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Private Investment and Political Institutions

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Recent research has demonstrated a negative link between macroeconomic and political uncertainty and levels of private investment across countries. This raises the question whether certain types of government institutions might help reduce this uncertainty. North and Weingast (1989) propose that political institutions characterized by checks and balances can have beneficial effects on investment by allowing governments to credibly commit not to engage in *ex post* opportunism with respect to investors. In this paper I develop and test a modified version of their hypothesis, suggesting that checks and balances, on average, improve possibilities for commitment, but that they are not a necessary condition for doing so. Results of heteroskedastic regression and quantile regression estimates strongly support this proposition.

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1. Introduction

There has been increasing recognition in recent years that the irreversibility of many forms of private investment creates a credibility problem for governments. If a firm fears that a government will have an incentive to make *ex post* changes in taxes or regulations, it may prefer to delay or cancel a proposed project. Both sides would be better off if a government could somehow commit not to acting opportunistically. This finding has major implications for developing countries, where investors may be particularly wary of the potential for radical swings in economic policy. One possibility proposed in a seminal article by North and Weingast (1989) is that political institutions characterized by checks and balances can allow governments to credibly commit.

In this paper I argue in favor of a modified version of the North and Weingast hypothesis. While increasing the level of checks and balances in a country can increase policy stability, existing theory also shows how governments *without* checks and balances may establish credibility through a more simple mechanism; if investors are convinced that there is little danger of opportunism, because a policy maker's own political supporters would suffer a loss from actions such as surprise increases in capital taxes. This suggests that governments in political systems with high checks and balances will, on average, find it easier to credibly commit, but we should also expect to see greater variability in levels of private investment within the set of observations where checks and balances are low. Ultimately, this prediction of greater variability in countries with few checks and balances reflects our absence of full information about the variation in preferences of policy makers across countries and over time. To illustrate why this should be the case, I draw on a simple political model of capital taxation developed by Persson and Tabellini (1994).

A further innovation of this paper is its use of new data which is specifically designed to measure the extent of checks and balances in a country's political system (collected by Beck *et al.*, 1999 and Henisz, 2000). In contrast with measures of democracy, such as the Gastil index, these two indicators are constructed according to a pre-specified and publicly available methodology. The other advantage of this new data is that it allows testing a more refined set of hypotheses. Rather than referring to the overall level of "democracy", the indicator I use is designed to measure something more specific: the extent to which a country's political institutions are characterized by checks and balances in government. This new data also allows more exact testing of political hypotheses than does data provided by risk assessment services such as BERI, ICRG, or the Economist Intelligence Unit. These latter measures have been useful for establishing links between poor protection of property rights and poor economic performance, but because they measure policy outcomes, they give us less sense of what real world political institutions are associated with better protection of property rights.

My findings are consistent with the above propositions. Using a data set on private investment in 74 developing countries and an index of political checks and balances developed by Beck *et al.* (1999), I estimate that the average long-run effect of moving from an authoritarian system to a political system where executive and legislature are controlled by separate parties would be an increase of 16% in private investment. Heteroskedastic regression estimates also suggest that within the group of countries with the latter set political institutions, the conditional variance of private investment is 9% lower than in the purely authoritarian systems. Using an index of political checks and balances developed by Henisz (2000) I estimate that a similar change in political institutions would also result in an 16% increase in average private investment, and that the conditional variance is 17% lower within the group of countries with checks and balances. Quantile regression results are also consistent with these findings. The pattern identified supports the proposition that checks and balances can improve credibility, but that they are not a necessary condition for doing so.

The remainder of this paper is organized as follows. Section 2 reviews the theoretical link between political institutions, uncertainty, and private investment. Section 3 presents the data. Sections 4 and 5 then conduct tests to examine the relationship between political checks and balances, levels of private investment and the variance of private investment within different groups of countries. Section 6 discusses robustness issues, and section 7 concludes.

