Capital Controls, Political Institutions, and Economic Growth: A Panel and Cross Country Analysis

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Abstract

Statistical studies on the relationship between capital controls and growth have paid little attention to how this relationship may differ across political institutional environments. Our causal argument for this difference is based on Bueno de Mesquita et al.'s formal model of public goods. Consistent with this model, we show that growth in authoritarian countries is negatively affected by capital controls, while growth in democratic countries is insignificantly affected. Our results hold across numerous panel and cross country specifications. Our findings suggest that the decision to liberalize capital flows should take careful account of the political context.

Is the liberalization of capital flows likely to lead to faster economic growth? Recent events suggest that the answer to this question is conditional on some other variable. For instance several crisis-afflicted East Asian countries with liberal capital flows experienced years of growth declines in the late 1990s while others experienced rapid recoveries.[1] Large variations in growth outcomes have also been noted among countries with capital controls.[2] The case study literature suggests a role for political institutions in accounting for some of these variations in outcomes.[3] However, the question of how and to what degree the relationship between capital controls and growth differs across political institutional environments has not received systematic attention in a large-n context. In this paper we seek to fill this gap in the literature.

We base our empirical predictions on Bueno de Mesquita et al.'s (2003) formal model of public goods provision. As this model implies we show that the relationship between capital controls and growth differs substantially depending on whether democratic or authoritarian institutions are present. We demonstrate a significant negative relationship between capital controls and growth in authoritarian environments across numerous panel and cross country specifications. On the other hand we demonstrate that capital controls have an insignificant effect on growth in democratic environments.

In addition to contributing to the literature on capital controls (summarized below) our paper also contributes to two broader literatures in political science. The first is the body of work on political institutions and financial globalization.[4] The second is

[1] See Haggard (2000).
[2] For example China vs. many countries in Africa.
[3] See the extensive case literature on the East Asian financial crisis.
[4] Notable contributions to this literature include Broz (forthcoming), and Clark and Hallerberg (2000).

the work on democracy and development.[5] Our main contribution to these literatures is to add to our understanding of how the relationship between financial globalization and economic performance is affected by the presence of democratic institutions.

In Section 1 of this paper we summarize ongoing theoretical debates over the impact of capital controls on growth, and outline the few major statistical contributions to this literature. Then, in Section 2, we present our causal story and alternative arguments for how the relationship between capital controls and growth can differ depending on the political institutional environment. We describe our data in Section 3. In Section 4 we present our results. Section 5, the conclusion, addresses the major policy implications of our findings.

1) The Debate Over Capital Controls

There are two contending sets of theoretical arguments on the relationship between capital controls and long term growth, with one positing that capital controls hurt growth and the other that they do not. The "neo-classical" theoretical case for a negative relationship between capital controls and long term growth rests on three arguments. First, it argues that dismantling capital controls helps developing countries gain access to funds from developed countries, enabling them to achieve investment levels that exceed their domestic savings rates, leading to faster long-term growth.[6] Second, environments with liberal capital flows allow domestic investors to diversify their portfolios by making international investments. This makes investors less vulnerable to domestic economic shocks, which enables them to achieve higher risk adjusted rates of return which encourage higher levels of saving and investment. Finally,

[5] For surveys see Alvarez et al. (2000) and Przeworski and Limongi (1993).
[6] Obstfeld 1998, 10.

the neo-classical view holds that fear of capital flight under liberal capital flows serves as a disciplining force on government policies. The imposition of capital controls removes this disciplining force and generates poor policies that create an environment which is unconducive to investment. Lower investment, in turn, retards growth.

The main criticism of the neo-classical view is that it does not take account of the increased propensity for massive financial crises under liberal capital flows.[7] Dani Rodrik, perhaps the best known opponent of the neo-classical view, lists four reasons why financial markets are inherently prone to such crises. First, "asymmetric information combined with implicit insurance results in excessive lending for risky projects."[8] Second, mismatches between short term liabilities and long term assets render countries vulnerable to financial panic and bank runs. Third, "when markets cannot observe the intrinsic quality of money managers, these managers are likely to place too little weight on their private information and exhibit herd behavior resulting in excess volatility and contagion effects."[9] Finally, since the prices of financial assets are based on earnings expectations, bubbles can easily emerge and burst, as a consequence of cycles of euphoria and negativity.[10] Rodrik argues that that the positive benefits that would derive from the neo-classical mechanisms described above are likely to be wiped out by frequent depression-inducing financial crises, resulting in an insignificant relationship between capital controls and long term economic growth.

