Convergence and Modernization Revisited*

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August 2012

Abstract

In an 80-country panel since the 1960s, the convergence rate for per capita GDP is around 1.7% per year. This "beta convergence" is conditional on an array of explanatory variables that hold constant countries' long-run characteristics. The introduction of country fixed effects generates a much higher—and, I argue, misleading—convergence rate. In a much longer time frame—28 countries since 1870—estimation with country fixed effects is more appropriate, and the estimated convergence rate is around 2.4% per year. Combining the point estimates from the post-1960s and post-1870 panels suggests that the conditional convergence rate is between 1.7% and 2.4% per year, an interval that contains the "iron-law" rate of 2%. In the post-1960s panel, estimation without country fixed effects supports the modernization hypothesis, in the form of positive effects of per capita GDP and schooling on democracy and maintenance of law and order. The long-term panel with country fixed effects also supports modernization, in the sense of a positive effect of per capita GDP on the Polity indicator for democracy. A measure of dispersion—the standard deviation of the log of per capita GDP across 25 countries—is reasonably stable since 1870. This lack of "sigma convergence" is consistent with the presence of beta convergence. For 34 countries—including China and India—observed since 1896, the dispersion of per capita GDP declines since the late 1970s, especially when the country data are weighted by population. This sigma convergence reflects particularly the incorporation of China and India into the world market economy. For 29 countries since 1919, the levels and trends in cross-country dispersion are similar for consumption and GDP.

*****This paper was prepared for presentation at the Nobel Symposium on Growth and Development, Stockholm, September 3-5, 2012. I appreciate excellent research assistance from David Hsu and comments from Philippe Aghion, Xavier Gabaix, Elhanan Helpman, Jong-Wha Lee, Andrei Shleifer, Ugo Troiano, and Jose Ursúa.

According to the "iron law of convergence," countries eliminate gaps in levels of real per capita GDP at a rate around 2% per year.[1] Convergence at a 2% rate implies that it takes 35 years for half of an initial gap to vanish and 115 years for 90% to disappear. Convergence-rate parameters are important to pin down because they provide guidance on how fast countries like China and India are likely to catch up to richer countries. The convergence rate may also reveal how fast a poor African country could develop or how rapidly North Korea could catch up to the South, and so on.

Empirically, the iron law takes the form of unconditional or absolute convergence in some samples of economies; those that are reasonably homogeneous in terms of long-run or steady-state characteristics. For example, a roughly 2% convergence rate emerged for per capita personal income in a long-term panel of U.S. states in Barro and Sala-i-Martin (1992).[2] This convergence was absolute in the sense of not having to be conditioned on a set of variables that capture differences in long-run positions. The results implied—in accordance with the data—that the U.S. South would not get close in per capita income to the rest of the country for about a century. Applying these results to East versus West Germany suggested that a short time frame for convergence was not a realistic expectation.[3] And, looking forward to the potential

[1]I first heard this term applied to my empirical findings on economic growth by Rudi Dornbusch. However, Larry Summers said that Rudi got the term from him. In any event, the term is reminiscent of the "iron law of wages." According to Wikipedia, this phrase came from Lassalle, but Marx and Engels argued that Lassalle got the idea from Malthus's theory of population and the terminology from Goethe.

[2]Baumol (1986, Figure 2) reported unconditional convergence from 1870 to 1979 for 16 countries (all subsequently OECD members), using data from Maddison (1982). However, De Long (1988) showed that Baumol's results depended on a sample-selection issue, whereby only countries that were rich toward the end of the sample (1979) were considered. Unconditional convergence did not hold for an expanded sample of 22 countries that were selected based on per capita income in 1870 (De Long [1988, Figure 2]). This sample-selection criticism of Baumol's (1986) findings was presented earlier by Romer (1986, pp. 1012-1013). Rodrik (2012) finds unconditional convergence in labor productivity across manufacturing industries for recent decades in 118 countries.

[3]Barro (2002) found that the predicted slow convergence between East and West Germany accorded with regional data on GDP per worker through the late 1990s. However, wage rates converged faster because of the German government's transfer and subsidy policies.

reunification of North and South Korea, the iron law presents a pessimistic outlook on how rapidly the large gap in per capita product could be eliminated.

The 2% convergence rate holds in contexts of conditional convergence for heterogeneous collections of economies that differ substantially in terms of long-run properties. This convergence is conditional in the sense of holding only with an allowance for differences in constant or slowly varying cross-economy characteristics, such as saving rates or fertility rates or quantity of human capital or institutional quality or colonial history or geographical features. For example, a convergence rate around 2% appeared in a cross section of 98 countries in Barro and Sala-i-Martin (1992, Table 3), after conditioning on an array of variables that differed by country.[4] Because of the conditioning variables, these results were more pessimistic than the iron-law convergence rate would suggest. Poor places—for example, many sub-Saharan African countries or North Korea or Burma or Bolivia or Venezuela—might not converge at all if key underlying variables, such as the quality of human capital and institutions, were not improved.

The present study uses updated cross-country panels to reexamine the iron law of convergence. One data set comprises a large number of countries with observations for many variables since the 1960s. Another data set exploits recent advances in long-term national-accounts information. These data cover over a century but apply to fewer countries and variables. In both contexts, the distinction between absolute and conditional convergence is important. And, within the context of conditional convergence, a key technical issue is whether the cross-country regressions include country fixed effects.

[4] In earlier work, Barro (1991) reported conditional convergence for the cross section of 98 countries but did not express the results in terms of a convergence rate.

Many analyses of economic growth stress effects from the quality of institutions, gauged particularly by maintenance of the rule of law and democracy.[5] A prominent feature of this analysis is two-way causation between economic development and institutional quality. Specifically, according to the "modernization hypothesis," economic development spurs the introduction and maintenance of higher quality institutions, including well-functioning representative democracy.[6] The validity of the modernization thesis is important for its own sake—particularly for understanding how democracy evolves—as well as for assessing institutional determinants of economic growth.

I use the two updated panel data sets to reassess the empirical status of the modernization hypothesis. From an econometric standpoint, the analysis of modernization turns out to have significant parallels with the study of convergence. Both types of results are sensitive to the treatment of country fixed effects.

I. Thoughts on Country Fixed Effects

Cross-country empirical findings concerning convergence and modernization are sensitive to the seemingly mundane issue of whether the panel regressions include country fixed effects. The incorporation of these fixed effects into cross-country panel regressions has become almost routine.[7] However, the merits of including these fixed effects are not straightforward, as they involve a tradeoff between two forces, highlighted by Nerlove (2000).

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[5] Knack and Keefer (1995) and Mauro (1995) studied growth effects from rule of law and corruption. Przeworski and Limongi (1993) and Barro (1997, Ch. 2) assessed growth effects from democracy. King and Levine (1993) examined effects of financial institutions on economic growth. Glaeser, La Porta, Lopez-de-Silanes, and Shleifer (2004) argued that institutions should be measured by basic legal constraints on the government, rather than political outcomes, which include official corruption and risk of expropriation.

[6] Contributions to the modernization literature include Aristotle (1932), Lipset (1959), Dahl (1991), and Huntington (1991). Marx (1913) extended the modernization idea to a predicted collapse of organized religion under capitalism.

[7] This approach applied to economic growth seems to have begun with Knight, Loayza, and Vllanueva (1993); Islam (1995); and Caselli, Esquivel, and Lefort (1996). Acemoglu, Johnson, Robinson, and Yared (2005, 2008) advocate the use of country fixed effects in studies of the modernization hypothesis.

To fix ideas, consider cross-country panel regressions for the growth rate of per capita GDP. Country fixed effects are attractive as a way to allow for unobserved, persistent country characteristics that influence long-run per capita GDP and are also correlated with observed per capita GDP. That is, rich countries tend to have prospered because they possess persistently favorable characteristics that lead to high steady-state per capita GDP. From this omitted-variables perspective, the exclusion of country fixed effects tends to bias upward the estimated effect of lagged GDP on current GDP and, thereby, bias downward the estimated convergence rate. One example of this effect is the tendency to estimate an absolute convergence rate near zero in a panel of heterogeneous countries. However, this bias may be small if the framework without country fixed effects includes a sufficiently rich set of explanatory variables so that little remains of omitted variables that are conditionally correlated with per capita GDP.

The second force involves the Hurwicz (1950)-type bias in the estimated coefficient of a lagged dependent variable. In samples that are small in the time dimension, Nickell (1981), Arellano and Bond (1991), Kiviet (1995), and Nerlove (2000), among others, show that this force biases downward the fixed-effects estimator (based on least squares with dummy variables) for the coefficient of the lagged dependent variable.[8] Although this bias vanishes as the sample becomes large in the time dimension, in the usual panel context with moderate time frames, the Hurwicz bias tends to be substantial. (As I discuss later, the effective time dimension cannot be expanded by observing the data more frequently; for example, annually, rather than every five or ten years.) On this ground, the estimated convergence rate tends to be overestimated (because the persistence in the level of the dependent variable is underestimated), thereby offsetting the omitted-variables bias.

[8]The Hurwicz bias arises because the realized error terms appear in the sample mean of the lagged dependent variable. Moreover, this effect co-varies negatively with the terms involving departures of the dependent variable from its sample mean.

When country fixed effects are included and the time dimension of the sample is moderate—say 20 or 40 years—the panel effectively comprises multiple cases of short time series. Since the regression for each country features a similar Hurwicz bias, this bias tends to apply also to the overall sample. In contrast, if country fixed effects are excluded, the observations are effectively stacked in the time and country dimensions. That is, with 80 countries and 10 time periods (as in the subsequent analysis of data since the 1960s), the 800 observations amount to a large sample in the relevant time dimension, and one would anticipate that the Hurwicz bias would be small. In other words, this bias may arise mostly because of the inclusion of country fixed effects. These conjectures are confirmed by Monte Carlo studies described in the appendix.

Inclusion of country fixed effects also affects the estimated coefficients of explanatory variables—X variables—other than lagged dependent variables. Coefficients on country variables that are constant (such as geographical features and colonial history) cannot be estimated at all, and variables that have little within-country time variation cannot be estimated with precision. In effect, the inclusion of country fixed effects throws out much of the information in isolating the effects of X variables on growth rates.

Problems in estimating coefficients of X variables in a fixed-effects context apply to the recent debate about the modernization hypothesis in Glaeser, la Porta, Lopez-de-Silanes, and Shleifer (2004); Glaeser, Ponzetto, and Shleifer (2007); and Acemoglu, Johnson, Robinson, and Yared (2005, 2008). The failure in the Acemoglu, et al. studies to find statistically significant effects on democracy from per capita GDP and education depends, in their main analysis, on the inclusion of country fixed effects. This result is not surprising because, with country fixed effects, it is challenging to estimate statistically significant coefficients on X variables that do not have a lot of independent variation over time within countries. In contrast, without country fixed effects, as in the Glaeser, et al. studies and Barro (1999), the typically substantial cross-sectional variation in the X variables makes it easier to isolate statistically significant effects.

The perspective changes in the context of panel data observed for over a century. In this setting, the econometric problems posed by the inclusion of country fixed effects should be much less serious. In particular, the Hurwicz bias is smaller in this context, as illustrated by the Monte Carlo studies discussed in the appendix.

II. The Framework of Conditional Convergence

This section summarizes well-known implications of the neoclassical growth model and its extensions for empirical analyses of conditional convergence. The model features a production function,

$$Y = A \cdot F(K, L), \tag{1}$$

where $F(\cdot)$ satisfies the usual neoclassical properties, including constant returns to scale in capital, K, and labor, L. Output per worker, $y \equiv Y/L$, depends on capital per worker, $k \equiv K/L$:

$$y = f(k). \tag{2}$$

The economy is closed, so that saving and investment coincide. In the baseline model, there is no government sector, but extensions allow for government purchases and taxes.

The labor force, measured as work-hours per year, is related in a fixed way to population, which grows at rate n. The labor force is fully employed and, therefore, corresponds to labor input, L, in equation (1). Given these assumptions, the quantities y and k, measured per worker in equation (2), can be interpreted as quantities per capita. In the baseline model, n is constant over time within an economy but may differ across economies.

In the baseline model, the gross saving rate (which equals the ratio of gross investment to GDP) is a constant, s (as in Solow [1956] and Swan [1956]), which may differ across economies. The first major extension of the Solow-Swan framework was to endogenize the saving rate. In a setting based on Ramsey (1928), Cass (1965), and Koopmans (1965), the representative household determines the optimal saving rate at each point in time in the context of an iso-elastic, time-additive utility function. The equilibrium value of s then varies over time within economies along a transition path to the steady state. Barro and Sala-i-Martin (2004, Ch. 2) show that, if $F(\cdot)$ is Cobb-Douglas, the transitional behavior of the saving rate is monotonic—always rising toward its steady-state value, always falling toward its steady-state value, or always constant at its steady-state value. (This result shows that the Solow-Swan setting is a special case of the Ramsey-Cass-Koopmans model.) With rising (falling) s, the convergence rate is slower (faster) than in the model with fixed s. With alternative utility functions, the transition may not be monotonic; for example, the transition path for s may be hump-shaped. Across economies, differences in preference and other parameters can shift the path of s when considered in relation to GDP per capita, y. An economy with higher s at a given y tends to grow faster.

