# Income and Democracy:
## Revisiting the Evidence*

Enrique Moral-Benito
Bank of Spain

Cristian Bartolucci
Collegio Carlo Alberto

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**Abstract**

It is well-known in the literature that income per capita is strongly correlated with the level of democracy across countries. In an influential paper, Acemoglu et al. (2008) find that this linear correlation disappears once they control for country-specific effects focusing on within-country variation. In this paper we find evidence of a non-linear effect from income to democracy even after controlling for country-specific effects. While a positive effect emerges for poor countries, this effect vanishes for rich countries.

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# 1 Introduction

More than 23 centuries ago, Aristotle already discussed economic prosperity as one of the factors that stimulate democracy (Aristotle, 1932); he argued that only in a wealthy society with relatively few people living in real poverty could democracy arise and irresponsible demagogues be avoided. This discussion was further developed and formalized by Lipset (1959), and thus this idea is often called the Lipset hypothesis. There is a number of mechanisms that could explain this effect, as for example increased education and an enlarged middle class (Lipset, 1959). The cross-country evidence examined in Barro (1999) confirms that the Lipset/Aristotle hypothesis is a strong empirical regularity.

However, in a recent paper, Acemoglu et al. (2008) find evidence against this hypothesis. In particular, after controlling for factors that simultaneously affect both income and democracy (i.e.

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country-specific fixed heterogeneity), they find that there is no evidence of a linear effect of income on democracy. They interpret this result as evidence in favor of the idea that societies embarked on divergent political-economic development paths at certain critical junctures in the distant past.

In this paper we revisit this hypothesis and find evidence in favor of an heterogeneous effect of income on democracy (heterogeneous across levels of income) even after controlling for country-specific effects in the framework of Acemoglu et al. (2008). More concretely, we find that the poorest countries in the world might be benefited in terms of higher democracy standards from an increase in income. However, once a certain level of economic development is reached, this effect vanishes.

As it is pointed out in North (1990), institutions are the rules of the game in a society, or more formally, are the humanly devised constraints that shape human interaction. That institutions affect the performance of economies is hardly controversial. It is then plausible that economies with good performances, in order to maintain the *status quo*, are going to be less prone to change their rules than economies with poor performances.

Figure 1 provides enlightening evidence in favor of this hypothesis. In particular, Figure 1 plots democracy[1] against log GDP per capita for rich an poor countries separately.[2] In order to control for factors that simultaneously affect income and democracy in levels (i.e. country-specific effects), both variables are expressed in deviations from country-specific time means so that only within country variation is plotted.[3] When we consider rich countries the within-country correlation between income and democracy is surprisingly weak. But on the other hand, when we consider poor countries, there is a strong positive within-country association between both indicators. This informal evidence suggests that the effect of income on democracy is not constant for every level of income, giving support to our working hypothesis: despite economic prosperity positively influences the democracy level in poor countries, once a certain threshold of economic development is reached this influence disappears.

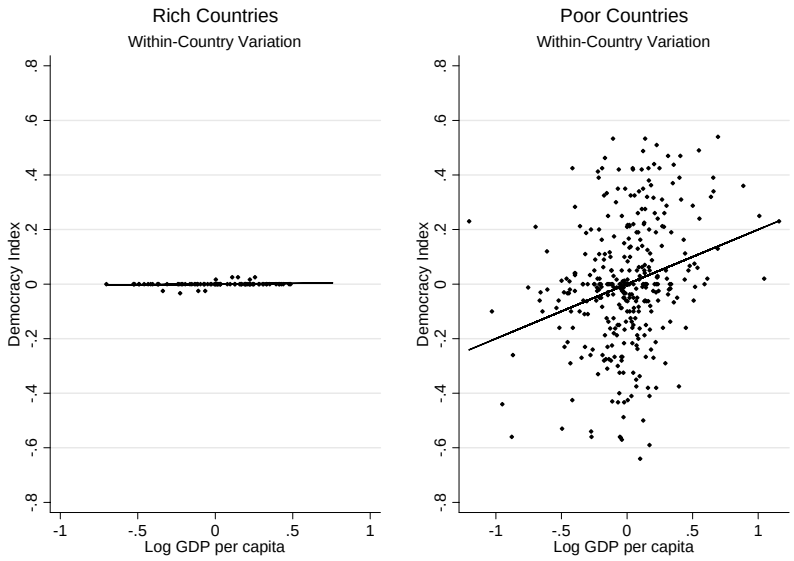
Usually, linearity is taken to be a convenient and homogeneous local approximation of a more general model. However, in this context we are interested in the comparison of very different groups of countries. If the effect of income on institutions is larger for poor countries than for rich countries, the local approximation is no longer tenable and the linear estimates might be biased. In view

[1]The measure of democracy corresponds to Democracy Index minus Autocracy Index from Polity IV (see the Data section for more details).

[2]Poor countries in Figure 1 are those in the bottom 80% of the income distribution in a given year (see the figure for more details). Although only the top-2 deciles might be a very restrictive group, it represents a large number of countries. It includes at least one observation of Antigua, Australia, Austria, Barbados, Belgium, Canada, Denmark, Finland, France, Germany, Greece, Iceland, Ireland, Israel, Italy, Japan, Luxembourg, Netherlands, New Zealand, Norway, Singapore, Spain, Sweden, Switzerland, United Kingdom and United States. This ad hoc partition of the sample is only for illustration and driven by the results discussed later in the paper (see Section 4). In any event, very similar results are found using alternative definitions of poor and rich, such as $70-30$ and $90-10$.