Like the theoretical literature, the literature on the statistical relationship between capital controls and long term growth yields conflicting results. On the one hand Rodrik

[7] Rodrik 1998, 2.
[8] Rodrik 1998, 4.
[9] Rodrik 1998, 4.
[10] Rodrik 1998, 4.

(1998), studying the period 1975-1989, finds an insignificant relationship between capital controls and long term growth. This finding is consistent with previous findings of Alesina et al. (1994) over the short term (annual data), and Grilli and Milesi-Feretti (1995) over the medium term (5 years). Using panel data that go as far back as 1880, Eichengreen and Leblang (2003) also find that "it is hard to find a robust effect of capital account liberalization on growth."[11] (They find that the positive effects of controls, limiting the downside effects of financial crises, and the negative effects, reduced investment under non-crisis conditions, effectively cancel out.) On the other hand, Dennis Quinn (1997), studying growth over 1960-89, finds that an increase in openness is significantly associated with higher long term growth. Edwards (2001) and Edison et al. (2002), find only partial support for this result, but the two papers are mutually contradictory as to which countries are likely to benefit from liberalization. Arteta et al. (2001), however, demonstrate that neither Edwards' nor Edison et al.'s positive findings are robust.

In the most recent contribution to this literature, Areendam Chanda (2005) finds that the impact of capital controls on long term economic growth depends on the degree of societal fractionalization. Studying the period 1975-1995 Chanda shows that there is a negative relationship between capital controls and growth in relatively fragmented societies, but not in homogenous societies.[12] However, Chanda's empirical findings are based exclusively on a cross-country empirical framework and could thus be contaminated by omitted variables bias.

[11] Eichengreen and Leblang 2003, 206.
[12] Chanda 2005, 441.

2) Political Institutions and the Relationship between Capital Controls and Economic Growth

We begin this section by presenting the institutional causal story that we favor. We then address alternative explanations, and describe how we confirm that it is our explanation rather than one of the alternatives that is likely to be the correct one.

A characteristic feature of the tightening of capital controls is that the government gains in its ability to decide which domestic economic actors will gain access to foreign currency. Controls on capital movements can have different effects on growth depending on the degree to which productive economic actors (i.e. those with growth enhancing projects) are affected by this increase in government discretion. First consider environments where governments have incentives to place a low priority on productivity and a high priority on rewarding government insiders when deciding who can exchange domestic for foreign currency. In such environments capital controls are likely to result in relatively productive actors becoming constrained in their ability to import goods that are essential for production. In contrast, where governments have incentives to prioritize productivity we should expect productive actors to be relatively unconstrained under a capital controls regime. It is thus in the former rather than in the latter environments that we should most expect the increased governmental discretion associated with capital controls to be associated with a decline in the productivity of investment. Ceteris paribus, we should thus most expect to observe a negative relationship between capital controls and growth in the former political environments.

The formal model of public goods developed by Bueno de Mesquita et al. (2003) gives us a basis for predicting which governments are likely to fall in the former and

latter categories. Define a winning coalition as the group of people within a country whose support is essential for an incumbent to retain office. Define the selectorate as the group of citizens who have a say in the choice of the leader of a country. Bueno de Mesquita et al. formally demonstrate that trading off the provision of important public goods (like growth) in favor of granting benefits to government insiders jeopardizes the survival of leaders of high W/S societies to a greater degree than leaders of low W/S societies. Assuming that incumbents seek to maximize the probability of retaining office, the implication is that as W/S increases the propensity to allocate goods in a manner that exclusively benefits members of the winning coalition decreases.

A defining characteristic of democracies is that they display a high ratio of the winning coalition to the selectorate (high W/S). Almost all authoritarian regimes display a low W/S. The logic of Bueno de Mesquita et al.'s model then suggests that under capital controls it is authoritarian regimes that are relatively likely to trade off growth as a priority in favor of ensuring that unproductive government insiders are relatively unaffected by the controls. We should thus expect authoritarian regimes to display a negative relationship between capital controls and growth. In contrast we should not expect democracies to display such a relationship, given that the sacrifice of an important public good like economic growth is likely to severely jeopardize the hold of the incumbent on office.

