

## **Capital Controls, Political Institutions, and Economic Growth**

### **Abstract**

The case study literature suggests that liberal international capital flows can have extremely different growth consequences depending on the political environment. Despite this, little systematic attention has been paid to how politics affects the relationship between capital controls and long term growth in a large-n context. Focusing on the conflict alleviating properties of democracy we demonstrate that authoritarian countries with a large number of societal divisions are negatively affected by capital controls, while neither democratic nor homogeneous countries suffer adverse growth effects from capital controls. We also challenge the prevailing wisdom on the causal links between capital controls, investment, and growth, systematically addressing the concerns of measurement error, reverse causation, and omitted variables bias that make it difficult to accurately assess the causal effects of capital controls. Our results suggest that the decision to liberalize capital flows should take careful account of both the political and societal context.

The case study literature indicates that a government's decision to operate with or without controls on international capital flows can have extremely diverse growth consequences. 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.<sup>1</sup> Similar variations in growth outcomes have been noted for countries with capital controls.<sup>2</sup> These variations have often been attributed to political institutions in the case study literature. However, little systematic attention has been paid to whether and how political institutions affect the relationship between capital controls and long term growth in a large n-context. Statistical studies on the relationship between capital controls and growth, primarily conducted by economists, largely ignore any role for institutions. In this paper we seek to show that democratic institutions have a critical effect on this relationship. We show that countries that are simultaneously authoritarian and societally fragmented are significantly negatively affected by capital controls. Democratic countries, however, do not suffer adverse growth effects from capital controls irrespective of the degree of societal division. We also show, in contrast to economists, that where controls adversely affect growth they do not do so by depressing the quantity of investment but rather by affecting the productivity of investment.

Justifying these claims calls for surmounting several methodological challenges; it requires much more than simply adding a political variable to economic specifications. It calls for both challenging the prevailing causal arguments relating capital controls to

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<sup>1</sup> See Haggard (2000).

<sup>2</sup> For example China vs. many countries in Africa.

growth as well as taking the endogeneity of institutions seriously, which we do. We also explicitly address many of the well known obstacles to precisely assessing the causal effects of capital controls including reverse causation from growth to controls, omitted variables bias, poor measures of controls, and difficulties in assessing the effects of societal (not just ethnolinguistic) fragmentation.

Our paper primarily contributes to two literatures in political science. The first is the international political economy literature on political institutions and financial globalization.<sup>3</sup> The second is the literature on democracy and development.<sup>4</sup> Our main contribution to these literatures is to highlight the importance of systematically studying the interplay of societal divisions and political institutions, specifically the conflict alleviating properties of democracy, when evaluating the effects of financial globalization on economic performance. As an ancillary contribution, we bring some symmetry to the international political economy literatures on trade and finance in an important realm. On the trade side numerous studies have concluded that, barring interventions for either infant industries or strategic trading reasons, barriers to free trade impede long term growth. However far less systematic attention has been paid to the analogous question on the finance side; are governmentally imposed controls on international capital flows beneficial or damaging for long term economic growth? Our research here is directly aimed at this question, albeit from a political perspective.

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<sup>3</sup> Notable contributions to this literature include Broz (forthcoming), and Clark and Hallerberg (2000).

<sup>4</sup> For surveys see Alvarez et al. (2000) and Przeworski and Limongi (1993).

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 assess how democratic political institutions can affect the relationship between capital controls and growth. We describe our data in Section 3. In Section 4, we present our main results. In Section 5, we address questions relating to endogeneity. In Section 6 we seek to identify the most plausible causal mechanism through which democratic institutions affect the relationship between capital controls and growth. Section 7 addresses the question of whether our main results hold across different definitions of latent conflict in a society. Section 8, 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.<sup>5</sup> 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,

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<sup>5</sup> Obstfeld 1998, 10.

abandoning controls helps eliminate the deadweight losses associated with rent seeking for capital import/export licenses.<sup>6</sup>

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.<sup>7</sup> 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.”<sup>8</sup> 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.”<sup>9</sup> 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.<sup>10</sup> 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.

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<sup>6</sup> Chanda 2005.

<sup>7</sup> Rodrik 1998a, 2.

<sup>8</sup> Rodrik 1998a, 4.

<sup>9</sup> Rodrik 1998a, 4.

<sup>10</sup> Rodrik 1998a, 4.

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 (1998a), 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) too find that “it is hard to find a robust effect of capital account liberalization on growth.”<sup>11</sup> (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.) Bordo and Eichengreen (1998) find this standard null result to hold even when taking account of selection effects (the choice of capital controls when they are likely to be most effective). 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, present results that challenge both Edwards and Edison et al.’s partially positive findings.

In the most recent contribution to this literature in Economics 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

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<sup>11</sup> Eichengreen and Leblang 2003, 206.

fragmented societies, but not so in homogenous societies.<sup>12</sup> The causal logic, presented in a formal model, is that an increase in the number of ethnic groups results in more rent seeking competition over licenses for the import/export of capital between these groups. Expenditures on rent seeking divert resources away from investment, resulting in a negative modifying effect for ethnic fragmentation on the relationship between capital controls and growth. Chanda's results offer a potential solution to the ambiguity surrounding the efficacy of capital controls; the ambiguity perhaps simply results from the fact that capital controls are harmful under some societal conditions, but not under others.

Aside from Leblang, mentioned above, political scientists have not entered into this statistical debate. Some political scientists have, however, published case analyses which suggest that political variables may be relevant to the debate. Studying several Asian countries with liberal capital flow regimes in the 1990s Haggard (2000) finds that the presence/absence of divided government accounts for diverse patterns in growth recoveries from crises. With a similar set of cases MacIntyre (2001) argues that an intermediate number of checks and balances helps countries respond to crises, because they help avoid incredible commitments and gridlock. Once again in the Asian context, Satyanath (2005) shows why democracies are exceptionally vulnerable to signaling-related banking crises under liberal capital flows. While these findings are suggestive, they still leave open the question of how and to what extent political institutions affect the relationship between capital controls and growth in a large-n context. This is the question that we address in the following sections.

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<sup>12</sup> Chanda 2005, 441.