[3]We consider here data in deviations from country-specific means due to its straightforward connection with the fixed effects estimators considered later in the paper (note that a simple OLS regression with the data in Figure 1 leads to the commonly-used fixed effects estimator).

Figure 1: Income and Democracy




This figure plots GDP per capita and the Polity IV index of democracy over the period 1960-2000 for rich and poor countries. In order to consider country-specific heterogeneity we plot the variables in deviations from its country-specific time mean. We split the sample in ten-year periods and each country-period pair is labeled as rich (poor) if its associated GDP per capita is above (below) the 80 percentile of the empirical cross-sectional density of this variable in a given period. See the Data section for more details on the sample and variables. The regression represented by the fitted line in the left panel (rich countries) yields a coefficient of 0.005 (standard error = 0.005), and the regression for poor countries in the right panel yields a coefficient of 0.203 (standard error = 0.049).

of the evidence presented in Figure 1, in this paper we relax the linearity assumption in order to further investigate the relationship that connects income and democracy. More concretely, we are interested in the measurement of the effect of income on institutions allowing for heterogeneous effects across income levels. Using the same dataset than Acemoglu et al. (2008), we find that income has a significant non-linear effect on democracy even after controlling for country-specific unobserved heterogeneity simultaneously affecting both variables. This effect is positive for lower values of income but it vanishes when income reaches a certain level.

Our first strategy is to partial out unobserved fixed heterogeneity in a within-group regression. By doing this we find a significant non-linear relationship between income and democracy. This evidence is equivalent to the one showed in Figure 1, but also controlling for cross-sectional correlations (i.e. time dummies) and the lagged dependent variable to capture state dependence. Fixed effects estimates in dynamic panels are biased when the number of time-series observations is small (e.g. Nickell, 1981). Moreover, in this context it is controversial to give a causal interpretation to the within-group estimates. Feedback effects from democracy to income are also present in these within-group correlations. As in Acemoglu et al. (2008), in order to further investigate causality issues we

consider first-differenced GMM (e.g. Arellano and Bond, 1991), finding again evidence in favor of a non-linear effect of income on democracy.

On the other hand, it is well-known that first-differenced GMM estimators are poorly behaved with persistent series such as GDP. This is so because lagged levels of the variables are only weakly correlated with subsequent first-differences (e.g. Blundell and Bond, 1998). We repeat the GMM estimation exercises using randomly generated instruments and find that the GMM estimates remain virtually the same; this result confirms that a problem of weak instruments is present in this particular application. In order to alleviate this problem, we also consider the likelihood-based counterpart of first-differenced GMM and confirm that our result is robust to finite sample biases due to weak instruments. Using the LIML counterpart of the GMM Arellano-Bond estimator we find no significant linear effect of income on democracy.[4] On the other hand, we do find significant evidence in favour of a non-linear effect, suggesting that income causes democracy only in low-income countries.

To understand if income has an effect on democracy is important for public policy decisions. If democracy is a consequence of economic development, international policies toward countries with authoritarian regimes should aim to foster the process of economic development, thereby hastening the eventual replacement of authoritarian regimes with more democratic successors. By contrast, if democracy is not caused by economic development, this may not be the route to free elections.

The rest of this paper is organized as follows. In Section 2 we briefly describe the data. Section 3 presents the empirical strategy and discusses the main results in the paper. Section 4 analyzes the potential problem of finite sample biases due to weak instruments and presents estimates of "causal effects" robust to this problem. Some concluding remarks are presented in the final Section.

# 2 Data

Many different indicators of democracy have been produced and employed in studies of comparative politics and international economics.[5] Our empirical analysis is based on the extensively used measures from the Polity Project.[6] Polity IV covers the period 1800-2000 for 184 countries. Its main advantage is that all of the indicators used in the construction of the aggregate measure are accessible and well documented. According to this index, democracy reflects three essential elements: the presence of institutions and procedures through which citizens can express preferences about alternative policies and leaders; the existence of institutionalized constraints on the power of the executive; and, the guarantee of civil liberties to all citizens. The original index ranges between $-10$ (pure autocracy) and 10 (full democracy). Following earlier literature considering this variable, the

[4]Using the Arellano-Bond estimator, Acemoglu et al. (2008) find a significantly negative linear effect of income on democracy. We argue that this result is mainly due to the small sample bias of GMM estimators.

[5]See Munck and Verkuilen (2002) for a survey.

[6]See Marshall and Jaggers (2002) for a detailed description of the main characteristics of the democracy indicators produced by the Polity Project.

original index is normalized to lie between 0 and 1.

In order to keep comparability of our results with Acemoglu et al. (2008), data on GDP per capita come from the Penn World Tables 6.1 (see Heston et al., 2002). In particular, GDP per capita is measured in constant 1996 USD at PPP. The resulting dataset represents an unbalanced panel of 137 countries over the post-war period 1960-2000. Following the recommendation in Barro and Sala-i-Martin (2003) we take as baseline the specification in which we split the sample in ten-year periods in order to avoid business cycle effects. In any case, we also present results considering data at five-year intervals.

