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The Impact of Capital Controls on Growth Convergence

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Both cross-sectional regression and panel data methods of growth convergence estimation are used to determine the impact of various forms of capital controls on economic growth and growth convergence. We suggest a method to control for measurement error in the cross-sectional regressions based on data quality estimates. The effect of capital controls is found to be strongly dependent on the specific type of control in place, with restrictions on current account payments having the most detrimental effect on growth. A key finding is the importance of interaction effects between capital controls and openness to trade, and capital controls and initial income. The interaction between capital controls and initial income suggests that not only do capital controls impair growth, they also reduce the rate of conditional growth convergence. The omission of these interactions may explain the failure of earlier cross-sectional studies to find effects of capital controls on growth.

I. Introduction

Fischer (1995) notes that the cross-country growth study of Sachs and Warner (1995) does not address the implications of the major change in the international system since Bretton Woods - the opening of the capital account. Multinational institutions have traditionally considered restrictions on trade to be significantly more harmful than impediments to capital mobility. For example, the Articles of Agreement of the International Monetary Fund condemn the use of trade restrictions, but condone the use of capital controls. Article 6, section 3 prescribes:[1]

> "Members may exercise such controls as are necessary to regulate international capital movements, but no member may exercise these controls in a manner which restricts payments for current transactions or which will unduly delay transfers of funds in settlements of commitments, except as provided in Article VIII, Section 3(b) and in Article XIV, section 2."

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1. See Guitián (1995) for a description of the rationale behind this provision.

Although barriers to international trade in goods have been greatly reduced, restrictions on capital movements continue to be widespread in many newly industrialized and developing countries. In contrast, restrictions to capital mobility have been greatly reduced in the most advanced nations in the recent period. An important step forward in this regard was made at the 1997 IMF/World Bank annual meetings in recognizing that the purposes of the IMF should be formally extended to encompass promotion of capital account liberalization, with an amendment to the IMF's charter giving it appropriate jurisdiction over capital movements.

However the wake of the recent Asian financial crisis, and in particular Malaysia's decision to impose controls on capital outflows in its midst raises the question of whether such controls will once again become widespread. The question of the effects of capital controls on growth is therefore of renewed importance.

While there is a vast literature studying the effects of openness to trade on growth, capital controls have not received as much attention. An exception is Rodrik (1998), who examines the link between capital account liberalization and per capita growth in GDP between 1975 and 1989 for a sample of almost 100 countries by means of cross-sectional regression. Controlling for initial per capita GDP, initial secondary-school enrolment rates, an index for the quality of government institutions and regional dummies he finds capital controls to be essentially uncorrelated with growth performance. He uses this finding to argue that the benefits of removing capital controls remain to be demonstrated, and to further argue against the implementation of IMF capital-account convertibility. This study has had some influence, especially with renewed interest in controls on capital flows by Russia and some Latin American governments.[2] Grilli and Milesi-Ferretti (1995) direct their attention to the issue of capital mobility, and carry out simple panel regressions using instrumental variables estimation. They do not find any robust correlation between their measures of capital controls and the rate of growth, although, in common with several other studies, they find evidence that countries with large black market premia grow more slowly.

In this paper we employ both cross-sectional and panel data methods of estimating growth convergence to examine the effects of capital controls more thoroughly, and thus determine the degree of empirical support for capital account liberalization. Several measures of capital controls are considered, and we explicitly allow for interaction effects between capital controls and other variables suggested by theory. In the process we utilize panel data methods to control for certain forms of measurement error, which is a problem endemic in cross-country comparisons of this nature. We find that the form which capital controls takes is important, with "restrictions on current account payments" having the strongest effect on growth convergence, and we find evidence of interaction effects between capital controls and openness to trade, and capital controls and initial income. The omission of these important interactions appears to have been a key determinant in the failure of the earlier cross-sectional studies cited to find effects of capital controls on growth.

2. The results have even made their way into the popular press. See "Currency Controls are Getting a Hearing Amid Asia Crisis", in the *Wall Street Journal* of September 4, 1998.

The structure of the paper is as follows. In Section II the theoretical arguments for and against the use of capital controls are summarized, with emphasis on their import for economic growth. Section III outlines the econometric models used in this paper. In Section IV the data used in this empirical study are described, while Section V, the main contribution of this paper, contains the empirical results and discusses them in the context of previous literature and the theoretical motivation provided in Section II. Finally Section VI concludes.

II. Pros and Cons of Capital Controls

Guitián (1995) enumerates the traditional benefits of capital account liberalization. Theoretically, the main benefit of an open capital account is that allowing individuals and firms access to international capital markets provides greater scope for risk diversification and intertemporal consumption smoothing. A second argument for an open capital account in developing countries is that capital inflows will finance increased investment, making it possible to have a larger capital stock and place the country on a potentially higher growth path than otherwise. Furthermore, capital controls can induce distortions and give rise to inefficiencies in the domestic financial system that will impair the competitiveness of the economy at large, through inhibiting certain types of external financial transactions, limiting access to international markets and restricting domestic financial market competition.

In terms of growth convergence the above arguments suggest several roles for capital mobility. Firstly, in so far as capital controls induce distortions in the economy, they may lower the steady state level of income. Secondly, the risk diversification argument suggests that in a stochastic framework, the stochastic steady state level of income will be less volatile in the presence of capital mobility. Finally, if capital mobility provides a way to mobilize resources and international funds, this would result in a faster rate of convergence.

Despite these enormous advantages that openness to capital flows offers, capital controls have been, and in many cases continue to be, widely used. Guitián (1995) and Grilli and Milesi-Ferretti (1995) discuss four categories of justification often used by supporters of the imposition of capital controls: i) Containment of Balance of Payments instability or undue exchange rate volatility, through limiting short-term capital flows, ii) Retention of domestic savings and prevention of excessive foreign ownership of domestic factors of production, iii). Taxation of domestic capital and financial transactions and iv) Reinforcement of domestic stabilization and reform efforts. Free capital flows can be destabilizing when a country implements a stabilization or structural reform plan and an extensive literature exists on the optimal sequencing of reform, stressing the effects of liberalizing the capital account on the real exchange rate.

In addition, theory does provide us with situations in which capital controls can increase growth. The theory of second-best gives that in the presence of other distortions in the economy, removing a proper subset of these distortions need not be Pareto superior to the prior situation. An extremely relevant case in point is emphasized by Brecher and Díaz-Alejandro (1977), who show that the impact of capital mobility in the presence of significant distortions to trade will be a misallocation of the world's capital and even a worsening of the economic well-being of the capital-importing country. The argument is that if capital flows freely into a labour-abundant country that protects its capital intensive industries, the world's

capital stock will be misallocated, the country's national product at world prices will be reduced, and the country's national income will be further reduced by the payment of returns to the foreign capital. This suggests that there may be interaction effects between capital controls and openness to trade. As a second example of second-best theory, if the domestic financial sector is distorted, the free flow of financial capital, which would be otherwise welfare-enhancing, could well be welfare-reducing. In this vein, Devereux and Saito (1997) show in an environment of incomplete markets, capital controls may increase world growth. In the presence of multiple equilibria, capital controls can prevent capital flight from shifting the economy to a low-level equilibrium.

