What We Have Learned About Policy and Growth from Cross-Country Regressions?

By Ross Levine and Sara J. Zervos*

Economists have been seeking to comprehend why some countries are rich and others poor for well over 200 years. A better understanding of the national policies associated with long-run growth would both contribute to our ability to explain cross country differences in per capita incomes and provide a basis for making policy recommendations that could lead to improvements in human welfare. Recently, economists have used cross-country regressions to search for empirical linkages between long-run growth and indicators of national policies (e.g., Roger Kormendi and Philip Meguire, 1985; Robert J. Barro, 1991). The large cross-country growth literature has identified various fiscal, monetary, trade, exchange-rate, and financial-policy indicators that are significantly correlated with long-run growth. Yet, Levine and David Renelt (1992) show that many of these findings are fragile to small alterations in the conditioning information set. That is, small changes in the right-hand-side variables produce different conclusions regarding the relationship between individual policies and growth. In this paper, we take stock of what the profession has learned from cross-country regression studies of policy and long-run growth.

*The World Bank, 1818 H Street, Washington, DC 20433, and Department of Economics, University of Rochester, Rochester, NY 14627, respectively. The findings, interpretations, and conclusions are the authors' own. They should not be attributed to the World Bank, its Board of Directors, its management, or any of its member countries. This research was supported by the World Bank research project "How Do National Policies Affect Long-Run Growth?" We thank Maria Carkovic, Rudiger Dornbusch, Stanley Fischer, Andrew Sheng, Mary Shirley, and David Zervos for helpful comments.

I. Limitations

To gauge what we have learned—and may potentially learn—from cross-country regressions, we should humbly face the substantial conceptual and statistical problems that plague cross-country investigations. Statistically, entries are sometimes measured inconsistently and inaccurately, and almost without exception, a person with detailed knowledge of a country can quickly identify contradictions between readily available data and what actually happened in that country. Even putting measurement difficulties aside, it is not clear that we should include vastly different countries in the same regression. Regression analysis presupposes that observations are drawn from a distinct population, but as argued by Arnold Harberger (1987), Thailand, the Dominican Republic, Zimbabwe, Greece, and Bolivia may have little in common that merits their being put in the same regression. Thus, the statistical basis upon which we draw inferences from cross-country analyses may be in doubt.

Furthermore, it is conceptually difficult to interpret the coefficients on regressions that involve data for over 100 countries averaged over 30 years during which time business cycles, policy changes, and political disturbances have influenced economic activity. Many papers interpret the coefficients as elasticities, suggesting that if a policy indicator changes by 1 percent, growth will change by a percentage corresponding to the coefficient on the policy indicator. These types of conceptual experiments should be treated skeptically, as cross-country regressions do not resolve causal issues, nor do the regressions "...describe a single piece of machinery through time" (Harberger, 1987 p. 256). Cross-country regressions should be viewed as evaluating the strength of partial correlations, and not as behavioral relationships

that suggest how much growth will change when policies change.

Finally, although economists seek a clear link between executable policy actions and long-run growth, cross-country regressions do not provide such a link. In theoretical models of policy and growth, economists typically represent policy distortions with the greek letter τ. Not only do international data sets such as the International Financial Statistics and the Robert Summers and Alan Heston (1988) data not contain data series called "τ," but it is very difficult to construct proxies that measure policy actions. Instead of measuring executable policies, cross-country regressions use policy indicators, such as the average ratio of exports to GDP or the average ratio of broad money to GDP over the past 30 years. Cross-country regressions, therefore, do not typically link executable policies with growth.[1]

The inherent statistical and conceptual obstacles to interpreting cross-country studies limit what economists can learn about policy and growth from cross-country regressions. Even if cross-country regressions yield very "strong" results, these results should be viewed as suggestive empirical regularities, not as behavioral relationships on which to measure responses to policy changes. Cross-country regressions, however, can be very useful. Along with other analytical methods, demonstrating that certain policy–growth relationships hold well across countries will influence beliefs about policy and economic performance. Similarly, beliefs about policy and growth that are not supported by cross-country evidence will tend to be viewed skeptically.

