, Alvarez, Cheibub, and Limongi (2000) contend that their evidence is not favorable to what they call the “endogenous” version of the modernization hypothesis. However, criticisms by Boix and Stokes (2003), Inglehart and Welzel (2005), and Epstein et al. (2006) contend that by using correct standard errors to estimate significance levels, incorporating new evidence whereby the ratio of regime switches to democracy against regime switches to autocracy increases, and expanding the sample back to 1850, there is clear support for the modernization hypothesis. See also Voigt (2011) for a brief discussion on the democracy causality issue.

7. Exponents of the critical junctures hypothesis are Moore (1966) and O’Donnell (1973).

8. Jong-A-Pin and De Haan (2008; 2011) report episodes of growth acceleration which are preceded by economic liberalizations. Additional evidence supportive of a beneficial effect of democratic institutions on growth is uncovered by Mobarak (2005), who finds that democracies enhance growth through the channel of reduced volatility given the inverse relation between political development and volatility. See also Persson and Tabellini (2009) for the role of democratic capital in stimulating growth by enhancing democracies’ stability.

capitalist institutions which are crucial for development along with human capital.⁹ A central indicator of economic freedom is quality of the legal infrastructure, in particular extent of the rule of law and independence of the judiciary, both of which can in the spirit of Lipset be interpreted as proxies for some social requisites for political development.

AJRY's basic results hinge on specifications which control for lagged democracy and country and time fixed effects, and in this context of limited residual variability they attempt to assess if income exerts an independent effect on democracy, and likewise for education.¹⁰ Needless to say this problem of little variance of democracy left to be explained by income or education is not mitigated and can be aggravated by the inclusion of economic institutions to the extent that economic freedom impacts democracy.¹¹

Moreover, economic freedom and income are highly correlated, as are economic freedom and education. The collinearities between income and economic freedom and between education and economic freedom, like the inclusion of economic freedom, reduce the likelihood of uncovering a statistically significant impact of income and/or education on democracy. Thus we are stacking the cards against the modernization hypothesis, and in this sense our tests are more demanding than those performed by AJRY.

A final reason for including economic freedom is that a research strand in the economics literature argues that economic freedom is a necessary condition for political freedom.¹² Economic freedom may be an important channel in explaining democracy that has gone missing in the modernization literature.

Another trait that distinguishes this paper from those of AJRY is methodological. Part of our empirical strategy is the application to our sample of the System Generalized Method of Moments estimator developed by Richard Blundell and Stephen Bond (1998). The System GMM estimator is particularly suited for identification tasks where the variables are highly persistent, which is the case with

9. For recent supportive evidence see Ashraf and Galor (2013), and for a fresh summary of the literature on deep determinants of economic development and the role of institutions see Spolaore and Wacziarg (2013).

10. To better appreciate the issue of reduced variability left to be explained by income, see Benhabib, Corvalan, and Spiegel (2011), who document that in democracy regressions using country five-year panels, the inclusion of lagged democracy along with country and time fixed effects accounts for 81% of total variation of the democracy variable, leaving little variability to be explained by income. A similar point is made by Paldam and Gundlach (2012, 164 n. 21): "This empirical model [referring to the specification used by AJRY (2008)] leaves virtually nothing to be explained by income, and consequently the effect of income becomes insignificant, and is declared spurious."

11. Indeed, components of economic freedom such as rule of law can promote democratization.

12. Among early proponents of this research strand are Friedman (1962) and Hayek (1944). The view has found recent empirical support in Lawson and Clark (2010).

income, education, and democracy.

We apply System GMM techniques to an unbalanced panel of countries spanning the 1970–2010 sample period,¹³ using quinquennial data and after controlling for economic freedom and democracy. We find that education and income predict democracy. Also, applying OLS to our data set and using a specification that captures long-run changes in democracy, we obtain results that support the modernization hypothesis.

The research in this paper is related to a number of recent studies, some of them motivated by the papers by AJRY (2005; 2008; 2009). First and perhaps the closest to ours, is a paper by Benedikt Heid, Julian Langer, and Mario Larch (2011), which finds support for the modernization hypothesis using the System GMM technique. But unlike Heid, Langer, and March (2011), we control for economic institutions and address the role of education as a driver of modernization.