2. Political institutions and private investment

Investment and uncertainty

Before considering how political checks and balances might reduce uncertainty, with knock-on effects of private investment, it is first worth reviewing the economic literature on investment and uncertainty. This shows that predictions about the sign of the uncertainty-investment link depend heavily on what assumptions go into one's model. If one assumes perfect competition, costless adjustment of factors other than capital, and constant returns to scale, then uncertainty actually raises the expected profitability of capital and therefore should lead to higher investment. More recent work shows that when one assumes that investments are irreversible, firms can be prompted to delay or forego investments out of the fear that the economic environment might change for the worse. Irreversibility implies that downward adjustments in capital stock are more difficult to make than are upward adjustments.[1]

The cross-country empirical literature on determinants of private investment provides support for the claim that higher macroeconomic uncertainty is associated with lower levels of private investment. While most of these studies are limited by the fact that they use only cross-sectional data, Serven and Solimano (1993) and Serven (1998, 1997) have estimated investment equations using panel data, finding significant support for the claim that there is a negative investment-uncertainty link. Serven (1998) is the most complete of these studies, using a data set covering a large group of developing countries over 26 years (1970-1995).

Political institutions and policy credibility

While existing work demonstrates that private investment is influenced by macroeconomic uncertainty, it would also be useful to consider how political conditions might affect perceived risks of opportunism for investors. As mentioned, irreversible investments may be subject to a credibility problem whereby a government has an incentive to change taxes or regulations *ex post* with the knowledge that investors cannot easily withdraw. One way of illustrating this is with the well known time-consistency problem in capital taxation. Given that capital investment decisions often involve a high degree of irreversibility, governments can face incentives to raise capital taxes *ex post*. To the extent that owners of capital anticipate this possibility, they will refrain from investing and in equilibrium both the government and investor will be worse off.

One proposed solution to the above problem is to adopt political institutions characterized by multiple veto points, raising the hurdles to policy change (North and Weingast, 1989). A veto point can be defined as a political institution, the holder of which has the power to block a proposed change in policy. A veto player, then, is the policymaker who controls a veto point. Multiple veto points can be created by constitutional provisions which specify, for example, that both an executive and a legislature must agree to any policy changes, or that multiple chambers of a legislature

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[1] See Dixit and Pindyck (1993). In order to demonstrate a negative link between uncertainty and investment one needs to assume not only irreversibility but also either risk aversion, imperfect

must approve any changes in laws. Multiple veto points can also exist as a consequence of electoral rules such as proportional representation, which favor the development of coalition governments. In coalition governments any one member of the coalition may be able to veto a policy proposal by threatening to withdraw from the government if its demands are not satisfied.[2]

While increasing the number of veto points in a political system may help to increase policy stability (and thus in this case the credibility of tax policies), making a firm prediction about their actual effect requires knowledge of policy maker preferences. To illustrate this, consider a simple two-period complete information model of capital taxation based on Persson and Tabellini (1994). Individuals derive a variable proportion of their income from land and from capital, as illustrated in equations (1) and (2) below where k_1 and l represent average per capita income from each factor, k_1^i and l^i represent land and capital income for the i th individual, and e is an exogenous parameter with mean zero.[3] So individuals with $e<0$ derive more income from capital than does the median member of society.

$$k_1^i = k_1 - e \qquad (1)$$

$$l^i = l + e \qquad (2)$$

In the first period, owners of capital decide whether to consume or to save their capital, earning utility $U_1 = (k_1^i - k_2^i)$ where k_2^i represents capital saved which earns a return of r.[4] In the second period a policy maker drawn from a political party p whose members each have an exogenous endowment e^p chooses the tax rates on land and capital

competition, or decreasing returns to scale.

[2] For a survey of political and electoral institutions, veto points, and their effect on policy making see Tsebelis (1995).

[3] The subscript refers to the period of the game.

