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ABSTRACT

When Does Capital Account Liberalization Help More than it Hurts?*

In this Paper we reconsider the evidence on capital account liberalization and growth. While we find indications of a positive association, the effects vary with time, with how capital account liberalization is measured, and with how the relationship is estimated. The evidence that the effects of capital account liberalization are stronger in high-income countries is similarly fragile. There is some evidence that the positive growth effects of liberalization are stronger in countries with strong institutions, as measured by standard indicators of the rule of law, but only weak evidence that the benefits grow with a country's financial depth and development. We find more evidence of a correlation between capital account liberalization and growth when we allow the effect to vary with other dimensions of openness. There are two interpretations of this finding, one in terms of the sequencing of trade and financial liberalization, the other in terms of the need to eliminate major macroeconomic imbalances before opening the capital account. By and large our results support the second interpretation.

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NON-TECHNICAL SUMMARY

There is now a substantial body of evidence that openness to foreign direct investment (FDI) is positively associated with growth. FDI is a conduit for the transfer of technological and organizational knowledge, suggesting that countries that welcome inward FDI should have higher levels of total factor productivity and to enjoy faster economic growth. In contrast, studies of the effects of financial capital flows are less conclusive. In part this reflects the difficulty of measuring a multi-dimensional phenomenon like financial openness in an economically meaningful way. In part it reflects the sensitivity of findings to the countries and periods considered.

In this Paper we seek to push this literature forward another step, by scrutinizing the robustness of the results reported in previous studies and addressing their implicit interpretation. We focus on the following questions. Is there a positive association of capital account liberalization with growth when the former is measured in an economically meaningful way? Is it robust? Is it evident only in certain times and places? If it is limited to high-income economies, does this reflect their more advanced stage of financial and institutional development, which mitigates the domestic distortions that cause capital account liberalization to have weak or even perverse effects in the developing world? Or do the effects of capital account liberalization hinge to a greater extent on the way it is sequenced with other policy reforms?

While we find some evidence of a positive association between capital account liberalization and growth, that evidence is decidedly fragile. The effects vary with time, with how capital account liberalization is measured, and with how the relationship is estimated. In our view, the evidence is insufficiently robust to support unconditional policy recommendations.

The evidence that the effects of capital account liberalization are stronger in high-income countries is similarly fragile. There is some evidence that the positive growth effects of liberalization are stronger in countries with strong institutions, as measured by standard indicators of the rule of law, but only weak evidence that the benefits grow with a country's financial depth and development.

In contrast, we find somewhat more evidence of a correlation between capital account liberalization and growth when we allow the effect to vary with other dimensions of openness. There are two interpretations of this finding, one in terms of the sequencing of trade and financial liberalization, the other in terms of the need to eliminate major macroeconomic imbalances before opening the capital account. By and large, our results support the second interpretation. While trade openness has a positive impact on growth, the effect of capital account openness is not *contingent* on openness to trade. Rather, it is

contingent on the absence of a large black market premium – that is to say, on the absence of macroeconomic imbalances. In the presence of such imbalances, capital account liberalization is as likely to hurt as to help.

If we are right, ours is the first systematic, cross-country statistical evidence that the sequencing of reforms shapes the effects of capital account liberalization. But our analysis also suggests that this result may be period-specific: the evidence that sequencing matters is more robust in the 1980s than in the 1970s or 1990s. Additional analysis is required to establish the generality of these findings.

When Does Capital Account Liberalization Help More than It Hurts?

Carlos Arteta, Barry Eichengreen and Charles Wyplosz¹
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1. Introduction

The literature on the effects of capital mobility falls under two headings, reflecting the traditional divide between the two branches of international economics. While work on the effects of capital movements in models of the real economy is well advanced, due in no small part to the important contributions of Assaf Razin, the same cannot be said of research in international finance on the effects of capital account liberalization and international capital flows.

There are two explanations for the contrast, one having to do with theory, the other reflecting the limitations of existing empirics. On the theoretical side there are reasons to think that the imperfect nature of the information environment does more to complicate the effects and consequently the analysis of financial than nonfinancial transactions. Information asymmetries are endemic in financial markets. In particular, it is unrealistic to assume that agents on both sides of a financial transaction have the same information.² This is especially true of international financial

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²The classic illustration is that a borrower will know more than a lender about his own desire and motivation to repay, although the point is more general. This is why banks and other financial institutions play a prominent role in the modern economy: by virtue of their investments in monitoring technologies characterized by economies of scale and scope, they aspire to bridge gaps in the information environment that decentralized markets cannot. These observations are widely cited in support of the notion that information asymmetries are pervasive in financial

transactions, in whose case information flows must travel additional physical and cultural distance. It is well understood that these imperfections in the information environment are a distortion in whose presence inward foreign financial investment can be welfare reducing. But the difficulty of characterizing the information asymmetry and therefore the incidence of the distortion means that there is no consensus on precisely when and where such immiserizing effects may take place.

In contrast, the limited conditions under which the transfer or accumulation of capital in a real trade model is immiserizing are well understood. Brecher and Diaz-Alejandro (1977) have pointed to import tariffs, while Brecher and Bhagwati (1991) have modeled the effects of rigid real wages, both of which can lead foreign capital to flow into the wrong sector with immiserizing effects. The transparency of this analysis leaves less controversy about the effects of capital mobility in models of the real economy.

The other explanation for the contrast, and the one we pursue in this paper, is that empirical studies of the effects of foreign direct investment (and, for that matter, trade in goods and services) have reached more definitive conclusions than those on portfolio capital flows. There is now a substantial body of evidence that openness to foreign direct investment is positively associated with growth. FDI is a conduit for the transfer of technological and organizational knowledge, suggesting that countries that welcome inward FDI should have higher levels of total factor productivity and to enjoy faster economic growth.³ In contrast, studies of the effects of financial capital flows are less conclusive. In part this reflects the difficulty of

markets.

³While this conclusion is not uncontroversial, we think that the bulk of the evidence points in this direction. See in particular Borensztein, De Gregorio and Lee (1995), Aitken and Harrison (1999) and Bradstetter (2000).

measuring a multi-dimensional phenomenon like financial openness in an economically meaningful way. In part it reflects the sensitivity of findings to the countries and periods considered (as we document below).

This controversy is summarized by a pair of recent studies by Rodrik (1998b) and Edwards (2001). Rodrik finds no correlation between capital account liberalization and growth and comes down against the presumption that opening an economy to financial capital flows has favorable effects. Substituting a more nuanced and presumably informative measure of capital account liberalization, Edwards reports in contrast a strong positive effect of capital account liberalization, one however limited mainly to high-income countries.

In this paper we seek to push this literature forward another step, by scrutinizing the robustness of these results and addressing their implicit interpretation. We focus on the following questions. Is there really a positive association of capital account liberalization with growth when the former is measured in an economically meaningful way? Is it robust? Is it evident only in certain times and places? If it is limited to high-income economies, does this reflect their more advanced stage of financial and institutional development, which mitigates the domestic distortions that cause capital account liberalization to have weak or even perverse effects in the developing world? Or do the effects of capital account liberalization hinge to a greater extent on the way it is sequenced with other policy reforms?

To anticipate our conclusions, while we find some evidence of a positive association between capital account liberalization and growth, that evidence is decidedly fragile. The effects vary with time, with how capital account liberalization is measured, and with how the relationship is estimated. In our view, the evidence is insufficiently robust to support unconditional policy

recommendations.

The evidence that the effects of capital account liberalization are stronger in high-income countries is similarly fragile. There is some evidence that the positive growth effects of liberalization are stronger in countries with strong institutions, as measured by standard indicators of the rule of law, but only weak evidence that the benefits grow with a country's financial depth and development.

More important than a country's stage of financial development, we find, is the sequencing of reforms. Capital account liberalization appears to have positive effects on growth only in countries that have already opened more generally. But there are significant prerequisites for opening, most obviously a consensus in favor of reducing tariff and nontariff barriers and an ability to eliminate macroeconomic imbalances in whose presence freeing up current account transactions is not possible. Which of these prerequisites turns out to matter may come as a surprise.