Just as the Ramsey-Cass-Koopmans analysis endogenized the fixed and exogenous saving rate of Solow-Swan, the model can be extended along the lines of Malthus (1798) to endogenize the population growth rate, n.[9] Barro and Sala-i-Martin (2004, section 9.2) allow for choices over time of the fertility rate for a given mortality rate. In the case where child-rearing costs rise linearly with k (because, as in Becker [1991], child-rearing is intensive in parental time, which is valued in accordance with the wage rate), n falls montonically during the

[9]Malthus had a model of endogenous population growth, but he tended to get the signs wrong. Specifically, he predicted a positive effect of real per capita GDP on fertility, whereas, in the modern data, the relation tends to go in the opposite direction. See Manuelli and Seshadri (2009).

transition to the steady state. This pattern reduces the convergence rate compared to the setting in which n is fixed.[10] However, the allowance for goods costs of child rearing can generate a non-monotonic pattern in which fertility first rises and later falls as per capita GDP, y, increases. Across economies, differences in preference and other parameters (including costs related to the rearing of children) can shift the path of n when considered in relation to y. An economy with lower n at a given y tends to grow faster.

In the baseline model, the productivity factor, A, can differ across economies. This factor may rise over time due to exogenous technical progress. The usual steady-state properties depend on technical progress taking the labor-augmenting form. In this setting, effective labor input, $\hat{L}$, replaces L in equation (1), $\hat{L}$ is the multiple e^{xt} of L, and $x \geq 0$ is the rate of technological progress. In this context, the results for the growth-rate transition go through in terms of quantities per effective labor; that is, as $\hat{y} \equiv Y/\hat{L}$ and $\hat{k} \equiv K/\hat{L}$. In the Cobb-Douglas case, labor-augmenting technical progress is equivalent to exogenous growth in A in equation (1). In extensions, differences in A (or in the path of A) within or across economies can reflect the quality of institutions, including maintenance of property rights and the efficiency of the tax system. That is, the economy's response to weak property rights or high marginal income-tax rates is essentially equivalent to its response to a reduction in productivity, A.

The research on endogenous growth due to endogenous technical change began with Romer (1987, 1990), Aghion and Howitt (1992), and Grossman and Helpman (1991, Chs. 3 and 4).[11] In these models, technological advances derive from purposeful and successful R&D

[10]In this model, a rise in the mortality rate would raise the fertility rate one-to-one, except that this response implies a larger flow of child-rearing costs. Because these higher costs work in the same way as higher depreciation on capital, a higher mortality rate tends to lower the growth rate of GDP at a given capital intensity.

[11]Other research on endogenous growth—exemplified by Romer (1986), Lucas (1988), and Rebelo (1991)—built on earlier work by Arrow (1962) and Uzawa (1965) and did not provide a theory of technical change. In these models, growth may go on indefinitely because the returns to investment in a broad class of capital goods, which include

activity. In "varieties" models (Romer [1987, 1990] and Grossman and Helpman [1991, Ch. 3]), discoveries of new types of intermediate inputs raise productivity by (metaphorically) expanding the number of inputs employed in production. In "quality-ladders" models (Aghion and Howitt [1992] and Grossman and Helpman [1991, Ch. 4]), innovations raise the quality of intermediate inputs within sectors (or, equivalently, improve the efficiency of production processes). From the perspective of the Solow-Swan model, the theories of technological progress effectively endogenize the growth rate of the productivity factor, A. Endogenous growth theory can be viewed, accordingly, as another extension of the Solow-Swan model to endogenize a key parameter; in this case, A, rather than s or n.

Endogenous growth theory has not played a major role in empirical studies of the determinants of economic growth across countries. The main empirical applications of the theory have involved effects of R&D outlays. This research builds on the (pre-endogenous-growth) approach described by Griliches (1973), which starts by computing TFP growth rates as Solow residuals in a growth-accounting framework.[12] The TFP growth rates can then be related econometrically to measures of R&D expenditures. This methodology was applied to U.S. firms and industries by Griliches and Lichtenberg (1984) and Griliches (1988). Coe and Helpman (1995) applied this framework to aggregate data for OECD countries and reported large positive effects of R&D outlays on economic growth.[13] However, a problem with this approach is that—in the absence of good instruments—a positive relation between R&D spending and economic

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human capital, may not diminish as economies develop. (This idea goes back to Knight [1944]). Spillovers of knowledge across producers and external benefits from human capital may be parts of the process because they help to avoid the tendency for diminishing returns to capital. Models of this type are sometimes described as "AK models," because the growth dynamics looks like that in a simple framework with constant returns to capital.

[12]This idea began with Solow (1957) and was extended to allow for changing quality of an array of inputs by Jorgenson and Griliches (1967). See also Barro and Sala-i-Martin (2004, section 10.4).

[13]More recently, Aghion, Bloom, Blundell, Griffith, and Howitt (2005) have used a model related to endogenous-growth theory as the framework for an empirical study of the relation between competition and innovation across firms.

growth can reflect reverse causation from growth opportunities to R&D, rather than effects of R&D and technological progress on growth.

One reason that endogenous growth theory has not played a major role in cross-country growth analysis is that the theory may apply mostly to the worldwide average growth rate. This perspective would apply if international trade, foreign investment, and the flow of ideas lead to rapid diffusion of technology across countries. The speed of diffusion of technology was assessed theoretically in Nelson and Phelps (1966), who stressed the role of human capital, and in subsequent models summarized in Barro and Sala-i-Martin (2004, Ch. 8). Caselli and Coleman (2001) found empirically for developing countries that imports of computers and other high-tech equipment—viewed as a proxy for technology absorption—were spurred by increased imports from technologically advanced countries. Technological diffusion was also higher when the home country had higher levels of school attainment at secondary and higher levels and better institutional quality.

A further extension of the neoclassical growth model, introduced by Mankiw, Romer, and Weil (1992), distinguishes human from physical capital. One way that this extension affects the dynamics of economic growth involves the greater difficulty in adjusting human capital, H, compared to physical capital, K. In this case, the growth rate of y tends to be a higher at a given y if the ratio H/K is higher. For an economy below its steady-state position, a higher H/K (perhaps generated from a war that destroyed much more physical than human capital) means that subsequent growth focuses on K, which is easier than H to expand rapidly.[14]

[14]Analogously, for an economy above its steady-state position, a higher H/K means that the reduction in H takes place slowly, thereby making the growth rate of y higher (perhaps less negative) than it would be otherwise.

Suppose that L is the number of workers and h is human capital per worker, so that quality-adjusted labor input is H=hL. The input H then replaces L in equation (1). Assume, further, that h relates to years of schooling along Mincerian lines, so that

$$h = e^{\lambda S},$$

where S is (average) years of schooling and λ is the rate of return on schooling (if the cost of schooling is the income foregone by not employing human capital in production). If the production function in equation (1) is Cobb-Douglas with exponents a on K and b on H, we can derive:

$$\text{(3)} \qquad \log(H/K) = (1/a)\cdot\log(A) - (1/a)\cdot\log(y) + \lambda\cdot(1 + b/a)\cdot S.$$

Therefore, for given A, a higher S signals a higher H/K at a given y, which predicts higher economic growth. However, for a given H/K, a higher S would signal a lower A at a given y, which predicts lower economic growth. In the empirical analysis, I assume that, although S and y are observable, A (corresponding to total factor productivity) cannot be measured (because K cannot be measured accurately). In this case, the overall effect of S on economic growth, for given y, is ambiguous. In the empirical analysis, the estimated effect on growth from the level of S turns out to be small and typically statistically insignificantly different from zero.

III. Cross-Country Growth Regressions

The preceding section implies that the growth rate of real per capita GDP, Dy_{it}, for country i at date t can be written as

$$\text{(4)} \qquad Dy_{it} = \Phi(\underset{(-)}{y_{it}}, \underset{(+)}{s_{it}}, \underset{(-)}{n_{it}}, \underset{(+)}{A_{it}}, \ldots),$$

where the negative sign under y_{it} reflects convergence, conditional on the other variables. These other influences include variables related to the saving rate, s_{it}, the population growth rate, n_{it}, and the level of productivity, A_{it}. In the main analysis, I estimate equation (4) using per capita growth rates of GDP averaged over 5- or 10-year periods. Sometimes the analysis includes constant terms that are specific to countries (country fixed effects). The estimation always includes constants for each time period (time effects)—therefore, the analysis does not attempt to explain variations over time of world average growth rates.

One well-known problem with cross-country growth regressions is endogeneity of some of the X variables. For example, it is unclear whether good institutions cause economic growth or are a reaction to rising living standards—or, perhaps, that GDP and institutional quality are responses to common influences. Previous studies have proposed instruments to deal with this problem. Examples are gravity variables including country size and trade restrictions that influence international trade (Lee [1993]); ethnolinguistic fractionalization (Mauro [1995]); population density and settler mortality at the time of colonial settlement (Engerman and Sokoloff [1997], Acemoglu, Johnson, and Robinson [2001, 2002]); the form of legal origins (La Porta, Lopez-de-Silanes, Shleifer, and Vishny [1998]); absolute degrees latitude and primary language (Hall and Jones [1999]); the presence of state religion (Barro and McCleary [2003]); and physical characteristics of islands (Feyrer and Sacerdote [2009]). One problem is that the proposed instruments typically do not vary over time in the sample within countries and, therefore, do not help when country fixed effects are included. A more basic issue, if one allows for the plausibly multi-dimensional set of X variables that matters for economic growth, is that there are never enough convincing instruments to allow for full identification.

The present empirical analysis uses lagged values of the X variables as instruments. For example, in considering the growth rate from 2000 to 2005, the average of the investment ratio from 2000 to 2005 enters into the regression equation but the investment ratio for 2000 is on the instrument list. This use of lagged X values as instruments helps to deal with endogeneity in some contexts (and also to alleviate problems of temporary measurement error). However, this approach is not fully satisfactory because of the strong serial correlation in some X variables.

In the main, the estimated effects on growth rates in the form of equation (4) reflect influences that are predictable with a 5- or 10-year lag—corresponding to the use of 5- or 10-year lagged values of the X variables as instruments for the right-hand-side variables. Contemporaneous growth-rate influences can also arise, corresponding, for example, to shifts in the productivity factor, A_{it}, over the 5- or 10-year periods over which growth rates are measured. In practice, the only variable used of this type is the contemporaneous growth rate of country i's terms of trade, which is taken as an exogenous influence on country i.[15]

Another well-known issue is the robustness of the results with respect to which X variables are included and in what functional form. Sala-i-Martin, Doppelhoffer, and Miller (2004) deal with this problem using a Bayesian model-averaging approach. This technique effectively weights each possible specification (among millions of possibilities) by the fits to the dependent variable (in their case, the growth rate of per capita GDP from 1960 to 1996). Sala-i-Martin, et al. apply this method to 67 X variables that have been proposed in the empirical growth literature, using data for 88 countries (see Barro and Sala-i-Martin [2004, Table 12.6]). The conclusion is that only 5 of the variables have posterior inclusion probabilities above 0.5 and

[15]The growth rate of the terms of trade is interacted with the openness ratio, measured as exports plus imports relative to GDP.

18 have probabilities above 0.1. However, with 67 variables considered, many are conceptually similar, so that low inclusion probabilities are not surprising.

My view is that one has to accept the idea that pinpointing precisely which X variables matter for growth is impossible. However, what is feasible is interpreting the results in terms of broad influences that matter for growth; for example, quality of institutions, openness to markets, and so on. In addition, one can interpret results on conditional convergence—gauged empirically by the estimated coefficient on the lag value of log(y)—holding fixed an array of X variables. These results on conditional convergence tend not to be highly sensitive to exactly which X variables are included.

IV. Empirical Results

Table 1 contains empirical results for the cross-country panel of 80 countries. Table 2 lists the countries in the sample. The dependent variable in the main specification is each country's growth rate of per capita GDP over 10 5-year intervals from 1960-65 to 2005-09. (The last period is shortened because of data availability.) Note that the growth rates are expressed per year, not per five-year period.

Column 1 of Table 1 includes as right-hand-side variables only time effects and the 5-year lag of the log of per capita GDP. The estimated coefficient on the lag is positive (indicating divergence rather than convergence) but statistically insignificantly different from zero. This result reproduces the typical pattern whereby absolute convergence of per capita GDP does not apply to a heterogeneous collection of countries. From the standpoint of equation (4), the interpretation is that the 5-year lag of the log of per capita GDP is positively correlated with determinants of the steady-state position that raise the long-run level of per capita GDP. This omitted-variables effect offsets the convergence force and leads, thereby, to an estimated coefficient on the lagged log of per capita GDP that is close to zero. The Monte Carlo analysis in the appendix (Table A1, line 15) reproduces this kind of result.

Column 2 adds country fixed effects, which are jointly highly statistically significant. The estimated coefficient of the lagged log of per capita GDP is now significantly negative, -0.0326 (s.e.=0.0048).[16] This result suggests conditional convergence (conditional on individual constants for each country) at a rate of 3.3% per year. A possible interpretation is that each country's fixed effect proxies for the influences of the various determinants of long-run per capita GDP that appear in equation (4)—at least to the extent that these determinants do not vary greatly over time within countries. Therefore, the estimated coefficient on the lagged log of per capita GDP now picks up the predicted convergence force indicated by the negative sign in equation (4). However, as noted before, there is also a tendency to overestimate convergence in the presence of country fixed effects because of the Hurwicz bias in the estimated coefficient of a lagged dependent variable. The Monte Carlo analysis (Table A1, lines 6-8) shows how these overestimates can be substantial.

Column 3 includes, instead of country fixed effects, an array of time-varying X variables for each country.[17] These variables are intended to proxy for the various growth determinants, aside from lagged per capita GDP, that enter into equation (4). Unlike in column 1, the estimated coefficient of the lagged log of per capita GDP is significantly negative, -0.0171 (s.e.=0.0022), and indicates convergence near the iron-law rate of 2 percent per year. This convergence is conditional in the sense of holding for given values of the X variables.

[16]In this and the subsequent regressions, one can think equivalently of the dependent variable as the log of the level of per capita GDP (observed for 1965, 1970, ..., 2009), with the five-year lag of this variable (for 1960, 1965, …, 2005) included on the right-hand side. In this version of the regression from Table 1, column 2, the coefficient on the lagged dependent variable is 0.837 (which equals 1 minus 5 times the estimated convergence coefficient of 0.0326 per year).