Jess Benhabib, Alejandro Corvalan, and Mark Spiegel (2011) report evidence favorable to the modernization hypothesis after employing panel nonlinear estimation methods that account for censored democracy data. As previously mentioned we find support for the modernization hypothesis using linear estimation methods, also used by AJRY (2005; 2008; 2009). However, our results rely on the Blundell-Bond System GMM estimator which is employed by neither AJRY (2005; 2008; 2009) nor Benhabib, Corvalan, and Spiegel (2011). Further, education is treated by Benhabib et al. (2011) as another covariate in addition to income in their main specification. We also perform regressions displaying horse races between income and education. In line with Lipset (1959; 1960) we attempt to evaluate education's predictive power of democracy independently of income.¹⁴ Consequently, we present regression specifications containing income but excluding education and, symmetrically, specifications that include education and exclude income.¹⁵

Carles Boix (2011) argues that AJRY's results are partly driven by the post-WWII sample period in which the effect of income on democracy is particularly weak. He finds support for modernization in long-run panels that use fixed effects spanning eighty or more years. By contrast, we find support for the Lipset hypothesis using a sample that focuses on recent decades commencing in 1970 and ending in 2010.

Eric Gundlach and Martin Paldam (2009) employ the Polity index as a proxy for democracy and use a sample that spans the period from 1820 to 2003.

13. Thus, similar to AJRY, we focus on a recent sample.

14. Lipset (1959) viewed education as a necessary condition for democracy.

15. *See* Glaeser, Ponzetto, and Shleifer (2007) for a theoretical development in which education is modeled as having a causal impact on democracy.

Estimating OLS and Two-Stage Least Squares cross-country regressions for each of the 184 years intervening between 1820 and 2003, Gundlach and Paldam find evidence that buttresses the democratic transition view. These scholars use this long-run procedure because in their view five-year panels offer a horizon too short to test the democratic transition hypothesis. Gundlach and Paldam write: “The Grand Transition view and the Democratic Transition hypothesis are about long-run trends that can be best handled by pure cross-section estimates, not by a combination of fixed effects and lagged adjustment over a short time horizon” (2009, 349-350).¹⁶ Nonetheless and as previously indicated, we find support for the modernization thesis using panels with a five-year frequency.

Paldam and Gundlach (2012) use the Gastil index as a proxy for democracy. They apply country and time fixed effects in a balanced panel of countries spanning the 1972–2008 period with frequencies of 18, 12, and five years. They find support for the modernization hypothesis using five-year panels and restricting the sample to the pre-1989 period.¹⁷ Like Gundlach and Paldam (2009), Paldam and Gundlach (2012) do not control for economic institutions,¹⁸ and they do not use dynamic specifications such as were employed by AJRY. Nonetheless, after applying OLS and IV methods to long-run cross-country specifications, both papers report strong support for the modernization hypothesis.

Daniel Treisman (2012) provides evidence which suggests that the impact of development on democracy takes place over a 10- to 20-year time span. The finding is particularly strong after 19th-century data is included. Treisman writes: “The new point I emphasize here is that the link between income and democracy is clearest and strongest *in the medium to long run*—i.e. panels of 10 to 20 years” (2012, 7, emphasis in original). Moreover, similar to Boix (2011) and to AJRY, Treisman (2012) reports that over the 1960–2000 period income does not predict democracy in panels of one-, five-, 10-, 15- and 20-year frequencies. However, Treisman does not apply Blundell-Bond methods to any sample period.

Ghada Fayad, Robert Bates, and Anke Hoeffler (2012) applied a Pooled Mean Group estimator (PMG), augmented with averages of all variables in the model to proxy for time-common factors, to a sample of countries with obser-

16. A similar argument is articulated by Paldam and Gundlach (2012, 152), who interpret their Granger causality test results as revealing “that the short to medium run is probably not well suited to identifying the main direction of causality between income and democracy.”

17. Unfortunately, Paldam and Gundlach (2012) do not indicate if the estimated standard errors used to assess the statistical significance of regression coefficients are robust to the presence of arbitrary heteroskedasticity and/or autocorrelation, or are clustered by countries. Reporting the type of standard error is relevant because the significance test may be invalid and the estimated p-value may change depending on the type of standard error used.