The above discussion shows that theory does not provide an unambiguous prediction as to the effects of capital controls on growth. In the end the consequences of relaxing capital controls will depend to a large extent on whether the domestic financial system is sound or weak. For example, Hanson (1995) attributes the disappointing experience of the Southern Cone countries in the late 1970's and early 1980's as largely due to lax banking regulations. Logically, opening to capital movements should speed convergence, but globalization of capital flows tends to punish bad policies and reward good policies more than previously. This suggests faster convergence may be possible for countries with good policies, but perhaps worse convergence will result for countries with bad policies.[3] This punishment-reward system may indirectly promote convergence through inducing convergence to "good" policies.

III. Econometric Model and Tests

1. Cross-sectional Regression

a. The Cross-sectional Model

The cross-sectional growth regression popularized by Barro (1991) is based on estimation of an equation of the form:

$$g_{it,T} = 100\left(\ln\left(y_{i,T}\right) - \ln\left(y_{i,t}\right)\right)/T = a + b\ln\left(y_{i,t}\right) + \Psi X_{i,t} + \varepsilon_{i,t} \qquad (1)$$

where $g_{it,T}$ is the percentage average growth rate between time t and T, X_{it} is a vector of variables that hold constant the steady state of the economy i, and $b = -100(1 - e^{bT})/T$. If the estimate of b is negative, we have conditional β-convergence.

b. Data Quality and Measurement Error

The standard approach in the literature consists of treating each country as an identical, independent observation insofar as estimation is concerned. However there are clear

3. The lack of suitable data measuring the adequacy of the domestic financial system in a large cross-section of countries prevents this hypothesis from being examined empirically.

differences in the quality of data from different countries. Summers and Heston (1991) provide a rough estimate of the quality of each country' s data in the form of an ten-category letter grade, ranging (A, A , B+, B, B ,C+, C, C , D+, D).

One would expect that the variance of the measurement errors increases with deteriorating data quality, but the extent to which this occurs is less obvious. Summers and Heston (1991) suggest the percentage accuracy of the price surveys used in constructing the Penn World Tables, interpreted in terms of 95 percent confidence intervals, ranges from 60 percent up or down for countries with GDPs per capita[4] less than a tenth of the United States, to 19 percent up or down for countries with between half and seven-tenths of the United States, and 15 percent for countries with as close as seven-tenths of the United States. This suggests the standard deviations are four times as great for the lowest quality countries as the highest. This provides a basis for weighted least squares, assigning A = 1, A = 2,...,D = 10 we let the variance be the grade multiplied by a scalar, and thus use the inverse of the square roots of the grades as regression weights.[5] The resulting regression places relatively more weight on the observations which have higher data quality, i.e. lower measurement error, and therefore the estimated coefficients will exh ibit less measurement error bias.[6] The weighted least squares approach used here can be viewed as treating measurement error in the dependent variable, and thus we improve the efficiency of the estimates by this form of GLS. In this case the ordinary least squares estimates are still taken to be unbiased, but inefficient. We also utilize panel data techniques as an alternative way of correcting for measurement error.

2. The Panel Data Approach

a. The panel data model

Islam (1995) shows that the standard neoclassical growth model can also be formulated in terms of a dynamic panel data model of the form:

$$y_{i,t} = \gamma y_{i,t-1} + \delta' x_{i,t} + \lambda_t + \alpha_i + u_{i,t} \qquad (2)$$

where $y_{i,t} = \ln(y_{i,t2})$, $y_{i,t-1} = \ln(y_{i,t1})$, $\gamma = e^{\beta t}$, $\tau = t2 - t1$, $\lambda_t = g(t2 - e^{\beta t} t1)$, $\alpha_i = (1 - e^{\beta t}) \ln A(0)$ and $u_{i,t}$ is the transitory error term that varies across countries and time periods, and has

4. Note these estimates are relative to per capita incomes, whereas the data quality grades they provide are not perfectly correlated with incomes.
5. An alternative would be to use a linear weighting scheme. This would further downweight observations on the poor data quality countries.
6. A potential downside of this method is that it may show convergence, not as a result of corrections for measurement error, but as a consequence of weighting more heavily countries with better data quality, such as the OECD countries, amongst which absolute convergence is more likely to occur. Our empirical analysis shows the coefficients do not change very much, alleviating this concern for this study.

mean zero. Again the parameter b represents the rate of conditional convergence. The a_i term represents individual country-specific effects, and can potentially capture institutional and technological differences between countries. The cross-sectional regression approach of Barro (1991) and Sachs and Warner (1995) ignores these effects, creating omitted variable bias. The panel data framework makes it possible for us to correct for this bias, and examine the influence of persistent cross-country technological and institutional differences on growth.

This panel data specification is obtained by moving from a single cross-section spanning the entire period (1960-1989 for our data) to cross-sections for the several shorter periods which constitute it. A key question which arises concerns the appropriate length of these time spans. The use of annual data gives maximum power for estimators relying on variation in the time dimension. However as Barro (1996) remarks, the underlying theory relates to long-term growth and the precise timing between growth and its determinants at high frequencies is not as well specified. Hence relationships at the annual frequency may be dominated by short-term or cyclical fluctuations. Data availability considerations mean that we opt for five-year time intervals, which is also the time span used by Islam (1995), among others. Thus considering the thirty year period 1960-89, we have six data (time) points for each country, with beginning years 1960, 1965, 1970, 1975, 1980 and 1985 respectively. With this setup the $u_{i,t}$ terms are now five calendar years apart, and hence may be thought to be less influenced by business cycle fluctuations and less likely to be serially correlated than with a yearly setup.

For our data set, the variables in $x_{i,t}$ consist of an education variable (taken at the start of each five-year period), five year averages of our measures of openness, capital controls, government spending to GDP, investment to GDP, and population densities. It also includes time-invariant variables measuring political stability[7] and whether or not the country is landlocked and in the tropics.

b. Estimation Techniques

The presence of a lagged dependent variable term in (2) means that the least-squares dummy variable (LSDV) estimator under the fixed effects formulation is inconsistent for the small T, large n case, although it is consistent in the direction of $T \to \infty$, fixed n case.[8] Theoretically, our sample is in the second situation, as the number of countries is fixed, whilst the time period will continue to grow indefinitely. However, in practice our sample resembles more the first case. This indicates that while we may expect the LSDV estimator to be consistent, its small sample properties may well be poor.

Alternatively, Anderson and Hsiao (1982) note that by differencing (2) we obtain:

7. Of course political conditions vary over time, but the Sachs and Warner data available consists of solely an average observation.