[4] I assume for simplicity that the stock of land is fixed and landowners do not earn first period income.

income, **&** and ✓ respectively, subject to a budget constraint g = ✓k_2 + **&**l (expressed here in per capita terms). Individuals receive utility U in the second period.

$$U_2=(1-✓)k_2^i + (1-\&)l^i \ . \qquad (3)$$

Owners of capital can have an incentive to save, but whether they do so will depend upon the anticipated tax rate on capital income ✓^{c}. The incentives for policy makers with regard to tax policy depend in a very straightforward fashion on whether the members of their party own mostly land or mostly capital. This can be shown by substituting (1) and (2) into (3).

$$U_2=(1-✓)k_2 + (1-\&)l + e^i(✓-\&) \qquad (4)$$

Any policy maker with $e^p > 0$ (from a landowning party) who maximizes (4) subject to the budget constraint will choose a capital tax rate of g/k_2. Capital owners will anticipate this incentive of a landowner government to satisfy the budget constraint exclusively with capital taxation, and thus they will consume all of their capital in the first period as long as $(1- g/k_2)<1/(1+r)$. The end result is that in equilibrium everyone will be worse off, and the budget constraint will need to be satisfied exclusively with taxes on land income. In contrast, if the policy maker is from a party of capitalists ($e^p<0$), no credibility problem will exist with respect to capital taxation.

How would the above scenario be altered if taxes had to be agreed to by multiple policy makers, each of which had an effective veto over any policy change? A general prediction would require full knowledge of how party preferences are distributed in different countries and what status quo tax policies exist. Given our lack of full information about this issue, one possibility is to consider what predictions emerge if one assumes that each veto point in a country is controlled by a party with an exogenous endowment of land and capital e randomly drawn from the uniform distribution $[-c,c]$, while the status quo capital tax rate $\checkmark^s$ is also randomly drawn from a uniform distribution [0,1] with e and $\checkmark^s$ i.i.d.[5]

Under the above assumptions, credible commitment is achieved as long as the status quo capital tax rate $\checkmark^s$ satisfies the inequality (1- $\checkmark^s$)>1/(1+r), and as long as one veto point is controlled by a party with e^p<0. This holds regardless of which player has agenda setting power, given the previous definition of a veto player as someone whose agreement must be obtained in order to change policy. This leads to a first prediction that the greater the number of veto points in a country, the greater the likelihood of credible commitment, because the likelihood of a policy maker with e<0 controlling at least one veto point is increasing in the total number of veto points. On average, then, levels of capital investment should be higher in countries with multiple veto points.

We can also derive a second significant prediction from this exercise; the variance of levels of capital investment should be higher within the group of countries without multiple veto points. This is because the smaller the number of veto points in the system, the greater the likelihood of obtaining one of the two following outcomes: either all veto players have e^p>0 and will set the capital tax rate at g/k_2, or all veto players will have e^p<0 and will prefer to collect all revenue from taxes on land.

While the assumptions about veto player preferences and status quo policies here are made here for illustrative purposes, the two above predictions are also likely to hold under much less restrictive assumptions. This has significant implications for cross-country tests of the relationship between private investment and political institutions. Say the level of private investment Y is estimated as a function of a vector of economic controls X and a vector of variables measuring the number of veto points in a political system Z One should then expect to observe a particular form of heteroskedasticity in

[5] The status quo here being the rate which prevails if veto players are unable to agree on a change in policy.

the regression $Y = \alpha + \beta_1 X + \beta_2 Z + \varepsilon$. The variance of the error term Var[ε] should be negatively correlated with Z, reflecting the greater variability in levels of investment in countries without checks and balances.[6]

Relationship of this argument to broader debates on politics and growth

The issue of checks and balances and their impact on private investment is closely related to the broader debate on democratic institutions and economic performance. In terms of theory, if many researchers have emphasized that democratic rights might help promote economic growth, others have emphasized how broadened political participation might lead to a deterioration in economic performance.[7] Empirical studies have generally failed to provide robust evidence in favor of either a positive or negative association of democracy with growth.[8] One possible reason for this is that democracy influences growth through numerous different channels, and these effects may have opposite signs. For example, to the extent that democratic systems tend to have more veto points than authoritarian systems, then democracy may reduce uncertainty and raise private investment. However, it also might generate increased pressures for redistribution, thus lowering allocative efficiency.[9]

Alesina and Perotti (1994) suggest another important reason for these inconclusive findings; dictatorships are a very heterogeneous group. Some authoritarian governments, like that of Singapore, have pursued policies that promoted fast growth. Others, like Mobutu's Zaïre, have tended towards kleptocracy. Still others, like Suharto's Indonesia, have at first been seen as models of stability, and subsequently, as prime examples of authoritarian misrule. The argument presented in this paper suggests why