[17]I considered some variables that were constant over time within countries—the absolute value of degrees latitude, land-locked status, and aspects of colonial history and legal origins—but these turned out to be unimportant.

The estimated coefficients of the X variables in column 3 can be viewed mostly as effects on long-run or steady-state positions for each country. For example, with respect to institutional quality, the results imply that a country's long-run economic position is enhanced by better maintenance of law and order (and the rule of law).[18] Greater democracy (gauged by the level and square of the Freedom House indicator for political rights[19]) tends initially to stimulate growth but later retards growth. The break point between marginal effects being positive or negative occurs roughly at the half-way mark between full dictatorship (value zero) and full representative democracy (value one). Analogous effects of democracy on economic growth were reported and interpreted in Barro (1997, Ch. 2).

Other findings are that countries' long-run positions are enhanced by a lower mortality rate (gauged by the reciprocal of life expectancy at birth), a lower fertility rate, greater openness to international trade, and higher female relative to male school attainment. The schooling effect does not relate to the overall level of human capital in the sense of total years of attainment. In fact, the estimated effect from a general increase in attainment—where average years of female and male schooling change by equal amounts—differs insignificantly from zero, a result that is consistent with the conceptual analysis of human capital presented earlier. A reasonable interpretation is that an expansion in female relative to male attainment—typically implying a decline in the gap between male and female average years of schooling—signals an improvement more generally in political and social arrangements that support economic growth.

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[18]The law-and-order variable (previously called rule-of-law) comes from *International Country Risk Guide*, which is produced by Political Risk Services. These data were first used in academic research by Knack and Keefer (1995). The law-and-order indicator starts in 1982 or later. The panel regressions in Table 1, column 3, use each country's first value available for this variable for the periods that start in 1980-85 or earlier. If the sample covers only the five periods that begin with 1985-90, the results are similar to those in column 3. For example, the coefficient on the lagged log of per capita GDP is -0.0209 (s.e.=0.0031) and that on the law-and-order variable is 0.0179 (s.e.=0.0088). An alternative to the ICRG data is the information on perceived quality of governance assembled by the World Bank (available at www.govindicators.org). However, these data have the serious shortcoming of being available only since the late 1990s.

[19]Analogous data from Bollen (1980) were used for 1960 and 1965.

Several explanatory variables have estimated coefficients that differ insignificantly from zero: the investment ratio (which would correspond to the saving rate in a closed economy), the government-consumption ratio, and the inflation rate. I discuss more findings below related to the investment ratio and the inflation rate. Finally, the results reveal a significantly positive contemporaneous impact on growth from improvements in a country's terms of trade.

As discussed before, the Hurwicz bias in the estimated coefficient of a lagged dependent variable is likely to be small in the context of column 3, where country fixed effects are absent. The estimated coefficient of the lagged log of per capita GDP would tend to be underestimated in magnitude if there are still important omitted determinants of long-run per capita GDP that are conditionally correlated with per capita GDP. However, given the substantial list of growth determinants included, this omitted-variables bias may be small (unlike in column 1). Therefore, it is possible that OLS without country fixed effects can deliver accurate estimates of the convergence rate, as confirmed in the appendix by Monte Carlo analysis (Table A2, lines 16 and 17).

Column 4 allows for country fixed effects along with the X variables. The fixed effects are still jointly highly statistically significant. The estimated coefficient of the lagged log of per capita GDP, -0.0445 (s.e.=0.0051), is negative, statistically significant, and larger in magnitude than in columns 2 or 3. Taken at face value, the indicated conditional convergence rate is 4.5% per year. However, as discussed before, the convergence rate tends to be seriously overestimated because of the Hurwicz bias (as confirmed by the Monte Carlo studies considered in the appendix; see Table A2, lines 7-10).

Many of the X variables that were statistically significant in column 2 are no longer statistically significant in column 4—because only within-country variation is now used to identify these coefficients. As an example, although the indicator for the maintenance of law and order was a significantly positive determinant of growth in column 3, this variable no longer has significant explanatory power in column 4. The likely explanation is not that institutional quality is unimportant for growth but, rather, that there is insufficient within-country variation in this quality (as measured) to isolate a statistically significant effect. This explanation is particularly plausible when one considers the many X variables included in the panel regression and the likely measurement error in the indicator of institutional quality.

Table 3 contains additional panel regressions for growth rates. Column 1 parallels Table 1, column 3, but uses OLS,[20] rather than instrumental variables (with lagged values of the X variables used as instruments). The results are similar in most respects, except that, under OLS, the estimated coefficient of the investment ratio is significantly positive, whereas that on the inflation rate is significantly negative. These last results likely reflect the joint short-run determination of GDP with investment and inflation, whereby the typical pattern is for the investment ratio to be procyclical and the inflation rate to be countercyclical. Hence, the OLS results on these estimated coefficients cannot be reliably interpreted as isolating causation from the investment ratio or the inflation rate to GDP growth.

Table 3, column 2, uses 10-year averages for growth rates, with corresponding adjustments in the definitions of the X variables. The results are similar to those for the 5-year case in Table 1, column 3, except that the standard errors for the estimated coefficients are higher in the 10-year specification; that is, some information is lost by averaging over 10 years, rather than 5. The estimated convergence rates per year are similar: -0.0163 (s.e.=0.0023) for the 10-

[20]Table 3, column 1 includes all the X variables on the instrument list but departs from OLS by allowing for correlation of the error term over time within countries. With full OLS, the estimated coefficients are the same but the standard errors of the estimated coefficients are somewhat smaller except for the one on the terms-of-trade variable.

year case, versus -0.0171 (s.e.=0.0022) for the 5-year. When country fixed effects are included, the results for 10-year periods (not shown) are also similar to those with 5-year periods (Table 1, column 4). The same general conclusions hold with annual observations on GDP growth rates. For the convergence rate, in a system without country fixed effects, the estimated convergence coefficient is -0.0176 (s.e.=0.0024), similar to those in Table 1, column 3, and Table 3, column 2.[21]

Overall, the results show that changing the time dimension of the sample by observing the data more or less frequently—shifting, say, from 10-year to 5-year to 1-year periods—has minor implications, particularly for the convergence rate. This result in the context of country fixed effects accords with the formula for the Hurwicz bias derived for a simple case in Nickell (1981, p. 1422).[22]

Table 3, column 3, goes further in the exploration of the implications of country fixed effects by allowing for two separate sets of country dummy variables—one applying to the first

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[21]The annual system is problematic because some of the data (school attainment, changes in the terms of trade, and the inflation rate) were constructed only over 5-year intervals. Moreover, other data—on life expectancy and the fertility rate—are provided at an annual frequency but are not really annual variables. In any event, the annual results (using interpolated values of the variables available at 5-year intervals) show much poorer fits than the systems at 5-year and 10-year intervals. The R-squared value for the annual case is 0.137, and the standard error of the regression is 0.049. (This system for 80 countries has 3478 total observations.)

[22]Nickell (1981, p. 1422) provides a formula for the bias in the least-squares-with-dummy-variables estimate for the coefficient of a lagged dependent variable when there are cross-sectional fixed effects, no X variables, the number of cross sections is large, and the initial values of the dependent variable are treated as given. It can be shown that what matters for the bias in his formula is the overall length of the sample, not the number of periods over which the data are observed. Suppose that β is the convergence rate per year. The coefficient of the lagged dependent variable, ρ in Nickell's formula, is given by $\rho=e^{-\beta\tau}$, where τ is the period length in years. Let T be the sample length in years (differing from Nickell's notation). Nickell's formula implies that the proportionate bias in the estimated β depends (as an approximation based on $\beta\tau$ being much less than one) only on the product βT. The formula is, if $\beta>0$:

$$[(\widehat{\beta} - \beta)/\beta] \approx \frac{2\cdot(e^{-\beta T}-1+\beta T)}{\beta^2 T^2-2\cdot(e^{-\beta T}-1+\beta T)} > 0.$$

Therefore, a change in the period length, τ, with T held fixed, does not affect the calculated bias, and the empirical findings (and Monte Carlo results) on convergence with fixed effects included are consistent with this result. Quantitatively, if β=0.02 per year, Nickell's formula generates an upward bias of 0.070 when T=40 years and 0.018 when T=139 years (corresponding to the long-term sample used later). Hence, although the bias approaches zero as T approaches infinity, the bias can be large in samples of realistic length. However, these results are only suggestive, because the Nickell formula depends on a number of unrealistic assumptions, particularly about the X variables. Detailed Monte Carlo results are in the appendix.

five periods (1960-65, …, 1980-85) and the other to the second five (1985-90, …, 2005-09). The break in the set of country fixed effects from the first half to the second half of the sample is jointly highly statistically significant, as are the fixed effects overall. Not surprisingly, the standard errors of the estimated coefficients of all the X variables are higher than those in Table 1, column 4. That is, the richer structure of country fixed effects makes it even more problematic to use the within-country variation to estimate the effects of X variables. For present purposes, the most interesting result is that the estimated coefficient on the lagged log of per capita GDP, -0.0783 (s.e.=0.0094), becomes even larger in magnitude (compared to Table 1, column 4), now indicating convergence at 8% per year. A reasonable interpretation is that the extra set of country fixed effects effectively cuts the time dimension of the sample by half (from 40 plus years to 20 plus years) and, thereby, intensifies the Hurwicz bias (see n. 22 and the appendix on Monte Carlo results). Hence, these findings provide a further warning that country fixed effects can cause serious upward bias in estimated convergence rates.

Finally, Table 3, column 4, replaces the Freedom House measure of political rights by the Polity measure of democracy/autocracy. The results are similar to those based on the Freedom House indicator (Table 1, column 3), although the estimated coefficients on the Polity variable and its square are not individually statistically significant. These variables are jointly statistically significant (p-value=0.045), as was also true for the Freedom House variables (p-value=0.004).

My inference from the cross-country data starting in the 1960s—covering 40-plus years for most countries—is that the most reliable estimates of convergence rates come from systems that exclude fixed effects but include an array of X variables to mitigate the consequences of omitted variables. That is, I would emphasize the results in Table 1, column 3. However, as I discuss later, the fixed-effects results seem more convincing in systems for the more limited set of countries with data over a century or more.

V. Modernization

The modernization thesis applied to democracy is that economic development—gauged particularly by per capita GDP and education—promotes democratic institutions. This idea was emphasized by Lipset (1959), who credits the concept to Aristotle (1932).[23] Glaeser, Ponzetto, and Shleifer (2007) provide a theoretical rationale for the effect of education on democracy through the channel of higher education motivating greater participation in political and other social activities. The Aristotle-Lipset hypothesis can be extended beyond democracy to apply to measures of institutional quality, including the indicator for maintenance of law and order (and the rule of law).

Barro (1999) provided empirical confirmation of the Aristotle-Lipset hypothesis in a cross-country panel, with stress on the Freedom House measure of political rights. Additional supporting analysis along these lines appears in Glaeser, la Porta, Lopez-de-Silanes, and Shleifer (2004) and Glaeser, Ponzetto, and Shleifer (2007). However, these results have been challenged by Acemoglu, Johnson, Robinson, and Yared (2005, 2008), who argue that education and per capita GDP do not have statistically significant causal influences on democracy. As in estimating convergence rates for economic growth, the essence of much of this recent empirical debate about modernization turns on whether one includes country fixed effects in the empirical analysis of data on democracy and other measures of institutional quality.

[23]According to Lipset (1959, p. 75): "From Aristotle down to the present, men have argued that only in a wealthy society in which relatively few citizens lived in real poverty could a situation exist in which the mass of the population could intelligently participate in politics and could develop the self-restraint necessary to avoid succumbing to the appeals of irresponsible demagogues."

Table 4 contains panel regressions in which the dependent variables are indicators of institutional quality. Columns 1 and 2 use the law-and-order measure from *International Country Risk Guide* (previous described by ICRG as maintenance of the rule of law). This dependent variable is observed for 1990, 1995, …, 2009 for 124 countries. Columns 3 and 4 use the Freedom House measure of political rights, observed in 1975, 1980, …, 2009 for 141 countries. Columns 5 and 6 use the Polity measure of democracy/autocracy, observed in 1965, …, 2009 for 133 countries.

In Table 4, the X variables are limited to those stressed in the recent literature on modernization—per capita GDP and measures of years of school attainment.[24] The analysis distinguishes average years of schooling by females and males and, in some cases, by primary versus upper (secondary and higher). The regressions also include five-year lagged values of the dependent variable. These forms are analogous to those used before for economic growth (which included lagged levels of the log of per capita GDP on the right-hand side). The method of estimation, using lags of the X variables as instruments, is analogous to that in Table 1.

Table 4, column 1, applies to the law-and-order indicator. The projected persistence over five years is given by the estimated coefficient of the lagged dependent variable, 0.708 (s.e.=0.026). This estimated coefficient differs significantly from one (which would indicate no adjustment over five years toward a target value) or zero (which would correspond to full adjustment over five year). The estimated coefficient implies that it takes 10 years to eliminate half an initial gap between the law-and-order indicator and its long-run target and 32 years to eliminate 90% of an initial gap.

[24]The schooling data come from the recent update of the Barro-Lee data set, as described in Barro and Lee (2012). For the data, see "Educational Attainment Data" at www.rbarro.com/data-sets.