8. See Hsiao, 1986 for a demonstration of this.

$$y_{i,t} - y_{i,t-1} = \gamma(y_{i,t-1} - y_{i,t-2}) + \delta'(x_{i,t-1} - x_{i,t-2}) + (\eta_t - \eta_{t-1}) + (\varepsilon_{i,t} - \varepsilon_{i,t-1}) \quad (3)$$

Then if the $\varepsilon_{i,t}$ are serially uncorrelated, either $y_{i,t-2}$ or $(y_{i,t-2} - y_{i,t-3})$ are valid instruments for the regressor $(y_{i,t-1} - y_{i,t-2})$. Arellano (1989) finds that for simple dynamic error components models the estimator that uses the levels $y_{i,t-2}$ rather than differences $\Delta y_{i,t-2}$ is to be preferred. Then for the first period we observe (3), i.e. for $t = 3$, we have that $y_{i,1}$ is a valid instrument. Then for $t = 4$, both $y_{i,2}$ as well as $y_{i,1}$ are valid instruments, and likewise for period T the set of valid instruments becomes $(y_{i,1}, y_{i,2}, \ldots, y_{i,T-2})$. We treat the regressors as predetermined variables, in which case the lagged $x_{i,t}$'s can also be used as instruments.[9] Using these instruments, one can obtain the Arellano and Bond (1991) preliminary one-step consistent estimator and two-step GMM estimators.[10] The one-step and two-step estimators are asymptotically equivalent if $\varepsilon_{i,t}$ are independent and homoskedastic across units and over time. The consistency of the Arellano-Bond GMM estimators depend critically on the assumption that $E(\Delta\varepsilon_{i,t}\,\Delta\varepsilon_{i,t-2}) = 0$, that is on lack of second-order serial correlation in the first-differenced residuals. We use the m_2 test statistic given in Arellano and Bond (1991) to test this assumption. The Arellano-Bond methodology is based on the presumption that there are unobservable individual effects in the data. We test this by means of a Holtz-Eakin(1988) test for the presence of these individual effects in dynamic panel data models, which utilizes the additional restrictions on sample moments implied by the absence of individual effects.

c. Measurement Error

Srinivasan (1994, p.4) provides a powerful and detailed warning of the problems with data measures for development analysis, noting that "...published data, national and international, suffer from serious conceptual problems, measurement biases and errors, and lack of comparability over time within countries and across countries at a point in time". He goes on to provide evidence of unreliability and bias in practically every category of variable commonly used in empirical studies of development analysis. This problem is exacerbated for the macro level data used in this study due to the interspatial and intertemporal extrapolation used in constructing data in the Penn World Tables.

Using panel data, Griliches and Hausman (1986) showed that one can identify and estimate a variety of errors in variables models without the use of external instruments. This

9. In a growth regression context, assuming the $x_{i,t}$ to be exogeneous is not desirable. The use of the lags as instruments can also help address possible endogeneity in capital account liberalization based on economic performance.

10. We do not utilize the additional moment conditions which can arise from assumptions about the initial value (as in Ahn and Schmidt (1995)) due to concerns about initial assumptions in a growth regression context, and also to the fact that IV estimators of this form can perform poorly in small samples.

correction can be particularly important in panel data models because the use of the fixed effects estimator to control for omitted variable bias can attenuate the bias arising from measurement error.[11] The Holtz-Eakin, Newey and Rosen (1988) methodology provides a way to test for the presence of measurement error. Suppose the true data $x_{i,t}$ and $y_{i,t}$ are unobserved and instead we observe noisy measures:

$$\tilde{x}_{i,t} = x_{i,t} + e^{x}_{i,t} \text{ and } \tilde{y}_{i,t} = y_{i,t} + e^{y}_{i,t} \quad (4)$$

where we assume the measurement errors $e^{x}_{i,t}$ and $e^{y}_{i,t}$ are uncorrelated with all x and y observations and are uncorrelated across time. Substituting (4) into (3) we obtain:

$$\tilde{y}_{i,t} - \tilde{y}_{i,t-1} = g\left(\tilde{y}_{i,t-1} - \tilde{y}_{i,t-2}\right) + \delta'\left(\tilde{x}_{i,t} - \tilde{x}_{i,t-1}\right) + w_{i,t} \quad (5)$$

where:

$$w_{i,t} = \left(u_{i,t} - u_{i,t-1}\right) + \left(e^{y}_{i,t} - e^{y}_{i,t-1}\right) - g\left(e^{y}_{i,t-1} - e^{y}_{i,t-2}\right) - \delta'\left(e^{x}_{i,t} - e^{x}_{i,t-1}\right) \quad (6)$$

The assumption that the measurement errors are uncorrelated across time means the vector $Z_{i,t} = \left[\tilde{y}_{i,t-3}, \ldots, \tilde{y}_{i,1}, \tilde{x}_{i,t-2}, \ldots, \tilde{x}_{i,1}\right]$ will be uncorrelated with $w_{i,t}$ and thus qualify as the vector of instrumental variables for Equation (5). That is we can modify the estimation procedure above to accommodate measurement error by simply using a different set of instrumental variables. Intuitively different differencing procedures to eliminate fixed effects have different implications for the measurement error bias, in a manner which depends on observables. The instrument vector $Z_{i,t}$ combines all the various moment conditions which arise from this idea. One can then test the null hypothesis of no measurement error by use of a Hausman test based on the estimators constructed from the two alternate sets of instruments.

d. Heterogeneous Slopes

Robertson and Symons (1992) show that failure to allow for slope parameter heterogeneity in a panel data set-up can lead one to erroneously conclude that a dynamic relationship exists, when the true model is static with heterogeneous slopes. Lee, Pesaran and Smith (1997, 1998) extend this result and apply it in the context of the growth convergence literature. They observe that heterogeneity in growth effects and in speeds of convergence renders Islam's estimator inconsistent, as his estimator depends critically on "poolability" - the presence of common growth rates across countries, and note when the slope coefficients differ across groups, there exists *no* consistent instrumental variables estimator.

11. Griliches and Hausman (1986) give sufficient conditions for the fixed effect estimator to suffer greater attenuation bias than the least squares estimator in this case.

This issue is of importance in both an econometric and a theoretical sense. We have argued in Section II that there is a theoretical case for capital controls and openness to trade to impact on the rate of conditional convergence as well. This suggests that one may allow for slope heterogeneity through the insertion of interaction terms between lagged per capita GDP and capital controls and/or openness. One can think of this heterogeneity of a limited form as an intermediate case between homogeneity and the "full" heterogeneity of Lee, Pesaran and Smith (1997) whom explicitly fit separate slope coefficients for each country.