Although measurement error is an important issue in our measures of democracy and income,[7] we are more aware of the consequences of the latter on the main results of the paper. This is so because whenever the within country variation of measurement error in the dependent variable is *iid* over time, it does not affect results. As the PPP adjustment of income in Penn World Tables 6.1 and 6.2 has been criticized (see Johnson et al., 2009), most of the results of the paper are replicated using the more stable harmonization of prices presented in later version of the Penn World Tables.[8]

# 3 Non-linearities in Income and Democracy

## 3.1 Econometric Model

Acemoglu et al. (2008) propose to estimate the effect of income on democracy considering the following model:

$$d_{it} = \gamma d_{it-1} + \beta y_{it-1} + \delta_t + \eta_i + v_{it} \tag{1}$$

where $d_{it}$ is the democracy score of country $i$ ($i = 1, ..., N$) in period $t$ ($t = 1, ..., T$), and $y_{it-1}$ is our variable of interest, the lagged value of log income per capita. Moreover, persistence and mean-reverting dynamics of democracy are captured by the coefficient of the lagged dependent variable on the right-hand side. $\eta_i$ captures country-specific fixed heterogeneity potentially correlated with the rest of variables on the right-hand side, and $\delta_t$ represents a set of time-specific shocks to democracy common to all countries in our sample. Finally, transitory shocks to democracy and other omitted factors in the model are represented in the term $v_{it}$.

In this linear specification, the effect of income on democracy (i.e. $\partial d_{it}/\partial y_{it-1} = \beta$) is assumed to be the same for all countries regardless of their level of income. Given the tentative evidence discussed in the introduction, we expect an heterogeneous effect of income on democracy depending on the income level. While democracy in rich countries might not react to changes in income, improving income per capita in poor countries is expected to have a positive effect on their democracy standards.

[7] See Treier and Jackman (2008) for evidence of measurement error in the Polity IV indicator, and Johnson et al. (2009) for inconsistencies in the PPP-adjusted measure of income presented in Penn World Table 6.1

[8] Results are presented in Table A2 in the Appendix.

The simplest specification that would capture this non-linearity is based on the inclusion of a square term of income in equation (1):

$$d_{it} = \gamma d_{it-1} + \beta_1 y_{it-1} + \beta_2 y_{it-1}^2 + \delta_t + \eta_i + v_{it} \tag{2}$$

where now the marginal effect of income on democracy is given by $\frac{\partial d_{it}}{\partial y_{it-1}} = \beta_1 + 2\beta_2 y_{it-1}$ which linearly depends on the level of income, and $\beta_2$ describes this dependence.[9] Provided $\beta_2 < 0$, the model in (2) represents a quadratic function with a maximum at $y^* = -\frac{\beta_1}{2\beta_2}$. Therefore, democracy in countries with GDP per capita levels below $y^*$ positively reacts to changes in income. However, once the income threshold $y^*$ is passed, this positive effect disappears.

Equations (1) and (2) might be estimated under different correlation structures between the the error term $\delta_t + \eta_i + v_{it}$ and the regressors $d_{it-1}$, $y_{it-1}$, $y_{it-1}^2$. In particular, we always allow for correlation between the country-specific effects ($\eta_i$) and the time effects ($\delta_t$) with the variables on the right hand-side ($d_{it-1}$, $y_{it-1}$, $y_{it-1}^2$). Fixed effects estimates accommodate this correlation and are consistent as $N \to \infty$ and $T \to \infty$. Pooled OLS is inconsistent in this framework which, as stated by Acemoglu et al. (2008), "*...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*".

With respect to the correlation between the transitory shock $v_{it}$ and the regressors, we consider two different working hypothesis in this paper. We first estimate the model under the strict exogeneity assumption presented in equation (3) which implies that both lagged democracy ($d_{it-1}$) and lagged income ($y_{it-1}$) are uncorrelated with the full path of shocks $v_i = (v_{i1}, ..., v_{it}, ..., v_{iT})'$:

$$E(v_{it} \mid d_i, y_i, \delta_t, \eta_i) = 0 \tag{3}$$

where $d_i$ and $y_i$ are the $T \times 1$ vectors $(d_{i1}, ..., d_{iT})'$ and $(y_{i1}, ..., y_{iT})'$. Fixed effects estimator is based on this assumption, which given the inclusion of the lagged dependent variable does not hold by definition because $d_{it-1}$ is correlated with $v_{is}$ for $s < t$, when $T$ is small (Nickell 1981). Moreover, under this strict exogeneity assumption, there is no feedback from democracy to income (i.e. income is not affected by changes in democracy).

In order to alleviate these two drawbacks of the strict exogeneity assumption in (3), we also consider the following working hypothesis:

$$E(v_{it} \mid d_i^{t-1}, y_i^{t-1}, \delta_t, \eta_i) = 0 \tag{4}$$

where $d_i^{t-1}$ and $y_i^{t-1}$ are the $t-1 \times 1$ vectors $(d_{i1}, ..., d_{it-1})'$ and $(y_{i1}, ..., y_{it-1})'$. We label this assumption as partial endogeneity because all past shocks up to the current period ($t$) $(v_{i1}, ..., v_{it})$ affect not only

[9] Assuming that the heterogeneity of the effect is fully described by a linear function of income might seem restrictive a priori. Table A1 in the Appendix presents results considering a more flexible polynomial specification providing evidence that the specification in (2) is flexible enough to capture the non-linearity in the income-democracy relationship.

democracy ($d_{it}$) but also income ($y_{it}$). However, future shocks ($v_{it+1}, ..., v_{iT}$) are not correlated with neither current democracy nor current income. Therefore, we allow for a feedback effect from changes in democracy to changes in income which seems to us a desirable property in the income and democracy relationship.[10] In order to accommodate the partial endogeneity assumption (4) and given the dimensions of our panel dataset[11] we resort to panel GMM estimators advanced by Holtz-Eakin, Newey and Rosen (1988) and Arellano and Bond (1991).