Our causal story also suggests the timing over which we should expect capital controls to adversely affect growth in authoritarian regimes. Assuming rational expectations of the tightening of controls, we should expect productive actors who need to import goods that are essential for production to stock up on these goods prior to the

imposition of controls. With rational expectations we should then expect productivity to be significantly affected only when these stocks have had time to run down. The implication is that we should expect the adverse effects of capital controls on growth via the productivity mechanism to only be felt several years after the tightening of controls.

The above mechanism, focusing on the productivity of investment, is not the only plausible way in which regime type can impact upon the relationship between capital controls and growth. We describe three of the most plausible alternatives below, and then describe our strategy for ruling out that any of these is the mechanism driving our result.

Recall that one of the ways in which capital controls can adversely affect growth is by removing the disciplining effect of anticipated capital flight on policy making, thereby generating policies that are less conducive to investment. Authoritarian regimes may be exceptionally prone to this problem since they lack free elections which can serve as alternative disciplinary force over government policies in the absence of free capital flows. If so, we should expect capital controls to be associated with lower investment and thus lower growth in authoritarian regimes.

For a second alternative causal story, recall that capital controls may also affect growth by limiting the ability of economic actors to diversify risks. This limitation, in turn, can depress domestic investment. It is plausible that the greater risk of arbitrary action by authoritarian governments means that economic actors in such environments have exceptionally acute needs for diversifying their risks. If so, it is in authoritarian regimes that we should most expect investment quantity and thus growth to be depressed by constraints on the ability to diversify risk.

Another way in which political institutions may impact on the relationship between capital controls and growth is through their effect on the propensity for financial crises. Studying four countries operating under liberal capital flows during the East Asian financial crisis MacIntyre (2002) has argued that "normal" democracies (democracies without unwieldy coalition governments of the order of Thailand in 1998) are less prone to crises of investor confidence than authoritarian regimes on account of their greater ability to credibly commit to sound policies. The implication of this argument then is that "normal" democracies should fare better than authoritarian regimes under liberal capital flows.

Note that in all these alternative stories the effects of regimes pass through the quantity of investment. This means we can rule out the possibility that our regressions are picking up the effects of these causal mechanisms simply by including a control variable for the quantity of investment in all our regressions, which we do.

3) Empirical Strategy and Data

Our core regressions are based on panel data analysis using six non overlapping five year periods, starting from 1970. Our dependent variable is the growth of average per capita GDP between successive five year periods, and we accordingly average values of right hand side variables over five year periods as well. We average over five years rather than use the country year as the unit due to the extreme instability and volatility of GDP data. Pritchett (2000) is the standard source referred to by economists for why averaging growth over several years constitutes best practice for growth regressions. As Pritchett puts it, "given the instability and volatility of output, moving to shorter and shorter time periods and eliminating long period variance are likely to entangle dynamics,

specification, endogeneity, and statistical power, which will ultimately confuse, not clarify, issues of growth, especially in developing countries."[13]

Our measure of capital controls is the latest (and now widely considered to be the best) measure, developed by Chinn and Ito (2006). Chinn and Ito's measure is considered preferable to the other measures because it attempts to capture the intensity of controls, rather than simply indicating the presence or absence of controls. (The latter is a serious problem with the widely disseminated measure from the IMF.) For example, the Chinn and Ito measure takes account of the fact that one can evade capital control measures via current account transactions such as over/underinvoicing. Thus, in addition to taking account of restrictions on capital account transactions, the Chinn and Ito measure also takes account of restrictions on current account transactions when assessing the intensity of capital controls. The measure also takes account of the requirement of the surrender of export proceeds and the presence of multiple exchange rates, both of which generate constraints on moving capital across borders. By construction, the Chinn and Ito series takes a mean value of zero. While the original series decreases in value with an increase in controls we have reversed this direction for ease of exposition. (See Chinn and Ito 2006 for further details about their measure.)

[13] Pritchett 2000, 235. Pritchett is especially eloquent on the problem of dynamic misspecification that results from using periods as short as a year. In his words, "arbitrarily parsing time series into shorter periods imposes the assumption that the dynamics are invariant across growth correlates…In fact although some growth effects are contemporaneous, especially macroeconomic and cyclical factors, others could take several years, such as transitional dynamics due to changes in investment incentives, and still others could take decades, such as the impact of changes that could affect the rate of technical progress. Some right hand side variables could have output or growth effects at all horizons-cyclical, transitional, and steady state-and there is no reason to believe that these effects are of similar magnitude, nor have the same sign, because some policy choices may lead to temporary booms but ultimately to busts." (Note that this latter effect is widely considered to be a possibility when it comes to the liberalization of capital flows.)