Pesaran and Smith (1995) discuss appropriate estimators under slope heterogeneity and conclude that there are very real difficulties in obtaining precise estimates of the speed of convergence. They suggest that one appropriate estimator is the mean group estimator, which involves running a separate regression for each country, and then averaging the coefficients to obtain the mean convergence rate. This is consistent for large n and T, in particular T must be large enough that it is sensible to run separate regressions for each group. They note that this procedure suffers from small sample downward bias in the coefficient on lagged real GDP per capita, where this bias still may be important even for T as large as 30. It was noted above that we had opted for 5-year time intervals for the panel estimation, resulting in $T = 6$. The lack of suitable data on certain variables, such as human capital, on an annual basis, together with the aforementioned business cycle concerns which arise when one attempts to use data of shorter time intervals means that we restrict our examination of slope heterogeneity to the interaction form discussed above.

IV. The Data

De Long (1988) provides a powerful demonstration that choosing the countries which have the most data available tends to result in overselection of industrialized countries, inducing an upwards bias in the estimated convergence rate. An overriding principle used in this paper was the intention to maintain as many countries as possible in the sample in order to alleviate concerns regarding sample selection bias. There was a tradeoff between this objective and the desire for a long time series in the panel, which increases the power of the panel estimation techniques. No observations were available for many developing countries until 1960, and again observations after 1989 were not complete. Thus this study focuses on the 30 year period 1960-89. This deficiency in the coverage period of the Penn World Tables is particularly vexing for studying the effects of capital controls given the massive increase in world capital flows during the 1990s, a period not covered by our sample.

The principal source of macroeconomic variables was the Penn World Tables 5.6. This was supplemented by human capital from the Barro-Lee data set and political and geographical variables from the Sachs-Warner data set. The variables used are for the most part common to the cross-country growth literature and hence discussion will be limited to the variables used to measure the presence of capital controls.

1. The Data on Capital Controls

Data from the IMF Annual Report on Exchange Arrangements and Exchange Restrictions are used to detail the presence of various forms of restrictions on capital flows. I

follow Grilli and Milesi-Ferretti (1995), who construct dummy variables for restrictions on payments for current and capital account transactions and multiple exchange rates, with the variable taking a value of one when a restriction was in place for a given year and zero otherwise. They focus on three forms of restrictions. The first is "Restrictions on Payments for Capital Transactions" (CAPCONTR) and refers exclusively to resident-owned funds. This is the measure used by Rodrik (1998). The second restriction is "Separate Exchange Rates for some or all capital transactions and/or some invisibles" (MULTER). This can be broadly interpreted as a form of control on capital flows, reflecting multiple currency practices. The third form of restriction is "Restrictions on Payments for Current Transactions" (CURRCON) and is included because current account transactions can be used to partially evade restrictions on capital flows through practices such as leads and lags in export billing and overinvoicing of imports.[12]

I also consider three measures for capital controls used by Lewis(1996). Firstly, she considers an indicator of whether countries are in interest arrears (ARREARS), which is intended to capture constraints on borrowing and other forms of international capital market transactions. Secondly, the dummy variables BILAT1 and BILAT2 indicate whether a country has bilateral payments arrangements with IMF members and non-members respectively. Countries that have currencies that are not heavily traded sometimes use bilateral payments arrangements, which enable payments for goods to be made with other goods in lieu of currency payments. Lewis suggests that residents of countries that find it necessary to enter into such arrangements may well face higher costs of transacting in world capital markets.

The use of dummy variables to measure capital controls and restrictions limits the extent to which one can examine the intensity of these controls. Grilli and Milesi-Ferretti (1995) argue that to some extent the current account restrictions dummy variable can proxy for the intensity of controls. Furthermore, the presence of multiple types of the restrictions enumerated above can further proxy for the degree of controls. To this end, the variable CONTROLS was constructed as an equally weighted index of CAPCONTR, CURRCON and ARREARS and is considered in the regressions that follow.

Table 1 summarizes the correlations between the seven measures of capital controls, the Sachs-Warner openness variable, the ratio of total trade to GDP, the real GDP per capita in 1960 and the average growth rate. As expected, there is positive correlation between the various forms of capital controls, however the correlation is weak in some cases indicating a reasonable degree of variability in the form which capital controls take. The first four capital control measures have a negative correlation with initial income, openness and growth, with this correlation greatest for CURRCON. The exchange rate variables have low correlations with growth. We note the expected positive correlation between openness to trade and economic growth, and a positive correlation between initial income and growth, indicating a lack of absolute convergence.

12. Quinn and Inclán (1997) use this same source to construct an index of financial openness on a 0-14 scale for 21 OECD countries. This approach is similar in spirit to the index of controls used here.

Following Sachs and Warner (1995) we classify the data into open and closed groups to focus on growth performance within each group. Within the closed group, average growth was 1.44 percent whereas within the open group average growth was 3.43 percent. Countries are then further classified according to the relative degree of capital controls present. Countries are classified as having high controls if the average level of capital controls in the country is greater than world median average level of controls, where the index measure CONTROLS described above is used as the measure of controls. Table 2 shows the frequency of growth rates above and below 3 percent per year for all countries in our sample. We see that among the open countries there were no low growth countries with high capital controls, whereas there were countries with low capital controls and low growth which were open, so that open economies with high capital controls grew faster on average than open economies with low capital controls. The largest group of countries in our table consists of closed economies with high capital controls that experienced low growth. Table 2 thus indicates that the effects of capital controls on growth are not as clear-cut as those of openness to trade, and suggests the possibility of interaction effects between openness to trade and the degree of capital controls.

Table 2 Growth, Openness and Capital Controls 1960-89

Growth Rate	Always Open		Not Always Open	
	High Capital Controls	Low Capital Controls	High Capital Controls	Low Capital Controls
Average growth > 3.0%	6	8	6	3
Average growth < 3.0%	0	8	42	15
Mean Growth	3.9	3.39	1.31	1.58

Test statistic of conditional independence based on Table 2

Conditional on:	Test Statistic	p-value
high capital controls	23.630	0.000
low capital controls	4.302	0.038
always open	4.714	0.030
not always open	0.193	0.660

Tests given are 2×2 chi-squared tests of the null hypothesis of no association, and have a χ^2_1 distribution under the null.

V. Empirical Results

1. Cross-sectional Growth Regressions

We now turn our attention formally to the question of the effects of capital controls on growth convergence. In order to facilitate comparison of our work with the vast body of empirical growth literature that precedes it, we first carry out some of the more standard income growth regressions. This enables the effects of differences in countries sampled and sampling period to be clearly seen. Table 3 follows the form of Table 11 in Sachs and

Warner (1995) and reports the results of regression analysis examining the relationship between growth in real GDP per capita between 1960 and 1989 and initial income in 1960. We see in regression 1 that for the entire sample of countries we get statistically significant *divergence* rather than convergence. In contrast, once we restrict attention to the open countries in regression 2, we obtain strong evidence of convergence. The coefficient on initial income suggests that within the group of open countries we observe absolute b-convergence at a rate of 1.54% per annum. Regression 4 shows the absence of convergence among the closed economies. In regression 5 we see the importance of openness continues to hold true in the presence of other explanatory variables in a Barro-style growth regression. In common with previous literature we find conditional convergence, with positive effects of educational attainment, positive effects of the investment-to-GDP ratio and negative effects of the ratio of government spending to GDP. The average number of revolutions and coups per year and the average number of assassinations have insignificant effects. Sachs and Radelet (1997) consider further the impact of geographical variables on growth, and we include two of these in regression 6, along with a dummy variable for Sub-Saharan Africa. In accordance with their results we find that both being landlocked and being in the tropics have significant negative impacts on average growth rates, and a further negative growth effect for Sub-Saharan countries. Once these variables were added the revolutions and coups, assassination and relative price of investment goods variables were still not significant and were dropped from the model. The estimated rate of conditional b-convergence in this model is 2.30% per annum. Regression 6 therefore provides the base model with which to investigate the effects of adding capital control measures.