II. Extreme-Bounds Analysis: Defining "Learn"

Researchers have found that many individual indicators of monetary, fiscal, trade, exchange-rate, and financial policies are significantly correlated with long-run growth in at least one cross-country regression (see Levine and Renelt, 1991). Should we alter our beliefs regarding the relationship between growth and policy based on these findings? To answer this question, we need a methodology for evaluating the strength of these findings.

Levine and Renelt (1992) and Levine and Zervos (1993) use Edward Leamer's work on extreme-bounds analysis (EBA) to evaluate the "believability" of cross-country regressions.[2] The EBA employs a linear, ordinary-least-squares regression framework:

$$(1) \qquad \mathrm{GYP} = \boldsymbol{\beta}_i \mathbf{I} + \beta_m M + \boldsymbol{\beta}_z \mathbf{Z} + u$$

where GYP is the growth rate in GDP per capita averaged over the 1960–1989 period for a cross section of up to 100 countries, $\mathbf{I}$ is a set of base variables always included in the regression, M is the policy indicator of particular interest, and $\mathbf{Z}$ is a set of up to three variables chosen from a pool of seven policy indicators. The EBA involves varying the $\mathbf{Z}$ variables to determine whether the coefficient on the policy indicator, M, is consistently significant and of the same sign when the right-hand-side variables change. If β_m is consistently significant and of the same sign we call the result "robust"; otherwise we term the result "fragile." The EBA helps clarify the degree of confidence that one can place in the partial correlations between growth and individual policy indicators. If a policy indicator is robustly correlated with long-run growth, then one should feel more confident about its association with growth than an indicator that has a fragile link.

In Levine and Zervos (1993), we perform the EBA using a different set of $\mathbf{I}$ and $\mathbf{Z}$ variables from that used in Levine and Renelt (1992). Specifically, the base set of variables includes the log of the initial (1960) secondary-school enrollment rate, the log of initial real GDP per capita, and the number

[1]Moreover, when studies measure the average inflation rate or average tax rate over the last 30 years, they do not distinguish between, say, a hyperinflationary episode lasting a few years and sustained high inflation lasting 30 years.

[2]EBA represents one, very limited, measure of believability. Many other tests should be performed before classifying a result as strong. See William Easterly et al. (1993).

of revolutions and coups.[3] We choose this new set of **I** variables because they correspond to the "Barro regressors" that have become popular in cross-country growth regressions. We began by using the complete set of Barro (1991) control variables but dropped the log of the initial primary-school enrollment rate, the number of assassinations, and the 1960 average deviation from unity of the purchasing-power-parity index for investment goods, since the inclusion of these variables did not alter our findings.[4]

The pool of variables from which we allow the EBA to choose **Z** variables includes the average inflation rate, the standard deviation of inflation, the government fiscal surplus ratio to GDP, imports plus exports as a share of GDP, the black-market premium, and the ratio of liquid liabilities to GDP, for a total of seven possible **Z** variables.[5] When we evaluate the robustness of each *M* variable, we restrict this pool of **Z** variables by excluding any variable which, a priori, may measure the same phenomenon. For example, when a trade-policy indicator is the *M* variable, we exclude the black-market premium from the **Z** pool, as both variables may reflect aspects of international policy. By eliminating such duplication, we give each *M* variable a better chance at achieving the "robust" status.

We generally confirm the results in Levine and Renelt (1992) with this new set of **I** and **Z** variables. We find that a wide assortment of fiscal, monetary, and trade-policy indicators have very fragile relationships with long-run growth. It is very difficult to find robust partial correlations between individual policy indicators and long-run growth. Moreover, most of these policy indicators are not significantly correlated with long-run growth in the base regression that excludes the **Z** variables. Thus, researchers must *search* for the "right" combination of right-hand-side variables to find significant partial correlations between many policy indicators and growth.