## **2) Political Institutions and the Relationship between Capital Controls and Long Term Economic Growth**

Economists' models of the effect of capital controls usually ignore the role of political institutions. Even Chanda's causal logic, despite the fact it is explicitly political, does not allow for the presence of institutions that moderate competition/conflict between societal groups. However, there are compelling arguments in the literature which indicate that democratic institutions can have such an effect.

Lijphart (1969) has described how democracies can develop consociational arrangements to implement cooperative solutions to ethnic disputes. Rodrik (1998b and 1999) has formally demonstrated how electoral and other checks and balances found in democracies can cause contending societal groups to anticipate that other groups will be constrained from making a grab for a limited resource. This dilutes the incentives for each group to make a grab for the resource, and thus alleviates the competition between societal groups. Elbadawi and Sambanis (2000) have showed how democracy alleviates civil war.

These papers suggest a plausible mechanism through which democratic institutions can affect the relationship between capital controls and growth. Assume, as per Chanda, that as the number of groups competing for capital licenses increases more resources are diverted from growth enhancing investment to rent seeking. Let us define a winning coalition as the set of citizens of a country whose continued support is essential for a chief executive to retain office. Democracies are regimes with relatively large winning coalitions. Thus, at any given level of societal fragmentation we would expect a larger proportion of societal groups to be represented in the winning coalition in a



democracy than in a dictatorship. Assume that the chief executive's goal is to retain office. In democracies with capital controls (as opposed to authoritarian regimes with such controls) we should then expect a relatively large proportion of groups to have access to capital licenses without resorting to aggressive rent seeking, simply by virtue of a larger proportion of these groups being essential to the chief executive's survival in office (i.e. being part of the chief executive's winning coalition).

This logic leads us to expect that we are most likely to observe a large number of societal groups engaged in competitive rent seeking for capital licenses in environments with controls where it is both the case that a) there is a high degree of diversity of preferences about how the licenses should be allocated (i.e. relatively fragmented environments), and b) a relatively high proportion of groups is forced to engage in aggressive rent seeking to acquire these licenses. In sum, it is in environments that are both relatively fragmented and undemocratic that we should most expect to observe capital controls cause the diversion of substantial funds from investment to competitive rent seeking, resulting in lower investment and slower growth. In contrast, environments which are either relatively unfragmented or democratic should be relatively immune to such effects, because either the societal (diversity of preferences) or political (inclusiveness) conditions generate less rent seeking competition. This intuition is easy to formally demonstrate with a simple extension to the Chanda model, which is available on request.<sup>13</sup>

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<sup>13</sup> We have excluded this purely for reasons of length. All that is required is to add a parameter which takes the value of (1-the proportion of societal groups whose support is essential for the incumbent's survival in office), and to introduce this parameter in a

While the mechanism presented above is one possible causal story of how democratic institutions impact on the relationship between capital controls and growth there is another plausible story as well. In such a story expenditures on rent seeking resulting from the presence of capital controls, while present, are unlikely to be so large as to have a major impact on a country's national investment level even in fragmented environments. The causal effect of capital controls on growth in fragmented environments then would not pass through the quantity of investment. Instead it would pass through the productivity of investment, as it is impacted by the efficiency with which capital licenses are allocated. The causal story here is as follows. The presence of numerous societal groups competing for licenses with bribes in fragmented societies generates incentives for the government to allocate licenses in return for bribes rather than allocating licenses to the most productive uses. As per Bueno de Mesquita et al. (2003), under the assumption that leaders maximize the probability of political survival, leaders of large coalition polities (democracies) have more powerful incentives to provide public goods such as economic growth than leaders of authoritarian regimes. This implies that, in the face of bribe offers from competing societal groups to deviate from efficiency-based criteria for allocating licenses, leaders of democracies are likely to accept such offers/deviate from efficiency-based criteria to a smaller degree. This is so because slower growth (which is the likely consequence of such deviation) jeopardizes their hold on office to a greater extent (Bueno de Mesquita 2003.) Like in the first

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multiplicative relationship with the ethnic fragmentation term in the Chanda model. The above result then follows from the plausible assumption that this new parameter takes lower values in a democracy.

causal story this mechanism implies that we are most likely to see a significant negative relationship between capital controls and growth in authoritarian regimes that are also relatively fragmented (and thus subject to intense rent seeking competition over licenses). Also like in the first argument, in relatively unfragmented societies the low level of rent seeking competition between groups over licenses should generate an insignificant relationship between capital controls and growth irrespective of regime type. The observable implications of the second causal mechanism, however, differ from those of the first in one critical respect. The first mechanism implies that in fragmented authoritarian environments we should observe significant negative relationships both between capital controls and growth, and between capital controls and investment quantity. The second mechanism implies a significant negative relationship for capital controls in the growth regression but not in the investment regression.

The following two hypotheses are suggested by both of the above causal stories. Our primary hypothesis is that capital controls are significantly associated with slower growth in fragmented societies that have authoritarian regimes, but not in those which have democratic regimes. The secondary hypothesis is that in relatively homogenous societies capital controls are not robustly associated with growth whether a country is authoritarian or democratic.

If the two hypotheses above are confirmed we can distinguish between the two causal mechanisms in the following fashion. If we see a significant negative relationship between capital controls and investment quantity in highly fragmented authoritarian societies, but no such relationship in relatively unfragmented and democratic societies the first causal mechanism would be supported. However, if we instead observe an

insignificant relationship between capital controls and investment quantity irrespective of regime type or societal fragmentation this supports the latter causal mechanism. This is because, in the absence of a significant relationship between capital controls and investment quantity, the most plausible explanation for a difference in the relationship between controls and growth between fragmented authoritarian environments and other environments would lie in variations in the productivity of investment resulting from differences in the efficiency with which capital licenses are allocated. (This of course assumes that we have adequately addressed omitted variable bias with fixed effects etc., which we take pains to do.)