For democracy, in our main specifications we use Przeworski et al.'s measure of democracy called REG.[14] We do so because both our causal arguments hinge on the risk to the chief executive of losing office, and Przeworski's measure is a behavioral as opposed to a purely subjective measure of this risk. (Democracies are countries which have experienced turnover in office following an election.) We also conduct robustness checks using the well known Polity measure from Polity IV in which the degree of authoritarianism is deducted from the degree of democracy, yielding a democracy scale running from -10 to 10.[15] Since our causal argument depends on the ratio of the winning coalition to the selectorate we additionally conduct robustness checks using Bueno de Mesquita et al.'s (2003) measure of this ratio (W/S).

All our panel specifications include country and time fixed effects and standard errors are clustered by country. We thus follow the conservative strategy of focusing on growth responses to changes in the intensity of capital controls within countries. We do so in recognition of widespread concerns about omitted variable bias when studying such variations across countries.

Across our various specifications we also control for the major control variables used in the capital controls literature, namely, investment, inflation, schooling, life expectancy, per capita GDP in the initial period, and trade openness (exports + imports divided by GDP). The data for all these variables is from the World Bank (GDNDG).[16] Summary statistics are provided in the Appendix.

[14] The data is available at http://pantheon.yale.edu/~jac236/DATASETS.htm
[15] http://www.cidcm.umd.edu/inscr/polity/
[16] The data is available at http://www.nyu.edu/fas/institute/dri/

At the end of the paper we present some cross country specifications in which we study the effects of ethnic fragmentation on our results. The goal is to see whether our results hold up in the face of Chanda's findings on the effects of ethnic fragmentation, which cannot be evaluated in a rigorous panel setting on account of time invariance in ethnic fragmentation data. Since our goal is to check the robustness of our arguments to Chanda's claims we follow him in using the ELF measure of ethnic fragmentation. ELF measures the likelihood that any two randomly drawn individuals in a country will be members of different groups. The source for ELF is Atlas Narodov Mira, obtained from Easterly and Levine (1997). This is the most widely used measure in the literature, one that has been used in extensively cited growth papers relating to societal divisions by Easterly and Levine (1997) and Rodrik (1999). We also conduct regressions in which we test the robustness of our results to the Alesina et al. (2003) measure of ethnic fragmentation and the Montalvo and Reynal-Querol (2005) measure of ethnic polarization.

4) Results

4.1) Main results (panel specifications)

Table 1 displays the first set of regressions in support of our hypotheses. Recall that our main claims are: a) that authoritarian regimes experience negative effects from capital controls while democratic regimes are insignificantly affected, b) that these significant negative effects are likely to be observable after a lag of several years, and c) that they are likely to be present even after controlling for the quantity of investment. In order to capture the lagged effects posited above we include our measure of capital controls lagged by one and two five year periods. We also add control variables for

investment levels lagged by one and two five year periods to soak up the effects of alternative causal mechanisms described in Section 2. (Since contemporaneous values of capital controls and investment are likely to be endogenous to growth we exclude them from our specifications.) We also include country and time fixed effects with standard errors clustered by country. As a proxy for the level of development (wealth) we use years of secondary education averaged over the previous five year period as a control variable. The correlation between this variable and lagged GDP is 0.8. (We use a proxy variable for development level because, while all our results are robust to using lagged per capita GDP as a control variable, we believe that the coefficients in such specifications are biased by the presence of the same term on the left hand side of the equation.[17])

Columns 1 and 2 in Table 1 explore the different effects of capital controls in democracies and dictatorships using our core measure of democracy, Przeworski's REG measure. In this measure a score of 1 indicates an authoritarian regime while 0 indicates a democracy. In order to address endogeneity we split the sample based on the value of REG in the previous decade. Thus, the first column refers to the effect of capital controls on growth in countries that had authoritarian regimes during the previous decade, while the second column captures this effect for democracies.

Column 1 indicates that capital controls are associated with significantly slower GDP growth in authoritarian environments at the 95% level of significance. We observe this effect for a lag of two periods. A one standard deviation increase in the intensity of capital controls reduces the growth in per capita GDP by approximately 1 percentage

[17] The left hand side is: $(GDP_{i,t} - GDP_{i,t-1})/ GDP_{i,t-1}$.

point in authoritarian regimes. Column 2, on the other hand, indicates that capital controls are insignificantly associated with growth in democratic environments.

