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Has Latin America's post-reform growth been disappointing?

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Abstract

After years of poor macroeconomic performance, many Latin American countries undertook ambitious programs of macroeconomic stabilization and structural reform during recent years. The change in policy created high expectations for the region. Some observers question, however, whether actual growth outcomes in several Latin American countries have measured up to such expectations. This paper offers some evidence that the response of economic growth to reforms in Latin America has not been disappointing. Because of the significant changes in policies achieved in Latin America by the 1990s and in spite of the global slowdown, Latin America did well to return to its historic rate of growth of 2 percent per capita in 1991–93. Latin American growth has responded to changes in policy variables as would have been predicted by the experience of other times and places, as summarized by a panel regression spanning a large number of countries and multi-year periods from 1960 to 1993. In order to obtain consistent estimates of the parameters linking policy variables and growth, this paper uses a dynamic panel methodology that both controls for unobserved time- and country-specific effects and accounts for the likely joint endogeneity of the explanatory variables.

Keywords: Latin America; macroeconomic stabilization; dynamic panel methodology

JEL classification: O11; O54; E60

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1. Introduction

After years of poor economic performance, many Latin American countries undertook ambitious programs of macroeconomic stabilization and structural reform during recent years.[1] The change in policy created high expectations for the region. Many observers question whether actual growth outcomes in several Latin American countries have measured up to such expectations. Paul Krugman (1995) used Latin American examples to note that "the real economic performance of countries that had recently adopted Washington consensus policies... was distinctly disappointing." (p. 41) The World Bank's Vice President and Chief Economist for Latin America noted in 1995 that "after all the reforms, the efforts, and the accolades from the financial media, the region as a whole is making little progress towards breaking out of the quagmire of poverty." (Burki and Edwards, 1995). Sebastian Edwards, the aforementioned World Bank Chief Economist for Latin America, said in a separate publication about Latin America, "the results in terms of growth and social progress have not yet met expectations" (Edwards, 1995). In like vein, Dornbusch and Edwards (1995) conclude a review of Latin American (and Middle Eastern) reforms: "Even though structural reforms appear to be a necessary condition for growth, they are not a sufficient one."

Economic growth *has* been faster in the region during the nineties than during the previous decade (Fig. 1). But the growth recovery in the aggregate can be judged inadequate by several standards—in comparison with contemporaneous growth in the East Asian Miracle countries (Fig. 1 again), or with "desirable" growth rates in the region itself—that is, rates of growth sufficient to reduce the incidence of poverty, or to restore previous levels of per capita income in a reasonably short time. Would expectations of faster growth have been unreasonable, given the reforms actually implemented? Is there a puzzle why Latin American growth has not been faster? Does general disappointment about Latin American growth reflect overemphasis on particularly vivid cases, like that of Mexico?

The simplest approach to these questions would be to analyze whether reforming countries are growing faster than non-reforming ones and/or faster than before the reforms. Such an approach is complicated, however, by a number of factors. First, non-reform determinants of growth may be quite different across countries, so that reforming and non-reforming countries may have grown differently even without the reforms. Second, Latin American countries differ in reform initiation and duration, as well as in depth and breadth of the reforms. Third, we would like to evaluate how the growth payoff to a given amount of reform in Latin America in the 1990s compares to reform experiences in other regions and times.

[1] Excellent overviews of the stabilization and structural components of reform in the region are provided by Edwards, 1995.

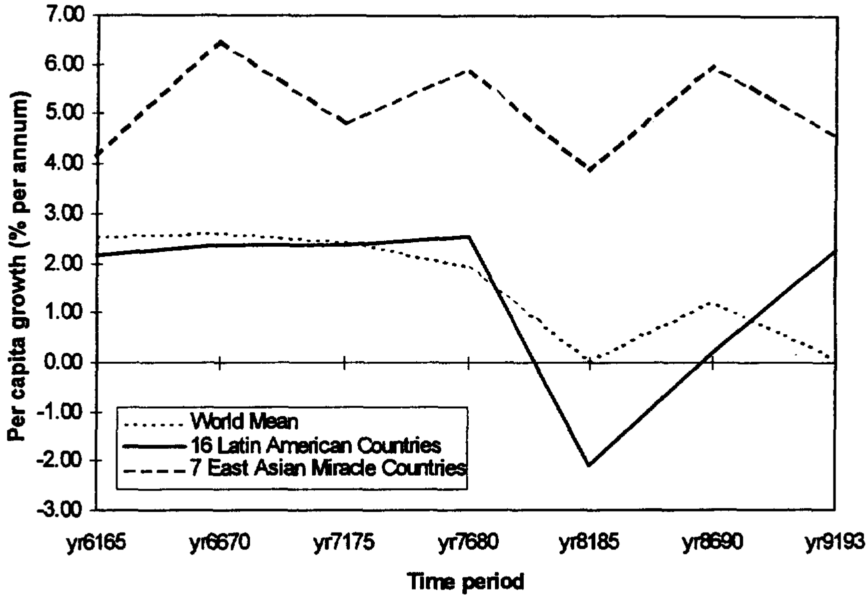

Fig. 1. Per capital growth rates by region.

These questions point toward a cross-country framework in which we relate countries' changes in growth rates to changes in economic policies, controlling for other factors and for initial conditions. Our procedure will be to estimate a growth equation based on this framework for all regions and time periods for which data are available, and then ask how incremental growth achieved by Latin America in the 1990s compares to the sample as a whole. The estimation of the average effects of reforms on growth using this approach will allow us to identify countries whose experience differs significantly from what would be predicted on the basis of the reforms they have undertaken. We will thus be able to address the question of whether there is a "growth puzzle" for specific countries. We will examine the experience of all Latin American countries that have the complete set of data on growth and policies.

This framework builds on the existing cross-country growth literature that quantifies the effects of a variety of policies on long-term growth, after controlling for non-policy variables. Previous exercises in this literature in the same spirit include Barro and Lee's (Barro and Lee, 1993a,b) examination of winners and losers in economic growth; Easterly and Levine's (Easterly and Levine, 1996) examination of the low average growth in Africa; Bouton, Kiguel, and Jones' (Bouton et al., 1994) examination of the payoffs to reform in Africa; and Corbo and Rojas' (Corbo and Rojas, 1993) and De Gregorio's (De Gregorio, 1992) examination of macroeconomic adjustment and growth in Latin America (see also the early treatment of Cardoso and Fishlow, 1989). Use of panel data similar to ours in the literature is given in Barro and Sala–i–Martin (1995) and Caselli, Esquivel, and Lefort (Caselli et al., 1995). Our framework makes a new contribution to this literature by performing an evaluation of Latin America's reforms based on the *changes* in growth performance associated with reforms, while explicitly addressing some of the econometric problems created by the first-difference specification.