### **3) Empirical Strategy and Data**

Since our primary interest is in long term growth (as opposed to short term fluctuations in growth), and because the theoretical claims about the benefits of liberal capital flows described earlier all relate to the long term benefits of liberal flows, in our core specifications we conduct an OLS cross country analysis of 76 countries, with the dependent variable defined as average annual per capita GDP growth between 1975 and 1995.<sup>14</sup> (This facilitates comparison with Chanda's results which are for the same period.) We address questions of endogeneity (described below) using the technique of instrumental variables. In addition, in recognition of the well known concerns about omitted variable bias in cross country growth regressions, we test the robustness of our core results in a panel with five year periods using country and time fixed effects. We

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<sup>14</sup> Our GDP data is from the World Bank's Global Development Network Growth Database (GDNGD). The data is available at <http://www.nyu.edu/fas/institute/dri/>

average over five years in the panel analysis rather than use the country year as the unit thanks 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. In brief, 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.”<sup>15</sup>

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<sup>15</sup> 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.)

A serious concern that arises in any analysis of the effects of capital controls is the difficulty of measuring capital controls. As a means of increasing our confidence that our results are not being driven by imperfections in measurement we use three measures of capital controls. Our main measure is the latest (and widely considered to be the best) measure, one developed by Chinn and Ito (2005). 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. (See Chinn and Ito 2005 for details.) We refer to this variable as CAPCON3. In our instrumental variables specifications we use the initial period value of CAPCON3 as our instrument, for reasons that we explain in Section 5.

We also present results for the measure of capital controls used by most contributors to the literature, a dummy variable provided annually by the IMF in its Exchange Arrangements and Exchange Restrictions publication, with 1 indicating that capital controls were present in a given year. Note that the IMF's dummy variable is an extremely general indicator of capital controls. It simply indicates whether or not each country had restrictions on international capital transactions in any given year. We call our IMF-based measure CAPCON1.

A third measure that appears in some of our regressions, called CAPCON2, is one developed by the Fraser Institute.<sup>16</sup> The CAPCON2 measure assesses restrictions on the ability of citizens to own foreign currency bank accounts, domestically and overseas. Absence of restrictions in one of the two areas results in an intermediate score. The Fraser Institute calculates this score based on data from Currency Data and Intelligence

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<sup>16</sup> These data were provided to us by Areendam Chanda.

and the World Currency Yearbooks, in addition to the IMF source mentioned above. The correlation between our capital controls variables ranges between .7 and .8, which allows for the possibility of different results.

In our specifications we allow the effect of capital controls on growth to be modified by societal fragmentation and by democracy. We use several different measures of societal fragmentation. The first measure we use is the index of ethno linguistic fractionalization (ELF), which 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). Alesina et al. (2003) have recently criticized the ELF measure on the grounds that it does not adequately capture racial differences. We thus conduct robustness checks using Alesina et al.'s measure of ethnic fractionalization. We also conduct robustness checks using Montalvo and Reynal-Querol's (2005) measure of ethnic polarization. Finally, we also conduct robustness checks on the effects of societal fragmentation using the Gini (WIDER) measure of income inequality.

For democracy, in our main specifications we use Przeworski et al.'s measure of democracy called REG.<sup>17</sup> 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

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<sup>17</sup> The data is available at <http://pantheon.yale.edu/~jac236/DATASETS.htm>

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.<sup>18</sup> Since our causal argument depends on the concept of a winning coalition we additionally conduct robustness checks using Bueno de Mesquita et al.'s (2003) measure of the size of the winning coalition (W). Another indicator of the presence/absence of democracy is offered by the CHECKS measure provided by the Database of Political Institutions (Beck et al. 2001), in which countries which do not have competitive elections generally score below 3 points. We thus conduct regressions using the CHECKS measure as well.

To address concerns about the endogeneity of political institutions in several specifications we use the initial period value for the REG variable (in place of the contemporaneous value) to split the sample between democratic and authoritarian regimes. As a further check against endogeneity in one specification we use the distance of the mid point of a country from the equator to divide the sample between democratic and authoritarian regimes. This variable is often used as an exogenous source of cross country variation in institutions because it is inarguably free of reverse causality. It is also strongly correlated with our REG measure at the 1% level of significance even when we use our most extensive battery of controls.

Our approach to controls is standard. In our core specifications we employ the controls used both by Rodrik (1998a) and Chanda (2005) in their core specifications, namely, per capita GDP in the initial period, a measure aimed at capturing human capital

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<sup>18</sup> <http://www.cidcm.umd.edu/inscr/polity/>



(the average years of secondary schooling),<sup>19</sup> and regional dummies. In our robustness checks we add controls for more potential determinants of growth, namely the inflation rate, life expectancy, and trade openness (exports + imports divided by GDP). To assess which of our causal arguments receives more support we also run several regressions with the log of the investment rate on the left hand side. The data for all these variables is also from the World Bank (GDNDG).<sup>20</sup> Summary statistics are provided in the Appendix while a list of countries (not included to meet the length restriction) is available upon request.

#### **4) Main Results**

Our main results are summarized in Figure 1. The y-axis displays the coefficient for the relationship between our core measure of capital controls, CAPCON3, and average growth over 1975-95. A positive coefficient for CAPCON3 indicates that capital controls have a negative effect on growth. 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 CAPCON3 and growth is significant. A comparison of the slopes of the lines for authoritarian regimes (REG=1) and democracies (REG=0) immediately reveals that societal fragmentation has a far less detrimental effect on the coefficient for CAPCON3 in democracies. The figure indicates that authoritarian regimes are significantly adversely affected by controls at high levels of fragmentation, but not at low levels. Democracies are insignificantly affected by controls at high levels

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<sup>19</sup> The data comes from the updated Barro and Lee (1993) dataset.

<sup>20</sup> The data is available at <http://www.nyu.edu/fas/institute/dri/>

of fragmentation and are slightly positively affected by controls at very low levels of fragmentation. (As we show below the very last positive result drops to insignificance in robustness checks while all the preceding ones are extremely robust, which is consistent with our hypotheses.) We now delve deeply into the underpinnings of these results.