The most important reason, but also the most controversial one, that justifies our preference for the Arellano-Bond estimator over withing-group, is the aim to give a causal interpretation to our estimates, that is robust to feedback from democracy to income. However, this kind of estimators is also interesting in this context because, as pointed out in Section 2, the measure of income used in this study is likely to have measurement error. The within-group transformation may exacerbate the attenuation bias due to measurement error while, under some conditions, panel GMM estimators are robust to measurement error in regressors.[12]

## 3.2 Empirical Results

Table 1 presents the results when estimating equations (1) and (2) under strict exogeneity (i.e. Fixed effects OLS) and partial endogeneity (i.e. Arellano-Bond GMM) using the panel of five-year data. First of all, in column (1) we report the estimates if fixed effects ($\eta_i$) are not included in the model so that the estimation technique is pooled OLS. In this case we observe a strong positive association between income and democracy in levels as Lipset (1959) hypothesized. In contrast, column (2) illustrates that once we control for country-specific effects and we only use within-country variation this positive correlation clearly disappears (these two columns replicates columns 1 and 2 of Table 3 in Acemoglu et al. (2008)). This result would imply that during the post-war period, increases in the income level were not associated with improvements in the democracy score across countries. However, in column (3) we can appreciate evidence of a significant non-linear association between

[10] In addition to strict exogeneity and partial endogeneity, there is also a third possible configuration labeled as strict endogeneity in which democracy and income are correlated with the full path of shocks from $t = 1$ to $T$. Estimating the model under this assumption would require the availability of additional country-specific time-varying variables uncorrelated with past, present and future values of democracy but correlated with the income level. Given the difficulty and controversy of this task we prefer to work with the somehow less ambitious partial endogeneity assumption.

[11] The panel dataset we consider for ten-year data has $T = 4$ and $N = 107$, so small $T$ oriented estimators seem to be more appropriate than other time-series oriented estimators such as Anderson and Hsiao (1982). Nevertheless in the appendix we show that the non-linear effect of income on democracy is also found to be significant using the estimator proposed in Anderson and Hsiao (1982), see Table A1.

[12] Whenever measurement error is free of serial correlation, the panel dimension of the data is helpful for dealing with attenuation bias because it provides internal instruments. The list of moments employed in the Arellano-Bond estimator is only a subset of the list of moments available when there is measurement error in models with unobserved fixed heterogeneity. GMM estimators based on this type of moments were proposed by Griliches and Hausman (1986).

changes in income and democracy after the sixties that somehow confirms the intuition of a positive correlation in poor countries that vanishes after the $y^*$ threshold.

Table 1: Linear vs. Quadratic Specification — Five-year data

| | Pooled OLS | Fixed Effects OLS | | Arellano-Bond GMM | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| | Dependent variable is democracy | | | | |
| Democracy Index $_{t-1}$ | 0.749 | 0.449 | 0.446 | 0.590 | 0.611 |
| | (0.034) | (0.063) | (0.059) | (0.106) | (0.094) |
| Log GDP per capita $_{t-1}$ | 0.053 | −0.006 | 0.388 | −0.351 | 1.368 |
| | (0.010) | (0.039) | (0.210) | (0.127) | (0.401) |
| (Log GDP per capita $_{t-1}$)$^2$ | | | −0.024 | | −0.082 |
| | | | (0.012) | | (0.022) |
| Fixed Effects | NO | YES | YES | YES | YES |
| Time Effects | YES | YES | YES | YES | YES |
| Income threshold ($y^*$) | | | 8.08 | | 8.34 |
| Hansen $J$ Test | | | | 0.03 | 0.02 |
| AR(2) Test | | | | 0.39 | 0.33 |
| Hausman Test | | | | 0.00 | 0.00 |
| Observations | 854 | 854 | 854 | 747 | 747 |
| Countries | 136 | 136 | 136 | 114 | 114 |
| $R$-squared | 0.77 | 0.82 | 0.82 | | |

Dependent variable is the Polity IV measure of democracy. Column (1) reports pooled cross-sectional OLS without country-specific effects; columns (2) and (3) presents Fixed effects OLS regressions under the strict exogeneity assumption of income and democracy; finally, in columns (4) and (5) we present results when applying the panel GMM estimator proposed by Arellano and Bond (1991) which accommodates partial endogeneity of both democracy and income. The sample period is 1960-2000 at five-year intervals. The income threshold ($y^*$) is the (log) level of income per capita (at 1996 USD PPP) beyond which the positive effect of income on democracy disappears. In all cases, standard errors clustered by country are in parentheses.

In columns (4) and (5) we aim to give a causal interpretation to our estimates and we allow for partial endogeneity of the income per capita variable. In particular, column (4) replicates the result in Acemoglu et al. (2008) (column 4 of Table 3), taking into account the possible feedback from democracy to income there is no evidence of a positive causal effect from income to democracy. Nevertheless, by including a square term of the income variable in the model (column 5) we obtain a significant and non-linear effect of income on democracy. This is clear evidence in favor of our hypothesis of heterogeneous effects across different levels of income. Below a certain level of wealth, increases in income generate increases in democracy confirming the Lipset hypothesis, but only for

poor countries.