[6] This holds as long as higher levels of Z correspond to higher numbers of veto points. One way to deal with this heteroskedasticity might be to include some direct measure of the taxation preferences of different political parties or interest groups in different countries, but no suitable cross-country data set currently exists for this purpose.
[7] See for example Huntington (1968).
[8] See Barro (1994), Alesina and Perotti (1994), and Leblang (1997)

authoritarian regimes may be more heterogeneous in terms of performance; they can be characterized as political systems with a single veto point, and the preferences of the groups on whose authority authoritarian rulers depend for support are likely to vary across countries and over time.

In contrast to empirical studies on growth, there is a more statistically significant link between levels of private investment and overall levels of democracy. Serven (1997) finds that the Gastil index of civil liberties is significantly correlated with private investment in a panel data estimation which controls for other investment determinants. One reason for this result may be that focusing on private investment as dependent variable excludes some of the negative effects which democracy might have on economic performance via increased demands for redistribution.[10] As Serven himself notes, however, it is unclear exactly what phenomenon the Gastil indices are capturing.[11] This suggests a need for improved measures of political institutions. Second, even if civil liberties and private investment are significantly correlated in Serven's study, it remains possible that the effect of civil liberties varies substantially from country to country within his sample. Ideally, one would want to know if this heterogeneity reflects heterogeneity among dictatorships.

In addition to the finding that measures of democracy are correlated with levels of private investment, several cross-sectional studies have identified a link between private investment and the measures of institutional uncertainty developed by risk

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[9] Another avenue of inquiry is to investigate the relationship between political checks and balances and economic growth, as in Henisz (2000). See also Durham (1999).

[10] Focusing on private investment rather than overall investment is also preferable when considering the effect of uncertainty, because Aizenman and Marion (1996) have shown that in cases where high uncertainty leads to a decline in private investment, public investment often increases in compensation.

[11] In terms of measurement, since the Gastil index is subjective, and no methodology is publicly provided for its assessment, the index may actually be measuring the overall institutional environment in a country, rather than just political or civil rights. There may also be an endogeneity bias if assessors are influenced by recent economic performance in deciding to what extent political and civil liberties have been present. The Polity III database's measure of democracy is also significantly correlated with levels of private

assessment agencies. Brunetti and Weder (1999) and Poirson (1998) both find that indices of the rule of law, bureaucratic quality, and corruption are significantly correlated with levels of private investment, controlling for other determinants. These findings have made a significant contribution to the literature, but they have two shortcomings. First, indices such as the "rule of law" do not give any indication of which actual government institutions are associated with better provision of the rule of law. Second, like the Polity III and Gastil indices, measures of institutional quality may be subject to an endogeneity bias whereby their designers are influenced by overall economic performance in judging to what extent the rule of law, for example, is present. The political measures in this paper capture differences in actual political institutions while avoiding endogeneity bias by using objective formulae.

3. Data issues

The private investment dataset I use is an updated version of that used in Serven (1998) which calculates annual levels of new private investment in 74 developing countries. While the number of countries included is large, the dataset is unbalanced, with private investment figures for several countries only being available from the late 1980s (see annex table A3). The summary statistics in Table 1, below, present information on constant-price private investment as a ratio of GDP.

In order to control for determinants of private investment which are not related to political uncertainty, I follow existing studies by including several macroeconomic variables.[12] The annual growth rate of real GDP is included to capture the conventional accelerator effect of growth on investment. The standard deviation of the inflation rate is included, because variability of inflation creates uncertainty about the profitability of

investment. While, unlike the Gastil indices, the methodology for calculating the Polity III democracy index is made public, it remains subjective in its construction.

investment projects.[13] The level of private sector credit (as a share of GDP) should also be a determinant of private investment, and probably a more important one than the real interest rate in this sample, since many of the countries in the sample utilized direct instruments of monetary policy during the period considered. A measure which captures the income effects of terms of trade shocks is also included in the regressions to control for the possibility that these shocks will have a significant effect on investment decisions.[14] Finally, a variable which measures fuel & mineral exports as a share of total exports is included, based on the logic that other things being equal, countries with significant natural resource rents may receive higher levels of private investment.[15]

The principal goal of this paper is to examine the link between political institutions and private investment, and to do so I make use of two newly developed measures of political checks and balances: the "political constraints index" developed by Henisz (2000) and an index of checks and balances in the political system developed by Beck *et al.* (1999).