Table 3 Regressions Explaining Income Growth

Independent	Regression								Measurement Error
Variable	1	2	3	4	5	6	7	8	9
Constant	–0.944	13.629	0.190	9.571	11.668	10.113	13.727	17.094	17.525
	(–0.827)	(5.481)	(0.120)	(5.618)	(6.645)	(5.222)	(6.512)	(6.405)	(7.803)
LGDP60	0.400	–1.239	0.177	–1.175	–1.499	–1.311	–1.678	–2.088	–2.131
	(2.723)	(–4.334)	(0.808)	(–5.383)	(–7.364)	(–5.821)	(–7.105)	(–6.869)	(–8.334)
OPEN					1.652	1.408	1.476	1.639	1.258
					(6.076)	(4.937)	(5.822)	(6.919)	(5.431)
SECM60				0.035	0.046	0.037	0.032	0.038	0.031
				(2.021)	(3.848)	(3.201)	(3.684)	(3.643)	(3.691)
GOVT/GDP				–0.083	–0.084	–0.070	–0.057	–0.058	–0.046
				(–2.793)	(–2.576)	(–2.035)	(–1.778)	(–1.916)	(–2.017)
REVCOUP				–0.681	–0.028	0.041			
				(–1.126)	(–0.043)	(0.066)			
ASSASSP				–1.690	–1.175	–1.223			
				(–0.838)	(–0.785)	(–0.789)			
PPI60DEV				–0.226	–0.260	–0.277			
				(–0.677)	(–0.808)	(–0.903)			
INV6089				0.147	0.120	0.120	0.100	0.089	0.100
				(5.907)	(5.192)	(5.184)	(4.678)	(4.824)	(5.891)

Table 3 (Continued)

Independent	Regression								Measurement Error
Variable	1	2	3	4	5	6	7	8	9
POP60/LAND						0.653	0.614	0.492	0.576
						(2.726)	(3.813)	(3.624)	(4.253)
ACCESS							–0.687	–0.578	–0.460
							(–2.179)	(–2.050)	(–1.857)
TROPICS							–0.838	–1.058	–1.206
							(–2.713)	(–3.299)	(–3.759)
SSAFRICA							–0.657	–0.422	–0.533
							(–1.927)	(–1.386)	(–1.575)
CURRCON								–6.451	–6.715
								(–2.628)	(–2.846)
CURRCON×LGDP								0.852	0.847
								(2.760)	(2.811)
Summary Statistics									
Adjusted R^2	0.031	0.496	–0.008	0.452	0.569	0.584	0.666	0.713	0.885
Sample Size	112	25	74	93	86	86	86	85	85

Source: Author's regressions based on data described in appendix.

The dependent variable is G6089, the real annual per capita growth in GDP over 1960-89. **T-statistics** are shown in parentheses and are based on White-corrected standard errors. Regressions are carried out via OLS, except regression 9, where data quality weights are used.

LGDP60 is the natural log of real GDP per capita in 1960.

OPEN is a dummy variable set equal to one for open economies.

SECM60 is the percentage of "secondary school attained" in the male population in 1960.

GOVT/GDP is the ratio of real government expenditures to real GDP, averaged over the period 1960-89.

REVCOUP is the number of revolutions and coups per year, averaged over the period 1970-85.

ASSASSP is the average number of assassinations per million population, 1970-85.

PPI60DEV measures the relative price of investment goods. It is the deviation of the log of the price level of investment from the cross-country sample mean in 1960.

INV6089 is the ratio of real gross domestic investment to real GDP, averaged over the period 1960-89.

POP60/LAND is the population density in 1960.

ACCESS is a dummy variable set equal to one for a landlocked country.

TROPICS is the approximate proportion of land area subject to a tropical climate.

SSAFRICA is a dummy variable taking the value of one for countries in SubSahara Africa.

CURRCON is a dummy variable taking the value of one when restrictions on payments for current account transaction are in place.

a. Adding Capital Controls to the Standard Model

Table 4a summarizes the results of cross-sectional regression estimations with different measures of capital controls. In each regression the variable indicated is added to the regression model in Column 6, Table 3. The first six regressions add separately each of the six measures of capital controls averaged over the period 1960-89, and in the seventh we use an index measure constructed by equally weighting the first four controls listed. Looking

at these results, one would conclude that none of these measures of capital controls have a significant impact on growth, which is in accordance with the results of Rodrik (1998). However, our theoretical discussion on the pros and cons of capital controls suggests that this simplistic model is not sufficient. Firstly, theory (and Table 2) suggest that there may be interaction effects between openness to trade and capital controls. Regressions (viii) through (xi) investigate this. First we find that using our controls index, again there is no significant effects. However, once we consider specific forms of the capital controls, the conclusion changes. Restrictions on current account payments have a negative level effect, and a positive interaction effect. This indicates that the presence of such capital controls is associated with a reduction in annual growth rates, but that this effect is lower for open countries than closed countries, formalizing the insights gained from Table 2. For interest arrears and the multiple exchange rate variables we obtain no significant interactions.

Secondly, the theoretical discussion shows that in addition to affecting the growth rate, capital controls may influence the rate of convergence. Exploring this possibility, we found that the interaction between capital controls and initial GDP per capita was significant only for restrictions on current account payments, as summarized in row (xii) of Table 4a. We see that the presence of these restrictions has a significant large negative effect on growth rates, and furthermore reduces the rate of conditional convergence.[13] Finally in row (xiv) we determine whether there are both interaction effects with openness to trade and initial GDP. We see the latter interaction term is stronger, whilst the interaction term with openness is no longer significant. This suggests the model in row (xii) is most appropriate, and full regression results for this model are shown in Column 7 of Table 3. The other variables maintain their signs, and the coefficient on initial income is now more negative. This shows faster convergence for countries without controls on current account payments than in Regression 5 which pools all countries together. The rate of conditional b-convergence for countries without capital controls is estimated to be 3.2% per annum, which is almost a full percent faster than the result from Regression 5. The regional dummy for Sub-Saharan Africa is now insignificant, indicating that allowance for the effects of capital controls can explain lower growth in this region.