Table 2 show results where the relationship between income and democracy is analyzed at lower frequency by estimating similar regressions using the data over ten-year intervals. The results are similar to those with five-year data in Table 1. There is no evidence of a positive linear association between income and democracy once the fixed country heterogeneity is partialled out, but a positive effect of income on democracy, exists only for low levels of income. Again, this pattern holds for both fixed-effects and first differenced GMM, but results are more conclusive when using the latter.

Hansen tests do not reject joint consistency of moment restrictions in any specification presented in Table 1 (column 4 and 5) and Table 2 (column 4 and 5). AR(2) tests on residuals in the Arellano-Bond GMM estimations indicate that there is no further serial correlation in both samples —at five or ten-year intervals— and for both specifications —with and without the squared log-GDP—. In addition, Hausman tests reject the null of equality between the coefficients estimated by fixed effects OLS (efficient under the null) and Arellano-Bond GMM (consistent under the null but robust to partial endogeneity) in both samples and in both specifications. The null of equality of coefficients would be true if the exogeneity of income holds.[13] This result supports our preference for estimators robust to partial endogeneity of income such as Arellano-Bond GMM.

We also show in the Appendix that our results are robust to alternative estimation methods and specifications (Table A1), and to alternative sources of income data (see Table A2). In Table A1 we consider the Anderson and Hsiao (1982) estimator, a more flexible polynomial specification, a specification without lagged dependent variable and, finally, a simple segmented regression that instead of the square term includes an interaction term between log GDP and a dummy variable for rich countries. In all cases we obtain the same heterogeneous effect from income to democracy: in poor countries, higher income is associated with higher democracy standards; however, this result does not hold in rich countries.

# 4 The Problem of Many Weak Instruments

The intuition behind the panel GMM estimates presented in Tables 1 and 2 is that lagged levels of the regressors are used as instruments for the same variables in first differences. Since income is a persistent variable, its first differences might be weakly correlated with its lagged levels, which implies that the relevance condition for validity of these instruments may be violated.[14] Moreover, the number of observations in the cross-section dimension ($N$) is small in this cross-country setting

[13]Fixed effects OLS produces efficient estimates related to Arellano-Bond GMM estimates because it is equivalent to use all the available lags and leads as instruments.

[14]As illustrative evidence of this weak correlation, Table A3 in the appendix shows results from the first stage regressions of the first difference of income on some of the lags available in the data. The covariances of every lag of income with the current first difference are not significantly different from zero. On the other hand, as indicated by the $R$-squared, less than 1 percent of the variation of the first differences of income is explained by each lag of income.

Table 2: Linear vs. Quadratic Specification — Ten-year data

| | Pooled OLS | Fixed Effects OLS | | Arellano-Bond GMM | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| | Dependent variable is democracy | | | | |
| Democracy Index $_{t-1}$ | 0.555 | 0.060 | 0.062 | 0.309 | 0.431 |
| | (0.052) | (0.091) | (0.079) | (0.134) | (0.127) |
| Log GDP per capita $_{t-1}$ | 0.098 | 0.007 | 0.549 | −0.368 | 1.545 |
| | (0.017) | (0.070) | (0.326) | (0.190) | (0.523) |
| (Log GDP per capita $_{t-1}$)$^2$ | | | −0.033 | | −0.090 |
| | | | (0.019) | | (0.030) |
| Fixed Effects | NO | YES | YES | YES | YES |
| Time Effects | YES | YES | YES | YES | YES |
| Income threshold ($y^*$) | | | 8.32 | | 8.58 |
| Hansen $J$ Test | | | | 0.01 | 0.08 |
| AR(2) Test | | | | 0.38 | 0.75 |
| Hausman Test | | | | 0.00 | 0.00 |
| Observations | 419 | 419 | 419 | 302 | 302 |
| Countries | 114 | 114 | 114 | 107 | 107 |
| $R$-squared | 0.64 | 0.77 | 0.77 | | |

Dependent variable is the Polity IV measure of democracy. Column (1) reports pooled cross-sectional OLS without country-specific effects; columns (2) and (3) presents Fixed effects OLS regressions under the strict exogeneity assumption of income and democracy; finally, in columns (4) and (5) we present results when applying the panel GMM estimator proposed by Arellano and Bond (1991) which accommodates partial endogeneity of both democracy and income. The sample period is 1960-2000 at ten-year intervals. The income threshold ($y^*$) is the (log) level of income per capita (at 1996 USD PPP) beyond which the positive effect of income on democracy disappears. In all cases, standard errors clustered by country are in parentheses.

and the first stage coefficients in the GMM framework proliferate as $T$ increases. Under these conditions, the first-differenced GMM estimator (Arellano-Bond) is poorly behaved (see Blundell and Bond (1998) amongst others) because we have a setting with many weak instruments and small samples.[15]

In order to further investigate this weak instruments problem in our framework, we repeat the panel GMM estimation exercises in columns (4) and (5) of Table 2 but substituting the real instruments (i.e. lagged levels of the regressors) by a set of random instruments completely independent of the regressors. This technique was previously applied to the Angrist and Krueger (1991) data by

[15]Note that consistency of this estimator is based on fixed-$T$ and $N \to \infty$, so that finite sample biases emerge due to small $N$ configurations, and worsen as $T$ increases.