Figure 1 is derived from the regression displayed in the first column of Table 1, which offers a conventional regression table for our cross country specifications. This table is solely intended to be an entry point for our detailed results which are presented in the following tables. 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 Table 1, then, the coefficients and standard errors for capital controls presented in Row 1 refer to the effect of capital controls when 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. Table 1 suggests a negligible effect for capital controls on growth when ethnic homogeneity is high and democracy is present (REG=0). The coefficient for capital controls is either insignificant or fails to achieve the 5% level of significance across specifications using our core CAPCON3 measure with the full sample of 76 countries, dropping OECD countries, using the CAPCON1 measures of capital controls, and using the CAPCON2 measure of capital controls. **(Note that our core CAPCON3 measure runs in the opposite direction to the others, with a positive coefficient indicating that capital controls have a negative effect on growth.)**

While these results are partially revealing, we need to go beyond a conventional regression table to assess our hypotheses. 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 and ethnic fractionalization. Specifically, we need to recalculate standard errors, taking account of covariances between the modifying variables.<sup>21</sup> We do this in the following tables.

Table 2 allows us to assess our hypotheses using our core Przeworski measure of democracy, REG. Panel 1 shows the effects of capital controls at relatively low levels of ethnic fragmentation (0.05 is the 25<sup>th</sup> percentile and .2 is the median) when democracy is present (REG=0) and absent (REG=1). As may be observed, capital controls do not have a robust significant relationship with growth at low levels of fragmentation irrespective of regime type. This is consistent with our second hypothesis.

Panel 2 displays the effects of capital controls at relatively high levels of fragmentation (0.6 is the 75<sup>th</sup> percentile and .8 is the 90<sup>th</sup> percentile.) As the panel shows capital controls robustly have an adverse effect on growth at high levels of fragmentation when democracy is absent (REG=1). Since a positive coefficient for CAPCON3 indicates that capital controls adversely affect growth, the table indicates that in a fragmented society an authoritarian regime experiences a 1% decline in the annual growth rate as a consequence of a one standard deviation increase in capital controls. However, capital controls display an insignificant relationship with growth when democracy is present. All this is consistent with our primary hypothesis that it is

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<sup>21</sup> See Brambor et al. 2006.

authoritarian regimes in relatively fragmented societies that are most likely to be adversely affected by capital controls.

Table 3 repeats the same exercise replacing the Przeworski measure of democracy with the well-known Polity measure. Note that in this case a country is considered to be a democracy if its Polity score is at least 5. As may be seen in Panel 1 capital controls have an insignificant effect on growth at low levels of ethnic fragmentation irrespective of regime type. However, at relatively high levels of fragmentation authoritarian regimes (Polity score of less than 5) display a robustly significant adverse effect for capital controls, in contrast to democratic regimes.

Table 4 displays the results using two alternative ways of capturing democracy. The table first displays the results for average W (winning coalition) scores for 1975-95 at the 25th, 50th, 75th, and 90<sup>th</sup> percentiles. At low levels of fragmentation (first two columns) capital controls display an insignificant relationship with growth in democracies and authoritarian regimes alike. However, at high levels of fragmentation (third and fourth columns) capital controls display a significant adverse effect on growth in authoritarian regimes, but not in democratic regimes.

Next, Table 4 displays the results for the CHECKS measure. Authoritarian regimes score below 3 on this measure. Once again at low levels of fragmentation capital controls do not display a significant relationship with growth, irrespective of regime (first two columns). However, at high levels of fragmentation, capital controls show a significant adverse effect at a Checks score of 1 (where most authoritarian regimes fall). This effect drops below the 5% level of significance once the CHECKS score reaches 3.

## 5) Questions of Endogeneity-

Anecdotal evidence indicates that capital controls could be endogenous to growth; countries could choose to impose controls when growth is slow (when capital is likely to flow out), but choose to liberalize in periods of fast growth (when capital is likely to flow in.) (Malaysia in 1998 is an oft cited example of the former phenomenon.) The above results could thus be contaminated by reverse causality. The question is, to what degree? In our cross country sample the correlation between capital controls and growth is approximately .2 for all three of our measures of capital controls while in the panel the correlation lies between .1 and .2. Moreover growth is never significant when we regress changes in controls on growth with the controls above. (Not shown.) We thus conclude that, despite the anecdotal evidence, our results are at most contaminated to a small degree by reverse causation from growth to controls over the long term. (This is not to say that such an effect is absent over periods as short as a year or two, which we do not consider for reasons mentioned earlier.)

Since we cannot rule out a small degree of bias resulting from reverse causality we check if our core results hold up when this bias is addressed. Our strategy for doing so is a conventional one in the face of concerns about simultaneity; it is to use the initial period (1975) value of capital controls as the instrument for average capital controls over 1975-95. The assumption here, of course, is that the 1975 value of capital controls is unlikely to be subject to reverse causality from average growth over 1975-95. This strategy is only acceptable if the first stages do not generate absurdly high t-statistics, which would be indicative of overly high serial correlation, and if we are confident that there is only a small amount of reverse causality. We have provided evidence to support

the latter view, and the tables to follow will support the former. In addition, we make efforts to address standard concerns about the exclusion restriction.

Since our focus is on establishing if the significant results seen in the previous regressions hold up when endogeneity is addressed, and since the only significant results were for authoritarian environments with ELF scores of  $>0.2$ , we focus exclusively on these environments in the instrumental variables regressions. (In other words our interest here is in establishing if the significant adverse effect of capital controls on growth in fragmented authoritarian environments is the product of endogeneity or not.) To this end we construct a sample which exclusively consists of fragmented authoritarian countries, and conduct two stage least squares regressions on this sample. (This strategy, as opposed to one of instrumenting for numerous interaction terms, is necessary for gaining a significant first stage for capital controls.)

Columns 1-4 in Table 5 show the instrumental variables results for our core cross country analysis. Note that the first stage results are presented in the lower panel, while the second stage results are presented in the top panel. The first column displays results for fragmented countries ( $ELF > .2$ ) with an average REG score over 1975-95 that exceeds .5. (An average REG score of over .5 is indicative of a country that spent more time as a dictatorship than as a democracy over 1975-95.) The upper panel shows that the significant negative effect of capital controls on growth in fragmented authoritarian environments is robust to instrumenting for CAPCON3 with its initial period value. The lower panel shows that the first stage t statistic for initial period CAPCON3 is not absurdly high (it is approximately 3).