Henisz (2000) develops a method for measuring the extent of "political constraints" placed on a government's decision makers by drawing inferences from a spatial model of political choice. He incorporates information covering (1) the number of formal constitutional veto points in a political system (executive, number of houses of the legislature, federal sub-units, and judiciary), (2) whether these veto points are

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[12] When not otherwise specified, the source for all data is the World Bank's *World Development Indicators*.
[13] This is measured as the standard deviation of a country's inflation rate over the previous seven years. Similar results were obtained when using alternative periods for calculating inflation variability.
[14] The variable *terms of trade shock* measures the income effect as a percent of GDP of the change in a country's terms of trade in a given year.
[15] Other variables, such as the relative price of capital goods and the overall fiscal balance (after grants) should also logically be included as economic determinants of investment, but due to data limitations, inclusion of these two variables would have significantly reduced overall sample size. This would introduce a bias to the extent that the process for excluding observations was not a random one. As an alternative approach, I have chosen to use a procedure to impute missing values of these two variables. Results of investment regressions using these partially imputed variables were consistent with the results of regressions reported here. Similar problems with missing data arose with variables for real exchange rate instability and the level of public investment.

controlled by different parties, and (3) the cohesiveness of the majority which controls each veto point. The justification for this last criterion is the claim that an executive facing a legislature controlled by a coalition of opposition parties will be less constrained than one who faces a legislature where a single opposition party is in control.[16] As a proxy for the cohesiveness of legislative majorities, Henisz adjusts his index according to levels of party fractionalization. The Henisz data are available for the entire period of the private investment sample (1971-1994). I have used the version of Henisz's index (called *political constraints* here) which excludes data regarding federal sub-units (which may not have veto power over the policy decisions which matter to investors) and the judiciary.[17]

The second measure of checks and balances I use is the one developed by Beck *et al.* (1999) which is available beginning with the year 1975. Their key innovation is to provide direct evidence on the number of parties within legislative majorities, rather than inferring this from fractionalization data, as in Henisz (2000). Their index, referred to in this paper as *checks*, is a count of the number of veto players, based on whether the executive and legislative chamber(s) are controlled by different parties in presidential systems, and on the number of parties in government for parliamentary systems. Counting all of the parties in government in parliamentary systems derives from the idea that each party which is a member of a coalition government will have effective veto power over policy proposals. This is an assumption also discussed by Tsebelis (1995), but which may for some cases exaggerate the veto power enjoyed by smaller parties. The

[16] In contrast, if a legislative majority is politically aligned with the executive, then the executive will be more constrained in his/her actions when this majority is a coalition of several different parties as opposed to a single party.

[17] This is the index called POLCONIII in the Henisz dataset. I have excluded the judiciary, because no accurate cross-country data is available to determine when and where the judiciary acts as a veto player with respect to policies which matter for investors. In constructing an alternative index which includes the judiciary as a potential veto player, Henisz (2000) uses data from risk assessment agencies to judge whether the judiciary is an independent veto player, but as noted above, risk assessment agency data has several significant shortcomings. For a full description of the formula used to calculate Henisz's index see Henisz (2000).

index is then modified to take account of the fact that certain electoral rules will affect the cohesiveness of governing coalitions.[18]

One potential uncertainty with this index is whether it should be entered into an investment regression in linear form, or whether it should instead be transformed to reflect the fact that the effect of adding an additional veto point is likely to be non-linear (moving from 1 to 2 veto players will result in a greater change than moving from 4 to 5, for example).[19] Since this seems quite plausible, I have used a log version of the Beck *et al.* index in the regressions in sections 4 and 5.