18. See Gundlach and Paldam (2009) for a justification of institutions-free analyses of democratic transition.

variations that commence in 1955 and end in 2007.¹⁹ They find that income is negatively and significantly related to democracy. Parameter estimates associated with world income and world democracy enter positively and significantly, predicting greater democratization at the country level. Figure 1 in Fayad, Bates, and Hoeffler (2012, 5) graphs world democracy and world per capita income starting in 1960 and ending in 2008 for a sample of 105 countries. World income mostly rises over the sample period whereas the democracy index falls during 15 consecutive years from 1960 through 1975. Yet, over the following 33 years the world democracy index rises along with income. Thus the Fayad, Bates, and Hoeffler (2012) Figure 1 is generally consistent with the modernization hypothesis.

Furthermore, the heterogeneous PMG estimator used by Fayad, Bates, and Hoeffler (2012) estimates individual country coefficients thus requiring long time series for each country included and excluding countries with a time-invariant dependent variable.²⁰ Due to this long time series requirement, countries that transition to democracy such as the Czech Republic, Estonia, Latvia, Lithuania, and Slovak Republic are not included in the sample. Among the time-invariant consistent democracies excluded are Australia, Austria, Belgium, Canada, Denmark, Finland, Ireland, Italy, Japan, Netherlands, New Zealand, Norway, Sweden, Switzerland, United Kingdom, and United States. Among the consistent autocracies excluded are Cuba, Libya, and Vietnam. Fayad, Bates, and Hoeffler (2012, 14) write: “Both the sample choice and the methodology thus led us to our results.” In other words, the methodology constrains the sample, leaving out potential important sources of information.

Additionally, Fayad, Bates, and Hoeffler show in Table 2 (2012, 11) that, both for their sample of 105 countries and for AJRY’s sample, OLS fixed effects estimates of income per capita are insignificant only when conditioning on year fixed effects. As previously mentioned, the PMG estimator does not allow for year effects.

The OLS Pooled Error Correction Model (PECM) admits controls for country and time fixed effects, however. The results of Fayad, Bates, and Hoeffler’s main sample using OLS (PECM) are shown in their Table 5 (2012, 14). In three out of four different lag structure models, income per capita at the country level enters

19. The PMG estimator does not allow for year fixed effects because parameters are estimated separately for each country. To correct for this shortcoming of the PMG methodology, the Fayad et al. (2012) model is augmented with world income and democracy.

20. In other words, regression coefficients are calculated for every country in the sample, as opposed to, say, OLS, which estimates one slope coefficient for all the countries. The PMG estimator by design eliminates time-invariant dependent variables. This is not so extraordinary (e.g., the fixed-effect methods employed by AJRY also by design discard time-invariant variables, which appear frequently among explanatory variables).

significantly negative, albeit only at a 10% significance level. They write: “However, estimating the pooled error correction model while using the AJRY (bigger) sample yields long-run coefficients on income per capita that are insignificant, regardless of the number of lags” (ibid., 13).

Interestingly, when Fayad, Bates, and Hoeffler apply the PMG estimator without accounting for their proxies for time effects, “the coefficient on income per capita is instead positive and significant” (2012, 12 n. 11). Overall, this evidence they offer may lead one to suspect that their results are also sensitive to the methodological procedure due to the technical impossibility of controlling for year effects using annual dummies when the PMG method is employed.

In light of the findings of Treisman (2012) and particularly Boix (2011), a potentially interesting robustness check of the Fayad, Bates, and Hoeffler (2012) findings may be to expand the sample coverage to earlier periods. This extension may alleviate sample attrition containing relevant information.²¹

Data attributes and sources

Our sample period comprises the years 1970 through 2010. In this regard our paper follows a common practice in the democratization literature of using data less afflicted by measurement problems than data prior to the Second World War.

We pool cross-section data with time series data in order to exploit the time dimension of the data, allowing us to investigate the impact over time of variables which proxy for socio-economic development, such as real income and human capital, on democracy. Specifically, exploiting the within-country variation in the data permits us to evaluate whether, as a country becomes more socio-economically developed, relative to its mean, it also turns out to be relatively more democratic.