With respect to X variables, the estimated coefficient on the log of per capita GDP is significantly positive, 0.0248 (s.e.=0.0066). The effect from overall years of schooling is also significantly positive (p-value=0.016),[25] but the results show a positive and statistically significant coefficient for male schooling (0.0164, s.e.=0.0068) and a negative but statistically insignificant coefficient for female schooling (-0.0093, s.e.=0.0062). The three indicators of economic development—the log of per capita GDP and average years of female and male schooling—are jointly highly statistically significant (p-value=0.000). Hence, the system that excludes country fixed effects provides strong evidence of modernization with regard to effects from GDP and schooling on institutions that maintain law and order (and the rule of law).

Table 4, column 2, shows the impact from allowing for country fixed effects, which are jointly highly statistically significant. The results change compared with column 1 in ways familiar from the analysis of economic growth: the estimated coefficient of the lagged dependent variable falls substantially (to 0.332, s.e.=0.045), and the standard errors of the estimated coefficients of the X variables rise sharply. The latter effect is strong enough so that none of the three X variables are now individually statistically significant. Moreover, these three variables become jointly insignificant (p-value=0.62). Hence, with country fixed effects, there is no longer empirical support for the modernization hypothesis with respect to effects of GDP and schooling on maintenance of law and order. It is worth stressing that, in this fixed-effects context, there is also no statistically significant effect of the law-and-order variable on economic growth (Table 1, column 4). In other words, with country fixed effects, the law-and-order variable seems uninteresting overall.

Results with the Freedom House indicator of political rights (a measure of democracy) as the dependent variable are in Table 4, columns 3 and 4. In the system without country fixed

[25]This result corresponds to the sum of the coefficients on female and male school attainment.

effects in column 3, the persistence over five years is given by the estimated coefficient of the lagged dependent variable, 0.796 (s.e.=0.018). Hence, the estimated persistence again differs significantly from zero or one. In this case, it takes 15 years to eliminate 50% of an initial gap between the democracy indicator and its long-run target and 51 years to eliminate 90% of the gap.

The X variables again include per capita GDP and now also distinguish female and male schooling by average years at the primary and upper levels. The five X variables are jointly highly statistically significant in Table 4, column 3 (p-value=0.000). The effect from the log of per capita GDP, 0.0112 (s.e.=0.0067), is positive but not quite statistically significant at the 5% level. School years overall are significantly positive (p-value=0.034).[26] Particularly important positive predictors of political rights are average years of female primary schooling (0.0439, s.e.=0.0138) and average years of male upper schooling (0.0344, s.e.=0.0142). Overall, this system without country fixed effects provides strong evidence for modernization with respect to effects of GDP and schooling on political rights; that is, for democracy.

In column 4, the incorporation of country fixed effects again reduces the estimated persistence—the estimated coefficient of the lagged dependent variable becomes 0.456 (s.e.=0.034). Also as before, the standard errors of the estimated coefficients on the X variables blow up, thereby leading to joint statistical insignificance for the five variables considered (p-value=0.59). Therefore, with country fixed effects, there is no longer empirical support for the modernization hypothesis with regard to effects of GDP and schooling on political rights. It is also true with fixed effects that political rights do not have a statistically significant effect on economic growth (Table 1, column 4). Thus, as with the law-and-order indicator, the inclusion of country fixed effects makes the political-rights variable look uninteresting.

[26]This result corresponds to the sum of all of the coefficients related to school attainment.

Results for the Polity democracy/autocracy indicator are mostly similar to those for the Freedom House political-rights variable. Without country fixed effects in Table 4, column 5, the coefficient of the lagged dependent variable is about the same as that in column 3. Total school years and the overall set of X variables are highly statistically significant in column 5. With country fixed effects included in column 6, the one difference from before (column 4) is that the X variables are jointly statistically significant. However, much of this significance stems from the negative impact estimated for male average years of upper schooling. Thus, we would conclude with the Polity variable that the modernization hypothesis is strongly confirmed in the system without fixed effects (column 5) but not in the system with fixed effects (column 6).

Acemoglu, et al., rely mainly on results with country fixed effects to argue that the modernization hypothesis is empirically not supported in cross-country panel data. They do not mention that, in the same context, the quality of institutions lacks explanatory power for economic growth. In any event, my inference is that the results signal the dangers from the inclusion of country fixed effects: this procedure seriously reduces the information contained in the panel data (reflected in the blowing up of coefficient standard errors for the X variables) and biases downward the estimated persistence effects, as revealed by the coefficients of lagged dependent variables.[27] I show in a later section that the results with country fixed effects look different—and supportive of the modernization hypothesis—in systems estimated over time frames of over a century.

[27]Acemoglu, et al. (2008, p. 820) argue that this bias in the fixed-effects estimated coefficient of a lagged dependent variable can be mitigated by observing the data at a higher frequency, such as annually: "Column 6 [of Table 3] estimates … with OLS using annual observations. This is useful since the fixed effect OLS estimator becomes consistent as the number of observations becomes large. With annual observations, we have a reasonably large time dimension." The results in n. 22 imply that this argument is incorrect. The proportionate bias in the estimated coefficient of a lagged dependent variable depends in Nickell's (1981) analysis on the overall length of the sample in years (or, actually, on the product βT, where β is the convergence rate per year and T is the sample length in years), not on the frequency of observation of the data.

For systems estimated with data spanning intervals of roughly 25 to 45 years—starting between 1965 and 1985 and ending in 2009—the most reliable (though imperfect) information on the determinants of institutional quality likely comes from systems that exclude country fixed effects but include measures of GDP and schooling. Thus, in Table 4, I would emphasize the findings in columns 1, 3, and 5. These results strongly support the modernization hypothesis with regard to effects of GDP and schooling on the determination of the quality of institutions, gauged by law and order and democracy. As with economic growth, the fixed-effects estimation seems more appropriate in systems for the limited group of countries with data over a century or more. These results are discussed in the next section.

VI. Long-Term Panels

Until recently, the best long-term macroeconomic panel data were the per-capita GDP series assembled by Maddison (2003). These series constitute a monumental contribution that has been widely used. However, the data have serious shortcomings, discussed and largely rectified in a new data set on per capita GDP and consumer expenditure assembled by Ursúa and me and described in Ursúa (2011).[28] The construction of these data was challenging, described by Ursúa as macroeconomic archeology, and various methods were implemented to include periods and countries originally missing or inadequately treated in standard sources. The new data set covers 42 countries and goes back at least to 1913 and in many cases to 1870 or earlier.

For the present long-term analysis, I use a sample of 28 countries that have annual data on per capita GDP starting between 1870 and 1896 and also have data at least since 1901 from

[28]The data are available as "Barro-Ursúa Macroeconomic Data" at www.rbarro.com/data-sets. Full annual time series back to 1913 are provided for 40 countries for real per capita GDP (with a missing observation for Greece in 1944) and 28 countries for real per capita personal consumer expenditure. The data on the website are expressed as indexes for each country, with the value in 2006 normalized to 100. For the present study, the values were converted to levels comparable across countries based on data on 2005 PPP-adjusted real per capita GDP from the World Bank's *World Development Indicators*.

Polity on the democracy/autocracy indicator. The list of countries is in the notes to Table 5, which has panel regressions using these long-term data.

A. Growth rates of per capita GDP

Columns 1-4 of Table 5 use as the dependent variable the growth rate of real per capita GDP over 5-year periods from 1870-75 to 2005-09. These results parallel those in Table 1, except that the array of X variables is now limited to the Polity indicator and its square. Table 5, column 1, includes as a regressor only the log of the five-year lag of per capita GDP (along with time effects). Unlike in Table 1, column 1, the estimated coefficient on the lagged log of per capita GDP is significantly negative, -0.0051 (s.e.=0.0013). However, the indicated convergence rate is only around 0.5% per year.

Table 5, column 2, adds country fixed effects. The estimated coefficient of the log of lagged per capita GDP, -0.0251 (s.e.=0.0043), shows convergence at 2.5% per year. This value is lower in magnitude than that found in Table 1, column 2.[29]

Table 5, column 3, includes the Polity indicator and its square as explanatory variables but excludes country fixed effects. The estimated coefficient of the log of lagged per capita GDP, -0.0092 (s.e.=0.0018), is larger in magnitude than that in column 1. However, the convergence rate is still only 0.9% per year. Most likely, this convergence rate is substantially underestimated because—with only the Polity indicator and its square included as X variables—there would be serious omitted-variables bias from the exclusion of growth determinants that are conditionally correlated with per capita GDP.

[29]A reasonable concern is that the comparison between the long-term and short-term results depends on sample selection with respect to which countries have long-term data. However, the post-1960s results in Table 1 do not differ greatly if the sample comprises only the 28 countries included in the long-term sample in Table 5. For this 28-country sample, the result corresponding to Table 1, column 2 (with country fixed effects included), is an estimated coefficient on the lagged log of per capita GDP of -0.0362 (s.e.=0.0131). The result corresponding to Table 1, column 1 (without country fixed effects), is -0.0043 (s.e.=0.0020).

The estimated coefficients of the Polity variables in Table 5, column 3, each differ significantly from zero, and the p-value for joint significance is 0.001. However, the pattern in the coefficients is the opposite to that found previously (Table 1, column 3)—negative on the level and positive on the square. Because data availability precludes the inclusion of other X variables in the regression—notably the law-and-order measure considered before—the Polity variables should be regarded as proxies for institutional quality broadly construed, rather than for democracy, per se. Hence, the particular sign pattern in the coefficients is probably not meaningful.

Table 5, column 4, includes country fixed effects along with the Polity variables. The estimated coefficient on the log of lagged per capita GDP, -0.0244 (s.e.=0.0041), indicates convergence at a slightly lower rate than that in column 2. The results on the Polity variables are similar to those in column 3, except that—with country fixed effects—the standard errors of the estimated coefficients are substantially higher.

In the long samples considered in Table 5, the lack of information on the array of explanatory variables considered in Table 1, column 3, suggests that the omitted-variables bias would be substantial in systems that omit country fixed effects. Thus, the estimated convergence rate of less than 1% per year in Table 5, column 3, is likely to be a serious underestimate of the true convergence rate. On the other hand, because of the long time series, the Hurwicz bias on the coefficient of the log of lagged per capita GDP—in columns 2 and 4—is likely to be less serious than that in Table 1, columns 2 and 4. (See n. 22 for a quantitative analysis of this bias in Nickell's [1981] model and the appendix for Monte Carlo analyses.) Therefore, the estimated convergence rate of around 2.4% per year in Table 5, column 4, may be only a small overestimate of the true conditional convergence rate.

We can put the long-term results from Table 5 together with the shorter-term results from Table 1. From Table 1, the best estimate of the convergence rate probably comes from the system without country fixed effects but with an array of X variables—column 3. The estimated conditional convergence rate here is 1.7% per year, with a two-standard-error band of 1.3% to 2.2%. This result may be a small underestimate of the true convergence rate—to the extent that the system still omits long-run growth determinants that are conditionally correlated with per capita GDP. From Table 5, the best estimate likely comes in column 4 from the system with country fixed effects and the limited X variables available (based on the Polity indicator). The estimated convergence rate here is 2.4% per year, with a two-standard-error band of 1.6% to 3.3%. This result may be a small overestimate of the true convergence rate—to the extent that the Hurwicz bias still operates in this long-term sample.

Combined results from the two panel data sets suggest that the true conditional convergence rate is, in terms of point estimates, bounded between 1.7% and 2.4% per year. Interestingly, this interval contains the iron-law rate of 2% per year.

B. Polity indicator

Columns 5 and 6 of Table 5 return to the modernization hypothesis by considering long-term panel regressions for the Polity indicator. Column 5 excludes country fixed effects. The estimated coefficient on the 5-year lag of the dependent variable, 0.817 (s.e.=0.023), is similar to that in Table 4, column 5. From the standpoint of the modernization hypothesis, the important result in Table 5, column 5, is the significantly positive coefficient, 0.0489 (s.e.=0.0095), on the log of per capita GDP.[30] This finding parallels the result in Table 4, column 5, concerning the joint statistical significance of the group of variables comprising the log of per capita GDP and

[30]The regression includes the average of the log of per capita GDP over the 5-year periods; that is, the average of the first through fifth lag. The instrument list includes the 6-year lag of the log of per capita GDP.

four schooling variables. Since these schooling measures are unavailable for the long-term sample,[31] we can think of the estimated coefficient on the log of per capita GDP as proxying for the overall effect from per capita GDP and education.

Table 5, column 6, adds country fixed effects. This change reduces the estimated coefficient on the lagged dependent variable to 0.743 (s.e.=0.028). Although this change is in the same direction as before (Table 4, column 6), the effect is now much smaller in magnitude. This result is expected because of the much longer time series employed in Table 5. With the variables observed over 139 years, the Hurwicz bias may be small even in the presence of country fixed effects (see n. 22 and the Monte Carlo results in the appendix).

The standard error of the estimated coefficients of the X variables—in this case, only the log of per capita GDP—goes up, as before, with the introduction of country fixed effects. However, this effect is now smaller, and the estimated coefficient of the log of per capita GDP, 0.0619 (s.e.=0.0244), remains significantly positive in Table 5, column 6. Hence, even with the inclusion of country fixed effects, the long-term sample supports the modernization hypothesis with regard to effects of per capita GDP on the Polity indicator for democracy.[32]

VII. Natural Experiments

An attractive way to avoid estimation problems related to the endogeneity of X variables is to exploit natural experiments in which cross-economy differences are clearly exogenous and substantial. This approach has been applied particularly to studies of the consequences of

[31]Some countries have long-term data on school enrollment rates, but the quality of these data is unclear.
[32]Acemoglu, et al. (2008, Table 7) argue that lagged per capita GDP lacks significant explanatory power for the Polity indicator over long samples for 25 countries starting in 1875. However, their analysis relies on the questionable Maddison data and—because of limitations in the availability of the Maddison data—considers effects only with a lag of 25 years. (They also fail to find significant effects at 50-year lags for a larger sample of countries.)

differences in institutional quality. Two frequently mentioned cases of this kind are the post-WWII separations of Germany into East and West and of Korea between North and South.