2. Previous Literature

The most widely-cited study of the correlation of capital account liberalization with growth is Rodrik (1998b).⁴ For a sample of roughly 100 industrial and developing countries, using data for the period 1975-1989, Rodrik regresses the growth of GDP per capita on the share of years when the capital account was free of restriction (as measured by the binary indicator constructed by the IMF), controlling for determinants suggested by the empirical growth literature (initial income per capita, secondary school enrolment, quality of government, and regional

⁴This section draws on Eichengreen (2000).

dummies for East Asia, Latin America and sub-Saharan Africa).⁵ He finds no association between capital account liberalization and growth and questions whether capital flows enhance economic efficiency.⁶

Given the currency of this article among economists, it is striking that the leading study of the question in political science reaches the opposite conclusion. For 66 countries, using data for the period 1960-1989, Quinn (1997) reports a positive correlation between capital account liberalization (the change in capital account openness) and economic growth.⁷ That correlation is statistically significant at standard confidence levels.

What accounts for the contrast is not clear. Quinn's study starts earlier (it may matter that growth in his sample period is not dominated to the same extent by the "lost decade" of the 1980s), his list of control variables is longer, and he looks at the change in capital account openness rather than the level. Edwards (2001) has emphasized that Quinn uses a more nuanced and therefore more informative measure of capital-account liberalization. For 58 countries over the period 1950 to 1994 and an additional 8 countries starting in 1954, Quinn distinguishes 7 categories of statutory measures. Four are current account restrictions, two are capital account

⁵The capital-account-openness variable is taken from line E2 (restrictions on payments for capital transactions) of the table on exchange and capital controls in the IMF's *Exchange Arrangements and Exchange Restrictions* annual.

⁶The results are depicted visually, using scatter plots of the partial correlation between growth and the share of years when the capital account was liberalized over the period; the author does not report the t-statistic on the capital account liberalization variable. In fact precisely the same result, using the same data, was published some three years earlier by Grilli and Milesi-Ferretti (1995), who report standard errors along with their point estimates.

⁷Quinn includes a long list of ancillary variables that have been shown to explain some of the cross-country variation in growth rates, drawing on Levine and Renelt (1992).

restrictions, and one measures international agreements like OECD membership constraining the ability of a country to limit exchange and capital flows. For each of these categories, Quinn codes the intensity of controls on a two-point scale (in half point increments from 0 to 0.5, 1, 1.5, 2, with 0 denoting most intense and 2 denoting no restriction), producing an overall index of current and capital account restrictions that ranges from 0 to 14 and a capital account restrictions measure that ranges from 0 to 4.

Also important may be that Quinn's country sample is different; in particular, he considers fewer low-income developing countries. The recent literature gives reason to think that the effects of capital account liberalization vary with financial and institutional development. Removing capital controls may be welfare and efficiency enhancing only when major imperfections in the information and contracting environment are absent; this is an implication of the theory of the second best. Portfolio capital inflows stimulate growth, the argument goes, only when markets have developed to the point where they can allocate finance efficiently and when the contracting environment is such that agents must live with the consequences of their investment decisions. The Asian crisis encouraged the belief that countries benefit from removing controls only when they first strengthen domestic markets and institutions generally; thus, we should expect a positive association with growth only when prudential supervision is first upgraded, the moral hazard created by an excessively generous implicit and explicit financial safety net is limited, corporate governance and creditor rights are strengthened, and transparent auditing and accounting standards and efficient bankruptcy and insolvency procedures are adopted. While these institutional prerequisites are difficult to measure, there is a presumption that they are most advanced in high-income countries. This is one way of understanding how

Alesina, Grilli and Milesi-Ferretti (1994) find evidence of a positive association between capital account liberalization and growth -- their sample is limited to 20 high-income countries -- when Grilli and Milesi-Ferretti (1995) find a negative association in a sample dominated by developing countries.⁸

Edwards (2001) addresses the hypothesis that capital account liberalization has different effects in high- and low-income countries. Using Rodrik's controls but Quinn's measure of the intensity of capital account restrictions in 1973 and 1988, he finds that liberalization boosts growth in the 1980s in high-income countries but slows it in low-income countries. (The dummy variable for capital-account openness enters negatively, in other words, while the interaction between capital-account openness and per capita income enters positively.) Edwards shows further that the significance of capital controls evaporates when the IMF index used by Rodrik is substituted for Quinn's more differentiated measure. Thus, it is tempting to think that the absence of an effect in previous studies is a statistical artefact. And there is some suggestion that capital account liberalization is more beneficial in more financial and institutionally developed economies.⁹

⁸The evidence is not overwhelming: Alesina, Grilli and Milesi-Ferretti report a positive coefficient that differs significantly from zero at standard confidence levels in one of four regressions. Moreover, the specification in question does not control for initial per capita income; once it is added, the correlation in question dissolves. Bordo and Eichengreen (1998) explicitly compare industrial and developing countries, and report weak evidence that controls have a positive impact on growth in the industrial countries, and a negative impact on growth in developing countries, although the operative word is "weak."

⁹Quinn (2000) uses a very different methodology to address this question but reaches a similar conclusion. He estimates bivariate VARs using growth rates and his measures of capital account liberalization, individually for a large number of middle- and low-income countries. He finds scant evidence that capital account liberalization has had a positive impact on growth in the poorest countries, but some positive evidence for middle-income countries, especially those that

But do these differences really reflect the level of financial and institutional development?

Kraay (1998) is the one direct attempt to test the hypothesis that the effects of capital account liberalization depend on the strength of the financial system, the effectiveness of prudential supervision and regulation, and the quality of other policies and institutions, rather than seeking to infer this from differences between industrial and developing economies.¹⁰ The results are not encouraging: the interaction of the quality of policy and institutions with financial openness is almost never positive and significant, and, it is sometimes significantly negative. But while these findings are intriguing, their generality is an open question; the other work reviewed in this section raises questions about whether they are sensitive to country sample, specification, and in particular how financial openness is measured. To the extent that capital account liberalization has different effects on growth in high- and low-income countries, the reasons for the contrast remain unclear.

3. Basic Results

In this section we scrutinize the claim that the effects of capital account liberalization differ between high- and low-income economies -- and, specifically, that they are positive in the former but not the latter.

have other characteristics likely to render them attractive to foreign investors.

¹⁰Kraay uses the ratio of M2 to GDP and the ratio of domestic credit to the private sector relative to GDP as ex ante proxies for the level of financial development, and one minus the average number of banking crises per year as an ex post indicator of financial strength. As an indicator of the strength of bank regulation, he uses authorization for banks to engage in nontraditional banking activities such as securities dealing and insurance. And to capture the broader policy and institutional environment, he uses a weighted average of fiscal deficits and inflation, the black market premium, and indices of corruption and the quality of bureaucracy.

Our point of departure is Edwards' model. Edwards regresses economic growth in the 1980s (approximately the same period considered by Rodrik) on the decennial average investment rate, years of schooling completed by 1965 (as a measure of human capital), the log of real GDP per capita in 1965 (as a measure of the scope for catch-up), and Quinn's index of capital account openness. He reports that this measure of capital account openness has a positive and generally significant effect on growth. Moreover, when capital account openness is entered both on its own and interacted with per capita incomes, the first coefficient is negative and the second positive.¹¹ The inflection point where the effect of capital account openness becomes positive coincides with the per capita incomes achieved by the 1980s by such relatively-advanced emerging markets as Hong Kong, Israel, Mexico, Singapore and Venezuela.

Four aspects of Edwards' data and specification are worthy of comment. First, while he uses annual data spanning the 1980s for his other variables, Edwards has Quinn's measure of capital account openness for only 1973 and 1988.¹² The 1973 value is arguably too early to have a first-order effect on growth in the 1980s, while the 1988 value is arguably too late.¹³ Similarly, the difference in the level of openness in 1973 and 1988 may tell us how policies toward the capital account changed in the 1970s and 1980s, but these two snapshots are equally compatible with the possibility that the change took place before the beginning of the sample period or at its

¹¹In contrast, the IMF measure of capital account openness bears no association to growth, as mentioned above.

