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# From Education to Democracy?

By Daron Acemoglu, Simon Johnson, James A. Robinson, and Pierre Yared\*

The conventional wisdom, since at least the writings of John Dewey (1916), views high levels of educational attainment as a prerequisite for democracy. Education is argued to promote democracy both because it enables a “culture of democracy” to develop and because it leads to greater prosperity, which is also thought to cause political development. The most celebrated version of this argument is the modernization theory, popularized by Seymour Martin Lipset (1959), which emphasizes the role of education as well as economic growth in promoting political development in general and democracy in particular. For example, Lipset (1959 p. 79) argues that

Education presumably broadens men’s outlooks, enables them to understand the need for norms of tolerance, restrains them from adhering to extremist and monistic doctrines, and increases their capacity to make rational electoral choices

and he concludes (p. 80) that

If we cannot say that a “high” level of education is a sufficient condition for democracy, the available evidence does suggest that it comes close to being a necessary condition.

Recent empirical work, for example, by Robert Barro (1999) and Adam Przeworski et al. (2000), provides evidence consistent with this view. Edward Glaeser et al. (2004) go further

and argue that differences in schooling are a major causal factor explaining not only differences in democracy, but more generally in political institutions, and they provide evidence consistent with this view.

Existing literature looks at the cross-sectional correlation between education and democracy rather than at the *within* variation. Hence existing inferences may be potentially driven by omitted factors influencing both education and democracy in the long run. A causal link between education and democracy suggests that we should also see a relationship between changes in education and changes in democracy. In other words, we should ask whether a given country (with its other characteristics held constant) is more likely to become more democratic as its population becomes more educated. We show that the answer to this question is no. Figure 1 illustrates this by plotting the change in the Freedom House democracy score between 1970 and 1995 versus the change in average years of schooling during the same time period (see below for data details). Countries that become more educated show no greater tendency to become more democratic.

We further investigate these issues econometrically. We show that the cross-sectional relationship between schooling and democracy disappears when country fixed effects are included in the regression. Although fixed-effects regressions are not a panacea against all biases arising in pooled ordinary least-squares (OLS) regressions, they are very useful in removing the potential long-run determinants of both education and democracy. We also document that the lack of a relationship between education and democracy is highly robust to different econometric techniques, to estimation in various different samples, and to the inclusion of different sets of covariates.

The recent paper by Glaeser et al. (2004) also exploits the time-series variation in democracy and education and presents evidence that changes in schooling predict changes in democracy and other political institutions. However,

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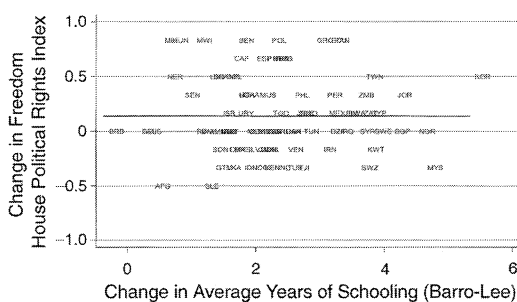


FIGURE 1. DEMOCRACY GROWTH AND EDUCATION GROWTH, 1970-1995

we document below that this result stems from their omission of time effects in the regressions, so it reflects the over-time increase in education and democracy at the world level over the past 35 years. Once we include year dummies in their regressions, the impact of education on democracy disappears entirely. Motivated by the Glaeser et al. (2004) paper, we also show that there is no effect of education on other measures of political institutions.

This paper is part of our broader research. The companion paper (Acemoglu et al., 2004) investigates the other basic tenet of the modernization hypothesis, that income (and economic growth) causes democracy. In that paper, using both fixed-effects OLS and instrumental-variable regressions, we show that there is little evidence in favor of a causal effect from income to democracy either. We also offer a theory for the differences in long-run factors causing the joint evolution of education, income, and democracy, and we provide supporting evidence for this theory.

### I. Education and Democracy

We follow the existing literature in economics and measure democracy using the Freedom House Political Rights Index. This index ranges from 1 to 7, with 7 representing the least amount of political freedom and 1 the most freedom. Following Barro (1999), we supplement this index with the related variable from Kenneth Bollen (1990) for 1955, 1960, and 1965, and we transform both indexes so that they lie between 0 and 1, with 1 corresponding to the most-democratic set of institutions. Our

basic data set is a five-yearly panel, where we take the democracy score for each country every fifth year. We prefer using the observations every fifth year to averaging the five-yearly data, since averaging introduces additional serial correlation (the results are robust to using five-year averages).