Bound, Jaeger and Baker (1995). It is striking that the results reported in columns (2) and (6) of Table 3 look reasonable even with no information about the regressors in the randomly generated instruments. The coefficient estimates are similar to the estimates obtained with the real instruments (lagged levels of the regressors) presented again in columns (1) and (5) of Table 3 for the sake of comparison. In view of these results, it seems to be that the presence of (many) weak instruments represents a problem in this particular application.

Table 3: The Problem of Many Weak Instruments

| Estimation Method | Arellano-Bond GMM | | sub-system LIML | | Arellano-Bond GMM | | sub-system LIML | |
|---|---|---|---|---|---|---|---|---|
| Instrument for diff income | lagged income | random number | lagged income | random number | lagged income | random number | lagged income | random number |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Democracy Index $_{t-1}$ | 0.309 | 0.349 | 0.193 | 0.292 | 0.431 | 0.296 | 0.231 | 0.292 |
| | (0.134) | (0.167) | (0.085) | (0.116) | (0.127) | (0.159) | (0.086) | (0.117) |
| Log GDP per capita $_{t-1}$ | −0.368 | −0.311 | −0.043 | 0.000 | 1.545 | 1.294 | 1.169 | −0.000 |
| | (0.190) | (0.232) | (0.093) | (0.101) | (0.523) | (0.844) | (0.464) | (0.012) |
| (Log GDP per capita $_{t-1}$)$^2$ | | | | | −0.090 | −0.088 | −0.068 | 0.005 |
| | | | | | (0.03) | (0.044) | (0.028) | (0.014) |
| Income threshold ($y^*$) | | | | | 8.58 | 7.35 | 8.60 | 0.01 |
| Hansen $J$ Test | 0.01 | 0.01 | | | 0.08 | 0.02 | | |
| AR(2) Test | 0.38 | 0.41 | | | 0.75 | 0.33 | | |
| Observations | 302 | 302 | 302 | 302 | 302 | 302 | 302 | 302 |
| Countries | 107 | 107 | 107 | 107 | 107 | 107 | 107 | 107 |

Dependent variable is the Polity IV measure of democracy. Columns (1) and (5) present the results when applying the panel GMM estimator proposed by Arellano and Bond (1991) using the real instrument for first differenced income, i.e. lagged level of income (these are the same as columns (4) and (5) in Table 1. Columns (2) and (6) repeat the Arellano-Bond estimation exercise but substituting lagged income by a random number as instrument for first differenced income. Columns (3), (4), (7), and (8) present the results in columns (1), (2), (5), and (6) respectively, but employing the sub-system LIML estimator instead of first-differenced GMM. The sample period is 1960-2000 at ten-year intervals in all columns. The income threshold ($y^*$) is the (log) level of income per capita (at 1996 USD PPP) beyond which the positive effect of income on democracy disappears. In all cases, standard errors clustered by country are in parentheses.

In the single equation case, it is well documented in the literature that the effect of weak instruments on the distribution of two-stage least squares (2SLS) and limited information maximum likelihood (LIML) differs substantially in finite samples despite the fact that both estimators have the same asymptotic distribution. Although the distribution of LIML is centered at the parameter value, 2SLS is biased toward ordinary least squares (OLS). Moreover, Anderson, Kunitomo and Sawa (1982) concluded that LIML was to be strongly preferred to 2SLS, particularly if the number of instruments is large. In the panel data setting considered in this paper, Monte Carlo evidence provided in Moral-Benito (2011) illustrates that the recommendation in Anderson, Kunitomo and Sawa (1982) for cross-sections is also true in our panel setting; despite both estimators are asymp-

totically equivalent, sub-system LIML (i.e. the LIML counterpart of panel GMM estimators) seems to be strongly preferred to first-differenced GMM in terms of finite sample[16] performance when weak instruments are present (see the Appendix for more details).

Results when employing the panel LIML estimator to estimate equations (1) and (2) are reassuring. In contrast to first differenced GMM, coefficient estimates in columns (4) and (8) of Table 3 indicate that sub-system LIML is robust to the weak instrument problem previously discussed. If randomly generated instruments are considered instead of real instruments, point estimates completely change becoming zero and the confidence intervals are much wider, pointing to the lack of information contained in the random instruments. Therefore, we consider the results in columns (3) and (7) of Table 3 as our preferred estimates of the effect of income on democracy. This is so because in addition to address partial endogeneity of the regressors, they alleviate the finite sample biases of Arellano-Bond estimates due to weak instruments.

According to these results, there is no evidence of a linear (and homogeneous) effect of income on democracy during the post-war period (Table 3, column 3). Note that the Arellano-Bond estimate of the coefficient of lag income, when the effect is assumed to be linear, is significantly below zero. At a different confidence levels, this is true in most of results presented in Acemoglu et al. (2008). These counter-intuitive results are consequence of the bias due to small sample size of the Arellano-Bond estimator.

Although there is no significant evidence of an homogeneous effect of income on democracy, in column 7 of Table 3, we show evidence of a non-linear effect from income to democracy; income might cause democracy at low levels of income per capita, but this effect vanishes at a certain threshold level. In particular, if income per capita is higher than $5,431$ PPP USD ($exp(8.60) = 5,431$) the positive effect of income on democracy vanishes. Approximately 80 per cent of the country-year pairs are below this threshold in our baseline sample.[17] Countries such as Afghanistan, China or Libya are always in the positive income-democracy effect region.