We want to be clear that we are evaluating the change in Latin American growth associated with recent reforms, not the long run average growth of Latin America compared to some standard of long-run performance. The long run average growth of Latin America could be disappointing in other ways than the recent expressions of disappointment about the response of Latin growth to reform. However it is only the latter that we evaluate in this paper.

The rest of the paper is organized in three sections. Section 2 describes the empirical methodology. The results are described in Section 3, and the concluding section summarizes the implications of the exercise for the process of economic reform in Latin America.

2. Measuring the average effects of reforms on growth

2.1. Cross-section versus time series

The existing empirical literature on growth has mainly used cross-country data without much consideration of their time-series dimension. In principle, it would be possible to apply the standard techniques to our problem by relying on cross-country growth regressions based on observations averaged only over the most recent (reform) years. For our purposes, however, the alternative of employing panel estimation has several advantages. First, our question is inherently a time series question: how did growth change when policy changed? Second, because the variables of interest vary significantly over time, their time series provide a considerable wealth of information ignored in cross-sectional averages. The gain in degrees of freedom by employing panel data is particularly important when a relatively large number of explanatory variables are used, as will be necessary to characterize the multiple dimensions of reform in Latin America. Third, the use of panel data allows us to control for, and assess the importance of, time-specific effects (in the form of world economic conditions) as well as country-specific effects. Fourth, the likely endogeneity of some explanatory variables can be accounted for by using previous observations of the variables in the panel as instruments. For these reasons, the estimates of policy (and non-policy) effects on growth obtained using panel data will be more consistent and efficient than those using only cross-sectional data.

At the same time, the use of the time-series dimension presents some problems for growth regressions. The effects of policies on growth are likely to exhibit complicated dynamics, which may be obscured by temporal effects emanating, for example, from the business cycle. Cross-section regressions avoid these problems by focusing on long-run effects. We will attempt to do the same here, while exploiting the time-series variation in the data, by using panels based on five-year averages.

Our estimation strategy will be based on the following regression equation:

$$GR_{i,t} = \phi' RV_{i,t} + \psi' CV_{i,t} + \lambda' TE_t + \gamma' CE_i + \mu_i + \eta_t + \epsilon_{i,t} \quad (1)$$

where the subscript i denotes a given country; time periods are normalized so that the subscript t refers to a five-year interval; GR is the average growth rate of per capita GDP; RV is the set of variables that measure the extent of economic reform; CV is the set of control variables for which panel data are available; TE is the set of time-specific variables; CE is the set of country-specific variables; μ and η are, respectively, the unobserved country-specific and time-specific effects; and ϵ is a white-noise error term. The sets CV, TE, and CE all contain growth determinants arguably independent of the reform process. These variables control for the non-reform determinants described above.

2.2. Variables

As indicated in the introduction, the empirical cross-country growth literature has identified a number of both policy and nonpolicy variables that are correlated with growth performance across countries. Our strategy is to rely on such variables both to serve as indicators of the depth and breadth of reform as well as to control for non-reform determinants of growth performance. Operationally, the following set of variables provides a reasonably complete and objective measure of the various dimensions of economic reform implemented in Latin America (the set RV):

a. With regard to *macroeconomic stabilization*, we rely on both the log of one plus the average inflation rate and the log of the average ratio of government consumption to GDP. Lowering inflation has been the central objective of stabilization policy in Latin America. While Bruno and Easterly (1995) have pointed out that growth has little relation to inflation at rates below about 40 percent annual, Latin America has provided precisely the kind of one-time high inflation experiments whose growth effects Bruno and Easterly found to be easily detectable in panel data like that used by the current paper (Fischer, 1993 and De Gregorio, 1992 found such growth effects).[2] Government consumption is arguably a good indicator of credible and permanent fiscal adjustment, and has played a prominent role in previous empirical growth studies (Barro, 1991; Barro and Sala-i-Martin, 1995).

[2]We will also explore a dummy variable for whether inflation is above 40 percent annual.

b. Our indicator of *financial reform* is the traditional measure of financial deepening—the average ratio of broad money (M2) to GDP. The work of King and Levine (1993) has shown a strong and robust association between financial depth and subsequent growth.[3]

c. We have tried to capture *reform of the external sector*, encompassing both trade reform and liberalization of regulations governing foreign exchange transactions, by using the log of the average black market premium and the average trade share of GDP as explanatory variables. Among a large number of alternative measures of increased openness and international market orientation, the black market premium has proven to be robustly correlated with growth performance in previous studies (e.g. Levine and Zervos, 1993; Fischer, 1993; Easterly, 1994; Barro and Sala–i–Martin, 1995). The trade share has fared less well in empirical studies (e.g. Harrison, 1996), but we include it here to see if it works better under our econometric approach.

d. Finally, we have attempted to capture the effects of *other structural reforms*, such as privatization of public enterprises, the resolution of debt-overhang problems, liberalization of the foreign direct investment regime, etc., through the inclusion of the log of the average ratio of investment to GDP. Investment to GDP is also obviously endogenous to unobserved characteristics that influence growth (or possibly to growth itself), but we will see in the next section that we address endogeneity of this variable as well as all the other variables by instrumenting with previous values.

All told, then, our regression includes a total of six variables intended to measure the extent of various types of economic reform. Undoubtedly, many other variables could have been used for this purpose. Our choices were guided by the criteria that the variables chosen to represent the reform phenomenon should be sufficiently varied and extensive as to capture the diverse aspects of reform in Latin America, should be in wide use in the growth literature as indicators of policy stance, and should be available for a large group of countries, particularly in Latin America. We consider that an appropriate set of reform indicators should both capture the process of reform in Latin America—i.e., they should move in the qualitative direction associated with reform for the countries in our sample—and should explain growth. As just mentioned, judgments regarding the latter are based on performance in previous studies. With regard to the former, Table 1 presents changes in the average values of each of our reform variables for all of the Latin American countries in the sample from 1986–90 to 1991–93. Of sixteen such countries, thirteen registered a reduction in the rate of inflation, nine registered

[3] We deflate end-of-year nominal M2 by the end of year CPI, and nominal GDP by the average-of-year CPI. This deflation is an important correction to the usual practice of taking the ratio of end of year nominal M2 to nominal GDP-otherwise the M2/GDP ratio will be artificially high for high inflation countries. Though known to practitioners, we are not aware that this correction has been used in the literature before.