Column 1 does not address the endogeneity of democracy to growth, since the average REG score for 1975-95 could be influenced by reverse causality. In column 2 we thus identify authoritarian regimes using an exogenous source of variation in institutions. The countries addressed here are those which are fragmented ( $ELF > .2$ ) and which have a geographical mid-point which is located between the Tropics of Cancer and Capricorn ( $LAT = 1$ ). (As mentioned earlier the latter is highly correlated with REG.) Column 2 indicates that our result is robust to addressing the endogeneity of institutions in this way. A comparison with column 1 also suggests that we were justified in being concerned about reverse causality from growth to institutions since the coefficient in column 2 is smaller.

In column 3 we address reverse causality vis a vis institutions in a third way, using the value of REG in 1975 as the basis for identifying authoritarian countries ( $REG = 1$  in 1975). (As before  $ELF > .2$  is considered indicative of relatively high fragmentation.) In order to address concerns about omitted variable bias as well as about the violation of the exclusion restriction, in this specification we also add controls for variables that are plausibly correlated with any omitted characteristics of countries that could adversely affect growth or ways in which initial period capital controls can directly influence growth. The variables we add are inflation over 1975-95, trade volume over 1975-95, and life expectancy in 1975. As may be seen our results are unaffected by these changes. (Note that we cannot conduct over-identification tests for the exclusion restriction because we are exactly identified.)

One of our claims is that we primarily need to be concerned with the negative effects of capital controls in fragmented authoritarian environments, rather than in

authoritarian environments across the board. If this claim is correct the effect of capital controls on growth observed in columns 1-3 should disappear if we cease to restrict our attention to highly fragmented environments and consider all authoritarian countries instead. Column 4 shows that this is precisely what occurs.

All of the above findings were derived from a cross country framework. This means that, even though we use an extensive battery of controls, concerns about omitted variables bias may persist because cross country analyses do not lend themselves to the use of country fixed effects. In order to address this concern we present results for a panel analysis with country and time dummies in column 5. As mentioned earlier the panel analysis uses non-overlapping five year periods as the basis of the analysis. As a means of addressing the potential endogeneity of institutions we identify authoritarian regimes as those which had an average REG score exceeding .5 over the first five year period ( $REGP1 > .5$ ). As may be seen, our results are robust. (We add the caveat that this is an OLS specification; we did not get a significant first stage using lagged CAPCON3 as the instrument with fixed effects.) The results are unchanged when we add all the controls from the previous columns to the fixed effects (not shown).

The panel results in column 5 were for fragmented authoritarian countries. In column 6 we examine if the result in column 5 disappears if we consider fragmented democracies. As column 6 shows the sign of the coefficient switches when we consider democracies, which is consistent with what we saw in Table 2 using our core CAPCON3 measure. We add the caveat that the sample size is relatively small, but it is reassuring that the insignificant result is not the consequence of a substantially higher standard error in column 6 relative to 5. It is also reassuring that a non-negligible number of developing



countries fall in the category of countries which are fragmented and yet democratic. (The list includes Guatemala, India, Papua New Guinea, Sri Lanka, Thailand, and Trinidad and Tobago.)

## **6) Which Causal Story?**

Having established that the core specifications support both our causal stories we now need to address which of them is better supported by the data. Recall that an insignificant relationship between capital controls and investment quantity across the board favors the causal mechanism involving the efficiency of license allocation rather than heavy expenditures on rent seeking. Table 6 shows precisely such results across both cross sectional and panel regressions.<sup>22</sup> Column 1 shows that in an OLS cross country regression there is an insignificant relationship between CAPCON3 and investment in fragmented authoritarian environments. Column 2 shows that this result holds up when we instrument for CAPCON3. Column 3 shows that this is the case for the entire sample of countries. In Column 4 we show that the result is robust to a panel analysis on fragmented authoritarian countries with time and country fixed effects. Adding investment as a control to our specifications in earlier tables does not alter our results and only results in minor changes in our point estimates (not shown). In sum, the results reported in this section lead us to favor a causal mechanism that passes through the productivity of investment (as influenced by efficiency of license allocation) over one passing through the quantity of investment (as influenced by the diversion of funds from investment to rent seeking).

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<sup>22</sup> We use the largest possible sample size allowed to us by the investment data.

## **7) Does democracy modify the conflictual effects of ethno-linguistic fragmentation alone or alleviate latent conflict in general?**

In all the specifications displayed so far we have used the ELF measure of ethnic fragmentation. One characteristic of this measure is that it focuses on one type of societal fragmentation, ethno-linguistic fragmentation. This raises the question of whether our results also hold when using other measures of latent conflict in a society.

We address this question in Table 7 where we run the regressions in Table 2 replacing the ELF measure successively with the Alesina et al. measure of ethnic fragmentation (which gives a higher weight to racial fragmentation than ELF), Montalvo and Reynal-Querol's measure of ethnic polarization, and the Gini measure of income inequality.<sup>23</sup> Table 7 displays results that are similar to those displayed in Table 2. Like Table 2, the top panel of Table 7 shows that at low levels of latent conflict there is no robust relationship between capital controls and growth irrespective of regime type. In fragmented authoritarian environments four out of six specifications indicate a significant negative relationship between capital controls and growth. The signs of all six specifications are consistent. While the signs of the coefficients for fragmented democratic environments are identical to those seen in Table 2, Table 7 differs from Table 2 in displaying significant coefficients across the board for such environments. We are reluctant to over-interpret the significance of the latter coefficients since it is not echoed elsewhere.

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<sup>23</sup> We would also like to have used Posner's (2004) measure of fragmentation, but the data is unfortunately only available for Africa.

## **8) Conclusion**

The literature on the liberalization of capital flows has by and large indicated that capital controls are not correlated with long term economic growth. The one published paper that finally finds an unambiguous correlation under specific conditions, Chanda (2005), overlooks the possibility that political institutions can affect the degree of conflict/competition between social groups. In this paper, we have showed that political institutions are, in fact, of critical importance in influencing the impact of capital controls on economic growth. Whereas authoritarian regimes in highly fragmented societies are likely to be adversely affected by controls, democracies are likely to be insulated from these adverse effects irrespective of the level of societal fractionalization.

Our findings have significant and concrete implications. First, democratic transitions in Africa in particular, the most ethnically fragmented region of the world, may significantly alleviate the dampening effect of capital controls on growth. For instance, our results indicate that a country like Zimbabwe could raise its annual growth rate by 1-2% if it experiences a democratic transition.