¹²Quinn has also made available his 1958 estimates to other investigators, but these are irrelevant to the current exercise.

¹³Including only the 1988 value but instrumenting it using the 1973 index, as Edwards does in some of his analysis, does not obviously solve this problem.

end, two scenarios which presumably imply very different effects.¹⁴

Second, Edwards weights his observations by GDP per capita in 1985. We worry that placing an especially heavy weight on rich countries with well-developed institutions biases the case in favor of finding a link between capital account liberalization and growth, since these are the countries in which such an effect is most plausibly present.¹⁵

Third, Edwards instruments his measure of capital account liberalization with a vector of concurrent and lagged economic, financial and geographical variables. While we are sympathetic to the idea that policies toward the capital account may be affected by as well as affect growth, useful instruments -- variables that are exogenous but also correlated with capital account liberalization -- are hard to come by. In particular, we are skeptical that geographic variables are usefully correlated with capital account liberalization (although previous work has shown that they importantly influence the level of income and/or the rate of growth), and we would question whether the economic and financial variables invoked in this context are properly regarded as exogenous with respect to the policy.¹⁶

¹⁴Edwards' two sets of estimates use, alternatively, the Quinn index for 1988 and the difference between Quinn's 1973 and 1988 values.

¹⁵In addition, heavy weights on high income countries, which were also the relatively fast growing countries in some of the subperiods we consider (see below), increases the danger of finding a correlation between capital account liberalization and growth because of reverse causality (insofar as the high-income countries were the ones most likely to open the capital account).

¹⁶In addition, Edwards derives his results from estimates of a two-equation system, where the dependent variables are GDP growth and TFP growth and the list of independent variables is the same across equations. Since the second dependent variable is derived (in part) from the first, errors in GDP growth are likely to produce (positively correlated) errors in TFP growth. When the correlation between the error terms in the two equations is taken into account via system estimation, the econometrician may therefore obtain a spuriously strong correlation with the

Fourth, neither Edwards nor other contributors to this literature include competing measures of economic openness and the macroeconomic policy regime. Typically, countries that open the capital account also open their economies to other transactions (for example, they will have reduced tariff and nontariff barriers to trade). In the absence of measures of these other policies, it is not obvious that the index of capital account openness is picking up the effects of financial openness as opposed to the openness of, say, the trade account. Similarly, governments may wait to open the capital account until they have first succeeded in eliminating macroeconomic imbalances that would precipitate capital flight through newly-opened channels; a positive effect of capital account liberalization on growth may reflect the growth-friendly effects of macroeconomic stabilization, in other words, rather than international financial policies per se.

We show some “Edwards regressions” in Table 1 (single equation estimates for the 1980s, with and without weights, using the 1988 level of the Quinn index and, also following Edwards, the change in Quinn’s index between 1973 and 1988).¹⁷ The controls -- the investment ratio, human capital, 1965 per capita GDP -- are consistently significant and have their anticipated signs. In the unweighted least squares regressions, both the level of capital account openness in 1988 and its change between 1973 and 1988 enter with positive coefficients, but only the latter differs from zero at the 95 per cent level.¹⁸ In the weighted least squares regressions, the converse is

independent variables.

¹⁷Details on the data underlying this and subsequent tables can be found in the data appendix.

¹⁸We also augmented the list of controls to include distance from the equator, since this variable is included among the instrumental variables we experiment with below, and others (e.g. Rodrik 2000) have argued that it has an independent impact on economic growth. While this variable often enters with a significant coefficient, adding it only reinforces our results (as we

true: the level of capital account openness differs from zero at the 95 per cent confidence level, but the change does not. There is some evidence, then, of a positive association between capital account liberalization and growth, although it is decidedly fragile.

Following Edwards, we re-estimated these equations substituting the binary measure of capital-account restrictions based on information in the IMF's *Exchange Arrangements and Exchange Restrictions* annual, constructing this variable as the share of years in the sample period when the capital account was open. As in his analysis, none of the coefficients on this variable approached significance at conventional confidence levels.¹⁹ This is support for Edwards' first point, that the growth effects of capital account liberalization are more evident when the latter is proxied by the (presumably more informative) Quinn measure.

When we interact the Quinn measure with per capita GDP in 1980 (that being the start of Edwards' sample period), we find little support for the notion that capital account liberalization has different effects in high- and low-income countries. In the unweighted least squares regressions (columns 1-4 of Table 1), the coefficient on the interaction of the Quinn index and 1980 per capita GDP is positive and significantly different from zero at the 95 per cent confidence level, as if the effects of liberalization are larger in high-income countries.²⁰ But we find no such effect in the weighted least squares regressions or when we measure capital account liberalization

explain below).

¹⁹These regressions are not reported but are available from the authors on request.

²⁰The coefficient on the level of the Quinn index by itself (column 3) is negative but insignificantly different from zero, lending no support to the hypothesis that liberalization damages growth in low-income countries. It is however the case that we can reject the null that both coefficients (that on the level of the Quinn index and the interaction term) are zero at the 95 per cent confidence level.

as the change in the Quinn index between 1973 and 1988. When we adopt this last specification, the pattern of coefficients instead suggests *smaller* growth effects of liberalization in high income countries.²¹ A joint test of the significance of the capital-account-openness measure measured in changes and the corresponding interaction term (not reported) allows us to reject the null that both coefficients are zero in both column 8 (the weighted least squares estimates) and in column 4 (the OLS estimates). But, to repeat, the pattern of signs is inconsistent with the notion that liberalization has negative effects in low-income countries and positive effects in high-income ones.²²

Edwards obtains more precise coefficients on the relationship between capital account liberalization and growth, both entered linearly and interacted with per capita GDP, when using instrumental variables. Though his estimates of the nonlinear effect differ from ours not just by the use of instruments but also by being estimated as a system of two equations (where capital account liberalization affects both aggregate and TFP growth), we were led to wonder whether differences between our results and his are driven by the use of instrumental variables. We therefore re-estimated the equations in Table 1 using two alternative sets of instruments. We first used the Hall-Jones (1999) instrument set (distance from the equator, a dummy variable for whether the country is landlocked, a dummy variable for whether it was an island, the share of the

²¹The results are essentially the same, although a little weaker, when we use membership in the OECD in place of per capital GDP when constructing the interaction term. In the weighted least squares regressions, all interaction terms are insignificantly different from zero at conventional confidence levels.

²²Edwards' key results -- those obtaining nonlinear effects for capital account liberalization (negative at low levels of per capital income, positive at high levels) -- are derived using three-stage least squares.

population speaking English, and the share of the population speaking a major European language). None of the measures of capital account liberalization -- its level or change, entered by itself or interacted with per capital GDP -- entered with a coefficient that approached significance at standard confidence levels.²³ While these instruments are plausibly exogenous, either they are not usefully correlated with capital account liberalization or the latter in fact has no independent impact on growth.

The second set of instruments is our attempt to replicate those used by Edwards: whether the capital account was open or closed in 1973, the ratio of liquid liabilities to GDP in 1970 and 1975, distance to the equator, and a dummy variable for OECD countries.²⁴ The results, in Table 2, differ sharply from before. The coefficient on the Quinn index in 1988 is still positive when entered on its own, albeit somewhat less well defined than in Table 1. However, when the interaction of capital account openness and per capita GDP is added, the coefficient on the level of the Quinn index turns negative (though insignificant), while the interaction term is positive and significantly different from zero at the 95 per cent confidence level. This is very close to Edwards' result. However, this pattern obtains only when we enter capital account openness in levels (rather than changes between 1973 and 1988) and only when we estimate by unweighted least squares (as opposed to applying per capita GDP weights). And it is sensitive to the choice

²³This was true both of the individual coefficients and, when Quinn openness was entered in levels and interacted with per capital GDP, of the pair. That is, the relevant F-statistic did not lead us to reject the null that the two coefficients were effectively zero. Note that this is where the decision of whether or not to include distance from the equator as an explanatory variable for growth could matter. It is reassuring, therefore, that adding it to the list of independent variables altered none of the results reported here.