Table 1
Indicators of economic reform in Latin America countries and different regions, 1991–93 relative to 1986–90

	Change in ratio of government consumption to GDP	Change in rate of inflation	Change in ratio of M2 to GDP	Change in ratio of investment to GDP	Change in black market premium	Change in ratio of volume of trade to GDP
Argentina	0.90	−518.73	1.80	−0.46	−0.35	6.05
Bolivia	−0.21	−28.56	19.03	3.64	0.09	3.98
Brazil	1.78	358.80	7.58	−3.01	−0.41	4.32
Chile	−0.88	−4.69	0.08	1.33	−0.11	11.65
Colombia	0.84	−3.13	0.80	−1.05	−0.08	9.18
Costa Rica	0.06	4.70	1.83	2.00	0.09	17.25
Ecuador	−3.35	−1.73	−2.22	0.84	0.08	5.72
El Salvador	−2.40	−8.99	4.33	2.39	−1.65	13.13
Guatemala	−1.50	−3.57	2.36	3.65	−0.37	6.97
Honduras	−2.62	7.04	−0.98	6.90	−0.45	−1.48
Mexico	0.77	−55.15	10.64	2.20	−0.10	13.15
Panama	−4.28	−0.20	18.00	9.70	0	7.05
Paraguay	3.15	−11.52	7.71	−0.63	−0.02	36.21
Peru	−1.68	−585.38	2.93	−3.38	−1.09	4.99
Uruguay	0.26	−8.77	−2.56	1.56	0.09	19.05
Venezuela	−0.89	−6.68	−4.36	1.11	−1.30	5.13
Latin America[a]	−0.63	−54.16	4.19	1.67	−0.35	10.15
Africa[a]	0.59	0.91	−1.89	−0.23	−0.55	0.56
East Asia[a]	−0.56	0.80	8.21	3.86	−0.01	22.05
World[a]	−0.10	−13.24	3.21	0.92	−0.24	10.07

[a]Simple average.

declines in the share of government consumption in GDP, and fifteen experienced an increase in the volume of trade as a ratio to GDP (openness). Twelve of the sixteen achieved some extent of financial deepening by our measure, while the average black market premium declined in eleven countries. Finally, eleven countries achieved an increase in the share of investment in GDP. Overall, then, we conclude that the indicators we have chosen do seem to capture the reform phenomenon for the Latin American countries in our sample.[4]

As mentioned above, we intend to control for non-reform determinants of economic growth. Panel-data control variables (the *CV* set in regression Eq. (1)) are initial per capita GDP, average population growth, initial average number of secondary-school years of the adult population, and average terms-of-trade changes. The first three of these are standard variables in one form or another in

[4]Some of the variables we have chosen to capture the reform process can best be understood as intermediate targets of reform, rather than direct policy instruments. However, the dearth of cross-country data on more direct measures of individual policies has rendered variables such as those listed above the policy indicators of choice in the empirical growth literature.

virtually all growth regressions. Including initial per capita GDP in the model is necessary in order to control for the possibility of "mean reversion" as an explanation for improved growth performance following a recessionary period; controlling for "mean reversion" is especially important for our purposes because reforms tend to be implemented in periods of poor growth performance, and, thus, their effect on growth could otherwise be confused with the simple dynamics of growth recovery.

The effect of terms-of-trade changes on growth appears in many studies (such as Easterly, Kremer, Pritchett, Summers, (Easterly et al., 1993); Fischer, 1993; Barro and Sala–i–Martin, 1995). We are careful to include it here because of our relatively short time units (5-year periods) and because we want to distinguish growth associated with reform from that associated with external factors. (Note that except for initial per capita GDP and educational achievement, we use period averages for all variables of interest.)

As explained next, time-specific (the *TE* set) and country-specific variables (the *CE* set) are eliminated in the process of controlling for their unobserved counterparts.

For both efficient estimation and comparison purposes, we intend to use all countries for which data are available in the period 1960 to 1993. Our sample, dictated by data availability considerations, consists of an unbalanced panel of 70 countries. Time periods will be defined as non-overlapping 5-year intervals, except the last one which consists of only three years. The composition of the panel and data sources are described in Appendix B.

2.3. Econometric procedures

The estimation of the parameters of interest poses two econometric problems that our methodology can address: the presence of unobserved effects and the likely endogeneity of some of the regressors. Controlling for unobserved time- and country-specific effects is necessary because they may be correlated with the right-hand side variables, and thus bias the coefficients if omitted. The unobserved time-specific effects are controlled for by using time-period dummies; this entails the elimination of information related to those variables that vary across time periods but not across countries. After controlling for time specific effects, regression Eq. (1) can be rewritten as follows:[5]

[5]Renaming the sets of variables as in Eq. (2) makes the explanation of the econometric methodology, especially in the appendix, easier. Eqs. (1) and (2) are related as follows. Concerning the dependent variable, if we normalize the period length to one, the growth rate, $GR_{i,t}$, in Eq. (1) is equal to the log difference of per capita GDP, $y_{i,t}-y_{i,t-1}$, in Eq. (2). Concerning the explanatory variables, initial per capita GDP, $y_{i,t-1}$, in Eq. (2) belongs to the set of panel control variables, $CV_{i,t}$, in Eq. (1); lastly, the rest of control variables and all reform variables, $RF_{i,t}$, are included in the set $x_{i,t}$ in Eq. (2).

$$y_{i,t} - y_{i,t-1} = (\alpha - 1)y_{i,t-1} + \beta' x_{i,t} + \mu_i + \epsilon_{i,t} \tag{2}$$

where y is the natural logarithm of per-capita GDP, x is the set of explanatory variables for which time and cross-sectional data are available, and the time periods are normalized so that the time subscript t refers to a five-year interval.

Following Anderson and Hsiao's (Anderson and Hsiao, 1981) procedure to account for unobserved country-specific effects, all variables in Eq. (2) are first-differenced. This eliminates not only the unobserved country-specific effects but also all variables for which only cross-sectional information is available. Using differences has an appealing intuition when we are trying to answer the question, "how did growth respond to reform?", since we are actually relating *changes* in growth to *changes* in policy.

After first-differencing and rearranging the terms of the dependent variable, regression Eq. (2) becomes,

$$y_{i,t} - y_{i,t-1} = \alpha(y_{i,t-1} - y_{i,t-2}) + \beta(x_{i,t} - x_{i,t-1}) + (\epsilon_{i,t} - \epsilon_{i,t-1}) \tag{3}$$

Note that first-differencing introduces a correlation between the new error term and the differenced lagged-dependent variable. Therefore, OLS estimation of Eq. (3) would produce biased results, even when the set of variables x is strictly exogenous. Assuming that the $\epsilon_{i,t}$ are serially uncorrelated, that is, $E(\epsilon_{i,t}\epsilon_{i,s})=0$ for $t \neq s$, values of y lagged two periods or more are valid instruments in the equations in first differences.

The second econometric problem to be addressed is the likely endogeneity of some of the regressors. Given that the problem of reverse causation applies to most variables in the set x, assuming that they are strictly exogeneous would lead to inconsistent estimation. Assume, rather, that x are only weakly exogenous in the sense that $E(x_{i,t}\epsilon_{i,s})=0$ for $s>t$. Then, values of x lagged two periods or more are valid instruments in the equations in first differences. (In actual estimation, we assume that all variables are only weakly exogenous except for two, which are taken as strictly exogenous; they are the average number of secondary school years in the adult population and the change in the terms of trade.)