A final group of regression variables is designed to capture the effect of political instability on investment. Non-constitutional transfers of executive power (*coups*) are particularly likely to increase uncertainty.[20] One reason for this is that, as Londregan and Poole (1990) have shown, experiencing one coup tends to increase the probability that a country will suffer subsequent coups. When it is feared that an extra-constitutional transfer of power might take place, the number of formal veto points in a political system becomes increasingly irrelevant.

While *constitutional* transfers of political power are likely to generate less turmoil than will coups d'état, they still may be associated with increased uncertainty about the future course of government policies to the extent that election winners are not known beforehand and to the extent that the preferences of future election winners are not well

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[18] This is based on the assumption that greater internal party cohesion in closed list systems will be synonymous with a lower level of checks and balances. The index is also adjusted downwards for countries where there are significant restrictions on electoral competition. When political competition is heavily restricted it seems less likely that veto players with heterogeneous preferences will hold office. For presidential systems, *checks* is the sum of 1 for the president and 1 for each legislative chamber. The value is modified upwards by 1 if an electoral competition index developed by Ferree, Singh, and Bates is greater than 4 (out of a possible 7). Also, in closed list systems where the president's party is the 1st government party, then the relevant legislative chambers are not counted. For parliamentary systems, *checks* is the sum of 1 for the prime minister and 1 for each party in the governing coalition. If elections are based on a closed list system and the prime minister's party is the 1st government party, then this sum is reduced by one. As for presidential systems, the value of *check* is modified upwards by 1 if value of the Ferree, Singh, and Bates index for electoral competition is greater than 4.

[19] This problem does not arise with the Henisz (2000) index.

known. I have included two separate variables to capture this effect, both of which are based on data collected by Henisz (2000). Constitutional changes in the executive are measured by the zero-one dummy: *executive turnover*. Constitutional changes in the legislature are measured by the variable *legislative turnover*, a continuous variable that measure the extent of legislature turnover in a given year.[21]

4. Pooled investment regressions

Table 2, below, presents results of several pooled investment regressions with *checks* and *political constraints* used as alternative measures of checks and balances. Regressions 1 and 2 were estimated using pooled OLS without controlling for unobserved country-specific effects. The coefficients on *checks* and *political constraints* are positive and significant in the case of the latter variable.[22] It would be unwise to draw inferences from these estimates, though. First, standard likelihood ratio tests suggest that groupwise heteroskedasticity is present.[23] Second, the theory reviewed in section 2 suggests that the variance of the residuals should also not be constant across different levels of checks and balances. Visual examination of the bivariate relationship between investment and *checks* suggests that this may in fact be the case (Figure I), and Breusch-Pagan tests for heteroskedasticity support the view that the conditional variance of private investment is negatively correlated with both *political constraints* and *checks*. This suggests that more efficient estimates could be obtained with a GLS model of the following form where, rather than assuming homoskedasticity of the residuals, the

[20] This variable is based on the Polity III data set.

[21] The formula for calculating this variable is $(\sum |S_2 - S_1|)/2$ where S_1 is a party's share of seats in the legislature in the previous year and S_2 is a party's share of seats in the legislature during the current year.

[22] Since I have used a dynamic panel specification here by including lagged private investment as a right-hand side variable, the distinction between the short-run and long-run effect of a change in *checks* and *political constraints* is relevant. The long-run coefficient for these variables is simply $B_{\text{checks}}/(1-B_{\text{lagged investment}})$.

[23] In both cases the null of homoscedasticity across groups was rejected at the p<.001 level. The test used was that presented in Greene (2000) p.511. While the use of heteroskedastic consistent standard errors can

variance of the residuals is estimated as a function of a set of country dummies and alternatively, *checks* or *political constraints*. The first equation below, then, is the specification for estimating the conditional mean and the second equation is the specification for estimating the conditional variance of private investment.