# 5 Concluding Remarks

It is well-known in the literature that income per capita is strongly correlated with the level of democracy across countries. However, Acemoglu et al. (2008), controlling for factors that simultaneously affect both income and democracy (i.e. country-specific fixed heterogeneity), find that there is not evidence of a linear effect of income on democracy, supporting the idea that societies embarked on divergent political-economic development paths at certain critical junctures in the distant past. In this paper we revisit the evidence connecting both variables allowing for a more flexible specification. We argue that the effect of income on democracy may be different for different levels of income.

[16]Note again that we are referring to fixed-$T$, $N \to \infty$ asymptotics so that finite sample biases arise because of small $N$.

[17]This result motivates our partition of rich and poor countries in Figure 1.

This might be because stability of institutions is highly correlated with economic performance (e.g. North, 1990); therefore, democracy in poor countries with more fragile institutions will be affected by changes in income. However, in rich countries institutions are more stable and thus changes in income will not have any effect on the democracy level.

Using the same dataset as Acemoglu et al. (2008), we find that income has a significant non-linear effect on democracy even after controlling for country-specific unobserved heterogeneity simultaneously affecting both variables. This effect is positive for lower values of income, but it vanishes when income reaches a certain level. This result is robust to different econometric specifications and estimation techniques.

Our first strategy is to partial out unobserved fixed heterogeneity in a within-group regression. Although fixed-effects estimates confirm our hypothesis, there are good reasons to go beyond these results. Fixed effects regressions in dynamic panels are biased when the number of time-series observations is small. In addition, the indicator of income used is likely to suffer from measurement error and the attenuation bias is exacerbated when only within variation is exploited. Finally, there might also be feedback effects from democracy to income, and hence income would be partially endogenous to democracy. We also consider first-differenced GMM (e.g. Arellano and Bond, 1991) and its likelihood-based counterpart. Both estimators are robust to the mentioned cases of partial endogeneity but the latter one is expected to be preferred in terms of finite sample performance when instruments are weak. Both sets of results give significant evidence of a non-linear (causal) effect from income to democracy; income might cause democracy at low levels of income per capita, but this effect vanishes at a certain threshold level.

This evidence is relevant in the sense that it legitimates international policies toward poor countries with authoritarian regimes to encourage economic development, thereby hastening the eventual replacement of authoritarian regimes with more democratic successors.

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# A Appendix

## A.1 Likelihood-Based Estimation of Dynamic Panels

In the single equation, as stated in the main text, it is well-known that the effect of weak instruments on the distribution of two-stage least squares (2SLS) and limited information maximum likelihood (LIML) differs substantially in finite samples. One possible explanation for this result is that LIML imposes a symmetric normalization of the coefficients while the normalization implied by 2SLS is non-symmetric (see Hillier (1990)).

Intuitively, the LIML model in the single equation case consists of the gaussian likelihood function derived from the assumption of joint normality of the error terms in the structural and the reduced-form equations conforming the 2SLS model. In the panel data case considered in this paper we would have a set of structural form equations (one for each time period) and a set of reduced-form equations (thus we can label the approach as sub-system LIML instead of simply LIML or Full Information Maximum Likelihood — FIML —). In a recent paper, Moral-Benito (2011) writes down this likelihood function for dynamic panels with partially endogenous regressors.[18] In particular, the structural equation in (1) is completed with an unrestricted feedback process which is specified in the form of period-specific linear projections of the partially endogenous variables (lagged democracy and log GDP per capita) on all available lags. Once the full set of equations is specified the likelihood function is derived under normality of the shocks. It is important to remark here that the resulting (pseudo) maximum likelihood estimator is consistent and asymptotically normal regardless of non-normality.

To be more precise, this (pseudo) maximum likelihood estimator is asymptotically equivalent to the GMM estimator proposed in Arellano and Bond (1991) because the resultant first order conditions correspond to a GMM problem with a convenient choice of weighting matrix. On the other hand, simulation experiments serve to evaluate the finite-sample behavior of this likelihood-based estimator. In particular, results in Moral-Benito (2011) show that the estimator has negligible biases in contrast to the commonly-used Arellano and Bond's (1991) GMM estimator, which might have large biases in small samples, especially when the generated series are persistent over time. This can be interpreted as a generalization of the result in Anderson et al. (1982) for the single equation case.

[18] Likelihood-based approaches for dynamic panel models with unobservable individual effects and exogenous regressors are well-known in the literature (e.g. Bhargava and Sargan (1983); Alvarez and Arellano (2003)). However, these approaches do not consider settings with partially endogenous regressors.