The remaining columns of Table 1 check if these results hold up with different definitions of democracy. W/S scores of 0.5 and below are considered by the creators of this variable to be indicative of authoritarian regimes. Column 3 shows that capital controls are associated slower growth in such environments at the 95% level of significance. Once again the result is for a lag of two periods. Column 4 confirms that capital controls display an insignificant relationship with growth in democratic environments as per the W/S measure.

Columns 5 and 6 indicate that these results hold up when we use Polity's measure of democracy. In this measure a country's score on a ten point authoritarian scale is deducted from its ten point score on a democratic scale to identify countries that are more democratic than authoritarian. A Polity score exceeding 0 is thus indicative of a country that is more democratic than authoritarian. Column 5 shows that capital controls are associated with significantly slower growth in the presence of an average Polity score of 0 or below over the previous decade. Column 6 shows that this effect is absent in environments that were more democratic than authoritarian on average in the previous decade.

In Table 2 we conduct robustness checks to see whether our core results (presented in columns 1 and 2 of Table 1) hold up when we add more controls. In this table we sequentially add controls for lagged inflation, trade openness, and life expectancy. Columns 1 and 2 show that the difference between democratic and authoritarian regimes observed in Table 1 holds up with the addition of lagged inflation.

Authoritarian regimes continue to be adversely affected by capital controls at the 95% level of significance, while democracies are not. Columns 3 and 4 show that this result is unaffected by the addition of a measure of trade openness as a control. Columns 5 and 6 shows that this difference also holds up when we add a control which captures the health of the population (life expectancy) which is itself very strongly associated with economic growth.

In sum, the adverse effect of capital controls on growth in authoritarian regimes is robust to specifications with a large number of controls that are well rooted in growth theory. (The country effects alone amount to the at least a third of the sample size in each specification.) Furthermore, the coefficient size is virtually the same across numerous specifications, giving us further confidence in the substantive implications of our result.

4.2) Additional Results (cross country specifications)

Recall that the only political story in the capital controls literature, that of Chanda (2005), is based on a cross country approach. Chanda argues that ethnic fragmentation contributes to competitive rent seeking for capital licenses between ethnic groups, which retards growth. Consistent with this story he finds that ethnically fragmented societies are adversely affected by capital controls while homogenous societies are not. Chanda's main causal variable, ethnic fragmentation, is time invariant, so he relies on cross country regressions. Since the effects of time invariant variables cannot be captured in panel regressions with fixed effects we can only examine the robustness of our results to the effects of ethnic fragmentation by also subjecting our hypotheses to a cross country

analysis. We do so below with a full awareness of the concerns about omitted variables bias associated with such regressions.

We adhere closely to Chanda's paper when checking the robustness of our results to the inclusion of ethnic fragmentation. Like Chanda we use interaction effects here instead of splitting the sample. (Splitting the sample of 65 countries by regime type and fragmentation level results in sample sizes that are much too small.) We examine robustness in two ways. One is by controlling for ethnic fragmentation along with Chanda's economic controls and interacting capital controls with our REG measure (a single interaction specification). The second is by interacting capital controls with ethnic fragmentation and REG (a double interaction specification). The single interaction specification is displayed in the first column of Table 3. As per Brambor et al. (2006), in a specification that includes interaction effects the coefficient and standard error for each individual constituent term of an interaction in a regression table refers to the effect of the constituent variable when the other modifying variable(s) take the value of 0. In column 1 of Table 3, then, the coefficient and standard error for capital controls presented in Row 1 refer to the effect of capital controls when REG takes the value of 0. The first row thus only gives us information relevant to democratic regimes. Column 1 suggests a negligible effect for capital controls on growth when democracy is present.

While this result is partially revealing, we need to go beyond a conventional regression table to assess the effect of capital controls in authoritarian regimes. This is because a conventional regression table does not give us the standard error for the total effect of capital controls at non-zero scores for political regime. Specifically, we need to recalculate standard errors, taking account of the covariance between the modifying

variables.[18] This effect (with the correct standard error) is presented in the first column of Table 4. As may be observed capital controls have a negative effect on growth in authoritarian regimes at the 95% level of significance, while democracies are insignificantly affected.