One attraction of these "natural experiments' is that they isolate sharp differences in the quality of institutions (West Germany being superior to East Germany and South Korea being better than North Korea), while (as in the labor-economics literature on twins) arguably avoiding substantial differences in most other X variables. Most importantly, the sources of the better institutions are plausibly exogenous political events, rather than responses to economic conditions. It is, therefore, convincing in these cases that the reason for higher GDP growth rates post-separation in the places with better institutions is the better institutions.

Although these kinds of natural experiments are useful, a serious drawback is the selection issue about which cases to consider. There is a tendency to focus on examples that work; that is, where the underlying differences in institutional quality are clear and due to exogenous political events and also where these differences are, in fact, accompanied by large growth-rate differentials in the hypothesized direction.

Consider as an example the peaceful split of Czechoslovakia into two independent countries in 1993. It would have been easy to argue at the time (as I recall doing myself) that the more highly developed Czech Republic retained high-quality institutions, whereas the Slovak Republic ended up with an institutional void. And, if the relative growth performances after 1993 had strongly favored the Czech Republic, this case would likely have joined Germany and Korea as a clear demonstration of the importance of institutional differences in promoting economic growth.[33] Yet, in practice, real per capita GDP in the Slovak Republic grew from

[33]However, unlike North and South Korea, the Czech and Slovak parts of Czechoslovakia had major differences in ethnic and cultural backgrounds (differences that also applied for East versus West Germany). Although the ethno-cultural division explained the way that Czechoslovakia was divided up, the timing of the split in 1993 and the fact of the breakup at all can still be regarded as mainly exogenous.

1993 to 2009 at 4.7% per year, whereas that in the Czech Republic grew by 3.1% per year—so that the ratio of Czech to Slovak per capita GDP fell from 1.48 in 1993 to 1.15 in 2009. This outcome seems consistent with usual convergence behavior without having to condition on major differences in institutional quality. However, in the usual analysis of natural experiments, the Czech/Slovak case is not taken as contrary evidence to the hypothesis that institutional quality is the key to economic growth. Instead, this case is typically ignored.

Similar observations apply to many other cases that could have been considered; for example, effects on institutional quality and economic growth from breakups of the Soviet Union and Yugoslavia into independent republics. To carry out this kind of analysis in an organized and global manner, it is necessary to have a comprehensive sample that includes measures of institutional quality, just as in the cross-country panel regressions of Table 1. Therefore, in the end, the appeal to natural experiments does not avoid many of the estimation issues associated with usual regression analyses.

VIII. Cross-Country Dispersion of Per Capita GDP and Consumption

The concept of convergence discussed thus far pertains to whether countries that are poorer (in absolute terms or in relation to their own steady-state positions) tend to grow faster than richer ones. In Barro and Sala-i-Martin (1991), this concept is called β-convergence and is distinguished from another form (σ-convergence) that relates to a tendency for the cross-sectional dispersion of per capita GDP to decline over time. This dispersion can be measured in proportionate terms by the cross-sectional standard deviation of the log of per capita GDP.

If all countries have the same steady-state per capita GDP, then the existence of β convergence tends to reduce the cross-sectional dispersion over time. However, if individual country shocks are present, these shocks tend to raise dispersion. With purely idiosyncratic country shocks, the cross-sectional variance tends to approach a value that depends positively on the variance of the shocks and negatively on the rate of β convergence. The cross-sectional variance tends to fall over time if it starts above its steady-state value but tends, otherwise, to rise over time (even though β convergence is present). If the sample comprises a large number of countries that have existed with fixed underlying parameters for a long time, the cross-sectional variance will tend at any point in time to be close to its long-run value.[34]

The long-term data on per capita GDP and consumption described in Ursúa (2011) can be used to study the long-run evolution of cross-country dispersion (see n. 28). Figure 1 applies to the longest feasible sample, 1870-2009, for which 25 countries (20 of which were subsequently OECD members) have annual data on real per capita GDP. The countries are listed in the note to the figure. Dispersion is measured by the standard deviation across countries of the log of real per capita GDP. The blue line weights countries equally, and the red line weights by population (thereby corresponding under some conditions to the dispersion of income for persons rather than countries).

The blue line (equally weighted) in Figure 1 shows small changes over time, when considered in relation to subsequent figures. The range is from 0.58 in 2009 to 0.71 in 1946. The main movement away from the mean of 0.65 associates with WWII—the standard deviation rose from 0.62 in 1938 to 0.71 in 1946. During this crisis period, shocks had a high spatial correlation and affected groups of countries differentially, thereby violating the assumption of purely idiosyncratic country shocks.[35] Otherwise, the main finding is that the cross-sectional

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[34]The notion that a tendency for the poor to grow faster than the rich implies a negative trend in dispersion or inequality is a fallacy; in fact, it is Galton's Fallacy (Galton [1886, 1889], Quah [1993], Hart [1995]), which Galton applied to the distribution of heights across a population. For generations of an extended family, height has positive persistence but tends to revert to the population mean, thereby constituting a form of β convergence. Nevertheless, the dispersion of heights across the overall population typically changes little over time.

[35]Similarly, in Barro and Sala-i-Martin (1991, Figure 4), the large dispersion in per capita personal income across the U.S. states in 1880 reflects the differential impact of the Civil War on the South versus the North. However,

standard deviation of the log of per capita GDP is remarkably stable since 1870 around its mean of 0.65.

The red line (population weighted) tells a similar story, except that this measure of dispersion is more sensitive to the major crises during the world wars and the 1990s in Russia (a relatively poor country with a large population). In 2009, the population-weighted standard deviation of 0.60 is close to the equally-weighted value of 0.58.

Figure 2 extends to a larger sample by using the 34 countries with GDP data starting at least by 1896. This sample is less subject than the 25-country group used before to the sample-selection problem of tending to include countries that were rich toward the end of the sample. The note to the figure lists the countries. Most importantly, this sample adds the world's two largest countries by population, China and India.

The dispersion measured by the blue line (equally weighted) in Figure 2 is higher than that in Figure 1 because the expansion of the sample adds several countries with per capita GDP well below the mean. Compared to Figure 1, the blue graph in Figure 2 also shows more substantial changes over time, with the standard deviation starting at 0.87 in 1896 and rising during the Great Depression and WWII to 1.04 in 1946. After not changing greatly through the mid 1970s, the standard deviation falls from 1.07 in 1974 to 0.79 in 2009. The decline in the last phase reflects particularly the strong growth in developing countries, including China, India, and Indonesia.[36] We can anticipate that, in the long run, the standard deviation in this 34-country sample will fall toward the average value of 0.65 found in Figure 1—because the added

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across the U.S. states, the long-run standard deviation for the log of per capita personal income was only around 0.2, much smaller than that across countries.

[36]Thus, the sample-selection criterion in Figure 1 (25 countries having GDP data back to 1870) actually understates σ convergence compared to that in Figure 2 (34 countries having GDP data back to 1896). The results in Baumol (1986, Figure 1) were the reverse—with the restriction of the sample to 16 countries with data from Maddison (1982) back to 1870 tending to overstate σ convergence.

developing countries seem to be joining the richer group selected in Figure 1 (by the criterion of having GDP data back to 1870).

The red line (population weighted) in Figure 2 starts with higher dispersion than the blue line (equally weighted) because the largest countries by population, China and India, begin far below the world mean for per capita GDP. Otherwise, the trend in the population-weighted series is similar to that for the equally-weighted series through the mid 1970s. Thereafter, the dispersion in the population-weighted series falls sharply, going from 1.58 in 1974 to 0.85 in 2009. This recent trend, highlighted in terms of the world distribution of income by Sala-i-Martin (2006), reflects particularly the strong growth in China since the late 1970s and in India since the mid 1980s.

To study the dispersion in consumption, the sample has to begin later to include a substantial number of countries. Figures 3 and 4 cover 29 countries observed for per capita personal consumer expenditure, C, since 1919. Note that this sample, listed in the note to Figure 3, includes India but not China (because C data for China start only in 1952).

Figure 3 shows the equal-weight dispersion in GDP and C for the 29 countries. The levels and patterns in the two series do not differ greatly, although C dispersion is lower than GDP dispersion in most years. In 2009, the two values are similar—0.69 for GDP and 0.68 for C. Both series show modest declines since the mid 1970s—GDP dispersion falls from 0.85 in 1973 to 0.69 in 2009, while C dispersion falls from 0.79 in 1973 to 0.68 in 2009.

Figure 4 has the population-weighted measures. The exclusion of China is important when comparing with the population-weighted series for GDP in Figure 2. The GDP and C series in Figure 4 again show similar levels and trends. The C series is lower in most years but higher since 1994. The two series show declines since 1979 but not as much as in Figure 2 because of the exclusion of China in Figure 4.

IX. Summary Observations

For a large panel of countries since the 1960s, the estimated conditional convergence rate for per capita GDP is around 1.7% per year in systems with a rich array of explanatory variables but no country fixed effects. The results feature statistically significant influences on economic growth from institutional quality, measured by maintenance of law and order and democracy. Analogous settings support the modernization hypothesis, in the sense of significantly positive impacts of per capita GDP and schooling on the indicators for maintenance of law and order and democracy.

The inclusion of country fixed effects produces much higher convergence rates, eliminates statistically significant effects of the institutional measures on economic growth, and removes the statistical support for modernization. I argue that these problematic findings reflect econometric issues associated with the inclusion of country fixed effects in panels with moderate time dimension.

In a panel for a limited number of countries observed since 1870, the estimated conditional convergence rate in the presence of country fixed effects is about 2.4% per year. This setting supports the modernization hypothesis in the sense of statistically significant, positive effects from per capita GDP on democracy. These "reasonable" results with country fixed effects likely arise because the econometric problems posed by country fixed effects may not be serious in samples with long time frames.

A combination of the results from short and long panels implies that the conditional convergence rate is in the neighborhood of the "iron-law" rate of 2% per year. The similar findings from these two very different empirical settings suggest that a conditional convergence rate of around 2% may be a robust empirical regularity. The two settings considered jointly also provide strong support for the modernization hypothesis.

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Table 1 Growth-Rate Regressions for Cross-Country Panel Five-year periods: 1960-65, …, 2005-09 (all equations include time effects)				
	(1)	**(2)**	**(3)**	**(4)**
		Fixed effects		**Fixed effects**
Log(lagged per capita GDP)	0.0019 (0.0012)	-0.0326** (0.0048)	-0.0171** (0.0022)	-0.0447** (0.0051)
1/(life expectancy at birth)	--	--	-2.59** (0.65)	-1.15 (1.22)
Log(fertility rate)	--	--	-0.0340** (0.0046)	-0.0351** (0.0083)
Law & order (rule of law)	--	--	0.0165** (0.0059)	0.0045 (0.0103)
Investment ratio	--	--	0.021 (0.013)	0.014 (0.026)
Female school years	--	--	0.0035* (0.0014)	0.0062 (0.0037)
Male school years	--	--	-0.0036* (0.0015)	-0.0107** (0.0035)
Government consumption ratio	--	--	-0.021 (0.025)	-0.083 (0.059)
Openness ratio	--	--	0.0066* (0.0027)	0.0129 (0.0082)
Terms-of-trade change	--	--	0.103** (0.027)	0.092** (0.029)
Democracy indicator	--	--	0.054** (0.019)	0.021 (0.027)
Democracy squared	--	--	-0.055** (0.017)	-0.027 (0.024)
Inflation rate	--	--	-0.0138 (0.0087)	-0.0315* (0.0156)
R-squared	0.095	0.381	0.349	0.511
s.e. of regression	0.0274	0.0239	0.0236	0.0216
No. countries, observations	80, 783	80, 783	80, 760	80, 760

*Significant at 5% level.

**Significant at 1% level.

Notes to Table 1

The dependent variable is the annual growth rate of real per capita GDP for the ten periods: 1960-65, 1965-70, …, 2005-09. The last period ends in 2009 because of data availability. Lagged per capita GDP is for 1960, 1965, …, 2005. Values for 1959, 1964, …, 2004 are used as instruments. Other regressors are averages over periods, with lagged values used as instruments. The error terms are allowed to be correlated over time within countries. Standard errors of coefficient estimates are in parentheses. The sample criterion was to include countries only if they have data starting by the 1965-70 period. The democracy indicator is the political-rights variable from Freedom House. Joint p-values for the two democracy variables are 0.004 in column 3 and 0.26 in column 4. The fixed effects in columns 2 and 4 are dummies for 79 of the countries shown in Table 2.

Definitions and sources:

PPP-adjusted real per capita GDP is from Penn World Tables (www.pwt.econ.upenn.edu), version 7.0, in units of 2005 international dollars. Also from this source are the ratios to GDP of investment (private plus public) and government consumption and the openness ratio (exports plus imports relative to GDP). These ratio variables use current-price information.

Life expectancy at birth and total fertility rate are from the World Bank's *World Development Indicators* (*WDI*).

The law-and-order indicator (converted from seven categories to a 0-1 scale, with 1 representing highest maintenance of law and order) is from Political Risk Services, *International Country Risk Guide*.

Average years of school attainment for females and males and at various levels of schooling are from Barro and Lee data set. Source: "Educational Attainment Data," available at www.rbarro.com/data-sets.

The terms-of-trade change (growth rates over five years of export prices relative to import prices) is from International Monetary Fund, *International Financial Statistics*, and *WDI*. This variable is interacted with the openness ratio.

The political-rights variable (converted from seven categories to a 0-1 scale, with 1 representing highest rights) is from Freedom House (www.freedomhouse.org). Data on an analogous concept for 1960 and 1965 are from Bollen (1980).