Our main right-hand-side variable, average years of schooling in the total population of age 25 and above, is from Barro and Jong-Wha Lee (2000) and is available in five-year intervals between 1960 and 2000. The value of this variable in our base sample ranges from 0.04 to 12.18 years of schooling, with a mean of 4.44.

Table 1 provides our main results using the Freedom House data. Column (i) shows the pooled OLS relationship between education and democracy by estimating the following model:

$$(1) \quad d_{it} = \alpha d_{i,t-1} + \gamma s_{i,t-1} + \mu_t + v_{it}$$

where  $d_{it}$  is the democracy score of country  $i$  in period  $t$ . The lagged value of this variable is included on the right-hand side to capture persistence in democracy and also potentially mean-reverting dynamics in democracy (i.e., the tendency of the democracy score to return to some equilibrium value for the country). The main variable of interest is  $s_{i,t-1}$ , the lagged value of average years of schooling. The parameter  $\gamma$  therefore measures whether education has an effect on democracy. The parameter  $\mu_t$  denotes a full set of time effects, which capture common shocks to (common trends in) the democracy score of all countries, and  $v_{it}$  is an error term, capturing all other omitted factors.

Column (i) shows a statistically significant correlation between education and democracy. The estimate of  $\gamma$  is 0.027 with a standard error of 0.004, which is significant at the 1-percent level (all the standard errors are robust for arbitrary heteroscedasticity and clustering at the country level). If causal, this estimate would imply that an additional year of schooling increases the “steady-state” value of democracy by 0.093 ( $\approx 0.027/[1 - 0.709]$ , where the long-run effect is calculated as  $\gamma/[1 - \alpha]$ ). This is a reasonably large magnitude relative to the mean of democracy in the sample, which is 0.57. Notice that this estimate includes both the direct and the indirect effect of education on democracy

TABLE 1—FIXED EFFECTS RESULTS

Independent variable	Base sample, 1965–2000 (5-year data)								
	Pooled OLS (i)	Fixed-effects OLS (ii)	Arellano- Bond GMM (iii)	Fixed-effects OLS (iv)	Arellano- Bond GMM (v)	Fixed-effects OLS (vi)	Arellano- Bond GMM (vii)	Fixed-effects OLS (viii)	Arellano- Bond GMM (ix)
Democracy <sub><i>t</i>-1</sub>	0.709 (0.035)	0.385 (0.053)	0.507 (0.096)	0.362 (0.053)	0.493 (0.101)	0.369 (0.054)	0.510 (0.094)	0.351 (0.055)	0.499 (0.097)
Education <sub><i>t</i>-1</sub>	0.027 (0.004)	-0.005 (0.019)	-0.017 (0.022)	0.005 (0.020)	-0.013 (0.024)	-0.012 (0.019)	-0.013 (0.026)	-0.007 (0.020)	-0.020 (0.026)
Age-structure <i>F</i> test:				[0.08]	[0.31]			[0.19]	[0.27]
Log population <sub><i>t</i>-1</sub>				-0.124 (0.101)	-0.023 (0.115)			-0.042 (0.108)	0.049 (0.143)
Log GDP per capita <sub><i>t</i>-1</sub>						-0.012 (0.042)	-0.187 (0.110)	-0.001 (0.049)	-0.121 (0.182)
Time-effects <i>F</i> test:	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
Hansen <i>J</i> test:			[0.31]		[0.21]		[0.44]		[0.15]
AR(2) test:			[0.81]		[0.89]		[0.96]		[0.88]
Number of observations:	765	765	667	746	652	684	595	676	589
Number of countries:	108	108	104	104	101	97	93	95	92
<i>R</i> <sup>2</sup> :	0.71	0.78		0.78		0.76		0.77	

*Notes:* Fixed-effects OLS regressions are reported in columns (ii), (iv), (vi), and (viii) with country dummies and robust standard errors clustered by country in parentheses. Columns (iii), (v), (vii), and (ix) use GMM of Manuel Arellano and Stephen R. Bond (1991), with robust standard errors; columns (vii) and (ix) treat log GDP per capita<sub>*t*-1</sub> as predetermined and instrument its first difference with log GDP per Capita<sub>*t*-2</sub>. Year dummies are included in all regressions, and the time effects *F* test gives the *p* value for their joint significance. The dependent variable is the augmented Freedom House Political Rights Index. The base sample is an unbalanced panel, 1965–2000, with data at five-year intervals in levels where the start date of the panel refers to the dependent variable (i.e., *t* = 1965, so *t* - 1 = 1960). Columns (iv), (v), (viii), and (ix) include but do not display the median age of the population at *t* - 1 and four covariates corresponding to the percentage 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. Countries enter the panel if they are independent at *t* - 1. See text for data definitions and sources.

working through income (since greater education corresponds to greater income, which might also lead to more democracy).