We now proceed to our next cross country robustness check, using the double interaction specification displayed in column 2 of Table 3. Recall that as per Brambor et al. (2006), in a specification that includes interaction effects the coefficient and standard error for each individual constituent term of an interaction in a regression table refers to the effect of the constituent variable when the other modifying variable(s) take the value of 0. In column 3 of Table 3, then, the coefficient and standard error for capital controls presented in Row 1 refer to the effect of capital controls when ethnic fragmentation (ELF) and REG take the value of 0. The first row thus only gives us information relevant to conditions of extreme ethnic homogeneity in democratic regimes. Based on this specification we now recalculate standard errors taking covariances of modifying variables into account, to assess the effects of capital controls at different combinations of REG and ELF.

Our results for the double interaction specification are displayed in Figure 1. The y-axis displays the coefficient for the relationship between capital controls and average growth over 1975-95 (Chanda's period of analysis). The x-axis displays different levels of societal fragmentation as captured by the ELF measure. The stars indicate levels of ELF at which the relationship between capital controls and growth is significant at the 95% level. The figure indicates that democracies are insignificantly affected by controls at all levels of fragmentation. Authoritarian regimes are significantly

[18] See Brambor et al. 2006.

adversely affected by controls at levels of fragmentation exceeding .25. Since more three quarters of the authoritarian countries in the sample have ELF scores exceeding .25 we find this result supportive of our core findings. **(**Columns 2-5 in Table 4 display coefficient and standard error values at different values of ELF for authoritarian and democratic regimes respectively.)

Our results are robust to using the Alesina et al. (2003) and the Montalvo and Reynal-Querol (2005) measures of ethnic polarization, to using the Polity and W/S measures of democracy, as well as to dropping inflation outliers. (Tables available upon request.) In sum, our main result holds up across numerous panel and cross country specifications.

5) Conclusion

The literature on the liberalization of capital flows has by and large indicated that capital controls are not correlated with economic growth. We present results that differ from this view. We find that while growth in authoritarian regimes is likely to be adversely affected by controls, growth in democracies is unlikely to be affected.

Our findings have significant and concrete implications. First, capital flow liberalization may significantly serve growth in many highly ethnically fragmented African dictatorships. This is an important finding in the context of the proliferating literature on "Africa's Growth Tragedy."[19]

Second, democracies are unlikely to substantially boost growth by liberalizing capital flows, and should lay little credence to claims by international financial institutions that this will improve their growth performance. This is likely to be the case irrespective of the level of ethnic fragmentation. For instance India, a country that has

[19] See Easterly and Levine 1997.

long operated under a barrage of capital controls, has recently been under immense pressure from international financial institutions to liberalize capital flows. As per our dataset, India's level of ethnic fractionalization is extremely high, easily falling within the fourth quartile. According to current wisdom, India would benefit from liberalizing capital flows thanks to its societal divisions. However, our findings suggest that the Indian government should also take account of its political institutions when making the decision. When this is done the conclusion is very different. The fact that India is a democracy places it in the category of countries that will not benefit substantially from capital flow liberalization, despite its high level of ethnic fractionalization. Other fragmented countries that would not be adversely affected by capital controls include Bolivia, Guatemala, Papua New Guinea, Trinidad, and Ecuador.

In sum, our research suggests that the effects of capital controls on growth are highly contingent on domestic political institutions. Policy planners should thus take careful account of the political context when determining which countries should embark on the globalization of capital flows.

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Table 1: Core Results and Robustness to Alternative Measures of Democracy

	Reg = 1	Reg = 0	WoverS <=.5	WoverS >.5	Polity <=0	Polity > 0
Twice Lagged Capital Controls	-0.93*	0.32	-1.49*	-0.082	-0.84*	0.084
	(0.36)	(0.22)	(0.61)	(0.14)	(0.41)	(0.17)
Lagged Capital Controls	0.90	-0.17	0.90	0.027	1.04	0.12
	(0.62)	(0.31)	(1.28)	(0.25)	(0.72)	(0.26)
Twice Lagged Log of Investment	-1.26	-1.62	-2.41	-1.44	-1.37	-1.43
	(1.58)	(1.99)	(2.50)	(1.19)	(1.77)	(1.52)
Lagged Log of Investment	0.42	0.14	4.42	-0.086	0.37	0.80
	(1.81)	(1.29)	(3.26)	(0.99)	(1.98)	(1.11)
Lagged Average Schooling	3.36	-0.81	10.16	-0.16	2.47	-0.64
	(2.04)	(0.67)	(6.39)	(0.51)	(3.00)	(0.61)
N	140	127	58	211	122	145

All regressions include country fixed effects and time dummies. Coefficients and errors for these are not reported. Robust Standard Errors clustered by country in parentheses. Significantly different than zero at 95% (*), 99% (**) confidence.