The inflation rate (averaged over 5-year intervals) is calculated from retail-price indexes from International Monetary Fund, *International Financial Statistics*, and *WDI*.

Table 2
Sample of 80 Countries Used in Regressions in Table 1

Country	Starting period	Country	Starting period
Argentina	1960-65	Jordan	1965-70
Australia	1960-65	Japan	1960-65
Austria	1960-65	Kenya	1965-70
Belgium	1960-65	South Korea	1965-70
Bangladesh	1965-70	Sri Lanka	1960-65
Bolivia	1965-70	Luxembourg	1965-70
Brazil	1965-70	Morocco	1960-65
Botswana	1965-70	Mexico	1960-65
Canada	1960-65	Mali	1965-70
Switzerland	1960-65	Malawi	1965-70
Chile	1960-65	Malaysia	1960-65
China	1965-70	Niger	1965-70
Cote d'Ivoire	1965-70	Nicaragua	1965-70
Cameroon	1965-70	Netherlands	1960-65
Congo, Republic	1965-70	Norway	1960-65
Colombia	1960-65	New Zealand	1960-65
Costa Rica	1960-65	Pakistan	1965-70
Cyprus	1960-65	Panama	1965-70
Denmark	1960-65	Peru	1965-70
Dominican Republic	1960-65	Philippines	1960-65
Algeria	1965-70	Papua New Guinea	1965-70
Ecuador	1960-65	Portugal	1960-65
Egypt	1960-65	Paraguay	1960-65
Spain	1960-65	Senegal	1965-70
Finland	1960-65	Singapore	1965-70
France	1960-65	Sierra Leone	1965-70
Gabon	1965-70	El Salvador	1960-65
United Kingdom	1960-65	Sweden	1960-65
Ghana	1965-70	Togo	1965-70
Gambia	1965-70	Thailand	1960-65
Greece	1960-65	Trinidad	1960-65
Guatemala	1960-65	Tunisia	1965-70
Honduras	1960-65	Turkey	1965-70
Haiti	1965-70	Taiwan	1965-70
Indonesia	1965-70	Uganda	1965-70
India	1960-65	Uruguay	1965-70
Ireland	1960-65	United States	1960-65
Iceland	1960-65	Venezuela	1965-70
Italy	1960-65	South Africa	1960-65
Jamaica	1960-65	Zambia	1965-70

Table 3 Additional Growth-Rate Regressions for Cross-Country Panel (all equations include time effects)				
	(1)	**(2)**	**(3)**	**(4)**
	OLS	**Ten-year periods**	**Two sets of fixed effects**	**Polity variable**
Log(lagged per capita GDP)	-0.0173** (0.0022)	-0.0163** (0.0023)	-0.0783** (0.0094)	-0.0164** (0.0023)
1/(life expectancy at birth)	-2.72** (0.61)	-2.65** (0.76)	-2.80 (1.77)	-2.75** (0.66)
Log(fertility rate)	-0.0307** (0.0046)	-0.0317** (0.0050)	-0.0328* (0.0137)	-0.0326** (0.0048)
Law & order (rule of law)	0.0159** (0.0056)	0.0161* (0.0066)	0.0217 (0.0161)	0.0124* (0.0059)
Investment ratio	0.041** (0.012)	0.011 (0.015)	0.010 (0.039)	0.010 (0.014)
Female school years	0.0035* (0.0014)	0.0030* (0.0014)	0.0092 (0.0058)	0.0026 (0.0015)
Male school years	-0.0037* (0.0015)	-0.0033 (0.0015)	-0.0111* (0.0054)	-0.0030 (0.0015)
Government consumption ratio	-0.026 (0.023)	-0.024 (0.028)	-0.114 (0.090)	-0.036 (0.026)
Openness ratio	0.0061* (0.0025)	0.0057 (0.0032)	0.0263* (0.0107)	0.0061* (0.0029)
Terms-of-trade change	0.101** (0.027)	0.100* (0.041)	0.094** (0.030)	0.120** (0.028)
Democracy indicator	0.039* (0.016)	0.031 (0.024)	0.051 (0.033)	0.031 (0.021)
Democracy squared	-0.039** (0.014)	-0.035 (0.022)	-0.056 (0.031)	-0.036 (0.019)
Inflation rate	-0.0198** (0.0040)	-0.0157 (0.0138)	-0.0091 (0.0250)	-0.0108 (0.0091)
R-squared	0.351	0.472	0.628	0.327
s.e. of regression	0.0236	0.0174	0.0201	0.0231
No. countries, observations	80, 774	80, 359	80, 760	78, 691

*Significant at 5% level.

**Significant at 1% level.

Notes to Table 3

See the notes to Table 1.

Column 1 corresponds to column 3 of Table 1 but all regressors are on the instrument list (corresponding to OLS, except for allowing the error terms to be correlated over time within countries). The p-value for the two electoral-rights variables jointly is 0.027. Results from OLS have the same estimated coefficients, but the standard errors of these coefficients are somewhat lower (except for the terms-of-trade variable). For example, for the log of lagged GDP, the standard error declines from 0.0022 to 0.0018.

The other columns estimate by instrumental variables, as in Table 1. Column 2 corresponds to column 3 of Table 1, except that the dependent variable is the growth rate of per capita GDP for the five "10-year" periods 1960-70, …, 2000-09. The regressors are defined analogously for the 10-year periods.

Column 3 corresponds to column 4 of Table 1 but has one set of fixed effects for the five 5-year growth-rate observations from 1960-65 to 1980-85 and another set for the five observations from 1985-90 to 2005-09.

Column 4 is the same as column 3 of Table 1, except that the democracy indicator (the Freedom House measure of political rights) is replaced by the Polity measure of democracy less autocracy. The joint p-value for the two Polity variables is 0.045.

Sources: The Polity indicator is for democracy less autocracy (converted from a -10 to +10 scale to a 0-1 scale, with 1 representing highest democracy), from Polity IV (www.systemicpeace.org). Other sources are given in the notes to Table 1.

Table 4 Regressions for Indicators of Law & Order and Democracy (five-year periods; all equations include time effects)				
	(1)	**(2)**	**(3)**	**(4)**
	Law & Order: 1985-90, …, 2005-09,		**Political Rights (Freedom House): 1970-75, …, 2005-09,**	
		Fixed effects		**Fixed effects**
Lagged dependent variable	0.708** (0.026)	0.332** (0.045)	0.796** (0.018)	0.456** (0.034)
Log(per capita GDP)	0.0248** (0.0066)	-0.0353 (0.0395)	0.0112 (0.0067)	-0.0262 (0.0349)
Female school years	-0.0093 (0.0062)	0.0316 (0.0299)	--	--
Male school years	0.0164* (0.0068)	-0.0195 (0.0283)	--	--
Female primary school years	--	--	0.0439** (0.0138)	0.0028 (0.0394)
Male primary school years	--	--	-0.0279 (0.0146)	-0.0202 (0.0423)
Female upper school years	--	--	-0.0367* (0.0153)	-0.0201 (0.0442)
Male upper school years	--	--	0.0344* (0.0142)	-0.0075 (0.0434)
p-value for total school years	0.016	0.49	0.034	0.14
p-value for GDP, female schooling, male schooling	0.000	0.62	0.000	0.59
R-squared	0.764	0.842	0.775	0.830
s.e. of regression	0.119	0.110	0.176	0.165
No. countries, observations	123, 574	123, 574	140, 996	140, 996

*Significant at 5% level.

**Significant at 1% level.

Table 4, continued		
	(5)	**(6)**
	Democracy/Autocracy (Polity): 1960-65, …, 2005-09	
		Fixed effects
Lagged dependent variable	0.805** (0.016)	0.546** (0.032)
Log(per capita GDP)	-0.0022 (0.0061)	0.034 (0.033)
Female school years	--	--
Male school years	--	--
Female primary school years	0.0384** (0.0123)	0.0104 (0.0323)
Male primary school years	-0.0171 (0.0128)	0.0462 (0.0358)
Female upper school years	-0.0224 (0.0137)	0.0267 (0.0371)
Male upper school years	0.0218 (0.0130)	-0.0621 (0.0369)
p-value for total school years	0.000	0.41
p-value for GDP, female schooling, male schooling	0.000	0.005
R-squared	0.805	0.841
s.e. of regression	0.166	0.161
No. countries, observations	132, 1019	132, 1019

*Significant at 5% level.

**Significant at 1% level.

Notes to Table 4

See the notes to Tables 1 and 3.

The dependent variable in columns 1 and 2 is the indicator for law and order from *International Country Risk Guide*, observed at the five dates: 1990, 1995, 2000, 2005, and 2009. In columns 3 and 4, the dependent variable is the Freedom House measure of political rights, observed at the eight dates: 1975, …, 2009. In columns 5 and 6, the dependent variable is the Polity measure of democracy less autocracy, observed at the ten dates: 1965, …, 2009. The lagged dependent variable applies to 1985, …, 2005 in columns 1 and 2; 1970, …, 2005 in columns 3 and 4; and 1960, …, 2005 in columns 5 and 6. These lagged dependent variables are on the instrument lists. As in Table 1, the other regressors are averages over periods, with lagged values used as instruments. The error terms are allowed to be correlated over time within countries. Standard errors of coefficient estimates are in parentheses. Country fixed effects are included in columns 2, 4, and 6.

Table 5
Panel Regressions with Long-Term Data
1870-75, …, 2005-09
(all equations include time effects)

	Dependent variable: Growth rate of per capita GDP			
	(1)	**(2)**	**(3)**	**(4)**
		Fixed effects		**Fixed effects**
Log(lagged per capita GDP)	-0.0051** (0.0013)	-0.0251** (0.0043)	-0.0092** (0.0018)	-0.0244** (0.0041)
Polity democracy/autocracy	--	--	-0.0431* (0.0017)	-0.0431* (0.0202)
Polity squared	--	--	0.0468** (0.0145)	0.0423* (0.0176)
p-value for Polity variables	--	--	0.001	0.046
R-squared	0.230	0.265	0.212	0.253
s.e. of regression	0.0279	0.0278	0.0257	0.0255
No. countries, observations	28, 760	28, 760	28, 693	28, 693

	Dependent variable: Polity (democracy/autocracy)	
	(5)	**(6)**
		Fixed effects
Lagged Polity (democracy/ autocracy)	0.817** (0.023)	0.743** (0.028)
Log(per capita GDP)	0.0489** (0.0095)	0.0619* (0.0244)
R-squared	0.833	0.842
s.e. of regression	0.144	0.143
No. countries, observations	28, 693	28, 693

*Significant at 5% level.

**Significant at 1% level.

Notes to Table 5

The sample criterion was to include countries only if they have GDP and Polity data starting by 1901. This criterion selected 28 countries: Argentina, Australia, Austria, Belgium, Brazil, Canada, Chile, China, Denmark, France, Germany, Italy, Japan, Mexico, Netherlands, New Zealand, Norway, Peru, Portugal, Russia, Spain, Sweden, Switzerland, Turkey, United Kingdom, United States, Uruguay, and Venezuela. Standard errors of coefficient estimates are in parentheses.

Columns 1-4: The dependent variable is the annual growth rate of real per capita GDP for the 28 countries for 28 periods: 1870-75, 1875-80, …, 2005-09. Lagged per capita GDP is for 1870, 1875, …, 2005. Values for 1869, 1874, …, 2004 are used as instruments. The Polity variables are averages over the periods, with lagged values used as instruments. The error terms are allowed to be correlated over time within countries. The fixed effects in columns 2 and 4 are dummies for 27 of the countries.

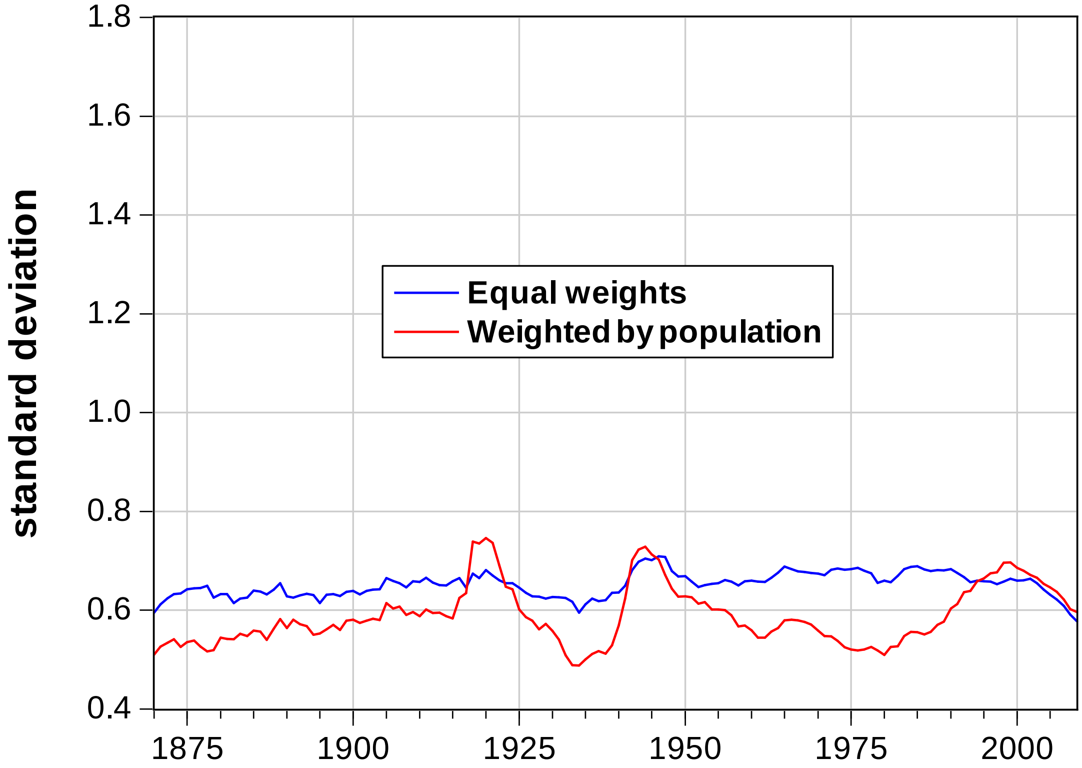
Columns 5-6: The dependent variable in columns 5 and 6 is the Polity measure of democracy less the measure of autocracy, observed for the 28 countries at 28 dates: 1875, 1880, …, 2009. The lagged dependent variable applies to 1870, 1875, …, 2005. These lagged dependent variables are on the instrument lists. The log(per capita GDP) variables are averages over the periods, with lagged values used as instruments. The error terms are allowed to be correlated over time within countries. Standard errors of coefficient estimates are in parentheses. The fixed effects in column 6 are dummies for 27 of the countries.