²⁴This is our reading of the abbreviations in the instrument list at the bottom of his Table 10.

of instrumental variables. For example, the one coefficient on the interaction term that was previously significantly positive goes to zero when either financial depth or lagged openness (or, for that matter, both) is dropped from the instrument list but the other instrumental variables are retained.²⁵

Thus, we confirm that an analysis of developing- and industrial-country experience in the 1980s yields somewhat more favorable results for the association of capital-account openness and growth when capital-account policies are measured using Quinn's index rather than the IMF measure. The evidence that this effect is stronger in high-income countries turns out to be extremely sensitive to specification and estimation.

4. Sensitivity Analysis

In this section we subject these results to two forms of sensitivity analysis. First, we adjust the timing of the dependent and independent variables in order to better identify the effects of capital account policies. Second, we compare the effects of capital account liberalization in different periods.

Recall that Edwards uses Quinn's measure of capital-account openness in 1973 and 1988. If capital account liberalization has a significant impact on growth, this should be most evident in the immediately succeeding years. Having analyzed growth in the 1980s as a way of rendering our results as directly comparable as possible to those of Edwards and other investigators, we

²⁵In addition, we worry about the validity of the instruments, specifically whether lagged liberalization and financial depth, which move slowly and are correlated with current liberalization and financial depth, are in fact capturing the exogenous component of these international financial policies. See the discussion above.

now focus on the effects of capital account liberalization in the years immediately following those for which we have Quinn’s capital account restrictions data: 1973 and 1988. The obvious stopping point for the period starting in 1973 is 1981, the eve of the Mexican debt crisis and the “lost decade” of the 1980s, when capital flows were subdued and their growth effects were plausibly different. Our second cross section (starting in 1988) ends in 1992, since that is when our data, drawn from the Penn World Tables Mark 5.6a, end.²⁶ This leaves a gap in the mid-1980s. Fortunately, we were also able to obtain Quinn’s measure of capital account openness for 1982.²⁷ Thus, we can analyze three cross sections covering the periods 1973-81, 1982-87, and 1988-92. We also pool the three cross sections. The pooled results will reassure readers worried that conditions during one or more of our periods were special (“1982-87 is unrepresentative because it is dominated by the debt crisis,” for example). Aggregating across periods limits the danger that our results are being driven by period-specific effects.

The results are in the Table 3. In the first four columns we enter capital account openness in levels; in the second four we also interact it with per capita GDP.²⁸ Given the questions about

²⁶In principle, it would be possible to extend this sample beyond 1992 using data from other sources. But given the far-reaching changes in capital-account restrictions that occurred in the 1990s, our measure of the structure of capital controls, circa 1988, would then capture little of the reality of capital-account policies in the last subperiod. Since we do not have Quinn-like data on the capital-account regime in the 1990s, we concluded that it makes sense to stick with a sample that ends in 1992.

²⁷We thank Dennis Quinn for sharing these data with us.

²⁸We report results using only the Quinn measure of openness, since when we substitute the IMF measure we obtain uniformly insignificant effects. We concentrate on unweighted regressions throughout. The use of weights and instrumental variables alters our results only slightly: the coefficients tend to be slightly less well determined, although the pattern of signs remains the same.

instrumentation raised in the last section, we estimate the equations by ordinary least squares.

The results remain generally plausible.²⁹ When entered exclusively in levels, the Quinn measure of financial openness is positively associated with growth in all three periods, but only in the third of these, 1988-92, is the effect significant at anything approaching conventional confidence levels. The coefficient is smallest in the period starting in 1982, when capital flows were depressed by the debt crisis, and largest in the post-1987 period, the year of the Brady Plan after which large scale portfolio capital flows resumed. But when we pool the three cross sections (adding fixed effects to differentiate the subperiods), the coefficient on capital account liberalization differs from zero at the 95 per cent confidence level. This is the strongest evidence so far of a positive association of capital account liberalization and growth, although it is clear that this result is heavily driven by one of our cross sections.

But there is still scant evidence of a stronger growth effect in high-income countries. We obtain a significant positive coefficient on the interaction term only for the post-1982 years. Perhaps capital account liberalization worked its magic more powerfully on high-income countries in these years. Alternatively, it may simply be that high-income (OECD) countries with open capital accounts were less affected by the debt crisis of the 1980s than developing countries with open capital account that had grown heavily dependent on foreign borrowing. Whether this in fact tells us anything about the differential effects of capital account liberalization in different developing countries is unclear.

²⁹Investment retains a consistently positive effect on growth, although the effects of human capital are somewhat weaker than before. The catch-up effect is weak in both the second and third periods, reflecting the heavy impact of the debt crisis of low- and middle-income developing countries starting in 1982 and their delayed post-1987 recovery.

In the pooled sample, the coefficient on the interaction term is indistinguishable from zero. However, the coefficient on capital account liberalization in levels continues to enter positively and differs from zero at the 90 per cent confidence level. Again, however, this result appears to be driven by the strong positive association in the post-1987 period.

Thus, more data and appropriate timing of the variables continue to provide indications a positive association of capital account liberalization with growth. However, that effect is robust only for the most recent period, that is to say, for the post-Brady Plan years. There is less evidence to this effect for earlier periods, whether these are the years of syndicated bank lending to developing countries or of the developing country debt crisis. Moreover, we find little support for the view that capital account liberalization has more favorable effects in high- and middle-income emerging markets than in poorer developing countries.

5. Do These Patterns Reflect Stages of Financial and Institutional Development?

We now ask whether the different effects of capital account liberalization in high- and low-income countries in fact reflect their different stages of financial and institutional development. To this end, we interact Quinn's index not with per capita GDP but with financial depth (proxied by the ratio of liquid liabilities to GDP) and institutional strength (the *International Country Risk Guide's* index of law and order).³⁰

The results are in Table 4, the first four columns for financial depth (post 1973, post 1982, post 1988, and pooled, reading left to right), the last four for law and order. Those for financial

³⁰Again, for details on this and the other variables used in the analysis, see the data appendix.

depth are unpromising: none of the coefficients in question is significant individually or as a pair.³¹ The results for the interaction between capital account openness and rule of law are more promising. In the first subperiod (1973-81) we obtain a negative coefficient on capital account openness in levels and a positive coefficient on the interaction term; the latter differs from zero at the 95 per cent confidence level. The interpretation is that capital account liberalization has no effect in countries with weak contract and law enforcement but a positive effect in those with stronger ones. The results for the second subperiod (1982-87) are more striking still: both terms again enter with the expected signs, and both now differ from zero at conventional confidence levels.³² According to this column at least, capital account liberalization hinders growth when a country rates low on the law-and-order index but helps when it rates high.³³ In comparison, the results for the most recent subperiod (1988-92) are disappointing: neither coefficient enters with its expected sign, and neither differs significantly from zero at standard confidence levels.³⁴

The results for the pooled sample reflect these contrasting subsample results. The coefficient on the level of Quinn openness is zero, but the coefficient on the interaction term is positive and significant at the 90 (but not the 95 per cent) confidence level.

Thus, we find scant support for the hypothesis that the effects of capital account

³¹Again, estimating these equations by instrumental variables changed nothing.

³²At the 90 and 95 per cent levels, respectively.

³³One of the coauthors is reminded of all the money deposited in Swiss banks by depositors from countries that rate low according to the law-and-order index and with porous capital accounts.

³⁴We also fail to reject the null that the two coefficients are jointly zero at conventional confidence levels.

liberalization reflect a country's stage of financial development. There is more support for the idea that the effects vary with the effectiveness of law and order, but the evidence is not overwhelming.