Second, our results indicate that, failing a democratic transition, capital flow liberalization may actually serve long term growth in many highly ethnically fragmented societies such as the dictatorships of Africa. This is an important finding in the context of the proliferating literature on “Africa’s Growth Tragedy.”

Finally, our findings suggest that fragmented democracies are unlikely to substantially boost long term growth rates by liberalizing capital flows, and should lay little credence to claims by international financial institutions that this will improve their growth performance. For instance in recent years India, a country that has long operated

under a barrage of capital controls, has 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 both the social environments of countries as well as their political institutions. The policy community may gain from taking account of this fact.

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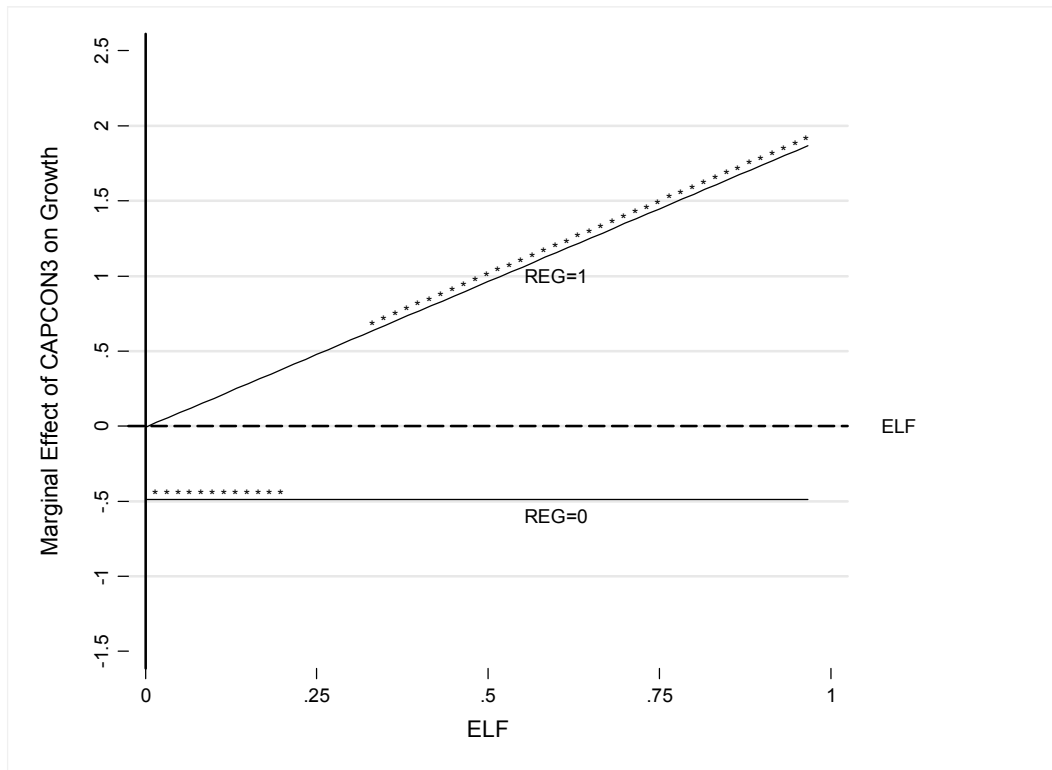
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**Figure 1: The Relationship Between Capital Controls and Growth as Modified by Democracy and Societal Fragmentation**





**Table 1: Basic Regression Table  
(Dependent Variable-Average Growth, 1975-95)**

	CAPCON3 ALL	CAPCON3 NO OECD	CAPCON1 ALL	CAPCON2 ALL
CAPCON	-0.49* (0.25)	-0.76* (0.44)	0.89 (0.81)	1.21* (0.74)
ETHNIC FRACTIONALIZATION	-2.26* (1.25)	-1.54 (1.96)	-0.77 (1.83)	-0.25 (1.48)
REG	0.44 (0.66)	0.52 (1.18)	2.85* (1.48)	0.61 (1.32)
CAPCON*ELF	0.001 (0.96)	0.50 (0.69)	-2.44 (2.99)	-3.74 (3.01)
CAPCON* REG	0.48 (0.57)	0.59 (0.69)	-3.03 (2.02)	-0.86 (1.92)
ELF*REG	-0.54 (2.36)	-0.94 (2.75)	-2.01 (3.62)	1.55 (3.57)
CAPCON*ELF*REG	1.94 (1.91)	1.86 (2.75)	0.47 (5.56)	-4.28 (6.01)
LOG GDP PER CAPITA (1975)	-0.75** (0.34)	-0.90** (0.35)	-1.00*** (0.33)	-0.91** (0.36)
YEARS OF SEC. SCHOOLING	0.87** (0.33)	1.49*** (0.44)	0.63** (0.31)	0.62** (0.26)
LATIN AMERICA	-1.97*** (0.50)	-1.83** (0.90)	-2.05*** (0.50)	-2.01*** (0.41)
SUB-SAHARAN AFRICA	1.35 (0.97)	0.12 (1.50)	1.64* (0.99)	1.98** (0.87)
EAST ASIA	-0.28 (1.46)	1.15 (1.07)	-0.32 (1.55)	0.89 (1.45)
Constant	7.37** (3.04)	7.46** (3.28)	9.17*** (2.87)	8.43*** (3.00)
Observations	76	57	76	76
R-squared	0.61	0.64	0.60	0.62
Robust standard errors in parentheses * significant at 10%; ** significant at 5%; *** significant at 1%				

**Table 2 Marginal Effects of Capital Controls in Dictatorships and Democracies (Dependent Variable-Average Growth, 1975-95)**

Panel 1: Marginal Effects of Capital Controls at Low Fragmentation

	CAPCON3		NO OECD		CAPCON1		CAPCON2	
	ELF=0.05	ELF=.2	ELF=0.05	ELF=.2	ELF=0.05	ELF=.2	ELF=0.05	ELF=.2
REG=0	-0.49** (0.24)	-0.49** (0.25)	-0.73* (0.41)	-0.66 (0.46)	0.77 (0.72)	0.41 (0.59)	1.02 (0.64)	0.46 (0.55)
REG=1	0.090 (0.47)	0.38 (0.36)	-0.050 (0.51)	0.30 (0.38)	-2.23 (1.58)	-2.52** (1.27)	-0.056 (1.47)	-1.26 (1.07)