Given that restrictions on current account payments have an interaction term with initial income, one should consider the possibility that openness to trade also interacts with initial income. Adding this interaction term to the model in regression 6, Table 3, one could not reject the null hypothesis of no interaction. Likewise the addition of additional capital control variables and regional dummies to this model had no significant effects. The insignificance of restrictions on capital account transactions may arise from the lack of cross-sectional variation in this measure, with 80% of the observations having this form of controls over the sample period, as opposed to 58% with restrictions on current account payments.

13. As a referee pointed out, this result also suggests that the restrictions on current account payments begin to have a positive effect on growth rates in the highest income countries. The countries in the sample with levels of income above this threshold all have no such restrictions, and thus one should be cautious in extrapolating the fitted linear interaction to the very high income group of countries.

Table 4a Adding Capital Controls to the Base Regression

Regression	Variables	Coefficient	t-statistic	p-value
I	CAPCONTR	0.440	1.679	0.097
ii	CURRCON	–0.013	–0.034	0.973
iii	MULTER	0.159	0.467	0.642
iv	ARREARS	0.185	0.528	0.599
v	BILAT1	0.103	0.375	0.709
vi	BILAT2	0.075	0.269	0.789
vii	Controls	0.132	0.310	0.758
viii	Controls	0.021	0.035	0.972
	Open×Controls	0.292	0.466	0.643
ix	CAPCONTR	0.158	0.322	0.748
	CAPCONTR×Open	0.499	0.901	0.371
x	CURRCON	–0.522	–1.121	0.266
	CURRCON×Open	1.243	2.402	0.019
xi	ARREARS	0.358	0.724	0.472
	ARREARS×Open	–0.481	–0.843	0.402
xii	CURRCON	–6.451	–2.628	0.011
	CURRCON×LGDP	0.852	2.760	0.007
xiii	CAPCONTR	–1.480	–0.540	0.591
	CAPCONTR×LGDP	0.236	0.725	0.471
xiv	CURRCON	–5.213	–2.151	0.035
	CURRCON×Open	0.645	1.286	0.202
	CURRCON×LGDP	0.654	2.069	0.042

Notes: **T-statistics** (shown in parentheses) are calculated based on White-adjusted Standard Errors.

In each case the Regression equation estimated has the variable(s) shown added to the model of Column 6 in Table 3.

Capital Control Variables in cross-sectional regressions are pooled time averages.

LGDP is the natural log of real GDP per capita in 1960.

OPEN is a dummy variable set equal to one for open economies.

ARREARS: Dummy variable taking the value one when the country is in interest arrears.

BILAT1: Dummy variable taking the value one when the country has a bilateral payments mechanism with a member country.

BILAT2: Dummy variable taking the value one when the country has a bilateral payments mechanism with a non-member country.

CAPCONTR: Dummy variable taking the value of one when capital controls are in place, zero otherwise. Capital controls are defined as "Restrictions on payments on capital transactions".

CURRCON: Dummy variable taking the value of one when restrictions on payments for current account transaction are in place, zero otherwise.

MULTER: Dummy variable taking the value of one when multiple exchange rates, defined by "Separate exchange rates for some or all capital transactions and/or some or all invisibles."

b. Measurement Error

Using data quality measures to carry out weighted least squares results in the estimates given in column 9 of Table 3. We see that the adjusted R^2 has increased from 0.713 to 0.885, indicating that the procedure suggested has been successful in eliminating noise in the observations. The parameter estimates do not appear to differ much from those in Regression 7, indicating that the presence of relative measurement errors does not have a great impact on our results.

c. Measurement of Openness to Trade

Fischer (1995) suggests that the extraordinary categorical results of Sachs and Warner regarding the effect of openness to trade on convergence may have to do with the noncontinuous nature of their openness variable. Furthermore, Rodriguez and Rodrik (1999) find that the Sachs-Warner openness dummy derives most of its power from two components - the black market premium and whether there is a state monopoly on exports. They contend that the former reflects underlying macroeconomic problems and the latter is essentially a Sub-Saharan Africa dummy, and hence captures very little about openness to trade. Given the necessity of capital market restrictions for a high black market premium to exist, it is possible that this openness measure is also capturing some of the effects of capital controls.

An alternative, continuous measure of openness is the ratio of exports plus imports to GDP.[14] This provides a less direct measure of the openness of the trade regime than the Sachs and Warner measure, and is more a reflection of the effect of trade volume rather than of a trade regime. Nevertheless, such a measure provides a robustness check, and by including population density and landlockedness we control for some of the effects of country size and geographical location on trade volume. Regressions 3 and 8 in Table 3 explore the sensitivity of the results to this alternate measure of openness to trade. Regression 3 supports Fischer's hypothesis in that controlling including only the new openness measure results in growth divergence. However, once one controls for additional variables, the results in regression 8 follow as before, albeit with less precision.[15]

d. Sensitivity Analysis

A common criticism of cross-sectional growth regressions is their lack of robustness to changes in the set of variables included. Table 4b shows the estimated coefficients on the CURRCON and CURRCONxLGDP variables for a variety of alternate model specifications. The variables retain their significance, and the coefficients do not change greatly in

14. This is similar to the approach of Milesi-Ferretti and Razin (1996) who use the ratio of exports to GDP to define the degree of openness of a country. Rodriguez and Rodrik (1999) detail other measures used in the literature, most of which are not available for many countries in our sample.

15. Although the coefficients on the current account restrictions are not significant in Regression 8, dropping the population/land ratio and the sub-Sahara Africa dummy results in significant coefficients at the 10% level.

magnitude, even when only an intercept and initial income are the only other right-hand side variables. This indicates that the interaction found is a robust one.

Table 4b Sensitivity Analysis of Model in Regression 7, Table 3

Other variables	Coeff. On CURRCON	T-stat	Coeff. On CURRCON×LGDP	T-stat
1. c, LGDP	−6.942	−2.406	0.853	2.290
2. c, LGDP,OPEN, SECM60, GOVT/GDP, POP60/LAND, ACCESS, TROPICS, SSAFRICA	−7.314	−2.383	0.950	2.521
3. c, LGDP,OPEN, SECM60, GOVT/GDP, POP60/LAND, ACCESS, TROPICS	−8.598	−2.663	1.113	2.814
4. c, LGDP,OPEN, SECM60, GOVT/GDP, POP60/LAND, ACCESS, TROPICS, SAFRICA, REVCOUP, ASSASSP, Openness	−7.011	−2.235	0.896	2.330

Results show the sensitivity of the coefficients on the CURRCON and CURRCON×LGDP variables as one changes the other right-hand side variables present.

2. Panel Data Results

Table 5 presents results from estimating the panel data form of our model. The model adds the CURRCON variable to our base model from Regression 6 in Table 3. In the first column we present the LSDV estimates, columns two and three present the Holtz-Eakin (1988) estimates under the null of no individual effects and the alternative of individual effects, and columns four and five present the Arellano and Bond one- and two-step estimates. Finally in Column 6 we present the Arellano and Bond two-step estimates in the presence of measurement error.