The assumptions that the error term is serially uncorrelated and that the explanatory variables are weakly exogenous imply a set of moment restrictions that can be used in the context of the Generalized Method of Moments (GMM) to generate consistent and efficient estimates of the parameters of interest. This methodology, more fully described in Appendix A, follows work by Chamberlain (1984), Holtz–Eakin, Newey, and Rosen (Holtz–Eakin et al., 1988), and Arellano and Bond (1991) on dynamic panel data estimation.

The consistency of the GMM estimator depends on whether lagged values of income and the other explanatory variables are valid instruments in the growth regression. A necessary condition for the validity of such instruments is that the error term $\epsilon_{i,t}$ be serially uncorrelated. To address these issues we present two specification tests, suggested by Arellano and Bond (1991). The first is a Sargan test of over-identifying restrictions; it tests the overall validity of the instruments by analyzing the sample analog of the moment conditions used in the estimation process. The second test examines the hypothesis that the error term in the differenced regression, $\epsilon_{i,t}-\epsilon_{i,t-1}$, is not second-order serially correlated, which implies that the error term in the level regression, $\epsilon_{i,t}$, is not serially correlated.[6] Under both tests, failure to reject the null hypothesis gives support to our model.

3. Results

3.1. Interpretations with and without investment

We estimated two versions of Eq. (3), with and without the investment variable. The reason is that the interpretation of the role of this variable is problematic even after we address its endogeneity. Including investment in the equation also has implications for the interpretations given to the coefficients of the remaining variables. We can think of the investment variable as capturing the effects on growth through the investment channel of all reforms, including those already in the equation. The coefficients on the included policy variables in the regression with investment included would then indicate the contribution of the relevant type of reform to productivity growth. Investment could also be capturing the effects of reforms that are difficult to quantify and are not included in the regression, like privatization, deregulation of labor markets, and so on. However, investment could conceivably be changing for exogenous reasons unrelated to reform, so we could be miscalculating the growth associated with reform when we call investment a reform variable. Hence we define the set of reform variables with and without investment.

The estimation results are presented in Table 2. Column (1) presents the results of the regression including the investment ratio; column (2) presents those obtained when the investment ratio is excluded. The key finding is that the variables we have identified perform very well in explaining changes in cross-country growth performance over five-year periods in our sample. In all cases, the policy and control variables have the theoretically-expected sign, and except for the ratio of government consumption to GDP, are statistically significant at

[6]Actually, lack of second-order serial correlation in the differenced residual is also consistent with the level residual following a random walk; in this case, however, there will also be no first-order serial correlation in the differenced residual. Tests of first-order autocorrelation applied to our data strongly reject the possibility of a random-walk level residual.

Table 2
Growth improvement determinants[a]

Dependent Variable	Real per capita GDP Growth	
	(1)	(2)
Volume of trade/GDP[b]	2.50	1.24
	(7.13)	(3.35)
Gov. consumption/GDP[b]	−0.46	−1.33
	(−1.32)	(−3.68)
Inflation[c]	−3.35	−5.33
	(−9.23)	(−16.43)
M2/GDP[b]	1.71	0.50
	(4.44)	(1.33)
Investment/GDP[b]	4.07	–
	(10.55)	
Initial GDP[b]	−4.75	−5.61
	(−9.03)	(−13.76)
Average Years of Secondary Schooling	0.12	0.26
	(1.24)	(2.17)
Black Market Premium[c]	−1.12	−1.92
	(−3.71)	(−6.49)
Terms of Trade Growth	7.76	7.39
	(9.39)	(12.04)
Population Growth	−0.99	−0.64
	(−11.93)	(−11.04)
Constant Term	−0.07	0.72
	(−0.33)	(3.76)
Period Dummy, 1991–93[d]	−1.66	−1.97
	(−8.13)	(−12.83)
Tests of GMM consistency (p-values):		
Sargan test	0.59	0.41
Serial-correlation test[e]	0.67	0.64
R^2	0.46	0.35
Std. error (for last period's differenced residuals)	3.38	3.50
Wald test of joint significance (p-value)	0.00	0.00
# of observations	352	355
Mean of dependent variable	1.84%	1.85%

[a] t-statistics given in parenthesis.
[b] In the regression, this variable is included as log (variable).
[c] In the regression, this variable is included as log (1 + variable).
[d] Dummies are included for all time periods in the regression in first differences.
[e] The null hypothesis is that the errors in the first-difference regression exhibit no second-order serial correlation, that is, $E((\epsilon_{i,t}-\epsilon_{i,t-1})(\epsilon_{i,t-2}-\epsilon_{i,t-3}))=0$.

conventional significance levels.[7] The procedure we are using improves the precision of estimates compared to conventional OLS growth regressions, so we actually find significant results on variables like openness that do not generally survive statistical tests in growth regressions. Overall, the explanatory power of the regression is 0.46 when investment is included and 0.35 when it is not.

3.2. Coefficient estimates

For our purposes, the magnitudes of the parameter estimates are of central importance. In what follows, we compare our results (in the model where investment is included) with those found elsewhere in the growth literature. In particular, we focus on two recent studies that also relied on panel data—those of Barro and Sala-i-Martin, 1995 (henceforth B–SM) and Caselli, Esquivel, and Lefort, (Caselli et al., 1995) (henceforth C–E–L). They are two of the best and most recent studies on growth using panel data. B–SM use a "random-effect" model, which assumes that the unobserved country-specific effects are uncorrelated with the explanatory variables. If, as we believe, these country-specific effects are correlated with the regressors, B–SM's estimates will be biased (upwards if the partial correlation between the regressor and growth-enhancing specific effects is positive, and vice versa). C–E–L's estimation procedure (based on time-differencing and the application of the generalized method of moments) is very similar to ours. B–SM consider two ten-year periods: 1965–75 and 1975–85 (data for 1960–65 and 1970–75 are respectively used as instruments for the two ten-year periods); and C–E–L consider five five-year periods from 1960 to 1985. Both B–SM and C–E–L attempt to account for possible joint endogeneity by using previous observations of each endogenous regressor as instruments for it.