We do, however, find a few robust results. Various indicators of the level of financial-sector development are robustly associated with long-run growth, confirming the findings in Robert G. King and Levine (1993a).[6] Since changes in these financial development indicators are closely associated with financial-sector policy changes in a few case studies considered in King and Levine (1993b), the link between financial-sector policies and growth deserves further study. In contrast to Levine and Renelt (1992), however, we find that the black-market exchange-rate premium is robustly related to long-run growth using the Barro regressors. These results, however, are particularly difficult to interpret because the black-market premium is often used as a general index of international distortions, and not as a trade or exchange-rate policy indicator per se.

III. Inflation: An Example

In this section, we choose one policy indicator, inflation, and move beyond simply studying whether the partial correlation between inflation and growth is robust or fragile. This illustrates additional complexities in identifying believable relationships using cross-country regressions. The EBA shows that inflation is not significantly negatively

[3]In Levine and Renelt (1992), when GYP is the dependent variable, the **I** variables are initial income, initial secondary-school enrollment, population growth, and the ratio of investment to GDP. In Levine and Renelt (1992), the robustness of the partial correlation between the investment share and each *M* variable is also investigated.

[4]In addition, we do not include the ratio of real government consumption less defense and education expenditures to GDP, which is part of Barro's (1991) set of control variables, because (i) this fiscal variable is a contemporaneous economic policy indicator and not a variable to control for initial conditions or political stability and (ii) it is averaged over the 1970–1985 period, rather than over the 1960–1989 period that we examine. We do, however, examine this fiscal-expenditure variable as an *M* variable.

[5]Levine and Renelt (1992) use inflation, the standard deviation of inflation, the ratio of government expenditures to GDP, the ratio of exports plus imports to GDP, the number of revolutions and coups, the growth rate of domestic credit, and the standard deviation of domestic credit growth.

[6]Levine and Renelt (1992) do not study financial development indicators.

correlated with long-run growth. More impressively, we could not find a combination of three **Z** variables that produced a significant negative association between growth and average inflation over the 1960–1989 period. Given the uncharacteristically unified view among economists and policy analysts that countries with high inflation rates should adopt policies to lower inflation in order to promote economic prosperity, the inability to find simple cross-country regressions supporting this contention is both surprising and troubling.[7]

The relationship between inflation and growth may, however, be discontinuous or nonlinear. Consider, for example, two alternative hypotheses about the growth–inflation relationship. First, inflation rates may have to reach extremely high levels before people significantly alter how they allocate their time and resources. Thus, marginal changes in moderate inflation rates—say, from 1–2 percent—may not be negatively associated with growth, but very high inflation rates—say, over 80 percent—may be associated with a breakdown in normal economic relationships and slower long-run growth rates. We call this the "high-π hypothesis" since the greek letter π often represents inflation.

Alternatively, people in countries with very high inflation for very long periods may become inured to inflation and develop a host of mechanisms for coping with inflation, so that growth is unrelated to very high inflation. Changes in inflation in moderate-inflation countries may, however, be negatively associated with growth, since moderate-inflation countries have not become "desensitized." We call this the "desensitize hypothesis."

These two hypotheses obviously do not cover the full range of potential explanations of the relationship between inflation and growth. Furthermore, perspectives on the inflation–growth nexus should be exemplified in models that clarify hypotheses and suggest appropriate econometric specifications. In Levine and Zervos (1993), we use simple dummy-variable procedures to identify and control for countries with very high inflation rates. We allow countries with very high inflation rates to have different slope and intercept coefficients from moderate-inflation countries. Then, we test versions of the high-π and desensitize hypotheses. This simple econometric approach allows us to illuminate a number of problems that plague broad cross-country analyses.

From a cross-country scatter plot of average inflation rates over the 1960–1989 period, the inflation rates of both 80 percent and 40 percent suggest relatively clear demarcations of where to define very high inflation. Therefore, we examine both. When we define high-inflation countries as those with average annual inflation rates over the 1960–1989 period of greater than 80 percent, we find support for the desensitize hypothesis; very high inflation is not negatively associated with growth, but increases in inflation in moderate-inflation countries are negatively linked with growth.