Table 2: Robustness to Additional Controls

	Reg=1	Reg=0	Reg=1	Reg=0	Reg=1	Reg=0
Twice Lagged Capital Controls	-0.89* (0.34)	0.32 (0.22)	-1.01** (0.29)	0.14 (0.27)	-1.02** (0.28)	0.072 (0.30)
Lagged Capital Controls	0.71 (0.52)	-0.19 (0.32)	0.92 (0.53)	-0.12 (0.31)	0.90 (0.53)	-0.15 (0.32)
Twice Lagged Log of Investment	-1.72 (1.82)	-1.49 (2.03)	-1.79 (1.79)	-1.10 (1.71)	-1.37 (1.60)	-1.35 (1.67)
Lagged Log of Investment	-3.32 (1.99)	0.063 (1.31)	-3.42 (2.19)	-0.92 (1.42)	-3.23 (2.07)	-1.19 (1.32)
Lagged Average Schooling	4.39* (2.10)	-0.78 (0.67)	3.54 (1.89)	-0.58 (0.67)	2.48 (1.56)	-0.53 (0.73)
Lagged Inflation	-0.057 (0.030)	0.00079** (0.00022)	-0.071* (0.030)	0.00089** (0.00024)	-0.072* (0.027)	0.00089** (0.00026)
Lagged Trade as Percent of GDP			0.029 (0.016)	0.054 (0.029)	0.030 (0.018)	0.068 (0.034)
Lagged Life Expectancy					0.34* (0.16)	0.24** (0.059)
N	120	127	114	124	114	124

All regressions include country fixed effects and time dummies. Coefficients and errors for these are not reported. Robust Standard Errors clustered by country in parentheses. Significantly different than zero at 95% (*), 99% (**) confidence.

Table 3: Cross Sectional Results

	Single Interaction	Double Interaction
Capital Controls	0.43 (0.25)	0.49 (0.30)
Reg	0.98 (0.52)	1.75* (0.72)
ELF	-2.30* (1.07)	0.13 (1.24)
Capital Controls * Reg	-1.27* (0.46)	-0.61 (0.89)
Capital Controls * ELF		-0.43 (0.92)
Reg * ELF		-3.96* (1.90)
Capital Controls * Reg * ELF		-1.79 (1.97)
Log GDP 1975	-0.95* (0.37)	-1.19** (0.39)
Average Schooling	0.41 (0.29)	0.38 (0.30)
Log Investment	1.24 (0.91)	1.11 (0.72)
Inflation	-0.0021** (0.00030)	-0.0017** (0.00035)
Life Expectancy 1975	0.086* (0.043)	0.150** (0.050)
Trade as Percent of GDP	-0.0079 (0.0043)	-0.0085 (0.0053)
Latin America	-1.76** (0.44)	-1.56** (0.54)
East Asia	-0.46 (0.81)	1.37 (1.00)
Sub Saharan Africa	1.42 (1.03)	1.61 (1.36)
Observations	65	65
R-Squared	0.75	0.78

Robust Standard Errors in Parentheses.
Significantly different than zero at 95% (*), 99% (**) confidence.