6. Sequencing

It could be that we are not capturing the full impact of capital account liberalization on growth because we are not controlling efforts to coordinate external financial opening with other liberalization measures. There is a large literature on sequencing that suggests that capital account liberalization initiated before the current account is opened can have strongly distortionary effects (see McKinnon 1991). If trade barriers continue to protect an uneconomical import-competing sector, foreign capital will flow there, attracted by rents and artificially inflated profits. Since the country has no comparative advantage in those activities, actually devoting more resources to import-competing production can be growth and welfare reducing. In particular, the cost of the resources that the country utilizes to service the foreign finance may exceed the cost of capital, reducing domestic incomes as well as starving other sectors of inputs to growth (Brecher and Diaz-Alejandro 1977). Similarly, the literature on the sequencing of financial liberalization measures cautions that it can be counterproductive to open the international accounts before domestic macroeconomic imbalances have been eliminated; the main effect will then be to provide avenues for capital flight.³⁵ If domestic financial markets are repressed, capital account liberalization allows savers to flee the local low-interest rate environment in favor of higher returns abroad. For all these reasons, capital account liberalization when macroeconomic

³⁵See for example Edwards (1994) and Johnston (1998).

policy is seriously out of balance is a recipe for disaster.

To capture these qualifications, we added the interaction between capital account openness, as measured by Quinn, and nonfinancial openness, as measured by Sachs and Warner (1995).³⁶ This is analogous to our earlier tests of the idea that the effects of capital account openness are contingent on financial depth and institutional development, but now the hypothesis is that they are contingent on the absence of trade and macroeconomic distortions. The Sachs-Warner index classifies a country as open if none of the five following criteria holds: the country had average tariff rates higher than 40 per cent, its nontariff barriers covered on average more than 40 per cent of imports, it had a socialist economic system, the state had a monopoly of major exports, and its black market premium exceeded 20 per cent. The first four criteria should allow us to test the notion that capital mobility is counterproductive for an economy whose trade is highly restricted and distorted.³⁷ The fifth is an indicative of macroeconomic policies and conditions inconsistent with a country's administered exchange rate; it should allow us to test the hypothesis that capital account liberalization is counterproductive if implemented before a country has eliminated macroeconomic imbalances.

The results are in Table 5, columns 1-4. The specification is analogous to that of Table 3 but for the addition of the interaction of the Sachs-Warner dummy with Quinn openness. We find

³⁶We thank Andrew Warner for providing these data.

³⁷Rodriguez and Rodrik (1999) note that the state monopoly of major exports variable is derived from a World Bank index of the degree of distortions caused by export marketing boards and is positive only for sub-Saharan African countries, whose growth performance was particularly disappointing in the sample period. Since there are only two sub-Saharan African countries in our (much smaller) sample, it is not plausible in our case that this is the proper interpretation of the results we obtain when we use the Sachs-Warner openness measure.

a strong positive effect of this interaction term, almost irrespective of period.³⁸ In the pooled sample, it differs from zero at the 99 per cent confidence level. This suggests that capital account openness stimulates growth when a country has eliminated major trade distortions and macroeconomic imbalances, but not otherwise.

We undertook some sensitivity analyses of this finding. We estimated the equations by weighted as well as unweighted least squares. We used Edwards' instrumental variables. We searched for and dropped outliers. We added the interaction between financial depth and financial openness and the interaction between law and order and financial openness as in Table 4 above. None of these changes weakened the result.

The one change that made a difference was adding Sachs-Warner openness in levels. We show the result of doing so in columns 5-8 of Table 5. The three openness measures (Sachs-Warner openness, Quinn capital account openness, and their interaction) are highly correlated, creating problems of multicollinearity. Only in the pooled sample is there much hope of distinguishing their effects. There, Sachs-Warner openness and Quinn openness both have (positive) coefficients that differ from zero at the 90 per cent confidence level, while their interaction is insignificant. This points less to the importance of sequencing than to separate, non-interdependent effects on growth of both Sachs-Warner and capital account openness. But multicollinearity makes it difficult to know which interpretation is appropriate. While the relevant F-test allows us to reject the null that the levels of both Sachs-Warner openness and Quinn openness are zero, consistent with the separate, non-interdependent-effects interpretation, it also

³⁸Only in the first subperiod, for 1973-81, is its coefficient not significantly greater than zero at the 95 per cent confidence level.

allows us to reject the null that Quinn openness and the interaction of Sachs-Warner openness with Quinn openness are both zero, consistent with the sequencing interpretation.

It turns out that we can get a better handle on which interpretation is more plausible by analyzing whether absence of a favorable impact on growth in countries that are closed according to the Sachs-Warner measure reflects distortionary trade policies or distortionary macroeconomic policies. We do so by breaking Sachs-Warner openness up into its two principal components, one reflecting the prevalence of tariff and nontariff barriers (distortionary trade policies), and the other reflecting the size of the black market premium (an indicator of macroeconomic imbalances).³⁹ If it is the interaction term involving the black market premium that matters, then we can say that it is the elimination of macroeconomic imbalances that is the essential prerequisite for capital account liberalization to have positive growth effects, a la McKinnon. If, on the other hand, it is the interaction involving tariff and nontariff barriers that is significant and important, we can say that it is the elimination of trade-related distortions that is key, a la Brecher and Diaz-Alejandro.⁴⁰

We measured tariff and nontariff barriers using the data of Barro and Lee (1994), which conveniently are also utilized by Sachs and Warner.⁴¹ For the black market premium, we constructed three alternative measures. First, we created a dummy variable which equaled unity if

³⁹As noted above, Sachs-Warner openness involves two additional criteria -- whether a country had a socialist economic system and the state had a monopoly of major exports -- which are likely to matter importantly for certain countries. We return to this point below.

⁴⁰This is similar to the approach taken by Rodriguez and Rodrik (1999), who find in growth regressions covering a longer period that it is the black market premium in which most of the explanatory power resides.

⁴¹Note that the Barro-Lee tariff and nontariff data do not vary with time. The same is true of the Sachs-Warner index (which makes use of the Barro-Lee measures), aside from a few selected changes imposed by its architects.

the black market premium was less than 20 per cent.⁴² While this follows Sachs and Warner as closely as possible, it does not use all the available information. We therefore also defined an alternative measure, 100 per cent minus the black market premium.⁴³ While this contains more information, the results obtained when using it are more likely to be dominated by a handful of extreme observations. This led us to create a third version of the variable, which truncated 100 per cent minus the black market premium at zero on the downside.⁴⁴

It turns out that it is the interaction term between capital account openness and the black market premium that most consistently matters. Columns 1-3 of Table 6 display pooled regressions using the three alternative measures of the premium. The interaction with the black market premium is positive, and its coefficient is significantly greater than zero at the 90 per cent confidence level, regardless of how that premium is defined and measured. The evidence that trade openness is a prerequisite for capital account openness to stimulate growth is less robust; while the coefficient on the interaction with tariff and Barro and Lee's trade openness measure is consistently positive, it approaches significance at conventional confidence levels in only one of the three regressions.⁴⁵

Again, we attempted to confirm the robustness of this finding. We added interaction

⁴²We refer to this in Table 6 as "black market premium 1."

⁴³This is "black market premium 2." We divide the premium by 100 so that the coefficients on this variable are scaled the same as for "black market premium 1."

⁴⁴Denoted "black market premium 3." Again, we divide this measure by 100 to make it as comparable as possible with "black market premium 1."

⁴⁵We see the same pattern when we consider the individual subperiods, although the coefficients, predictably, are less well defined than when we pool the data. We discuss the subperiod results later in this section.

terms involving financial depth and law and order, as in Table 4. We ran regressions using weighted as well as unweighted observations. In each instance the results were essentially unchanged. The one sensitivity analysis that mattered was adding Sachs-Warner openness in levels. The results are in the last three columns of Table 6. Evidently, the two measures of external policy with the most robust, consistent effects on growth are (i) Sachs-Warner openness and (ii) the interaction of the black market premium with capital account openness. In other words, there is strong evidence, as before, that countries that open externally in the sense of Sachs and Warner grow faster, other things equal. In addition, however, countries that open the capital account also grow faster *but only if they first eliminate any large black market premium*. Capital account openness has favorable effects, it would appear, only when macroeconomic imbalances leading to inconsistencies between the administered exchange rate and other policies have first been eliminated.