Our dependent variable and proxy for democracy measures is the Index of Political Rights from Freedom House published in 2010. In the Index, “political rights” include the existence of free and fair elections, competitive parties, an opposition that plays an important role in the political process, and whether those who are elected rule, among others. “Political rights are rights to participate meaningfully in the political process. In a democracy this means the right of all adults to vote

21. A recent paper that addresses the relation between education and governmental quality and, indirectly, the relation between education and democracy is Botero, Ponce, and Shleifer (2013). As they write: “Most studies find that education and development lead to improved government (e.g., Barro 1999, Glaeser et al. 2004, Bobba and Coviello 2007, Castello-Climent 2008, Murin and Wacziarg 2011), although some disagree (Acemoglu et al. 2005). In this paper, we ask *why* the quality of government improves with education and development, assuming that it does” (Botero et al. 2013, 2).

and compete for public office, and for elected representatives to have a decisive vote on public policies” (Gastil 1991, 7).²² The Index of Political Rights goes from one to seven, where one indicates most politically free and seven least free.

Our independent variables are lagged democracy, log of real income per capita, human capital, and the Economic Freedom of the World (EFW) index. Real income per capita is provided by the World Development Indicators published in 2010 by World Bank. Human capital is provided by Robert Barro and Jong-Wha Lee (2010)²³ and measures average years of education of the population 25 years and older. The EFW index—inspired by Milton Friedman, built over the years since 1997 by James D. Gwartney and Robert Lawson, and published by the Fraser Institute—is our proxy for capitalism. The EFW index contains the following areas: (1) “Size of Government: Expenditures, Taxes, and Enterprises”; (2) “Legal Structure and Security of Property Rights”; (3) “Access to Sound Money”; (4) “Freedom to Trade Internationally”; and (5) “Regulation of Credit, Labor, and Business” (Gwartney, Lawson, and Hall 2011). Thus the index controls for trade, inflation, regulation, government spending, taxes, rule of law, and quality of the judiciary. The ratings for the components of the EFW index range from zero to ten with higher ratings indicating more economic freedom. The summary ratings are an aggregation of the five area ratings, and they almost always fall within a range between three and nine.

Empirical strategy and results

Using quinquennial panels and an unbalanced panel of countries from 1975 to 2010 we estimate the following regression model:²⁴

$$PF_{i,t} - PF_{i,t-1} = \alpha \cdot PF_{i,t-1} + \beta \cdot EFW_{i,t-1} + \theta \cdot Y_{i,t-1} + \tau \cdot HK_{i,t-1} + \delta_i + \mu_t + \varepsilon_{it} \quad (1)$$

where change in political rights²⁵ is regressed against lagged political rights to capture persistence in democracy and also potentially mean-reverting dynamics. The main parameters of interest are θ associated with initial-period income, and τ associated with initial-period human capital. Specification (1) allows for country fixed effect dummies, with δ_i to control for country idiosyncratic time-constant factors, and for time period dummies, with μ_t to control for world trends in

22. The late Raymond Gastil directed Freedom House from 1977 to 1988 and made a decisive contribution to its indexes on political rights and civil rights, which are now published yearly.

23. This metric of human capital updates the Barro and Lee (2000) data set and corrects for measurement errors in educational attainment and takes account of criticisms made by Cohen and Soto (2007).

24. This functional form is used by Glaeser, La Porta, Lopez-de-Silanes, and Shleifer (2004).

25. $PF_{i,t}$ stands for level of political rights in country i at year t .

SHOULD THE MODERNIZATION HYPOTHESIS SURVIVE?

democracy. Finally, ε_{it} is a zero-mean error term, which captures the variation in democracy not explained by model (1).

Results shown in Table A indicate that, applying fixed-effects OLS over non-overlapping five-year periods comprising thirty-five years, initial-period (lagged) political rights (in columns 1 and 2) enter negative and statistically significant at a 1% level, suggesting the presence of mean reversion. Controlling for income (column 1) and human capital (column 2), the regression coefficient associated with economic freedom is negative and significant at a 5% level, consistent with the view that higher levels of economic freedom induce more democratic change. Income (column 1) enters significantly though with a positive sign, inconsistent with the modernization hypothesis implying that development leads to less democracy, whereas human capital (in column 2) does not predict democracy at a 5% significance level.

Column (3) presents our first horse race results between income and education. Human capital does not predict democracy. Income, however, enters significantly predicting *less* democracy.