Sources: GDP and consumption (personal consumer expenditure) are from "Barro-Ursúa Macroeconomic Data," available at www.rbarro.com/data-sets. The source of the Polity indicator is given in the notes to Table 3.

Figure 1

Cross-Country Dispersion of the Log of per capita GDP

25 countries, 1870-2009

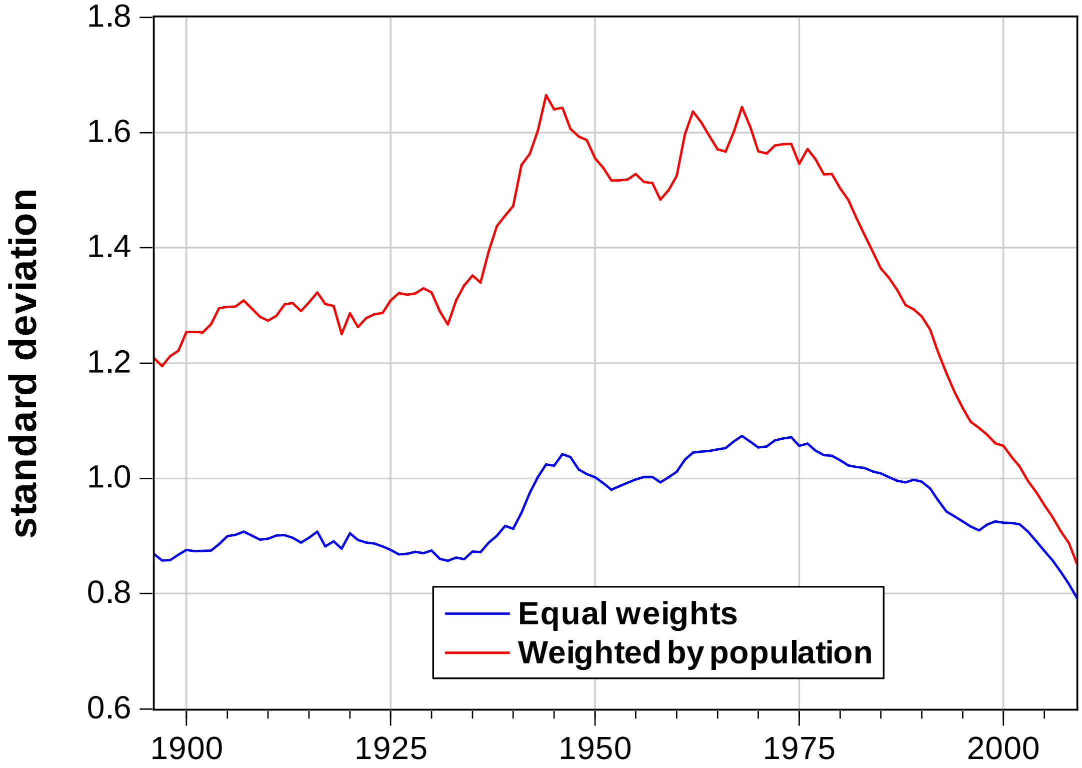


Note: The 25 countries included are Australia, Austria, Belgium, Brazil, Canada, Chile, Denmark, Finland, France, Germany, Iceland, Italy, Japan, Netherlands, New Zealand, Norway, Portugal, Russia, Spain, Sri Lanka, Sweden, Switzerland, United Kingdom, United States, and Uruguay. The graphs show the cross-sectional standard deviation of the log of real per capita GDP. The blue series has equal weights; the red series weights each country by population. The source of data (which also includes data on population) is given in the notes to Table 5.

Figure 2

Cross-Country Dispersion of the Log of per capita GDP

34 countries, 1896-2009

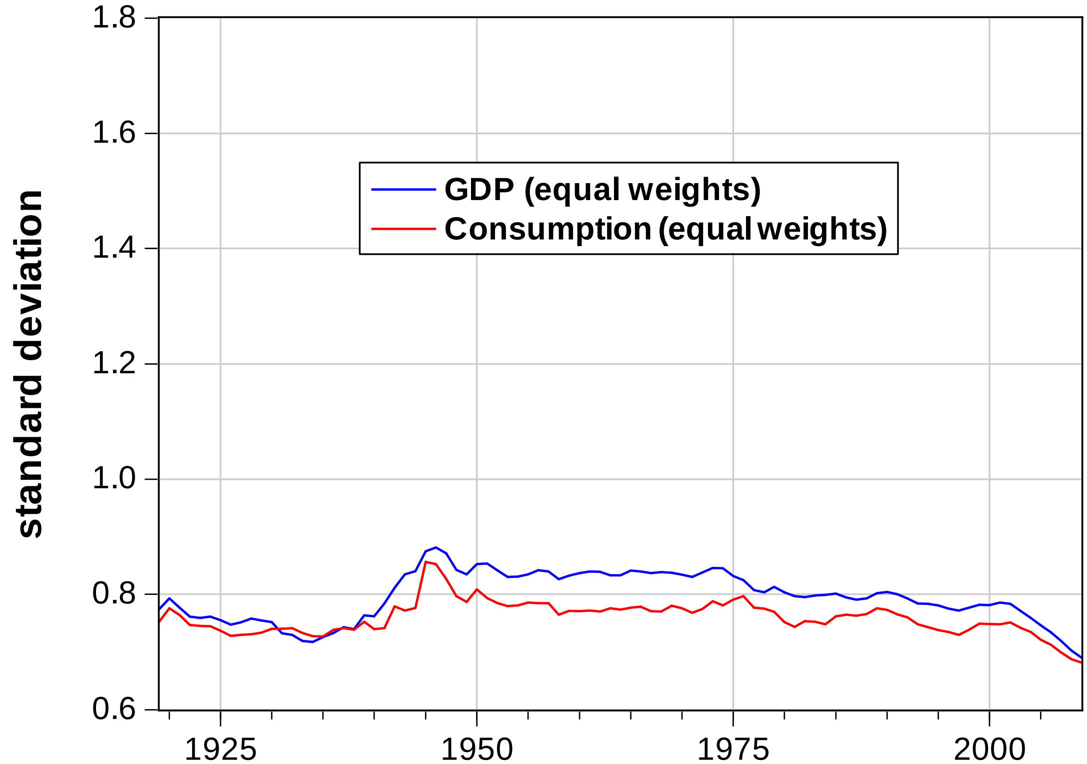


Note: The 34 countries included are Argentina, Australia, Austria, Belgium, Brazil, Canada, Chile, China, Denmark, Egypt, Finland, France, Germany, Iceland, India, Indonesia, Italy, Japan, Mexico, Netherlands, New Zealand, Norway, Peru, Portugal, Russia, Spain, Sri Lanka, Sweden, Switzerland, Turkey, United Kingdom, United States, Uruguay, and Venezuela. The graphs show the cross-sectional standard deviation of the log of real per capita GDP. The blue series has equal weights; the red series weights each country by population. The source of data (which also includes data on population) is given in the notes to Table 5.

Figure 3

Cross-Country Dispersion of Logs of per capita GDP and Consumption

29 countries, 1919-2009, equal weights

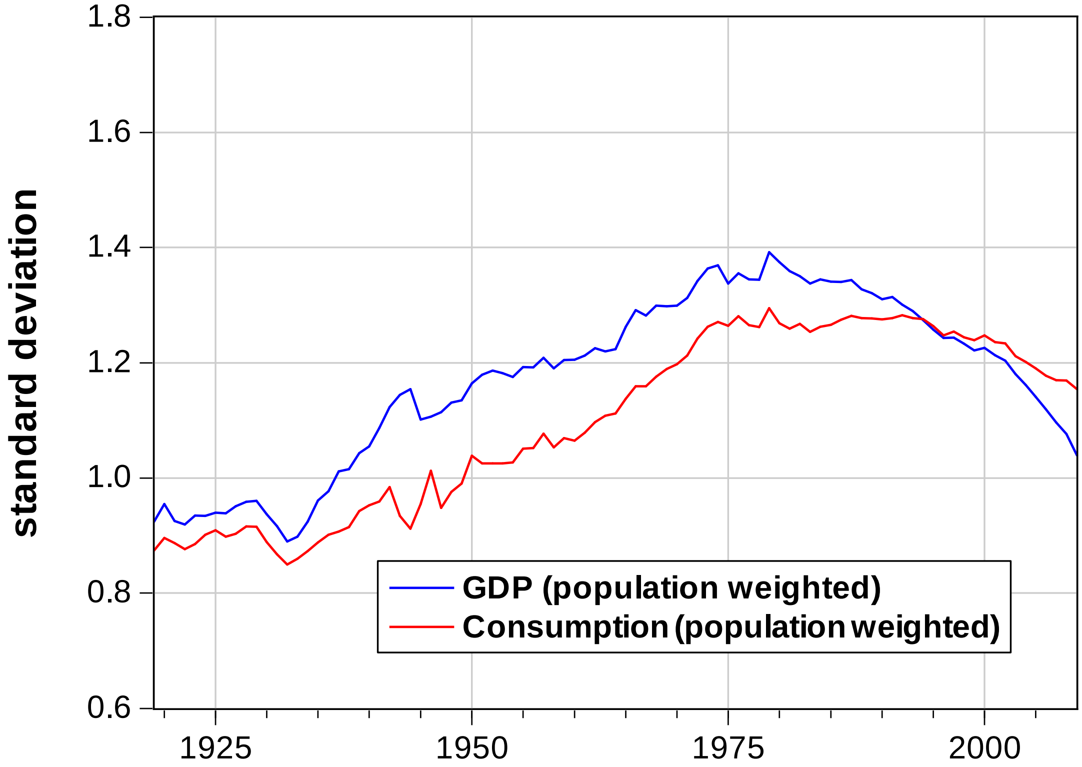


Note: The 29 countries included are Argentina, Australia, Belgium, Brazil, Canada, Chile, Denmark, Egypt, Finland, France, Germany, India, Italy, Japan, Mexico, Netherlands, New Zealand, Norway, Peru, Portugal, Russia, South Korea, Spain, Sweden, Switzerland, Taiwan, Turkey, United Kingdom, and United States. The graphs show the cross-sectional standard deviation of the log of real per capita GDP (blue graph) and the log of real per capita personal consumer expenditure (red graph), where countries have equal weights. The source of data (which also includes data on population) is given in the notes to Table 5.

Figure 4

Cross-Country Dispersion of Logs of per capita GDP and Consumption

29 countries, 1919-2009, weighted by population



Note: The note to Figure 3 shows the sample of countries. The graphs show the cross-sectional standard deviation of the log of real per capita GDP (blue graph) and the log of real per capita personal consumer expenditure (red graph), where countries are weighted by population. The source of data (which also includes data on population) is given in the notes to Table 5.

Appendix

Monte Carlo Analysis of Dynamic Estimation with and without Fixed Effects

The dynamic model with fixed effects and no time-varying X variables follows Hurwicz (1950), Nickell (1981), Arellano and Bond (1991), and Nerlove (2000):

(A1) $$y_{it} = \gamma \cdot y_{i,\, t-1} + \eta_i + \varepsilon_{it},$$

where i = 1, …, N represents "countries;" t = 1, …, T represents periods; y_{it} is per capita GDP or some other country-time variable; $0<\gamma<1$; and $\varepsilon_{it} \sim N(0, \sigma_\varepsilon^2)$ is an i.i.d. shock. I think of the fixed effect, η_i, as being drawn once for each country at the beginning of time, where $\eta_i \sim N(0, \sigma_\eta^2)$ is an i.i.d. disturbance. (The normality assumption should not be important here.) As stressed by Nerlove (2000), the initial sample value y_{i0} cannot be viewed as independent of η_i, because y_{i0} comes from the cumulation of equation (A1) from the indefinite past; hence, y_{i0} depends on η_i and past realizations of the ε_{it}. Specifically, we have $y_{i0} \sim N(\frac{\eta_i}{1-\gamma}, \frac{\sigma_\varepsilon^2}{1-\gamma})$. Therefore, a country that gets a high draw for η_i tends also to have a high y_{i0}.

The model with a time-varying, exogenous X variable and no country fixed effects is:

(A2) $$y_{it} = \gamma \cdot y_{i,\, t-1} + \alpha \cdot X_{it} + \varepsilon_{it},$$

(A3) $$X_{it} = \rho \cdot X_{i,t-1} + u_{it},$$

where γ and ε_{it} are defined as before, α is a constant, $0<\rho<1$, and $u_{it} \sim N(0, \sigma_u^2)$ is an i.i.d. shock. The initial sample value of the X variable is given accordingly by $X_{i0} \sim N(0, \frac{\sigma_u^2}{1-\rho})$. The initial sample value of y reflects its dependence on past shocks to ε and u from equations (A2) and (A3), and this dependence implies a relationship with X_{i0} (which depends on past shocks to u). Specifically, we can write

(A4) $$y_{i0} = \varphi \cdot X_{i0} + w_{i0},$$

where φ can be shown to equal $\alpha/[(1+\rho)\cdot(1-\gamma\rho)]$, and $w \sim N(0, \sigma_w^2)$, independently of X_{i0}. Therefore, if $\alpha>0$, a country that gets a high draw for X_{i0} tends also to have a high y_{i0}.