Table 7 reports a selection of subperiod results.⁴⁶ These reveal that the positive effect of capital account openness on growth, *contingent on the absence of a large black-market premium*, is driven by the 1982-87 subperiod. In addition, previously (in Tables 3-5) the coefficient on capital account openness in levels was either positive or zero. There was no evidence, in other words, that capital account openness was *bad* for growth in countries with underdeveloped financial markets, weak institutions, severe macroeconomic imbalances, or closed current accounts. Now the coefficient on the level of Quinn's index is strongly negative in 1982-87, as if countries with significant trade distortions and large black market premia grew more slowly if

⁴⁶These are discussed in the next paragraph. To conserve space, we report only the results for "black market premium 1." Those using the other measures of the black market premium are essentially identical.

they ill advisedly opened their capital accounts. That this effect is most evident in the debt-crisis years 1982-87 may be telling us that countries that poorly sequenced capital account liberalization suffered the most devastating effects of the curtailment of capital flows; they suffered a severe debt overhang and an intractable transfer problem when the debt crisis struck. It may be that improper sequencing does not actually damage growth so long as international capital markets are flush with funds, but that it can result in serious damage if lending suddenly dries up.

7. Conclusion

Economic theory creates a strong presumption that capital-account liberalization has favorable effects on growth. Yet the accidents and disappointments suffered by countries liberalizing their international financial transactions remind us that reality is more complex than theory. The quest for guidance is not helped by the fact that the data do not speak loudly. Some analysts have rejected the hypothesis that there is a positive association between capital account liberalization and growth, while others have reported evidence of a favorable effect.

The idea that the effects of capital-account liberalization are conditioned by a country's stage of financial and institutional development similarly has intuitive appeal. Not only are there good theoretical reasons to think that this might be the case, but it could be the failure of previous investigators to incorporate this idea that accounts for the weak and inconsistent results of their econometric studies. Yet our tests of the hypothesis are only weakly supportive. We find no evidence that the effects of capital account liberalization vary with financial depth, but somewhat more evidence that its effects vary with the rule of law.

In contrast, we find somewhat more evidence of a correlation between capital account

liberalization and growth when we allow the effect to vary with other dimensions of openness.

There are two interpretations of this finding, one in terms of the sequencing of trade and financial liberalization, the other in terms of the need to eliminate major macroeconomic imbalances before opening the capital account. By and large, our results support the second interpretation. While trade openness has a positive impact on growth, the effect of capital account openness is not *contingent* on openness to trade. Rather, it is *contingent* on the absence of a large black market premium -- that is to say, on the absence of macroeconomic imbalances. In the presence of such imbalances, capital account liberalization is as likely to hurt as to help.

If we are right, ours is the first systematic, cross-country statistical evidence that the sequencing of reforms shapes the effects of capital account liberalization. But our analysis also suggests that this result may be period-specific: the evidence that sequencing matters is more robust in the 1980s than in the 1970s or 1990s. If this investigation has taught us one thing, it is not to oversell such results. Considerable additional analysis is required to establish the generality of such findings.

Data Appendix

Our sample includes the following 61 countries: Argentina, Australia, Austria, Belgium, Bolivia, Brazil, Canada, Chile, Colombia, Costa Rica, Denmark, Dominican Republic, Ecuador, Egypt, El Salvador, Finland, France, Germany, Ghana, Greece, Guatemala, Haiti, Honduras, Hong Kong, India, Indonesia, Iran, Iraq, Ireland, Israel, Italy, Jordan, Korea, Liberia, Malaysia, Mexico, Myanmar, Netherlands, New Zealand, Nicaragua, Norway, Pakistan, Panama, Paraguay, Peru, Philippines, Portugal, Singapore, South Africa, Spain, Sri Lanka, Sweden, Switzerland, Syrian Arab Republic, Thailand, Tunisia, Turkey, United Kingdom, United States, Uruguay, and Venezuela.

Dependent Variable: Rate of growth of real GDP per capita, defined as the first difference of the log of real GDP per capita in constant dollars at 1985 international prices. Source: Penn World Tables, Mark 5.6a.

Controls:

- **Real investment share of GDP (%)** at 1985 international prices. The variables used in the regressions are averages of this variable over particular periods of time, as noted in the text and tables. Source: Penn World Tables, Mark 5.6a.
- **Average years of schooling** of the population over 15 years of age. This variable is available quinquennially for the years 1960-1990. For Tables 1 and 2, the 1965 value was used. For the other tables, the values for 1970 (for the 1973 cross-section), 1980 (for the 1982 cross-section), and 1985 (for the 1988-cross section) were used. (Given lack of 1970 data for Egypt, the value for 1975 was used in the 1973 cross-section for this country.). Source: Barro-Lee data set (see Barro and Lee 1996).
- **Log of GDP per capita** in constant dollars (chain index) at 1985 international prices. The value for 1965 is used in Tables 1 and 2. In the other tables, the value for the beginning of the corresponding period was used. Source: Penn World Tables, Mark 5.6a.

Financial and Institutional Development:

- **Financial depth**, defined as the ratio of liquid liabilities to GDP (%). Values at the beginning of the period were used. Source: Beck, Demirguc-Kunt, and Levine (1999).
- **Law and order index**, which ranges from zero to six, where a higher value represents a better institutional framework. Source: *International Country Risk Guide*. Since this index starts only in 1984, we use the 1984 value for 1973 and 1981.

Financial Openness:

- **Quinn index**, which ranges from zero to four in increments of 0.5, where a higher value represents a more open capital account. Values for 1973, 1982, and 1988 are available. The value for 1988 and the difference between the 1973 and 1988 values were used in Tables 1 and 2. In the other tables, the value for the beginning of the corresponding period was used. Source: personal correspondence with Dennis Quinn.
- **IMF capital account openness dummy**, constructed from line E2 (“restrictions on payments for capital transactions”) of the IMF *Annual Report of Exchange Arrangements and Exchange Restrictions*, various issues. The variable used was the share of years in the sample period when the capital account was open. Source: IMF.

Non-Financial Openness:

- **Sachs-Warner openness dummy**, defined as a binary variable equal to one if *none* of the five following criteria holds: the country had average tariff rates higher than 40 per cent, its nontariff barriers covered on average more than 40 per cent of imports, it had a socialist economic system, the state had a monopoly of major exports, and its black market premium exceeded 20 per cent. Source: Sachs and Warner (1995), via personal correspondence with Andrew Warner.
- **Barro-Lee trade openness dummy**, defined as binary variable equal to one if a country did not have average tariff rates higher than 40 per cent and its nontariff barriers did not cover on average more than 40 per cent of imports. Source: Barro and Lee (1994).
- **Black market premium**, defined as per cent premium over the official exchange rate. Source: personal correspondence with Andrew Warner.

Instruments:

- **Liquid liabilities to GDP** (as defined above), for 1970 and 1975. Source: Beck, Demirguc-Kunt, and Levine (1999).
- **Distance to the equator**. Source: Hall and Jones (1999).
- **OECD membership dummy**. Source: *World Development Indicators*, World Bank.
- **Language variables**, corresponding to: (a) the fraction of the population speaking English, and (b) the fraction of the population speaking one of the major languages of Western Europe: English, French, German, Portuguese, or Spanish. Source: Hall and Jones (1999).
- **Landlocked nation dummy**. Source: Andrew Rose’s website.

- **Island nation dummy.** Source: Andrew Rose's website.