According to AJRY, conditioning on fixed effects captures the spirit of the critical junctures hypothesis to the extent that it accounts for the effect of unobserved heterogeneity associated with time-invariant historical factors impacting both political and economic development. In their sample, income loses significance controlling for fixed country effects and time dummies, which is consistent with the critical juncture hypothesis. However, our results indicate that economic freedom predicts more democracy and income predicts less democracy. This evidence suggests that their democracy-income effects are sensitive to the presence of economic freedom in the model.²⁶

While fixed-effects estimation methods correct for biases induced by the omission of a complete list of country-specific unobserved heterogeneity variables correlated with the independent variables, parameter estimates are inconsistent if time-varying independent variables correlated with explanatory variables are omitted, violating consequently the strict exogeneity assumption. Moreover, fixed-effects estimates in dynamic specifications are biased, and in short time-period panels inconsistent,²⁷ due to the correlation between the transformed lagged

26. In our sample and using our functional form that regresses changes in democracy over five-year periods against initial-period democracy and income, and also allowing for time and country fixed effects but not controlling for economic freedom, income enters significantly at a 5% level predicting *less* democracy. These results, which are available upon request, are also at odds with the tenets of the modernization hypothesis.

27. However, these estimates become consistent as country time observations increase. More precisely, the fixed-effect estimator is consistent as T increases assuming both that there is no other source of correlation between lagged democracy and the error term and that remaining regressors are strictly exogenous (see Wooldridge 2002).

dependent variable and the transformed unsystematic error term ε_{it} inherent to the time-demeaned transformation. To overcome inconsistency of the fixed-effect estimator, and following AJRY, we apply the Difference Generalized Method of Moments (GMM) estimator proposed by Manuel Arellano and Stephen Bond (1991). In addition, we also apply the System GMM estimator introduced by Blundell and Bond (1998) to the following dynamic specification:²⁸

$$PF_{i,t} = \alpha' \cdot PF_{i,t-1} + \beta \cdot EFW_{i,t-1} + \theta \cdot Y_{i,t-1} + \tau \cdot HK_{i,t-1} + \delta_i + \mu_i + \varepsilon_{it} \quad (2)$$

where the dependent variable is the level of political rights for country i in period t .

Columns (4) through (9) of Table A present Arellano-Bond estimates. Economic freedom enters significantly and negative in column (4), whereas income enters significant at a 10% level and positive, thus with a sign at odds with Lipset's hypothesis. In column (5) controlling for time effects, economic freedom loses significance, the p-value being 0.103, and income again enters significantly at a 5% level but with the 'wrong' sign. In columns (6) and (7) we substitute human capital for income, and only lagged political freedom enters significantly. Horse race results between human capital and income using Arellano-Bond are presented in column (8) not controlling for time effects and in column (9) controlling for time effects. Lagged income enters significantly at a 10% level in column (8) and at a 5% level in column (9). However, in both cases income predicts less democracy. Human capital again does not predict democracy at conventional levels of significance.

Moreover, Sargan tests suggest that none of the models that apply Arellano-Bond methods are correctly specified. These results are qualitatively similar to those obtained by AJRY (2005; 2008; 2009) in that predictors of the modernization hypothesis, education and income, either enter not significantly or, if significantly, show up with associated regression coefficients bearing the wrong sign.

The Arellano-Bond estimator is based on the following moment conditions: $E(PF_{i,t-s} \Delta \varepsilon_{it}) = 0$ for $t \geq 3$ and $s \geq 2$. It is well known, however, that democracy, education and income are highly persistent variables,²⁹ and therefore instruments in levels are poorly correlated with first differences.³⁰ This low correlation originates a weak-instrument problem aggravating finite sample biases. To enhance

28. The estimating equations (1) and (2) are equivalent. Specification (1) is obtained subtracting lagged democracy on both sides of (2).

29. See, for example, Glaeser, Ponzetto, and Shleifer (2007) and Bobba and Coviello (2007).

30. To understand the poor correlation between the instrument in levels and subsequent differences when the series is highly persistent, consider a simple autoregressive process of order one (AR(1)), e.g., $PF_{it} = \alpha PF_{i,t-1} + \varepsilon_{it}$. Subtracting $PF_{i,t-1}$ from both sides, to transform this process in differences, yields $\Delta PF_{it} = (\alpha - 1)PF_{i,t-1} + \varepsilon_{it}$. The closer the value of α to 1 (the higher the persistence), the lower the correlation between ΔPF_{it} and $PF_{i,t-1}$, that is, between the difference and the level.