## A.2 Additional Results

Table A1: Alternative Specifications and Estimation Methods

| | Anderson-Hsiao Estimator | | Quartic Specification | | No lagged Democracy | | Interaction Dummy | |
|---|---|---|---|---|---|---|---|---|
| Sample | Five-year | Ten-year | Five-year | Ten-year | Five-year | Ten-year | Five-year | Ten-year |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| | Dependent variable is democracy ($d_t$) | | | | | | | |
| $d_{t-1}$ | 0.723 | 0.590 | 0.400 | −0.031 | | | 0.125 | 0.155 |
| | (0.138) | (0.205) | (0.038) | (0.066) | | | (0.070) | (0.084) |
| $y_{t-1}$ | 2.273 | 4.028 | 65.10 | 64.25 | 0.547 | 0.944 | 0.127 | 0.126 |
| | (1.072) | (1.830) | (20.55) | (36.60) | (0.266) | (0.441) | (0.058) | (0.062) |
| $y_{t-1}^2$ | −0.101 | −0.181 | −12.80 | −13.30 | −0.034 | −0.057 | | |
| | (0.053) | (0.089) | (3.78) | (6.71) | (0.015) | (0.026) | | |
| $y_{t-1}^3$ | | | 1.10 | 1.20 | | | | |
| | | | (0.307) | (0.544) | | | | |
| $y_{t-1}^4$ | | | −0.035 | −0.039 | | | | |
| | | | (0.009) | (0.016) | | | | |
| $y_{t-1}R_{t-1}$ | | | | | | | −0.173 | −0.169 |
| | | | | | | | (0.061) | (0.063) |
| Income threshold ($y^*$) | 11.2 | 11.1 | | | 8.04 | 8.28 | 8.60 | 8.60 |
| $F$-test | | | | | | | 0.17 | 0.24 |
| Observations | 832 | 335 | 744 | 330 | 764 | 341 | 744 | 330 |

Dependent variable ($d_t$) is the Polity IV measure of democracy. $y_{t-1}$ corresponds to log GDP per capita, and $R_{t-1}$ is a dummy variable for rich country-year pairs. In particular, $R_{t-1}$ takes the value 1 if the GDP per capita associated to a particular country-period pair is above the 80 percentile of the empirical cross-sectional density of GDP per capita in period $t-1$. Columns (1) and (2) report the estimates using the Anderson and Hsiao (1982) approach. In columns (3) and (4) we report the results when considering a more flexible polynomial specification which confirms the concave relationship between income and democracy obtained in the main text with the quadratic specification. In columns (5) and (6) we drop the lagged dependent variable from the model in equation (2) in the main text. Finally, columns (7) and (8) consider the interaction dummy specification in which we allow for a different income-democracy effect for poor and rich countries by including a dummy for rich countries interacted with income. The income threshold ($y^*$) is the (log) level of income per capita (at 1996 USD PPP) beyond which the positive effect of income on democracy disappears. $F$-test reports the p-value of the null that sum of coefficients on $y_{t-1}$ and $y_{t-1}R_{t-1}$ is zero (i.e. the effect of income on democracy for rich countries is zero). In all cases, standard errors clustered by country are in parentheses.

Table A2: Results with Penn World Tables 6.3

| Estimation Method | Fixed Effects OLS | | Arellano-Bond GMM | | sub-system LIML | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Democracy Index $_{t-1}$ | −0.01 | −0.01 | 0.30 | 0.49 | 0.59 | 0.62 |
| | (0.074) | (0.076) | (0.148) | (0.145) | (0.084) | (0.123) |
| Log-GDP per Capita$_{t-1}$ | −0.011 | 0.80 | −0.21 | 2.45 | −0.16 | 1.55 |
| | (0.072) | (0.426) | (0.172) | (0.914) | (0.073) | (0.488) |
| (Log GDP per capita $_{t-1}$)$^2$ | | −0.05 | | −0.12 | | −0.06 |
| | | (0.026) | | (0.047) | | (0.027) |
| Hansen $J$ Test | | | 0.004 | 0.13 | | |
| AR(2) Test | | | 0.45 | 0.84 | | |
| Observations | 332 | 332 | 332 | 332 | 332 | 332 |
| Countries | 125 | 125 | 125 | 125 | 125 | 125 |

Dependent variable is the Polity IV measure of democracy. In columns (1) and (2) you can find the fixed effects estimates. Columns (3) and (4) present the results when applying the panel GMM estimator proposed by Arellano and Bond (1991). Columns (5) and (6) present the results employing the sub-system LIML estimator instead of first-differenced GMM. The sample period is 1960-2000 at ten-year intervals in all columns. A newer estimation of PPP-adjusted income provided in the new version of Penn World Tables (i.e. PWT 6.3) is used as opposed to the chain index PPP-adjusted income. This income measure is based on a constant price extrapolation of Domestic Absorption from 2005 to earlier and later years. It uses the national growth rate of Domestic Absorption as the basis for the extrapolation. In all cases, standard errors clustered by country are in parentheses.

Table A3: First Stage Regressions

| | (1) | (2) | (3) |
|---|---|---|---|
| Dependent Variable | First differences of Log GDP per capita at $t$ | | |
| Log GDP per capita $_{t-2}$ | −0.021 | | |
| | (0.020) | | |
| Log GDP per capita $_{t-3}$ | | 0.027 | |
| | | (0.025) | |
| Log GDP per capita $_{t-4}$ | | | 0.016 |
| | | | (0.063) |
| Constant | 0.326 | −0.086 | 0.134 |
| | (0.165) | (0.197) | (0.487) |
| P-value of F-test | 0.30 | 0.27 | 0.97 |
| R-squared | 0.004 | 0.006 | 0.001 |
| Observations | 493 | 332 | 166 |

Each column represents an univariate regression of first-differenced log-income per capita on different lags of log-income per capita in levels. Standard errors in parentheses are robust to heteroskedasticity and clustered at the country level.