Equation (1) is similar to the regressions in the existing literature in that it does not control for country fixed effects. Thus the entire long-run differences across countries are used to estimate the effect of education on democracy. As a result, omitted factors that influence both democracy and education in the long run will lead to spurious positive estimates of  $\gamma$ .

The alternative is to allow for the presence of such omitted factors (that are not time-varying) by including country fixed effects, that is, by estimating a model of the form

$$(2) \quad d_{it} = \alpha d_{i,t-1} + \gamma s_{i,t-1} + \mu_i + \delta_i + u_{it}$$

which only differs from (1) because of the full set of country dummies, the  $\delta_i$ 's.

The rest of Table 1 reports estimates of  $\gamma$  from models similar to (2). Column (ii) is identical to column (i) except for the fixed country

effects, the  $\delta_i$ 's. The results are radically different, however. Now  $\gamma$  is estimated to be -0.005 with a standard error of 0.019; thus it is highly insignificant and has the opposite sign to that predicted by the modernization hypothesis [and to that found in the pooled OLS regression of column (i)].

In the regression in column (ii) because the regressor  $d_{i,t-1}$  is mechanically correlated with  $u_{is}$  for  $s < t$ , the standard fixed-effect estimation is not consistent in panels with a short time dimension. To deal with this problem, in column (iii) we use the generalized method-of-moments estimator (GMM) developed by Manuel Arellano and Stephen R. Bond (1991). The estimate for  $\gamma$  is now more negative, -0.017 (SE = 0.022). The AR(2) test and the Hansen *J* test, reported at the bottom of this column, indicate that the overidentifying restrictions implied by this GMM procedure are not rejected.

The remaining columns of Table 1 investigate the relationship between education and de-

mocracy when other covariates are included. Columns (iv) and (v) control for the age structure and population by including the fractions of the population in five different age ranges, the median age of the population, and the logarithm of total population (see the working-paper version [Acemoglu et al., 2004] for details and sources). These variables are correlated with education attainment of the population and might have a direct effect, making it impossible for us to identify the influence of education on democracy. We find that the age-structure variables are jointly significant at the 10-percent level using fixed effects OLS, but not GMM, while log population is not significant. The effect of education on democracy continues to be highly insignificant in both cases.

Columns (vi) and (vii) add GDP per capita. Education is still insignificant (and has a negative coefficient), and interestingly, GDP per capita itself is insignificant with a negative coefficient. The causal effect of income on democracy, which is the other basic tenet of the modernization hypothesis, is therefore also not robust to controlling for country fixed effects. We investigate this issue in greater detail in Acemoglu et al. (2004). Finally, columns (viii) and (ix) control for log population, age-structure variables, and GDP per capita simultaneously, again with similar results.

The working-paper version (Acemoglu et al., 2004) also shows that the same results apply when we exclude sub-Saharan Africa, formerly socialist countries, and Muslim countries, and when we use five-year-averaged data or different measures of democracy.

Overall, these results show that there is no empirical relationship between education and democracy once country fixed effects are included, and therefore they cast considerable doubt on the causal effect of education on democracy.

## II. From Education to Institutions?

The recent paper by Glaeser et al. (2004) argues that there is a causal effect of education on institutions. They substantiate this by reporting regressions similar to our model in (2), but with very different results, in particular showing a positive effect of education on democracy. Why are their results different from ours?

TABLE 2—FIXED-EFFECTS RESULTS: EDUCATION, DEMOCRACY, AND POLITICAL INSTITUTIONS: GLAESER ET AL. (2004) SAMPLE, 1965–2000 (FIVE-YEAR DATA)