precision of the point estimates, the Blundell-Bond System GMM estimator employs simultaneously the equation in levels and the equation in first differences, conforming to a system of equations which uses lagged differences as internal instruments for the equations in levels and lagged levels as instruments for the equation in differences. Thus, the procedure allows us to exploit additional overidentifying moment restrictions that may contribute to overcome the weak-instrument problem. These additional moment restrictions use internal instruments in differences which are assumed to be orthogonal to the country fixed effect plus the zero mean error term.³¹

In Table A, the Columns (10), (11), and (12) report Blundell-Bond estimates. The parameter estimate associated with income in column (10) is significant at a 5% level, with a p-value of 0.018, and the estimate is negative, suggesting that development predicts democracy. According to column (11) the point estimate of education is also negative and significant at a 5% level with a p-value of 0.018, indicating that education predicts democracy. Both specifications, used in columns (10) and (11), condition on time effects and in neither do the regression coefficients associated with economic freedom enter significantly. Further, according to the Hansen over identification test and second order autocorrelated disturbances in the first differences equations, AR (2), we fail to reject the Hansen test's null hypothesis that the instruments are valid and also the null hypothesis of no second-order autocorrelation.³²

Column (12) contains results of the horse race between income and education. Neither income nor education enters significantly. In fact, only the coefficient estimate of lagged democracy, the autoregressive variable, enters significantly.

Summing up the results of our horse races using high-frequency panels: parameter estimates associated with education do not enter significantly, and in the cases where income parameter estimates entered significantly they show up with the 'wrong' sign. Similarly non-instructive results are reported by AJRY (2005, 46) in their Table 1. This lack of meaningful results should not be surprising given the aforementioned low residual variability to be explained by income or education. The lowness of the residual variability stems from the inclusion of lagged democracy, economic freedom, time and fixed effects (see footnote 10). This

31. This is not to say that the Blundell-Bond estimation technique cannot be afflicted by weak instruments; see Bazzi and Clemens (2013) for cases in the economic growth literature. Alas, extant econometric methods do not provide standard tests to detect weak instruments in dynamic panel GMM settings.

32. Interestingly, when applying Blundell-Bond methods to specifications that do not control for economic freedom, income does not predict democracy, which underscores the appropriateness of our basic specification that includes economic freedom. These results are available from the authors upon request.

problem is exacerbated by the simultaneous inclusion of both of the democracy predictors, education and income, which are highly positively correlated.

Finally, columns (13), (14), and (15) of Table A evaluate the predictive power of income, human capital, and economic freedom on political rights over a longer time span and using traditional OLS methods. We use a specification where the dependent variable is the change in political freedom over 35 years from 1975 to 2010 and the independent variables are the initial values of political rights, economic freedom, income, and/or human capital.

Economic freedom in 1975 does not predict a change in political rights over the long run controlling for income (column 9) and human capital (column 10) in the year of 1975. By contrast, parameter estimates associated with income and human capital show up as highly significant in columns (9) and (10) respectively. Regression coefficient estimates are precisely estimated and appear with the hypothesized sign. The negative sign of 1975 political rights suggests the presence of mean reversion.

These findings are reassuring because they suggest that our results based on shorter time spans are not driven by sample characteristics. Indeed, the evidence is consistent with prior findings on the long-run effects of income and education on democracy.

Conclusions

Lipset's renowned quotation (1959, 75) suggests a gradual democratization process associated with greater socio-economic development. Thus, the empirical strategy of panel data with fixed effects, which assesses the within-country variation between relative wealth or education and democracy, captures econometrically the spirit of Lipset's hypothesis.

Applying fixed-effects OLS and Arellano-Bond methods to our post-World War II data set, using five-year high-frequency panels and conditioning on a proxy for capitalism, we obtain results qualitatively similar to those of AJRY to the extent that increasing both income and education do not induce greater political development. However, to account for weak instruments and endogeneity bias, we use a System GMM estimator advanced by Blundell and Bond, and we find that income and education predict democracy in five-year panels conditioning on economic freedom. Intuitively, as countries become relatively wealthier and their populations relatively more educated, the likelihood of these countries becoming relatively more democratic increases. We also find using OLS that democracy changes over a 35 year period are predicted by income and human capital in 1975. Thus, using our sample and the frequently used method of OLS, we corroborate

SHOULD THE MODERNIZATION HYPOTHESIS SURVIVE?

prior evidence that was consistent with the modernization theory.