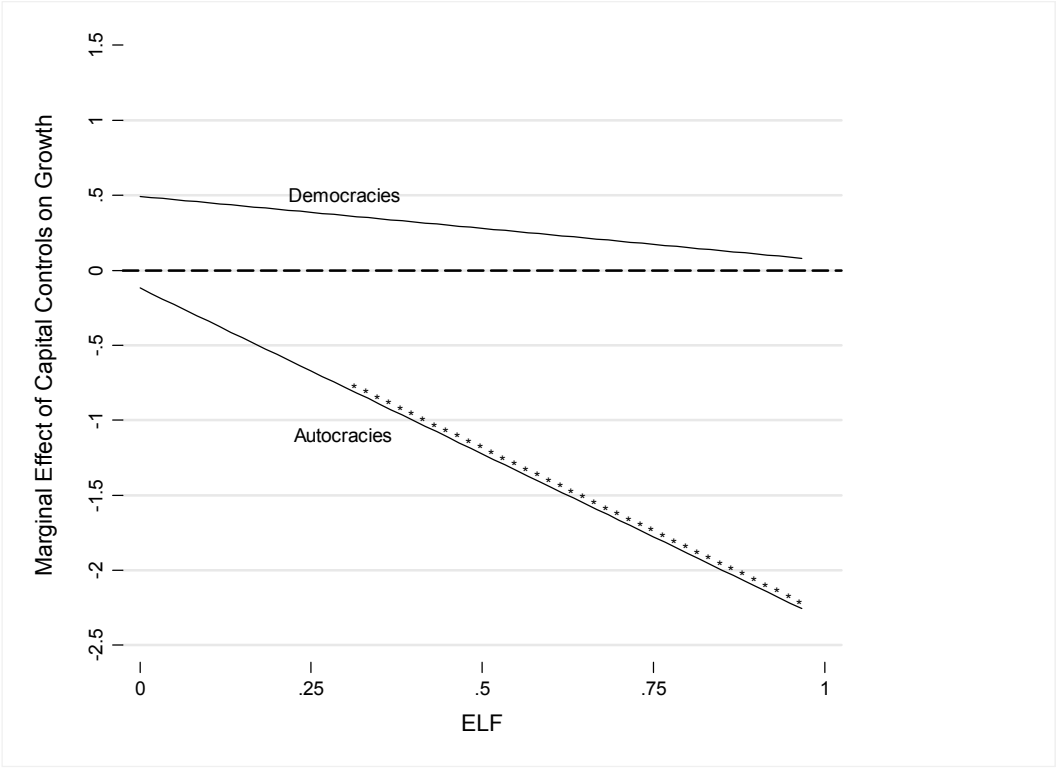
Table 4: Cross Sectional Results

Marginal Growth Effect of Tightening Capital Controls

	Single Interaction	Double Interaction			
		ELF=0	ELF=.2	ELF=0.6	ELF=0.8
Authoritarian Regimes	-0.85* (0.35)	-0.12 (0.79)	-0.56 (0.53)	-1.45** (0.40)	-1.89** (0.60)
Democracies	0.43 (0.25)	0.49 (0.30)	0.41 (0.27)	0.24 (0.49)	0.16 (0.65)

Robust Standard Errors in Parentheses.
Significantly different than zero at 95% (*), 99% (**) confidence.

Figure 1: Marginal Effect of Capital Controls on Growth by ELF in Democracies and Autocracies



* Indicates significance at the 95% level.

Appendix Table A: Summary Statistics for Key Variables (Panel)

Variable	Mean	Standard Deviation	Minimum	Maximum	N
Capital Controls	0.35	1.27	-2.62	1.75	255
Przeworski Autocracy Measure	0.81	0.37	0	1	274
Bueno de Mesquita: W over S	0.62	0.31	0	1	273
Polity	0.68	7.83	-10	10	268
GDP Growth	1.41	3.04	-11.43	10.56	323
Average Secondary Schooling	1.39	1.06	0.033	5.09	269
Average Log Investment	3.01	0.32	1.81	3.98	269
Average Inflation	1.33	9.75	-0.018	117.50	269
Average Life Expectancy	65.23	9.76	34.65	79.19	269
Average Trade as Pct. of GDP	64.20	48.25	11.80	439.03	269

Appendix Table B : Summary Statistics for Key Variables (Cross Country)

Variable	Mean	Standard Deviation	Minimum	Maximum	N
GDP Growth	1.21	2.07	-4.93	7.18	65
Capital Controls	0.030	1.19	-2.66	1.70	65
Przeworski Autocracy Measure	0.46	0.42	0	1	65
Ethno Linguistic Fractionalization	0.32	0.29	0	0.87	65
Log Real per capita GDP in 1975	7.91	0.95	6.13	9.52	65
Average Secondary Schooling	1.45	1.00	0.063	4.54	65
Life Expectancy 1975	60.66	10.05	37.26	74.87	65
Average Log Investment	3.08	0.25	2.39	3.68	65
Average Inflation	89.50	299.67	2.59	1810.98	65
Average Inflation (Outliers Excluded)	16.23	15.40	2.59	78.41	60
Average Trade as Pct. of GDP	62.16	47.15	15.45	369.98	65