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Table 1
Basic Regressions
(Dependent Variable: Average Growth Rate of GDP per Capita, 1980-1989)

	1	2	3	4	5	6	7	8
Investment Ratio, 1980-1989 average	0.192*** (4.44)	0.183*** (5.03)	0.176*** (4.44)	0.182*** (4.96)	0.160*** (4.46)	0.171*** (5.47)	0.155*** (4.44)	0.167*** (5.01)
Human Capital, 1965	0.720** (2.54)	0.735** (2.15)	0.587** (2.03)	0.776** (2.26)	0.500** (2.52)	0.579** (2.24)	0.481** (2.34)	0.621** (2.35)
Log GDP per capita, 1965	-2.911*** (-3.41)	-2.665*** (3.30)	-3.719*** (-4.25)	-2.588*** (-3.14)	-2.487*** (-3.29)	-2.015*** (-3.06)	-2.784*** (-3.55)	1.888*** (-2.88)
Level Quinn's Index, 1988	0.599 (1.48)	----	-0.081 (-0.17)	----	1.005** (2.35)	----	0.742 (1.28)	----
Interaction Level Quinn 1988 * 1980 GDP per capita	----	----	0.001*** (2.98)	----	----	----	0.001 (0.84)	----
Difference Quinn's Index, 1973-88	----	0.600** (2.28)	----	1.034** (2.60)	----	0.280 (0.91)	----	1.204** (2.26)
Interaction Diff. Quinn 1973-88 * 1980 GDP per capita	----	----	----	-0.001 (-1.24)	----	----	----	-0.001* (-1.81)
Constant	15.587*** (2.87)	15.027*** (2.84)	22.790*** (4.01)	14.389** (2.66)	13.009*** (2.77)	11.109** (2.59)	15.723*** (3.03)	10.073** (2.40)
Observations	61	61	61	61	61	61	61	61
R ²	0.52	0.52	0.57	0.52	0.53	0.44	0.53	0.46

OLS regressions. t-statistics derived using robust standard errors in parentheses. Columns 1-4 are unweighted. Columns 5-8 are weighted by GDP per capita in 1985. Significance at 10%, 5%, and 1% denoted by *, **, and *** respectively.

Table 2
Two-Stage Least Squares Regressions
(Dependent Variable: Average Growth Rate of GDP per Capita, 1980-1989)

	1	2	3	4	5	6	7	8
Investment Ratio, 1980-1989 average	0.173*** (3.58)	0.179*** (5.17)	0.130** (2.35)	0.177*** (5.19)	0.159*** (4.44)	0.176*** (6.08)	0.153*** (4.13)	0.174*** (5.21)
Human Capital, 1965	0.681** (2.31)	0.710* (1.84)	0.379 (1.08)	0.674 (1.61)	0.453** (2.27)	0.520* (1.84)	0.432** (2.12)	0.611* (1.85)
Log GDP per capita, 1965	-2.897*** (-2.83)	-2.712*** (-2.94)	-5.326*** (-2.89)	-2.716*** (-2.90)	-2.265** (-2.63)	-1.752** (-2.59)	-2.781 (-1.63)	-1.643** (-2.54)
Level Quinn's Index, 1988	0.802 (1.48)	----	-1.815 (-1.60)	----	1.088* (1.89)	----	0.465 (0.30)	----
Interaction Level Quinn 1988 x 1980 GDP per capita	----	----	0.001** (2.06)	----	----	----	0.001 (0.41)	----
Difference Quinn's Index, 1973-88	----	0.798 (1.59)	----	0.411 (0.31)	----	0.354 (0.85)	----	2.203 (1.36)
Interaction Diff. Quinn 1973-88 x 1980 GDP per capita	----	----	----	0.001 (0.33)	----	----	----	-0.001 (-1.14)
Constant	15.447** (2.39)	15.472** (2.58)	38.050** (2.64)	15.605** (2.58)	11.067** (2.12)	8.921** (2.13)	16.087 (1.11)	7.860** (2.04)
Observations	52	52	52	52	52	52	52	52
R ²	0.51	0.50	0.32	0.49	0.51	0.47	0.51	0.46

2SLS regressions. t-statistics derived using robust standard errors in parentheses. Instruments are liquid liabilities in 1970 and 1975, distance to the equator, OECD dummy and Quinn's index in 1973. Columns 1-4 are unweighted. Columns 5-8 are weighted by GDP per capita in 1985. Significance at 10%, 5%, and 1% denoted by *, **, and *** respectively. Significance at 10%, 5%, and 1% denoted by *, **, and *** respectively.

Table 3
Growth Regressions for Alternative Periods
(Dependent Variable: Average Growth Rate of GDP per Capita during Relevant Period)

	1	2	3	4	5	6	7	8
	1973-81	1982-87	1988-92	Pooled	1973-81	1982-87	1988-92	Pooled
Investment Ratio, period average	0.207*** (4.81)	0.179*** (3.27)	0.278*** (4.14)	0.223*** (6.46)	0.209*** (4.47)	0.189*** (3.44)	0.276*** (4.15)	0.226*** (6.40)
Human Capital, beginning of period	0.209 (1.06)	0.349 (1.60)	-0.478* (-1.73)	0.087 (0.63)	0.203 (1.03)	0.233 (0.99)	-0.420 (-1.50)	0.059 (0.42)
Log GDP per capita, beginning of period	-2.329*** (-3.41)	-0.986 (-1.20)	-1.113 (-1.09)	-1.546*** (-3.12)	-2.363*** (-3.11)	-1.646* (-2.00)	-0.845 (-0.63)	-1.718*** (-3.11)
Quinn's Index, beginning of period	0.264 (0.94)	0.095 (0.29)	1.131* (1.98)	0.487** (2.17)	0.246 (0.66)	-0.444 (-1.23)	1.380** (2.15)	0.365 (1.40)
Interaction Quinn * GDP per capita, beginning of period	----	----	----	----	0.001 (0.13)	0.001* (1.99)	-0.001 (-0.55)	0.001 (0.79)
Dummy for 1973-81	----	----	----	0.436 (0.87)	----	----	----	0.468 (0.92)
Dummy for 1982-87	----	----	----	-0.739 (-1.43)	----	----	----	-0.704 (-1.34)
Constant	15.663*** (3.82)	3.311 (0.63)	5.873 (0.91)	8.608*** (2.75)	15.932*** (3.31)	9.150* (1.70)	3.303 (0.34)	10.071*** (2.75)
Observations	62	62	60	184	62	62	60	184
R ²	0.34	0.29	0.27	0.26	0.34	0.33	0.27	0.27

OLS regressions. t-statistics derived using robust standard errors in parentheses. Significance at 10%, 5%, and 1% denoted by *, **, and *** respectively.

Table 4
Role of Financial and Institutional Development
(Dependent Variable: Average Growth Rate of GDP per Capita during Relevant Period)

	1	2	3	4	5	6	7	8
	1973-81	1982-87	1988-92	Pooled	1973-81	1982-1987	1988-92	Pooled
Investment Ratio, period average	0.189*** (4.55)	0.185*** (3.01)	0.304*** (3.92)	0.225*** (6.06)	0.199*** (5.16)	0.162** (2.66)	0.279*** (4.04)	0.214*** (5.92)
Human Capital, beginning of period	0.209 (0.98)	0.264 (1.49)	-0.508* (-1.69)	0.049 (0.36)	0.112 (0.62)	0.250 (1.15)	-0.471 (-1.55)	0.024 (0.17)
Log GDP per capita, beginning of period	-2.488*** (-3.22)	-0.784 (-1.04)	-1.123 (-1.03)	-1.594*** (-3.19)	-2.692*** (-3.66)	-1.569* (-1.88)	-1.070 (-0.95)	-1.882*** (-3.73)
Quinn's Index, beginning of period	0.342 (1.15)	-0.285 (-0.85)	1.142* (1.75)	0.343 (1.29)	-0.273 (-0.75)	-0.817** (-2.04)	1.194* (1.70)	0.005 (0.02)
Interaction Quinn * Financial Depth, beginning of period	0.003 (0.57)	0.002 (0.56)	-0.002 (-0.36)	0.002 (0.60)	----	----	----	----
Interaction Quinn * Law and Order, beginning of period	----	----	----	----	0.171** (2.12)	0.250** (2.44)	-0.016 (-0.11)	0.137** (2.18)
Dummy for 1973-81	----	----	----	0.421 (0.82)	----	----	----	0.369 (0.74)
Dummy for 1982-87	----	----	----	-0.690 (-1.33)	----	----	----	-0.769 (-1.48)
Constant	16.881*** (3.49)	2.508 (0.51)	5.859 (0.83)	9.212*** (2.86)	19.053*** (3.94)	8.896 (1.62)	5.450 (0.68)	11.833*** (3.51)
Observations	55	58	58	171	62	62	60	184
R ²	0.43	0.35	0.28	0.29	0.39	0.35	0.27	0.28

OLS regressions. t-statistics derived using robust standard errors in parentheses. Significance at 10%, 5%, and 1% denoted by *, **, and *** respectively.