If periods are of length τ (years), the persistence coefficient, γ, relates to the convergence rate, $\beta>0$, from $\gamma = e^{-\beta\tau}$. I assume that $\beta\tau$ is much less than 1, so that $\gamma \approx 1-\beta\tau$ and, hence, $\beta \approx (1-\gamma)/\tau$. For annual periods ($\tau$=1 year), the convergence rate per year is $\beta \approx 1-\gamma$.

A. Model with Fixed Effects

Consider first the model with a country fixed effect and no X variable in equation (A1). The standard deviation of the time-series shock, σ_ε, can be normalized to 1, so that σ_η represents the dispersion of the country (cross-sectional) shock, relative to the time-series shock. In Nickell (1981), the analysis applied as the number of cross sections, N, tended to infinity. In my Monte Carlo analysis, N=20 seems large enough to approximate this asymptotic setting. I use N=100 below.

I contrast the results using OLS without country fixed effects with those using country fixed effects (OLS with dummy variables). As discussed in the text, the estimated coefficients γ and β from the two methods involve a tradeoff between two types of biases. I begin with a heuristic description of the results.

For OLS without country fixed effects, the Hurwicz bias is unimportant—even if the time dimension, T, is small—but an omitted-variables bias applies. The omitted variables are the country fixed effects, η_i, which are positively correlated with the y_{it}. This effect biases up the estimated γ and, therefore, biases down the estimated β. This omitted-variables channel is more important the larger σ_η. For small enough σ_η, the omitted-variables effect is minor, and OLS without country fixed effects produces nearly unbiased estimates.

For OLS with country fixed effects, there are no omitted variables,[37] but the Hurwicz bias tends to be large. This effect biases down the estimated γ and, therefore, biases up the estimated β. The size of the Hurwicz bias depends on the length of the time series, T. If T is small—even 20 or 50 years—the bias is large in magnitude. However, for large enough T, the bias becomes small, so that OLS with country fixed effects produces nearly unbiased estimates. However, even a sample of 140 years (the largest time frame considered in the text) is not sufficient to make the bias negligible. Unlike the case without country fixed effects, the size of σ_η is unimportant for the bias—because the estimation takes account of the variations in the η_i.

The upper part of Table A1 applies to OLS estimation with country fixed effects. Two values of the convergence rate, $\beta = 1-\gamma$, are considered, 0.10 and 0.02 per year. The emphasis is on the effect of the time dimension, T, on the mean of the estimated $\hat{\beta}$, shown in column 5. The bias is always upward, reflecting the Hurwicz channel. For example, if β=0.02 per year, the mean of $\hat{\beta}$ when T=20 (line 6) is 0.151; that is, the upward bias is dramatic. The mean of $\hat{\beta}$ falls to 0.070 when T=50 (line 7) and 0.036 when T=140 (line 10). Hence, although the bias is much reduced compared to that applying to short time series, even T=140 years is insufficient to make the bias negligible. Changing the standard deviation σ_η of the fixed effect does not impact the results (lines 4 and 9 of the table) because the fixed effects are estimated; that is, there is no omitted-variables problem here.

The results for $E(\hat{\beta})$ from Nickell's formula, shown in column 4 of Table A1, are similar to the Monte Carlo results for the mean of $\hat{\beta}$ in column 5. Some differences arise because Nickell's formula applies as the number of cross sections, N, approaches infinity. More importantly, Nickell treated the initial sample values, y_{i0}, as given, rather than allowing for a

[37] If the equation for y_{it} includes X variables—not all of which are included in the empirical specification—there would still be an omitted-variables problem.

relation with the fixed effect, η_i. This issue is more important in the estimation without country fixed effects (not considered by Nickell [1981]).

The lower part of Table A1 applies to OLS estimation without country fixed effects. The focus is on the effect of the standard deviation, σ_η, for the fixed effects. The bias in the estimate $\hat{\beta}$ is always downward, reflecting the omitted-variables channel. For example, when β=0.02, T=100, and σ_η=1 (line 15), the mean of $\hat{\beta}$ in column 5 is 0.0003. Hence, the bias is sharply downward in this case. However, the mean of $\hat{\beta}$ rises to 0.011 when σ_η=0.1 (line 16) and 0.020 when σ_η=0.01 (line 18). Thus, because the Hurwicz bias is unimportant, the estimate $\hat{\beta}$ is virtually unbiased for a σ_η that is small enough to make the omitted-variables bias unimportant.

The time dimension, T, has a nonzero but moderate effect on the results without country fixed effects (lines 13 and 17 of the table). For example, reducing T from 100 to 20 (lines 16 and 17) actually lowers the bias: the mean of the estimate $\hat{\beta}$ goes from 0.0113 to 0.0127 (but the standard error of the estimate roughly doubles). I lack intuition for the reduced bias, but analytical results analogous to Nickell's formula could probably be generated for the setting without country fixed effects.

Another approach is to consider estimation other than OLS, with or without country fixed effects. For example, Arellano and Bond (1991) use instrumental procedures, using long lags of the dependent variable as instruments. A shortcoming is that these procedures are sensitive to the dynamic structure of the error term ε_{it} with respect to serial correlation.

It would also be possible to adjust the OLS estimates to correct for bias. For estimation with country fixed effects, the adjustment would be based on the time-series dimension, T. Such an adjustment—using the results in the upper part of Table A1—would be straightforward for the model in equation (A1). However, the adjustments are sensitive to the behavior of X variables when these are included in the model (below).

B. Model with an X Variable and No Fixed Effects

Table A2 has the results for the model in equations (A2)-(A4), which has an exogenous X variable and no fixed effects. The results from OLS without country fixed effects, shown in the lower part of the table, are straightforward. If the X variable is included in the regression (lines 13, 14, 16, and 17), so that there are no omitted variables, the OLS estimates of the convergence rate, β (and also for α, the coefficient of the X variable) are essentially unbiased. These results follow because the Hurwicz bias is again unimportant when fixed effects are excluded. Moreover, the findings apply independently of the value of β (0.10 and 0.02 are considered), the length of the sample (T=100 or 20 are in the table), and of the details of the specification of the process for the X variable (because this variable is exogenous and included in the regressions). However, if the X variable is excluded from the regression (lines 15 and 18), the estimate $\hat{\beta}$ is biased downward because of the omitted-variable effect described before.

The results for OLS with fixed effects are shown in the upper part of Table A2. The conclusions with respect to $\hat{\beta}$ depend on the sample size, T, and also on the details of the process for the X variable. For example, if β=0.02 and the X process is highly persistent (ρ=0.90), the mean of $\hat{\beta}$ is 0.022 (line 7) when T=100 and 0.035 (line 8) when T=20; that is, the bias is small with a 100-year sample but moderate with a 20-year sample. However, if the X process is less persistent (ρ=0.50), the bias is larger: the mean of $\hat{\beta}$ is 0.036 (line 9) when T=100 and 0.105 (line 10) when T=20.

The results suggest two circumstances when the regressions generate estimates of the convergence rate, β, without large bias. The first is for OLS with country fixed effects when the sample is large in the time domain. For example, when β=0.02, the fixed-effects model yields a mean of $\hat{\beta}$ of 0.036 when T=140 (Table A1, line 10), and the X-model yields a mean of $\hat{\beta}$ between 0.022 and 0.036 when T=100, depending on the persistence of the X variable and whether the X variable is included in the regressions (lines 7, 9, and 11 of Table A2). The second case is for OLS without country fixed effects in the X-model when the (exogenous) X variable is included in the regressions. This framework yields a mean of $\hat{\beta}$ of 0.021 (Table A2, line 16). OLS without country fixed effects also does well in the fixed-effects model when the variation in the fixed effects is small (Table A1, row 18).

The results can be used to get reasonable bounds on the convergence rate, β, as carried out in the text. The upper bound came from the estimate with country fixed effects using the long-term data back to 1870. Because T is large, the upper bound may be reasonably tight. The lower bound came from the estimate without country fixed effects using the shorter-term data since the 1960s with a rich array of X variables.

Table A1 Monte Carlo Results for Estimated Convergence Rates Model with Fixed Effects					
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
OLS with fixed effects					
	β	**T**	**σ_η**	$\boldsymbol{E(\widehat{\beta})}$ **(Nickell formula)**	$\boldsymbol{mean(\widehat{\beta})}$ **(Monte Carlo)**
(1)	0.10	20	1	0.231	0.206 (0.014)
(2)	0.10	50	1	0.147	0.140 (0.008)
(3)	0.10	100	1	0.122	0.120 (0.005)
(4)	0.10	100	0.1	0.122	0.120 (0.004)
(5)	0.10	140	1	0.115	0.113 (0.004)
(6)	0.02	20	1	0.165	0.151 (0.014)
(7)	0.02	50	1	0.076	0.070 (0.006)
(8)	0.02	100	1	0.046	0.043 (0.004)
(9)	0.02	100	0.1	0.046	0.043 (0.004)
(10)	0.02	140	1	0.038	0.036 (0.003)
OLS without fixed effects					
(11)	0.10	100	1	--	0.0054 (0.0008)
(12)	0.10	100	0.1	--	0.085 (0.005)
(13)	0.10	20	0.1	--	0.088 (0.007)
(14)	0.10	100	0.01	--	0.100 (0.004)
(15)	0.02	100	1	--	0.0003 (0.0002)
(16)	0.02	100	0.1	--	0.011 (0.001)
(17)	0.02	20	0.1	--	0.013 (0.003)
(18)	0.02	100	0.01	--	0.020 (0.002)

Notes: $\beta=1-\gamma$ is the convergence rate in equation (A1), T is the length of the time series, and σ_η is the standard deviation of the country fixed effect. The Monte Carlo results in column 5 show the mean and standard deviation of the estimate $\hat{\beta}$ from 100 iterations of the model in equation (A1). The upper part refers to OLS estimation with country fixed effects (country dummies). The lower part refers to OLS estimation without country fixed effects. All results shown use $\sigma_\varepsilon=1$ (for the time-series shock), N (number of countries) =100, and an observation period of $\tau=1$ year. Results with 50 iterations or N=20 are similar. Results with $\tau=5$ are also similar (see n. 28). For example, with fixed effects, the mean of $\hat{\beta}$ when $\beta=0.10$, T=100, $\sigma_\eta=1$, and $\tau=5$ is 0.125 (0.006). Without fixed effects, the mean of $\hat{\beta}$ for this case is 0.0047 (0.0008). Column 4 shows the expected value of $\hat{\beta}$ from the formula in Nickell (1981, p. 1422); see n. 22. This formula applies to OLS estimation with country fixed effects.

Table A2 Monte Carlo Results for Estimated Convergence Rates Model with X variable and no Fixed Effects							
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**
OLS with fixed effects							
	β	**ρ**	**T**	**incl. X?**	**F.E. signif?**	$mean(\widehat{\beta})$ **(Monte Carlo)**	$mean(\widehat{\alpha})$ **(Monte Carlo)**
(1)	0.10	0.90	100	y	n	0.103 (0.002)	0.501 (0.005)
(2)	0.10	0.90	20	y	n	0.127 (0.007)	0.506 (0.005)
(3)	0.10	0.50	100	y	y	0.112 (0.004)	0.503 (0.008)
(4)	0.10	0.50	20	y	y	0.181 (0.013)	0.506 (0.021)
(5)	0.10	0.50	100	n	n	0.084 (0.004)	--
(6)	0.10	0.50	20	n	y	0.172 (0.015)	--
(7)	0.02	0.90	100	y	n	0.022 (0.001)	0.503 (0.005)
(8)	0.02	0.90	20	y	n	0.035 (0.005)	0.497 (0.014)
(9)	0.02	0.50	100	y	y	0.036 (0.002)	0.501 (0.009)
(10)	0.02	0.50	20	y	y	0.105 (0.011)	0.487 (0.023)
(11)	0.02	0.50	100	n	y	0.033 (0.002)	--
(12)	0.02	0.50	20	n	y	0.124 (0.013)	--
OLS without fixed effects							
(13)	0.10	0.50	100	y	--	0.100 (0.004)	0.500 (0.007)
(14)	0.10	0.50	20	y	--	0.101 (0.008)	0.499 (0.021)
(15)	0.10	0.50	100	n	--	0.069 (0.004)	--
(16)	0.02	0.50	100	y	--	0.021 (0.002)	0.500 (0.008)
(17)	0.02	0.50	20	y	--	0.021 (0.006)	0.498 (0.020)
(18)	0.02	0.50	100	n	--	0.012 (0.002)	--

Notes: β=1-γ is the convergence rate in equation (A2), α is the coefficient on the X variable, ρ is the persistence coefficient for the X variable, and T is the length of the time series. The Monte Carlo results in columns 6 and 7 show the mean and standard deviation of the estimates $\hat{\beta}$ and $\hat{\alpha}$ from 100 iterations of the model in equation (A2). The upper part refers to OLS estimation with country fixed effects (country dummies). The lower part refers to OLS estimation without country fixed effects. All results shown use $\sigma_\varepsilon=1$ (for the time-series shock), $\sigma_u=1$ (for the X shock), α=0.5, N (number of countries) =100, and an observation period of τ=1 year. Column 4 indicates whether the regressions include the X variable. Column 5 indicates whether the standard test for statistical significance of the country fixed effects is satisfied at the 0.05 level. (These results show that statistical significant of the fixed effects is not a reliable guide to the bias in the estimate of β.)