Beginning with the policy variables, we are able to compare our results with those in the previous studies for four of the five policy variables. We estimated the coefficient of the ratio of government consumption to GDP as −0.46. Not being statistically significant, this estimate falls between the negative estimate reported by B–SM and the positive one by C–E–L. The estimated coefficient of the black-market premium on foreign exchange, −1.12, is of the same sign but smaller in absolute value than the estimates in B–SM and C–E–L, both of which

[7] When we try a dummy variable for inflation above 40 percent instead of the continuous inflation variable, we find it to be significant in a one-sided test at the 5 percent level in the regression without investment and insignificant in the regression with investment. Bruno and Easterly, 1995 show stronger results with such a dichotomous treatment of inflation when they test for growth differences between the high-inflation and low-inflation periods within each country.

are around -3.0.[8] Our estimated coefficient of 1.71 for financial depth is similar to B–SM's 1.6. Finally, we estimated a coefficient of 4.07 for the share of investment in GDP; this value is significant and somewhat larger in size than C–E–L's estimate. B–SM also find a positive but smaller and statistically insignificant estimate. B–SM claim that their result shows that the high positive correlation between investment and growth is due to causation from (expected) growth to investment and not the reverse. C–E–L's and our estimate show that there is also causation from investment to growth.

Turning to the control variables, the estimated coefficient of initial per capita GDP, -4.75, implies a convergence rate of 5.42% per year and, thus, a half life of 12.8 years. The "traditional" convergence rate found in papers based on cross-sectional regressions, such as Barro and Sala–i–Martin (1992) and Mankiw, Romer, and Weil (Mankiw et al., 1992), is 2.0%. Convergence rates estimated through panel-data regressions are higher than the "traditional" rate: B–SM's estimate is 3.0%, Loayza's (Loayza, 1994) estimate is 4.94%, and C–E–L's estimate is 10%. We obtained a larger (in absolute value) estimate of the effect of population growth (-0.99), than did B–SM (-0.63). Finally, with regard to changes in the terms of trade, our estimated effect, associated with a coefficient of 7.76 is in between those of C–E–L (5.66) and B–SM (11.0).

These comparisons show how sensitive the coefficient estimates are to the estimation procedure and to time periods. We believe our procedure is preferable to that of B–SM because of likely correlation between country effects and the righthand side variables, and preferable to C–E–L for our purposes because of the inclusion of the reform-relevant last 8 years (1985–93). We acknowledge some concern about the stability of the relationships we are estimating. This instability is reminiscent of the results by Levine and Renelt (1992) about the sensitivity of coefficients to regression specification. As in Levine and Renelt, however, the instability comes about because of difficulty in separating out effects of different policies; the estimated aggregate effects of policy packages are more stable than the estimated effect of each policy in isolation.

3.3. Validity of the instruments

As explained in Section 2, the validity of lagged values of income and the explanatory variables as instruments is crucial to the consistency of the GMM

[8] There are two reasons why our estimate is smaller: First, we include inflation in the regression, which is positively correlated with the black-market premium (the correlation coefficient is about 0.23). Second, they only use data up to 1985; when we performed the estimation with data from 1960 to 1985, we found an estimated coefficient of about -2.5; that is, the partial correlation between the black-market premium and growth in the last two periods (1986–1990 and 1991–1993) is weaker than in the previous five periods. For a given change in each of these variables in the reforming countries, then, our results would generate smaller effects of refoms on growth than would be obtained from previous estimates.

Table 3
Decomposition of changes in growth rates from 1986–90 to 1991–93—investment included

Country	Actual change in growth rates	Predicted change in growth rates	Contribution to predicted change in growth rates from			Regression residuals
			Six reform variables	Time effect[a]	Other variables	
Argentina	7.464	5.619	6.304	−1.73	1.044	1.845
Bolivia	1.561	2.836	3.308	−1.73	1.258	−1.275
Brazil	−0.252	−1.643	−0.344	−1.73	0.43	1.392
Chile	0.901	−1.501	0.944	−1.73	−0.715	2.402
Colombia	−0.049	0.109	0.644	−1.73	1.195	−0.157
Costa Rica	0.923	−0.41	0.712	−1.73	0.608	1.333
Ecuador	1.584	0.069	0.291	−1.73	1.507	1.516
Guatemala	1.197	1.187	2.142	−1.73	0.775	0.01
Honduras	1.141	0.698	1.393	−1.73	1.035	0.443
Mexico	1.116	2.424	3.393	−1.73	0.761	−1.308
Panama	9.178	3.722	3.34	−1.73	2.112	5.455*
Peru	4.504	5.399	5.289	−1.73	1.84	−0.895
Paraguay	−0.92	0.927	1.727	−1.73	0.93	−1.847
El Salvador	1.714	1.232	2.906	−1.73	0.056	0.482
Uruguay	0.255	−1.077	1.358	−1.73	−0.706	1.333
Venezuela	2.261	−0.408	1.346	−1.73	−0.024	2.668
Regional Average	2.036	1.199	2.172	−1.73	0.757	0.837

[a]The time effect corresponding to the last period is equal to the overall constant plus this period's dummy coefficient.
*Statistically different from zero at the 0.10 level of significance on a one-tail test.
**Statistically different from zero at the 0.05 level of significance on a one-tail test.

estimator. According to the Sargan and serial-correlation test statistics presented in Table 2, our econometric model specification cannot be rejected. In fact, in the regression that includes investment as an explanatory variable, the p-value for the Sargan test is 0.59, and that for the second-order serial correlation test is 0.67. When investment is excluded, the p-value for the Sargan test, 0.41, is lower but still above standard significance levels, and the p-value for the second-order serial correlation test, 0.64, clearly supports the assumption of lack of serial correlation in the growth-regression error term.[9]

3.4. Is Latin America under-achieving?

To calculate the contribution of the reforms to recent growth performance in Latin America, we calculated fitted values using both versions of Eq. (1) for each

[9]As mentioned in a previous footnote, the hypothesis of no first-order serial correlation in the differenced residual is strongly rejected, thus eliminating the possibility that the level residual follow a random walk.

Table 4
Decomposition of changes in growth rates from 1986-90 to 1991-93—investment excluded

Country	Actual change in growth rates	Predicted change in growth rates	Contribution to predicted change in growth rates from			Regression residuals
			Five reform variables[a]	Time effect[b]	Other variables	
Argentina	7.464	8.818	9.036	−1.246	1.029	−1.354
Bolivia	1.561	1.384	1.55	−1.246	1.081	0.177
Brazil	−0.252	−2.246	−1.222	−1.246	0.222	1.994
Chile	0.901	−1.584	0.722	−1.246	−1.059	2.485
Colombia	−0.049	−0.023	0.52	−1.246	0.704	−0.026
Costa Rica	0.923	−1.076	−0.075	−1.246	0.246	1.999
Ecuador	1.584	0.512	0.465	−1.246	1.293	1.072
Guatemala	1.197	0.563	1.292	−1.246	0.518	0.635
Honduras	1.141	0.01	0.538	−1.246	0.719	1.13
Mexico	1.116	2.161	2.801	−1.246	0.607	−1.045
Panama	9.178	1.471	0.607	−1.246	2.11	7.707**
Peru	4.504	9.103	8.75	−1.246	1.599	−4.598*
Paraguay	−0.92	−0.049	0.639	−1.246	0.559	−0.871
El Salvador	1.714	1.766	2.795	−1.246	0.218	−0.052
Uruguay	0.255	−1.54	0.521	−1.246	−0.814	1.795
Venezuela	2.261	0.544	1.994	−1.246	−0.203	1.716
Regional Average	2.036	1.238	1.933	−1.246	0.552	0.798