Table 5
Role of Sequencing
(Dependent Variable: Average Growth Rate of GDP per Capita during Relevant Period)

	1	2	3	4	5	6	7	8
	1973-81	1982-87	1988-92	Pooled	1973-81	1982-87	1988-92	Pooled
Investment Ratio, period average	0.176*** (4.15)	0.103* (1.68)	0.232*** (3.11)	0.173*** (4.53)	0.146*** (3.00)	0.094 (1.49)	0.218*** (2.81)	0.154*** (3.74)
Human Capital, beginning of period	0.113 (0.58)	0.272 (1.33)	-0.556* (-1.93)	-0.002 (-0.01)	0.123 (0.63)	0.260 (1.34)	-0.529* (-1.97)	-0.001 (-0.01)
Log GDP per capita, beginning of period	-2.382*** (-3.39)	-1.149 (-1.38)	-1.339 (-1.34)	-1.759*** (-3.56)	-2.325*** (-3.40)	-1.156 (-1.40)	-1.284 (-1.24)	-1.719*** (-3.53)
Quinn's Index, beginning of period	0.194 (0.55)	-0.361 (-1.17)	0.455 (0.61)	0.224 (0.85)	0.366 (0.93)	-0.128 (-0.36)	1.049 (1.38)	0.509* (1.82)
Interaction Quinn * SW Open Index	0.458 (1.67)	0.919*** (3.54)	0.917* (1.81)	0.720*** (3.45)	-0.134 (-0.22)	0.469 (0.90)	-0.386 (-0.37)	-0.001 (-0.01)
SW Open Index, beginning of period	----	----	----	----	1.754 (1.37)	1.197 (0.78)	3.441 (1.20)	2.020* (1.83)
Dummy for 1973	----	----	----	0.690 (1.40)	----	----	----	0.677 (1.39)
Dummy for 1982	----	----	----	-0.533 (-1.04)	----	----	----	-0.542 (-1.07)
Constant	16.872*** (3.95)	6.305 (1.17)	9.186 (1.47)	11.310*** (3.69)	16.487*** (4.00)	6.115 (1.16)	7.614 (1.11)	10.679*** (3.54)
Observations	60	60	59	179	60	60	59	179
R ²	0.41	0.36	0.31	0.32	0.43	0.37	0.33	0.33

OLS regressions. t-statistics derived using robust standard errors in parentheses. Significance at 10%, 5%, and 1% denoted by *, **, and *** respectively.

Table 6
Role of Trade Distortions and the Black Market Premium
(Dependent Variable: Average Growth Rate of GDP per Capita during Relevant Period)

	1	2	3	4	5	6
	Pooled	Pooled	Pooled	Pooled	Pooled	Pooled
Investment Ratio, period average	0.235*** (6.30)	0.239*** (6.50)	0.226*** (6.17)	0.182*** (3.74)	0.189*** (3.96)	0.181*** (3.77)
Human Capital, beginning of period	-0.059 (-0.35)	-0.059 (-0.36)	-0.068 (-0.40)	-0.120 (-0.73)	-0.119 (-0.72)	-0.123 (-0.73)
Log GDP per capita, beginning of period	-1.884*** (-3.30)	-1.978*** (-3.49)	-1.819*** (-3.12)	-1.686*** (-2.90)	-1.775*** (-3.06)	-1.652*** (-2.79)
Quinn's Index, beginning of period	-0.214 (-0.49)	0.138 (0.42)	-0.963 (-1.40)	-0.158 (-0.37)	0.182 (0.56)	-0.777 (-1.12)
Interaction Quinn * Barro-Lee	0.324 (1.14)	0.522** (2.01)	0.288 (1.10)	0.055 (0.19)	0.251 (0.94)	0.068 (0.26)
Interaction Quinn * Black Market Premium 1	0.546* (1.69)	----	----	0.520 (1.64)	----	----
Interaction Quinn * Black Market Premium 2	----	0.064** (2.52)	----	----	0.056** (2.50)	----
Interaction Quinn * Black Market Premium 3	----	----	1.257** (1.99)	----	----	1.091* (1.71)
SW Open Index, beginning of period	----	----	----	1.644** (2.22)	1.597** (2.14)	1.468** (2.02)
Dummy for 1973	0.124 (0.22)	0.259 (0.46)	-0.003 (-0.01)	0.268 (0.48)	0.397 (0.72)	0.159 (0.29)
Dummy for 1982	-0.773 (-1.30)	-0.675 (-1.11)	-0.881 (-1.52)	-0.728 (-1.25)	-0.640 (-1.08)	-0.822 (-1.45)
Constant	12.159*** (3.41)	12.562*** (3.53)	12.206*** (3.37)	11.291*** (3.20)	11.637*** (3.28)	11.390*** (3.17)
Observations	141	141	141	141	141	141
R ²	0.34	0.34	0.36	0.37	0.37	0.38

OLS regressions. t-statistics derived using robust standard errors in parentheses. Significance at 10%, 5%, and 1% denoted by *, **, and *** respectively.

Table 7
Role of Trade Distortions and the Black Market Premium, Subperiod Estimates
(Dependent Variable: Average Growth Rate of GDP per Capita during Relevant Period)

	1	2	3	4	5	6
	1973-81	1982-87	1988-92	1973-81	1982-87	1988-92
Investment Ratio, period average	0.241*** (5.53)	0.193*** (3.36)	0.289*** (3.83)	0.201*** (3.23)	0.133* (1.99)	0.249*** (2.72)
Human Capital, beginning of period	0.212 (0.96)	0.103 (0.48)	-0.649* (-1.79)	0.161 (0.71)	0.018 (0.08)	-0.649* (-1.86)
Log GDP per capita, beginning of period	-3.116*** (-3.69)	-0.834 (-1.04)	-1.429 (-1.21)	-2.904*** (-3.20)	-0.749 (-0.94)	-1.255 (-1.05)
Quinn's Index, beginning of period	0.537 (0.90)	-1.867*** (-3.00)	0.533 (0.73)	0.556 (0.96)	-1.614** (-2.47)	0.548 (0.73)
Interaction Quinn * Barro-Lee	0.359 (0.89)	0.532 (1.25)	0.243 (0.41)	0.219 (0.51)	0.139 (0.27)	-0.012 (-0.02)
Interaction Quinn * Black Market Premium 1	-0.284 (-0.54)	1.016** (2.46)	0.610 (1.05)	-0.319 (-0.61)	1.136** (2.66)	0.475 (0.90)
SW Open Index, beginning of period	----	----	----	0.948 (1.04)	1.560 (1.69)	1.736 (1.10)
Constant	20.889*** (4.03)	4.754 (0.90)	9.085 (1.24)	19.997*** (3.71)	4.766 (0.91)	8.036 (1.10)
Observations	47	47	47	47	47	47
R ²	0.46	0.46	0.34	0.47	0.49	0.37

OLS regressions. t-statistics derived using robust standard errors in parentheses. Significance at 10%, 5%, and 1% denoted by *, **, and *** respectively.