Table 5 Results from Panel Data Estimation

Model I: No interaction terms

	Method					
	LSDV	Holtz-Eakin		Arellano-Bond		Measurement Error
Variable		H0: no ind. effects	H1: ind. effects	one-step	two-step	two-step
LGDP	0.6778	0.8863	0.2047	0.4248	0.3741	0.3718
	(0.3439)	(0.0820)	(0.4771)	(0.0631)	(0.0728)	(0.0835)
TIME TREND	0.0135	–0.0200	0.0535	0.0321	0.0408	0.0511
	(0.0618)	(0.0327)	(0.0842)	(0.0097)	(0.0117)	(0.0132)
OPEN	0.0810	0.0974	0.0343	–0.0247	0.0143	–0.0192
	(0.3606)	(0.1178)	(0.3056)	(0.0411)	(0.0440)	(0.0758)
SECM	0.0013	0.0009	0.0008	0.0008	0.0009	–0.0026
	(0.0095)	(0.0034)	(0.0069)	(0.0012)	(0.0013)	(0.0022)
GOVT/GDP	–0.0037	–0.0016	0.0044	0.0030	0.0031	–0.0003
	(0.0175)	(0.0063)	(0.0175)	(0.0025)	(0.0028)	(0.0049)

Table 5 (Continued)

Model I: No interaction terms

	Method					
	LSDV	Holtz-Eakin		Arellano-Bond		Measurement Error
Variable		H0: no ind. effects	H1: ind. effects	one-step	two-step	two-step
INV	0.0103	0.0067	0.0173	0.0182	0.0171	0.0154
	(0.0158)	(0.0065)	(0.0128)	(0.0019)	(0.0022)	(0.0033)
CURRCON	–0.0236	–0.0077	0.0168	–0.0733	–0.0553	–0.0804
	(0.2094)	(0.0978)	(0.0885)	(0.0231)	(0.0276)	(0.0409)
PPIDEV	0.1325	0.0074	–0.0617	0.1182	0.0936	0.2475
	(0.5426)	(0.0998)	(0.5394)	(0.0647)	(0.0715)	(0.1326)
POP/LAND	0.1393	0.0317	0.4243	0.1396	0.1802	0.1805
	(0.6157)	(0.0520)	(0.5201)	(0.1137)	(0.1343)	(0.1480)
implied rate of conditional convergence:	0.0777	0.0244	0.3173	0.1711	0.1966	0.1978
	(0.1015)	(0.0186)	(0.4663)	(0.0297)	(0.0389)	(0.0447)
	Test of individual effects		M2 Test of overidentification:		Test of measurement error	
	L:	304.72	Test statistic	0.274	Test statistic	42.8478
	df	119	p-value	0.784	df	32
	p-value	0.000	p-value		p-value	0.095

Time periods are 1960-64, 1965-69, 1970-74, 1975-79, 1980-84, and 1985-1989.

The dependent variable is the real annual per capita growth in GDP over each five-year period. Asymptotic **Standard Errors** are shown in parentheses.

LGDP is the natural log of real GDP per capita at the start of each time period.

OPEN is a dummy variable set equal to one for economies open over the entire five year period.

SECM is the percentage of "secondary school attained" in the male population at the start of each time period.

GOVT/GDP is the ratio of real government expenditures to real GDP, averaged over the respective time period.

REVCOUP is the number of revolutions and coups per year, averaged over the respective time period.

PPIDEV measures the relative price of investment goods. It is the deviation of the log of the price level of investment from the cross-country sample mean in the respective time period.

INV is the ratio of real gross domestic investment to real GDP, averaged over the respective time period.

POP/LAND is the population density averaged over each time period.

CURRCON is a dummy variable taking the value of one when restrictions on payments for current account transaction are in place, averaged over each respective time period.

Tests given are asymptotic chi-squared test statistics.

The Holtz-Eakin test of no individual effects overwhelmingly rejects the null, indicating that significant individual country effects are present. This is reflected in the significant differences in the estimated parameters under the null of no individual effects as compared to the alternative. We note that the Holtz-Eakin estimates under the alternative have large standard errors, and the greater efficiency of the Arellano-Bond estimators which utilize more overidentifying restrictions is clearly seen. The estimated rate of conditional convergence is much higher than under the cross-sectional regression approach, which qualitatively concurs with the results obtained by Islam (1995) and Caselli, Esquivel and

Lefort (1996). Once we allow for individual effects, the estimated conditional convergence rate is now 20% for the Arellano and Bond two-step estimator, whereas we see the rate under the null of no individual effects in the Holtz-Eakin method is the same as that estimated under cross-sectional regressions. This is important as it indicates that it is not the division of our thirty-year period into five-year intervals that drives our results, but rather the allowance for individual effects.

The dynamic formulation of the model means that the magnitudes of the coefficients are not directly comparable with those from the cross-sectional regressions. In particular, now we are regressing the logarithm of GDP per capita on its lag and other variables, as opposed to regressing the growth rate on these variables in the cross-sectional approach. Estimation occurs by considering the influence of differences in these variables, and we see these effects are weaker, with fewer significant coefficients than in the cross-sectional regressions. Nonetheless, we find a significantly negative influence of restrictions on current account payments, and also a significantly positive time trend and significantly positive effect of investment. .The other variables are individually insignificant, and hence we note in particular the strong effect of openness found in the cross-sectional regression is no longer apparent in the panel data. The differencing procedure prevents estimation of the coefficients for time invariant variables such as ACCESS and TROPICS.[16]

The Arellano-Bond (1991) m_2 test of the overidentifying restrictions does not reject the null hypothesis of no second-order serial correlation in the first-differenced residuals.[17] Hence there is no evidence against the moment restrictions underlying the consistency of the Arellano and Bond estimator. We also present a Holtz-Eakin, Newey and Rosen test for the presence of measurement errors within the context of the Arellano and Bond model. In this context the GMM test of specification finds there is at most weak evidence against the null of no effect from measurement errors on our estimation, and we see the estimated conditional convergence rate and parameter values seem robust between columns 5 and 6.

We then consider whether our finding of interaction effects between capital controls and openness, and between capital controls and initial real GDP per capita apply in the dynamic panel data model. This was carried out for both restrictions on current account payments(CURRCON), and for restrictions on payments for capital transactions (CAPCON). In both cases no significant interaction between the relevant control variable and openness was found from any of the methods used in Table 5. Table 6 shows the Arellano-Bond 2-step estimators for these interaction models. The strongest evidence of an interaction is between CAPCON and lagged GDP per capita, however this is of at best marginal significance. Hence we conclude that the Arellano-Bond two-step estimates provided in Table 5 are robust to concerns of slope heterogeneity which occur through the channels of openness and capital controls.

16. One could estimate these effects by applying the Hausman-Taylor (1981) method in a dynamic context, but as our focus is not on the effects of these variables per se, this strategy is not pursued here.