[a]Set of reform variables does not include Investment/GDP.
[b]The time effect corresponding to the last period is equal to the overall constant plus this period's dummy coefficient.
*Statistically different from zero at the 0.10 level of significance on a one-tail test.
**Statistically different from zero at the 0.05 level of significance on a one-tail test.

of the sixteen Latin American countries in our sample. These fitted values are reported in Table 3 for the version of Eq. (1) that includes the investment ratio, and in Table 4 for the version that does not. The short answer to the question of whether Latin America's post-reform growth has been disappointing is given at the bottom of the last column in each table, which provides the average value of the residuals from the estimated regression for the sixteen countries in the table. The answer is no.[10] The average value of the growth residual for the region is positive, indicating that, when the effects of the reforms are estimated on the basis of international experience over the entire 1960–93 period, and when "permanent" characteristics of the countries themselves as well as the state of the international

[10]Another robustness check that we performed was to exclude, in the coefficient estimation stage, the Latin American countries in the last period, which is the focus of this paper. This is to check whether the Latin American countries in the last period are highly influential in the regression. If they had been highly influential the ability of the regression to describe Latin America's experience in the last period would have been in some sense tautological. However, we find that the regression results are very similar when the Latin American countries in the last period are excluded, ruling out this possibility.

economy are controlled for, Latin American countries have performed on average better than would have been expected.[11] Thus the growth puzzle for the region as a whole, if there is one, would be why it grew so rapidly, rather than so slowly, in the immediate aftermath of economic reform. However, a more appropriate conclusion for the region as a whole may be that there is no puzzle at all, since the average estimated residual for the sixteen countries is not significant in either table.

The experience of individual countries is of independent interest. Of the sixteen Latin American countries in the worldwide sample, eleven produced positive residuals in Table 3, and ten in Table 4. No negative residual was found to be significantly different from zero when the investment ratio was included in the regression, and only one, in the case of Peru, when the investment ratio was excluded. Thus the regional average reflects substantial uniformity across countries. The largest growth improvements were predicted for Argentina and Peru, two relatively recent and ambitious reformers. By contrast Chile, a very early reformer with little additional reform in 1991–93, was predicted to experience a growth slowdown in 1991–93, albeit one entirely driven by a less favorable international environment. It is interesting to note that Mexico, whose slow growth during recent years probably triggered much of the concern with growth in the region, did grow slower than predicted in Tables 3 and 4, although neither residual is significantly negative.

3.5. Comparing Latin America with the East Asia Miracle, 1991–93

Latin America is often unfavorably compared with East Asia. We can utilize our equation to see how Latin America's change in growth in 1991–93 compared to East Asia's, and how much of the relative change in growth between the two regions is explained by their respective policy changes.

In Table 5, we show a comparison between the East Asian Miracles and Latin America of the changes in growth and in country characteristics for 1991–93 on the right-hand side on our growth regressions in Table 2. (The six East Asian Miracle countries over which we average—because they have all the necessary data—are Indonesia, Japan, Korea, Malaysia, Singapore, and Thailand. Latin America means the average for the sixteen countries from Table 3.) Two facts jump out from the first two columns of Table 5. First, Latin America closed the growth gap in 1991–93 compared to 1986–90, since Latin America's growth went up and East Asia's growth went down. Latin American growth was still below East Asia's in 1991–93 (Fig. 1), but not as far below as in the previous period. Second, Latin America was not alone in reforming. The East Asian miracles also

[11] Bruno and Easterly (1995) have a similar, even stronger finding for countries stabilizing from high inflation: after stabilization, growth was significantly above both the world average growth and the country's own pre-crisis average growth.

Table 5
Explaining the difference in growth-rate changes between East Asia and Latin America (period 1991–93 compared to 1986–90)

	Average change between the periods 1986–90 and 1991–93		Difference East Asian miracles – Latin America	Predicted difference in growth-rate changes: East Asian Miracles – Latin America	
	East Asian miracles	Latin America		Including investment	Excluding investment
Per capita GDP growth	−1.12	2.04	−3.16	−2.77	−3.42
Policy indicators (total)				−1.15	−1.63
Volume of trade/GDP	14.58*	20.43*	−5.85	−0.15	−0.07
Government consumption/GDP	−5.27*	−5.10*	−0.17	0.00	0.00
Inflation rate	0.55*	−20.61*	21.16	−0.71	−1.13
M2/GDP	12.66*	17.13*	−4.47	−0.08	−0.02
Black market premium	−0.88*	−22.14*	21.26	−0.24	−0.41
Investment/GDP	10.55*	10.03*	0.52	0.02	–
Other determinants of growth (total)				−1.63	−1.79
Initial GDP per capita	28.65*	0.22*	28.43	−1.35	−1.59
Average number of secondary-school years in the labor force (initial)	0.20	0.14	0.06	0.01	0.02
Percent change in terms of trade	0.68	1.75	−1.07	−0.08	−0.08
Population growth	−0.42	−0.62	0.20	−0.20	−0.13

*Average percentage change (log difference) from 1986–90 to 1991–93. As in the estimation regression, the variables inflation and black market premium are presented as one plus the respective rate.

achieved increases in trade volume, financial depth, and investment ratios to GDP in 1991–93. However, Latin America's reduction in inflation and in the black market premia had no counterpart in East Asia, since East Asia did not have high inflation or a high black market premium in the previous period.

On balance, the policy changes in Latin America relative to East Asia correctly predict that Latin America's growth should have risen relative to East Asia's in 1991–93. The strongest effect comes from Latin America's reduction in inflation, with another strong effect from its reduction in the black market premium. Another important, non-policy determinant of growth is the change in initial level of income, which is strongly positive in East Asia and close to zero in Latin America. According to the standard convergence result, the increase in initial income was a disadvantage for East Asia in 1991–93.

Adding all effects together, our estimated equation explains most or all of the fall in the East Asia–Latin America growth differential. Our results suggest that there is nothing "miraculous" about the relative growth performances of East Asia and Latin America during the period of Latin American reform. Latin America's growth performance is *not* falling short relative to the degree of policy change, even compared to the "miracle" economies of East Asia. If Latin American reform did not close the gap with East Asia even more strongly, it was because financial depth, openness, and investment were also improving in East Asia.