Panel 2: Marginal Effects of Capital Controls at High Fragmentation

	CAPCON3		NO OECD		CAPCON1		CAPCON2	
	ELF=.6	ELF=.8	ELF=.6	ELF=.8	ELF=.6	ELF=.8	ELF=.6	ELF=.8
REG=0	-0.49 (0.53)	-0.49 (0.71)	-0.46 (1.11)	-0.36 (1.50)	-0.57 (1.37)	-1.06 (1.93)	-1.03 (1.41)	-1.78 (1.98)
REG=1	1.16** (0.49)	1.54** (0.70)	1.23** (0.53)	1.71** (0.76)	-3.31** (1.52)	-3.70* (2.07)	-4.46*** (1.65)	-6.06** (2.42)

Robust Standard Errors in Parentheses. Significantly different than zero at 90% (\*), 95% (\*\*), 99% (\*\*\*) confidence.

**Table 3: Marginal Effects of Capital Controls in Dictatorships and Democracies, Robustness Check with Polity (Dependent Variable- Average Growth, 1975-95)**

Panel 1: Marginal Effects of Capital Controls at Low Fragmentation

	CAPCON3		NO OECD		CAPCON1		CAPCON2	
	ELF=.05	ELF=.2	ELF=.05	ELF=.2	ELF=.05	ELF=.2	ELF=.05	ELF=.2
Polity=-10	0.046 (0.60)	0.45 (0.46)	-0.37 (0.75)	0.15 (0.56)	-3.02 (2.08)	-3.40** (1.63)	-0.34 (1.97)	-1.61 (1.43)
Polity=-5	-0.065 (0.45)	0.25 (0.34)	-0.31 (0.51)	0.094 (0.38)	-2.09 (1.52)	-2.46** (1.21)	-0.055 (1.43)	-1.16 (1.05)
Polity=0	-0.18 (0.31)	0.056 (0.25)	-0.27 (0.33)	0.037 (0.25)	-1.16 (1.01)	-1.53* (0.82)	0.23 (0.93)	-0.71 (0.69)
Polity=5	-0.29 (0.23)	-0.14 (0.20)	-0.22 (0.33)	-0.020 (0.26)	-0.22 (0.67)	-0.59 (0.54)	0.51 (0.59)	-0.26 (0.45)
Polity=10	-0.40 (0.27)	-0.34 (0.23)	-0.17 (0.52)	-0.077 (0.40)	0.71 (0.78)	0.35 (0.57)	0.80 (0.69)	0.19 (0.52)

Panel 2: Marginal Effects of Capital Controls at High Fragmentation

	CAPCON3		NO OECD		CAPCON1		CAPCON2	
	ELF=.6	ELF=.8	ELF=.6	ELF=.8	ELF=.6	ELF=.8	ELF=.6	ELF=.8
Polity=-10	1.53*** (0.57)	2.07** (0.81)	1.5*** (0.56)	2.24*** (0.82)	-4.43*** (1.50)	-4.95** (2.06)	-4.98*** (1.59)	-6.67*** (2.38)
Polity=-5	1.10*** (0.41)	1.53*** (0.58)	1.20*** (0.42)	1.75*** (0.62)	-3.47*** (1.14)	-3.98*** (1.55)	-4.09*** (1.19)	-5.56*** (1.76)
Polity=0	0.67** (0.30)	0.98** (0.42)	0.86** (0.37)	1.27** (0.52)	-2.51*** (0.90)	-3.01*** (1.20)	-3.21*** (0.93)	-4.45*** (1.35)
Polity=5	0.25 (0.29)	0.44 (0.39)	0.51 (0.43)	0.78 (0.60)	-1.55* (0.85)	-2.04* (1.18)	-2.32** (0.94)	-3.35** (1.34)
Polity=10	-0.18 (0.39)	-0.10 (-0.53)	0.17 (0.57)	0.29 (0.81)	-0.59 (1.03)	-1.07 (1.49)	-1.43 (1.22)	-2.24 (1.75)

Robust Standard Errors in Parentheses. Significantly different than zero at 90% (\*), 95% (\*\*), 99% (\*\*\*) confidence.

**Table 4: Alternative Measures of Democracy (Dependent Variable- Average Growth, 1975-95)**

	ELF=0.05	ELF=.2	ELF=.6	ELF=.8
W=.1	-0.17 (0.70)	-0.16 (0.23)	0.87* (0.49)	1.25* (0.70)
W=.5	-0.25 (0.38)	-0.0060 (0.3)	0.65** (0.34)	0.98** (0.47)
W=.6	-0.28 (0.31)	-0.036 (0.25)	0.60* (0.34)	0.92** (0.46)
W=1	-0.36 (0.26)	-0.16 (0.23)	0.38 (0.50)	0.65 (0.70)
Checks=1	-0.16 (0.47)	0.18 (0.36)	1.08** (0.5)	1.53** (0.71)
Checks=3	-0.32 (0.23)	-0.11 (0.20)	0.47 (0.35)	0.76* (0.47)
Checks=5	-0.49 (0.27)	-0.39 (0.25)	-0.13 (0.58)	-0.0040 (0.81)

Robust Standard Errors in Parentheses. Significantly different than zero at 90% (\*), 95% (\*\*), 99% (\*\*\*) confidence.