Finally, given the complex interplay between development, capitalism and democracy, we conjecture that one causation channel goes from institutions, such as economic freedom and human capital, to development, and that another channel leads from development to democracy. Thus economic freedom becomes an indirect catalyst of democracy through its impact on development.³³ This of course is not a fully resolved issue and is part of an ongoing research agenda.³⁴

33. James Gwartney (2013) in a personal communication states a similar view: “Finally, there is strong evidence that increases in economic freedom promote subsequent increases in income levels. With time, these higher income levels will also promote democracy. Thus, acting through income, increases in economic freedom will also tend to promote democracy. But, the lags between both (a) increases in economic freedom and higher income levels and (b) increases in income and moves toward democracy will be long and variable. Thus, when analyzed across time periods of even a decade or two, the economic freedom–democracy linkage will be quite weak.”

34. For some of the evidence on the link between economic freedom and development see Dawson (1998); Faria and Montesinos-Yufa (2009); Gwartney, Holcombe, and Lawson (2006); Hall, Sobel, and Crowley (2010); Rode and Coll (2012); and Bennett, Faria, Gwartney, and Morales (2013). On the relation between capitalism and democracy see Aixala and Fabro (2009); De Haan and Sturm (2003); Lundstrom (2005); Rode and Gwartney (2012); and Giuliano, Mishra, and Spilimbergo (2013).

TABLE A. Impact of income and human capital on democracy

Independent variables	Short-run fixed effects			Arellano-Bond					Blundell-Bond			Long-run OLS			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Initial political rights	-0.692 (0.000)	-0.665 (0.000)	-0.673 (0.000)	0.349 (0.000)	0.327 (0.000)	0.447 (0.000)	0.438 (0.000)	0.413 (0.000)	0.391 (0.000)	0.817 (0.000)	0.765 (0.000)	0.788 (0.000)	-0.735 (0.000)	-0.764 (0.000)	-0.821 (0.000)
Initial economic freedom	-0.199 (0.064)	-0.164 (0.106)	-0.204 (0.060)	-0.203 (0.006)	-0.141 (0.103)	-0.090 (0.258)	-0.082 (0.328)	-0.135 (0.111)	-0.124 (0.152)	0.078 (0.504)	0.006 (0.947)	0.017 (0.867)	-0.058 (0.770)	0.081 (0.621)	0.029 (0.875)
Initial income	0.407 (0.045)		0.418 (0.043)	0.514 (0.055)	0.638 (0.028)			0.570 (0.052)	0.744 (0.012)	-0.176 (0.018)		-0.071 (0.411)	-0.536 (0.001)		-0.110 (0.677)
Initial human capital		0.123 (0.138)	0.115 (0.167)			-0.013 (0.831)	0.058 (0.614)	-0.049 (0.438)	0.118 (0.301)		-0.081 (0.018)	-0.055 (0.159)		-0.374 (0.000)	-0.352 (0.008)
Time effects	yes (0.008)	yes (0.047)	yes (0.028)	no	yes (0.674)	no	yes (0.798)	no	yes (0.677)	yes (0.465)	yes (0.006)	yes (0.176)			
Number of observations	785	758	741	527	527	521	521	504	504	785	758	741	85	94	83
Residual AR(2) test				(0.174)	(0.170)	(0.601)	(0.643)	(0.536)	(0.589)	(0.302)	(0.740)	(0.731)			
Hansen OIR test										(0.117)	(0.235)	(0.979)			
Sargan OIR test				(0.005)	(0.004)	(0.003)	(0.001)	(0.011)	(0.005)						

Dependent variables are: for columns (1) to (3), change in political rights between $t-1$ and t ; for columns (4) to (12), level of political rights at t ; for columns (13) to (15), change in political rights between 1975 and 2010. Sample periods are: for columns (1) to (12), the eight five-year periods 1970–2010; for columns (13) to (15), the one 35-year period 1975–2010. An “initial” variable is the value of the variable at time $t-1$. A “change” in a variable is its value at time t minus its value at time $t-1$. P-values are in parentheses. Estimated coefficients are above p-values. Fixed effects p-values are calculated using clustered standard error by country. Arellano-Bond and Blundell-Bond p-values are calculated using robust standard errors.

Appendix

Data and code files used in this research can be downloaded from the *Econ Journal Watch* website ([link](#)).

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