4. Conclusions

The response of economic growth to reforms in Latin America has not been disappointing. Latin American growth has responded to changes in policy variables as would have been predicted by the experience of other times and places, as summarized by a panel regression spanning a large number of countries and (mostly) 5-year periods from 1960 to 1993. Part of the perception of disappointing growth in Latin America in the 1990s may really be reflecting the disappointing growth in the world as a whole, since the 1990s have registered poor growth rates in all countries on average, including reforming economies, non-reforming economies, and those that did not need to reform. Latin America did well to return to its historic rate of growth of 2 percent per capita in 1991–93 in spite of the global slowdown. This growth recovery was associated with the significant changes in policies achieved in Latin America by the 1990s.

Another part of the perception of disappointing growth may be because the standard of comparison may be not the *average* performance in the worldwide sample, but the *maximum* performance in the worldwide sample—namely, the East Asian Miracles. But even if East Asia is taken as the standard of comparison, the predicted change in the East Asia–Latin America growth gap is the right size and sign. Latin America's growth rose relative to East Asia's because it reformed more (since it had more to reform).

Perhaps when we compare Latin America's *policy* improvement with East Asia's, some disappointment is valid. Still we wonder how useful are the incessant policy comparisons of any and all industrial and developing regions with the best performing one, East Asia. Economic policy in the world as a whole cannot be like Garrison Keillor's Lake Woebegon, "where all the children are above average." By normal standards, Latin America's policy reforms and accompanying growth recovery have been an impressive achievement.

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Appendix A

Econometric procedure.[12]

Although the data used for estimation consist of an *unbalanced* panel, for expositional purposes consider a data set that consists of N individual time series, each having T periods.

Consider regression Eq. (3),

$$y_{i,t} - y_{i,t-1} = \alpha(y_{i,t-1} - y_{i,t-2}) + \beta(x_{i,t} - x_{i,t-1}) + (\epsilon_{i,t} - \epsilon_{i,t-1}) \tag{A1}$$

The error term and the lagged-dependent variable are correlated by construction. Therefore, OLS estimation produces biased results, even when the set of variables x is strictly exogenous. Assuming that the $\epsilon_{i,t}$ are serially uncorrelated, that is, $E(\epsilon_{i,t}\epsilon_{i,s}) = 0$ for $t \neq s$, values of y lagged two periods or more are valid instruments in the equations in first differences. Therefore, for $T \geq 3$, the model implies the following linear moment restrictions:

$$E[(\epsilon_{i,t} - \epsilon_{i,t-1})y_{i,t-j}] = 0 \quad (j = 2, \ldots, t-1; t = 3, \ldots, T) \tag{A2}$$

Given that the problem of reverse causation applies to most variables in the set x, assuming that they are strictly exogeneous would lead to inconsistent estimation. Assume, rather, that x are only weakly exogenous in the sense that $E(x_{i,t}\epsilon_{i,s}) = 0$ for $s > t$. Then, values of x lagged two periods or more are valid instruments in the equations in first differences. Therefore, for $T \geq 3$, the model implies the following additional linear moment restrictions:

$$E[(\epsilon_{i,t} - \epsilon_{i,t-1})x_{i,t-j}] = 0 \quad (j = 2, \ldots, t-1; t = 3, \ldots, T) \tag{A3}$$

Hansen (1982) and White (1982) propose an optimal estimator, the Generalized Method of Moments (GMM) estimator, based only on moment restrictions, that is,

[12] The presentation in the appendix follows Arellano and Bond (1991) and Caselli, Esquivel, and Lefort (Caselli et al., 1995).

in the absence of any other knowledge concerning initial conditions or the distributions of the $\epsilon_{i,t}$ and the μ_i. The moment Eqs. (A4) and (A5) can be written in vector form as $E[Z_i' v_i]=0$, where $v_i=((\epsilon_{i,3}-\epsilon_{i,2})\dots(\epsilon_{i,T}-\epsilon_{i,T-1}))'$ and Z_i, the instrument matrix, is a matrix of the form $Z_i=\text{diag }(y_{i,1}\dots y_{i,s}, x_{i,1}\dots x_{i,s})$, $(s=1,\dots, T-2)$. Note that the number of columns of Z_i, say M, is equal to the number of available instruments. Following Hansen (1982), the form of the GMM estimator of the $k\times 1$ coefficient vector $\theta=(\alpha\beta')'$ is given by

$$\hat{\theta}=(\bar{X}'ZA^{-1}Z'\bar{X})^{-1}\bar{X}'ZA^{-1}Z'\bar{y} \tag{A4}$$

where a bar above a variable denotes that it is in first differences; $\bar{X}$ is a stacked $(T-2)N\times k$ matrix of observations on $\bar{x}_{i,t}'$ and $\bar{y}_{i,t-1}'$; $\bar{y}$ is a stacked $(T-2)N\times 1$ vector of $\bar{y}_{i,t}'$; $Z=(Z_i'\dots Z_N')'$ is a $(T-2)N\times M$ matrix; and A is any $M\times M$, symmetric, positive definite matrix.

For arbitrary A, a consistent estimate of the asymptotic variance-covariance matrix of $\hat{\theta}$ is given by

$$AVAR(\hat{\theta}) = N(\bar{X}'ZA^{-1}Z'\bar{X})^{-1}\bar{X}'ZA^{-1}\left(\sum_{i=1}^{N} Z_i'\hat{v}_i\hat{v}_i'Z_i\right)A^{-1}Z'\bar{X}(\bar{X}'ZA^{-1}Z'\bar{X})^{-1} \tag{A5}$$

The most efficient GMM estimator for θ is obtained when the matrix A is chosen such that A is $V=E[Z_i'v_iv_i'Z_i]$, that is, when A is equal to the variance–covariance matrix of the moment conditions. This variance–covariance matrix can be consistently estimated using the residuals obtained from a preliminary, consistent estimation of θ.

Following this idea, Arellano and Bond (1991) suggest a two-step estimation procedure. In the first step, it is assumed that the $\epsilon_{i,t}$ be independent and homoskedastic both across units and over time; under these assumptions, the optimal choice of A is, without loss of generality, $A_1=(1/N)\ \Sigma_{i=1}\ Z_i'HZ_i$, where H is a $(T-2)$ square matrix that has twos in the main diagonal, minus ones in the first subdiagonals, and zeroes otherwise. In the second step, the asumptions of homoskedasticity and independence across units are relaxed. The residuals obtained in the first step are used to construct a consistent estimate of the variance–covariance matrix of the moment conditions. This matrix, say A_2, becomes then the optimal choice of A and is used to reestimate the coefficients of interest. Clearly, $A_2=(1/N)\ \Sigma_{i=1}\ Z_i'\hat{v}_i^1\hat{v}_i^{1\prime}Z_i$, where $\hat{v}_i^1$ are the residuals estimated in the first step.

Specification tests

Residual serial correlation test

The consistency of the proposed GMM estimator depends crucially on the $\epsilon_{i,t}$

being serially uncorrelated. Since the $v_{i,t}$ are first differences of $\epsilon_{i,t}$, the consistency of the GMM estimator does not require that $E(v_{i,t}\ v_{i,t-1})$ be zero; however, consistency does hinge heavily on the assumption that there is no second-order serial correlation in the residual of the regression in first differences, that is, $E(v_{i,t}\ v_{i,t-2})=0$.