In contrast, when we define high inflation countries as those with average inflation greater than 40 percent, we find evidence for the high-π hypothesis; countries with very high inflation rates have slower per capita income growth, but inflation increases in moderate-inflation countries are not negatively linked to growth.

To resolve these findings, we note that two countries that fall between the 40-percent and 80-percent inflation levels are two countries that experienced extreme political disruptions, Uganda and Nicaragua. Even though we attempt to control for political stability by including measures of the number of revolutions and coups in the regressions, we repeated the tests of the desensitize and high-π hypotheses without Uganda and Nicaragua. Support for both hypotheses breaks down. Removal of both countries causes a reversion to the original results, that inflation is unrelated to growth. Thus, cross-country results must be scrutinized carefully; different researchers may find conflicting, though equally appealing,

[7]We only study the average inflation rate. This average rate may be strongly influenced by a few extreme observations and therefore may not accurately represent the inflation rate in any time period. This further illustrates the need to consider country-specific experiences.

"facts" about the relationship between individual policy indicators and growth. Unless researchers study the sensitivity of their results to small variations in the sample of countries and changes in the conditioning information set, the results should be regarded with skepticism.

IV. Conclusions

We identify two broad findings. First, cross-country regressions show that indicators of financial development are strongly associated with long-run growth. Furthermore, since changes in these financial development indicators are linked to changes in financial-sector policies (see e.g., King and Levine, 1993b), the link between financial-sector policies and long-run growth deserves more attention. Second, it is extremely difficult to identify believable links between a wide assortment of indicators of individual policies and long-run growth, although there is some evidence that general indicators of international distortions are negatively associated with growth. Most notably, we could not find robust ties between indicators of monetary or fiscal policy and long-run growth. The empirical connection between policy indicators and growth seems to be quite sensitive to slight alterations in the right-hand-side variables and to small changes in the sample of countries. Future cross-country work on the relationship between policy and long-run growth will need to develop innovative ways of improving available policy indicators, defining policy regimes, and examining interactions among policies and their effects on growth.

REFERENCES

Barro, Robert J., "Economic Growth in a Cross-Section of Countries," *Quarterly Journal of Economics*, May 1991, *106*, 407–43.

Easterly, William, Kremer, Michael, Pritchett, Lant and Summers, Lawrence H., "Good Policy or Good Luck? Country Growth Performance and Temporary Shocks," mimeo, World Bank, Washington, DC, 1993.

Harberger, Arnold, "Comment," in Stanley Fischer, ed., *NBER Macroeconomics Annual*, Cambridge MA: MIT Press, 1987, pp. 253–7.

King, Robert G. and Levine, Ross, (1993a) "Finance and Growth: Schumpeter Might Be Right," *Quarterly Journal of Economics*, 1993 (forthcoming).

_____ **and** _____, (1993b) "Finance, Entrepreneurship and Development: Theory and Evidence," mimeo, University of Rochester, 1993.

Kormendi, Roger and Meguire, Philip, "Macroeconomic Determinants of Growth: Cross-Country Evidence," *Journal of Monetary Economics*, September 1985, *16*, 141–63.

Levine, Ross and Renelt, David, "Cross-Country Studies of Growth and Policy: Some Methodological, Conceptual, and Statistical Problems," World Bank (Washington, DC) Working Paper Series No. 608, 1991.

_____ **and** _____, "A Sensitivity Analysis of Cross-Country Growth Regressions," *American Economic Review*, September 1992, *82*, 942–63.

_____ **and Zervos, Sara,** "Looking at the Facts: What We Know About Policy and Growth From Cross-Country Analysis," mimeo, World Bank (Washington, DC) Working Paper Series No. 1115, 1993.

Summers, Robert and Heston, Alan, "A New Set of International Comparisons of Real Product and Price Levels Estimates for 130 Countries, 1950–1985," *Review of Income and Wealth*, March 1988, *34*, 1–25.