Independent variable	Executive (i)	Autocracy (ii)	Democracy (iii)	Autocracy (iv)
<i>A. No Time Effects</i>				
Institutions <sub><i>t</i>-1</sub>	-0.572 (0.072)	-0.547 (0.068)	-0.515 (0.065)	-0.864 (0.103)
Education <sub><i>t</i>-1</sub>	0.498 (0.119)	0.909 (0.179)	0.700 (0.180)	0.096 (0.071)
Log GDP per capita <sub><i>t</i>-1</sub>	0.038 (0.403)	-0.508 (0.630)	0.292 (0.606)	0.267 (0.202)
<i>R</i> <sup>2</sup> :	0.33	0.32	0.30	0.47
<i>B. Including Time Effects</i>				
Institutions <sub><i>t</i>-1</sub>	-0.618 (0.073)	-0.616 (0.071)	-0.580 (0.067)	-0.897 (0.106)
Education <sub><i>t</i>-1</sub>	-0.163 (0.192)	-0.318 (0.267)	-0.432 (0.298)	-0.137 (0.126)
Log GDP per capita <sub><i>t</i>-1</sub>	0.168 (0.360)	-0.317 (0.550)	0.477 (0.561)	0.292 (0.192)
Time-effects <i>F</i> test:	[0.01]	[0.00]	[0.00]	[0.08]
<i>R</i> <sup>2</sup> :	0.39	0.40	0.37	0.50
<i>C. Including Time Effects</i>				
Institutions <sub><i>t</i>-1</sub>	-0.617 (0.073)	-0.615 (0.071)	-0.579 (0.068)	-0.891 (0.107)
Education <sub><i>t</i>-1</sub>	-0.125 (0.182)	-0.389 (0.229)	-0.324 (0.289)	-0.088 (0.125)
Time-effects <i>F</i> test	[0.01]	[0.00]	[0.00]	[0.07]
<i>R</i> <sup>2</sup> :	0.39	0.40	0.37	0.49
Number of observations:	499	499	499	349

*Notes:* The table reports fixed effects OLS regressions in all columns, with country dummies and robust standard errors clustered by country in parentheses. Year dummies are included in panels B and C, and the time-effects *F* test gives the *p* value for their joint significance. The dependent variable in column (i) is change in Constraint on the Executive from Polity. The dependent variable in column (ii) is change in negative Autocracy Index from Polity. The dependent variable in column (iii) is change in Democracy Index from Polity. The dependent variable in column (iv) is change in negative Autocracy Index from Przeworski et al. (2000). The base sample in all columns is an unbalanced panel, 1965–2000, with data at five-year intervals, where the start date of the panel refers to the dependent variable (i.e.,  $t = 1965$ , so  $t - 1 = 1960$ ). See Glaeser et al. (2004) for data definitions and sources.

In Table 2A, we replicate their results, which use the constraint on executive from Polity, the autocracy score from Polity, the democracy score from Polity, and the autocracy score from Przeworski et al. (2000). (The only difference from their results is that we transform the variables so that all coefficients have the same sign. Note also that in this table, the indexes are no longer normalized between 0 and 1.) These columns exactly match their regressions, but are

different from our corresponding regressions, because they do not include time effects, the  $\mu_i$ 's in equations (1) and (2). In the absence of time effects, the parameter  $\gamma$  is identified from the over-time variation: in this context, the world-level increase in education and democracy. This clearly does not correspond to any causal effect.

Panels B and C of Table 2 report estimates with and without income per capita, but including time effects as in our basic specifications. In all cases, the effect of education is insignificant and has the *incorrect sign*, as in our basic results. Moreover, in all columns except one, the time effects are jointly significant at the 1-percent level or less, and in that one case they are significant at the 10-percent level [and interestingly, in that case, as column (iv) shows, education is insignificant even without time effects].

The evidence in Table 2 therefore shows that there seems to be no effect of education on democracy or on other political institutions.

### III. Concluding Remarks

A common view clearly articulated by the modernization theory claims that high levels of schooling are both a prerequisite for democracy and a major cause of democratization. The evidence in favor of this view is largely based on cross-sectional or pooled cross-sectional regressions. This paper documents that this evidence is not robust to including fixed effects and exploiting the within-country variation. This strongly suggests that the cross-sectional relationship between education and democracy is driven by omitted factors influencing both education and democracy rather than a causal relationship.

This evidence poses two important questions:

- (i) Is there no long-run causal relationship between education and democracy? It is important to emphasize that our paper does not answer this question. We have exploited the five-yearly variation in the post-war era. It is possible that changes in education have very long-run effects, say, over 50 or 100 years, that do not manifest themselves in the shorter time frame that we have examined.
- (ii) What are the omitted factors influencing both education and democracy, captured by the country fixed effects? We conjecture that these are related to the joint evolution of economic and political development ("the historical development paths"). In our companion paper (Acemoglu et al., 2004) we provide evidence consistent with this conjecture. We document that the fixed effects for the former European colonies are very highly correlated with the historical, potentially exogenous determinants of institutional development in this sample: in particular, the mortality rates faced by the European settlers and the density of the indigenous populations (see Acemoglu et al., 2001, 2002) as well as early experiences with democracy.

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