Consider the following notation $\hat{v}(t)_i \equiv [\hat{v}_{i,3}, \ldots, \hat{v}_{i,T}]'$, $\hat{v}(t-2)_i \equiv [\hat{v}_{i,1}, \ldots, \hat{v}_{i,T-2}]'$, $\hat{v}(t) \equiv [\hat{v}(t)_1, \ldots, \hat{v}(t)_N]'$, $\hat{v}(t-2) \equiv [\hat{v}(t-2)_1, \ldots, \hat{v}(t-2)_N]'$. The statistic

$$m_2 = \frac{\hat{v}(t-2)'\hat{v}(t)}{Q} \tag{A6}$$

is standard normal (Q serves as the standardization factor) and can be used as a test of the null hypothesis that the residuals in the first-difference regression are not second-order serially correlated, that is, $E[v_{i,t}v_{i,t-2}]=0$.

Sargan test

The second specification test is based on a Sargan test for over-identifying restrictions. The null hypothesis of the Sargan test is that the instruments are not correlated with the residuals in the first-difference regressions, that is, $E[Z_i' v_i]=0$. The test is based on the following statistic,

$$s = \hat{v}'Z\left(\sum_{i=1}^{N} Z_i'\hat{v}_i\hat{v}_i'Z_i\right)^{-1} Z'\hat{v} \tag{A7}$$

where $\hat{v} \equiv [\hat{v}_i', \ldots, \hat{v}_N']'$ consists of the residuals estimated in the second stage. Under the null hypothesis, the asymptotic distribution of the statistic s is χ^2 with $M-k$ degrees of freedom. As mentioned above, M is the number of instruments (equal to the number of columns of Z) and k is the number of explanatory variables.

Appendix B

Data

Country coverage

The following is the list of countries covered in our study. Since the panel data set is unbalanced, we also indicate the time periods for which observations are available in each of the 70 countries.

Country	1961–65	1966–70	1971–75	1976–80	1981–85	1986–90	1991–93
Algeria					x	x	x
Argentina	x	x	x	x	x	x	x
Australia	x	x	x	x	x	x	x
Austria	x	x	x	x	x	x	x
Bangladesh				x	x	x	x
Belgium	x						
Bolivia	x	x	x	x	x	x	x
Botswana						x	
Brazil	x	x	x	x	x	x	x
Cameroon		x	x	x	x	x	
Canada	x	x	x	x	x	x	x
Central African Rep.		x	x	x	x	x	x
Chile	x	x	x	x	x	x	x
Colombia	x	x	x	x	x	x	x
Congo						x	
Costa Rica	x	x	x	x	x	x	x
Cyprus			x	x	x	x	x
Denmark	x	x	x	x	x	x	x
Dominican Rep.	x	x	x	x	x	x	
Ecuador	x	x	x	x	x	x	x
Egypt				x	x		x
El Salvador	x	x	x	x	x	x	x
Finland	x	x	x	x	x	x	x
France	x	x	x	x	x	x	x
Gambia				x	x	x	x
Germany	x	x	x	x	x	x	x
Ghana		x	x	x	x	x	x
Greece	x	x	x	x	x	x	x
Guatemala	x	x	x	x	x	x	x
Haiti		x	x	x	x	x	
Honduras	x	x	x	x	x	x	x
Hong Kong				x	x		
India	x	x	x	x	x	x	x
Indonesia		x	x	x	x	x	x
Ireland	x	x	x	x	x	x	
Israel	x	x	x	x	x	x	x
Italy	x	x	x	x	x	x	x
Jamaica	x	x	x	x	x	x	x
Japan	x	x	x	x	x	x	x
Jordan					x*	x*	x*
Kenya		x	x	x	x	x	x
Korea		x	x	x	x	x	x
Malawi					x	x	x
Malaysia	x	x	x	x	x	x	x
Mali							x
Mauritius				x	x	x	x
Mexico	x	x	x	x	x	x	x
Netherlands	x	x	x	x	x	x	x
Nicaragua					x		
Niger		x	x	x	x	x	

Norway	x	x	x	x	x	x	x
Pakistan	x	x	x	x	x	x	x
Panama							x
Paraguay	x	x	x	x	x	x	x
Peru	x	x	x	x	x	x	x
Philippines	x	x	x	x	x	x	x
Portugal		x	x	x	x	x	x
Rwanda		x	x	x	x	x	x
Senegal		x	x	x	x	x	x
Sierra Leone							x
Singapore				x	x	x	x
South Africa	x	x	x	x	x	x	
Spain	x	x	x	x	x	x	x
Sri Lanka	x	x	x	x	x	x	x
Sudan	x	x	x	x	x	x	
Swaziland				x	x	x	x
Sweden	x	x	x	x	x	x	x
Switzerland	x	x	x	x	x	x	x
Thailand	x	x	x	x	x	x	x
Togo			x	x	x	x	x
Trinidad and Tobago				x	x	x	x
Tunisia	x	x	x	x	x	x	x
Turkey		x	x	x	x	x	x
Uganda						x	x
United Kingdom	x	x	x	x	x	x	x
United States	x	x	x	x	x	x	x
Uruguay	x	x	x	x	x	x	x
Venezuela			x	x	x	x	x
Zaire	x	x	x	x	x	x*	
Zambia		x	x	x	x	x	x
Zimbabwe					x	x	x

Note: * indicates that observations are available when investment data are not included.

Data sources

1. Data on the level and growth rate of per capita real GDP is calculated from the World Bank's real GDP and population data.
2. Data on total real imports and exports come from the World Bank.
3. Data on the ratio of real government consumption to GDP come from the World Bank.
4. Inflation rates are calculated using the IFS's CPI data.
5. Data on M2 and CPI come from IFS. Statistic is [M2/(end of year CPI)]/[GDP/(average year CPI)]
6. The average years of secondary schooling in the total population (15 years of age and over) for 1960, 65, 70, 75, 80, 85 and 90 come from the Barro–Lee data set.

7. Data on real investment shares of GDP come from the World Bank except for Jordan and Nepal whose investment data come from Summers–Heston Penn World Table 5.6.
8. Data on black market premium for time periods 1960–1984 are from Wood, A., "Global Trends in Real Exchange Rates, 1960 to 1984," World Bank, 1988. 1985's data are from World Currency Yearbook (1987–89). Data for 1990–1993 are from International Currency Analysis (1990–1993). Some missing observations are approximated by the data from the Barro–Lee data set.
9. Data on terms of trade are from the World Bank for 1965–1992. Data from Barro–Lee for the period 1960–64 are also used.
10. Data on population growth are from the World Bank.

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