**Table 5: Addressing Reverse Causality and Omitted Variables Bias  
(Dependent Variable- Growth)**

Panel 1: Second Stage						
	ELF>.2 & REG>.5	ELF>.2 & LAT=1	ELF>.2 & REG75=1	REG75=1	ELF>.2 & REGP1>.5	ELF>.2 & REGP1<.5
CAPCON3	2.88** (1.13)	2.00** (0.77)	2.23*** (0.61)	0.47 (0.71)	2.16** (1.01)	-0.14 (0.98)
Initial Log(GDP)	-1.13 (0.82)	-0.14 (1.46)	-1.38 (0.98)	-1.03 (0.66)		
Average Schooling	1.41 (1.14)	-0.10 (1.79)	-0.42 (1.73)	0.72 (0.90)		
Latin America	-3.99** (1.95)	-4.85** (2.18)	-1.06 (1.90)	-1.30 (0.99)		
East Asia	-5.20 (3.26)	-2.28 (1.60)	-1.23 (2.11)	1.27 (2.34)		
S-S Africa	-3.19** (1.19)	-3.55 (2.46)	-0.017 (2.35)	-0.99 (0.92)		
Average Inflation			-0.0024*** (0.00037)	-0.0021*** (0.00049)		
Initial Life Expectancy			0.18*** (0.053)	0.13** (0.052)		
Average Openness			-0.0033 (.0049)	-0.0015 (0.0055)		
Panel 2: First Stage						
Initial CAPCON3	0.58*** (0.19)	0.46*** (0.14)	0.49*** (0.16)	0.34*** (0.11)		
Initial Log(GDP)	0.058 (0.25)	0.47 (0.36)	0.13 (0.42)	0.13 (0.34)		
Average Schooling	0.022 (0.36)	-0.11 (0.41)	-0.071 (0.59)	-0.048 (0.33)		
Latin America	0.66 (0.45)	-0.80 (0.78)	-0.15 (0.59)	0.21 (0.39)		
East Asia	1.44*** (0.50)	0.35 (0.73)	0.81 (0.73)	1.13* (0.58)		
S-S Africa	-0.09 (0.43)	-0.67 (0.75)	-0.65 (0.67)	0.061 (0.47)		
Average Inflation			0.00011 (0.00034)	-0.00020 (0.00031)		
Initial Life Expectancy			-0.0055 (0.029)	0.0094 (0.029)		
Average Openness			0.0031 (0.0024)	0.0030 (0.0025)		
Panel with Fixed Effects	NO	NO	NO	NO	YES	YES
N	26	24	24	37	92	27

Constant terms not reported. Robust Standard Errors in Parentheses. Significantly different than zero at 90% (\*), 95% (\*\*), 99% (\*\*\*) confidence.

**Table 6: Which Causal Mechanism? (Dependent Variable-Log Investment)**

Panel 1: Second Stage

	ELF>.2& REG75=1	ELF>.2& REG75=1	All Countries	ELF>.2& REGP1>.5
CAPCON3	0.096 (0.060)	0.20 (0.17)	-0.050 (0.043)	-0.021 (0.13)
Initial Log(GDP)	0.17 (0.11)	0.14 (0.11)	0.042 (0.063)	
Average Schooling	-0.031 (0.21)	0.025 (0.23)	-0.013 (0.045)	
Latin America	-0.58*** (0.16)	-0.67*** (0.23)	-0.16** (0.06)	
East Asia	-0.50** (0.20)	-0.77 (0.51)	0.33*** (0.11)	
S-S Africa	-0.74*** (0.18)	-0.77*** (0.17)	-0.33** (0.13)	

Panel 2: First Stage

Initial CAPCON3		0.46** (0.17)	0.42*** (0.069)	
Initial Log(GDP)		0.20 (0.27)	0.45*** (0.16)	
Average Schooling		-0.41 (0.41)	0.24 (0.15)	
Latin America		0.75 (0.49)	-0.22 (0.23)	
East Asia		1.95*** (0.55)	0.85*** (0.32)	
S-S Africa		0.036 (0.49)	0.22 (0.31)	
Panel with Fixed Effects	NO	NO	NO	YES
N	23	23	63	51

Constant terms not reported. Robust Standard Errors in Parentheses. Significantly different than zero at 90% (\*), 95% (\*\*), 99% (\*\*\*) confidence.

**Table 7: Robustness to Alternative Measures of Latent Conflict**

Low Fragmentation						
	ETHFRAG		ETHPOL		GINI	
	0	0.2	0.05	0.2	30	40
Reg=0	-0.4 (0.26)	-0.48** (0.23)	-0.38 (0.33)	-0.46 (0.28)	-0.40 (0.25)	-0.62*** (0.23)
Reg=1	0.099 (0.37)	0.32 (0.32)	-0.080 (1.10)	0.13 (0.82)	0.48 (0.53)	0.54 (0.39)

High Fragmentation						
	ETHFRAG		ETHPOL		GINI	
	0.6	0.8	0.6	0.8	50	60
Reg=0	-0.71** (0.27)	-0.82** (0.34)	-0.71*** (0.25)	-0.83** (0.29)	-0.85*** (0.31)	-1.07** (0.45)
Reg=1	0.91* (0.50)	1.20* (0.66)	0.68* (0.39)	0.96 (0.61)	0.60* (0.36)	0.67 (0.46)

Robust Standard Errors in Parentheses. Significantly different than zero at 90% (\*), 95% (\*\*), 99% (\*\*\*) confidence.

**Appendix Table : Summary Statistics for Key Variables**

Variable	Obs.	Mean	Std. Dev.	Min	Max
Average Growth Rate	76	1.23	2.06	-4.93	7.18
Capcon1	76	0.78	0.34	0.00	1.00
Capcon2	76	0.59	0.38	0.00	1.00
Capcon3	76	-0.013	1.21	-1.70	2.66
Fractionalization (ELF)	76	0.34	0.30	0.00	0.89
Fractionalization (Alesina et al.)	76	0.46	0.28	0.01	0.96
Polarization (Montalvo and Reynal-Querol)	76	0.52	0.24	0.02	0.98
Democracy (Polity)	76	2.09	6.66	-9.00	10.00
Democracy (Przeworski)	76	0.49	0.43	0.00	1.00
Checks	76	2.74	1.47	1.00	7.46
W	76	0.65	0.27	0.12	1.00
Inequality (Gini)	73	43.69	8.93	27.27	62.61
Polity in 1975	76	0.11	7.83	-10.00	10.00
Average schooling 1975-1995	76	1.41	0.98	0.063	4.54
Log Investment 1975-1995	69	3.08	0.27	2.31	3.68
Average Inflation 1975 -1995	68	16.28	15.47	2.59	78.40
Life Expectancy 1975	74	60.06	10.81	34.98	74.87
Trade Openness 1975-1995	73	61.80	45.83	15.45	369.98