17. A Hausman test against the alternative of MA(1) errors also did not reject.

Table 6 Arellano-Bond (1991) Two-step Estimates for Alternative Models

Variable	Model II	Model III	Model IV	Model V
LGDP	0.3221	0.3439	0.3235	0.3237
	(0.0703)	(0.0706)	(0.0710)	(0.0698)
TIME	0.0485	0.0457	0.0484	0.0462
	(0.0120)	(0.0115)	(0.0118)	(0.0119)
OPEN	0.0326	–0.0098	0.0339	0.0154
	(0.0434)	(0.0522)	(0.0643)	(0.0429)
SECM	0.0007	0.0011	0.0010	0.0005
	(0.0013)	(0.0013)	(0.0013)	(0.0013)
GOVT/GDP	0.0029	0.0030	0.0016	0.0014
	(0.0028)	(0.0028)	(0.0028)	(0.0028)
INV	0.0169	0.0171	0.0174	0.0178
	(0.0022)	(0.0022)	(0.0022)	(0.0021)
CURRCON	0.0140	–0.0663		
	(0.2498)	(0.0299)		
CAPCON			–0.0086	–0.4219
			(0.0399)	(0.3076)
PPIDEV	0.0824	0.1048	0.0979	0.1035
	(0.0707)	(0.0717)	(0.0730)	(0.0708)
POP/LAND	0.2070	0.1645	0.2028	0.2167
	(0.1339)	(0.1346)	(0.1357)	(0.1350)
$LGDP_{t-1} \times CURRCON_t$	–0.0092			
	(0.0327)			
$LGDP_{t-1} \times CAPCON_t$				0.0525
				(0.0397)
CURR×OPEN		0.0616		
		(0.0606)		
CAPCON×OPEN			–0.0276	
			(0.0677)	
implied convergence rate	0.2268	0.2134	0.2256	0.2256
	(0.0437)	(0.0412)	(0.0441)	(0.0429)

Source: Author's regressions based on data described in appendix.

Time periods are 1960-64, 1965-69, 1970-74, 1975-79, 1980-84, and 1985-1989.

The dependent variable is the real annual per capita growth in GDP over each five-year period. Asymptotic **Standard Errors** are shown in parentheses.

LGDP is the natural log of real GDP per capita at the start of each time period.

OPEN is a dummy variable set equal to one for economies open over the entire five year period.

SECM is the percentage of "secondary school attained" in the male population at the start of each time period.

GOVT/GDP is the ratio of real government expenditures to real GDP, averaged over the respective time period.

REVCOUP is the number of revolutions and coups per year, averaged over the respective time period.

PPIDEV measures the relative price of investment goods. It is the deviation of the log of the price level of investment from the cross-country sample mean in the respective time period.

INV is the ratio of real gross domestic investment to real GDP, averaged over the respective time period.

POP/LAND is the population density averaged over each time period.

CURRCON is a dummy variable taking the value of one when restrictions on payments for current account transaction are in place, averaged over each respective time period.

VI. Conclusions

The cross-sectional analysis indicates that capital controls have a negative impact on growth; however the form these capital controls take is important. It appears that restrictions on current account payments a detrimental effect on both the growth rate of real GDP per capita, and on the speed of conditional convergence. On the other hand other measures of capital controls did not impact significantly on growth. A key finding was the importance of considering interaction effects between capital controls and openness, and between capital controls and initial real GDP per capita. The interaction with initial income suggests that growth convergence is slower in the presence of capital controls, in addition to the impact of capital controls on the economic growth rate. It appears this omission may have led Rodrik (1998) to erroneously conclude that capital controls are essentially uncorrelated with long-term economic performance. The method developed in this paper to account for the presence of measurement error, via the use of data quality weights, was successful in improving the fit of the model, and provides some indication of robustness of our results with respect to errors in measurement.

The panel data analysis generally supports the qualitative results emerging from the cross-sectional analysis. Again we find the form which capital controls take to be important, however we no longer find such strong evidence of interaction effects between capital controls and openness or initial income. There is strong evidence for the presence of individual country effects in the data, indicating an omitted variables bias in the cross-sectional results. The result is that cross-sectional regressions underestimate the rate of conditional convergence, which the panel data models estimate to be up to 20% per annum. As Islam (1995) observes, through controlling for more sources of difference in steady state levels of incomes, at the same time we render the obtained convergence hollower. This obtains as convergence is commonly referred to in the context of different countries in the world approaching the same or similar levels of income, and thus a finding that countries are converging at fast rates to very different points may not provide much comfort.

Ideally one would like to examine the effects of the intensity of various capital controls, rather than just their presence or absence. The measures of capital controls we have are crude in this respect, and this is especially problematic for panel data estimation, which utilizes within country variation to identify the effects of capital controls. This highlights the need for higher quality data in this area in order for future work to proceed. In addition, more frequent human capital data is needed to increase the time dimension power of panel data methods.

The theoretical discussion of the pros and cons of capital controls, while unable to provide an unambiguous answer as to the desirability of capital controls, does seem, on balance, to provide more evidence for the free movement of capital controls than against. This recognition provides the impetus behind recent moves to extend the role of the IMF to encompass the promotion of capital account liberalization. In light of the aforementioned provisos, one should be cautious in using the empirical results contained in this paper for policy recommendations. Nonetheless, our results do provide some support against the use of capital controls, and in particular highlight the importance of freedom from restrictions on payments for current transactions. Improved measures of capital controls are needed before

one can conclude, as Rodrik (1998, p.61) did, that "the data provide no evidence that countries without capital controls have grown faster...".

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Table 1 Correlations between Variables, averaged 1960-89[1]

	CURRCON	CAPCONTR	ARREARS	BILAT1	BILAT2	MULTER	OPEN	OPEN2	LGDP60	GDP Growth
Capital Controls Index	0.894	0.845	0.833	0.464	0.335	0.125	–0.448	–0.181	–0.552	–0.238
Restrictions on current account transactions	1.000	0.635	0.618	0.495	0.353	0.155	–0.510	–0.259	–0.571	–0.271
Restrictions on payments on capital transactions		1.000	0.559	0.295	0.184	–0.104	–0.233	–0.115	–0.374	–0.119
Indicator of Interest Arrears			1.000	0.386	0.314	0.265	–0.386	–0.068	–0.458	–0.210
Bilateral Payments with IMF member				1.000	0.801	0.334	–0.216	–0.283	–0.280	0.003
Bilateral Payments with non-member					1.000	0.427	–0.111	–0.162	–0.078	0.073
Separate Exchange Rates for capital transactions or invisibles						1.000	–0.270	–0.127	0.028	–0.102
Sachs & Warner openness indicator							1.000	0.303	0.541	0.530
(Exports+Imports)/GDP								1.000	0.059	0.329
Log of real GDP per capita in 1960									1.000	0.255
Annual growth in Real GDP per capita 1960-89										1.000

1. ARREARS, BILAT1 and BILAT2 are averaged over 1966-89.