$$\text{Private investment} = \alpha_i + \beta_1\, inv_{t-1} + \beta_2\, growth + \beta_3\, credit + \beta_4\, sdinflation + \beta_5\, coup + \beta_6\, leg.\ turn. + \beta_7\, exec\ turn. + \beta_8\, res.\ exports + \beta_9\, terms\ of\ trade + \beta_{10}\, checks + \varepsilon \quad (5)$$

$$\text{Var}[\varepsilon] = \alpha_i + \beta_1\, checks \quad (6)$$

Regressions 3 and 4 present the results of the GLS maximum likelihood estimates.[24] When estimating the conditional mean (equation 5) I have constrained individual country intercepts to be equal, but have allowed individual country intercepts in the conditional variance estimates (equation 6), given that groupwise heteroskedasticity is present. Both short and long-run coefficients of *checks* and *political constraints* are now highly significant when estimating the conditional mean. In addition, both *checks and political constraints* are negatively correlated with the conditional variance of private investment (Table 1II), supporting the theoretical propositions from section 2, although in the case of *checks* the coefficient is not significant at conventional levels.

Results for regressions 3 and 4 also show that coefficients on the economic determinants of investment such as GDP growth, private sector credit, and terms of trade have the expected signs and the coefficients on GDP growth are highly significant. Results with regard to the political instability variables are less conclusive. While the coefficient for extra-constitutional changes in government (*coup*) has the expected negative sign, and it is statistically significant in regression 3, the coefficients for constitutional changes and parliamentary turnover are not significant in either regression.

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provide consistent standard error estimates, OLS may still be inefficient in the presence of groupwise heteroskedasticity.

[24] Results of two-step estimates of this procedure were nearly identical to the ML estimates. The method used was that first proposed by Harvey and presented in detail in Greene 2000, pp.514-522.

While the results of regressions (3) and (4) suggest that high checks and balances are, on average, associated with high levels of private investment, they may be biased by the failure to control for unobserved country effects. Standard F-tests show that when added to either regression 3 or regression 4 which estimate the conditional mean, a set of country dummies was jointly significant.[25] A major problem here, however, is that the fact that *checks* and *political constraints* are relatively time-invariant can make it difficult to establish firm inferences when allowing for unobserved country effects.[26] While the institutional measure, *checks*, does vary over time, there are thirteen countries in the sample for which the variable remains constant. In each case these are purely authoritarian systems where *checks*=1, making *checks* perfectly collinear with the country dummy in these cases. Likewise, there are 19 countries where the level of *political constraints* remains constant at 0. Even in those countries where levels of *checks* and *political constraints* have varied over time, changes have generally occurred with low frequency.

Regressions 5 and 6 repeat the GLS estimation procedure while allowing individual country intercepts in both the estimation of the conditional mean and the estimation of the conditional variance. In the estimates of the conditional mean, the coefficients for *checks* and *political constraints* remain positive, but neither the short-run nor the long-run coefficient are significant at conventional levels. This result should be taken less as a sign that checks and balances do not matter than as an indication of the difficulty in establishing to what extent they matter relative to unobserved country effects. The discussion of robustness issues in section 6 considers to what extent *checks* and *political constraints* remain significant after controlling for other slow-changing features of countries, such as levels of democracy and levels of GDP per capita. The coefficients

[25] P-values for the test that all country dummies were equal were $P<0.01$ in both cases.
[26] The same problem also applies to the conditional variance estimates.

are also considerably smaller in magnitude. However, it is interesting to note that in these regressions the conditional variance of private investment remains negatively correlated with the level of checks and balances, and the coefficients are significant in both cases.

The results of regressions 3-6 should also be interpreted in substantive terms. Table 4 shows in terms of percentages the estimated effect of a one standard deviation increase in *log checks* and *political constraints* on the level of private investment and on the conditional variance of private investment. For *log checks* a one standard deviation increase (+0.56) is roughly equivalent to a move from a system with only one veto point (*log checks* =0) to one with two veto points where each veto point is controlled by a separate party (*log checks* =0.69). For *political constraints* a one standard deviation increase (+0.19) would be equivalent to a move from a system with one veto point (*political constraints*=0) to one with two veto points: an executive and a legislature controlled by a different party from the executive and with a fractionalization index of 0.72.[27]

5. Quantile regression estimates

So far I have used GLS methods to reduce the effect of groupwise heteroskedasticity on my estimates and to simultaneously test the prediction that greater checks and balances imply both higher investment on average and a lower conditional variance of investment across countries and over time. This section examines the

[27] As an additional note, while the results from GLS estimates allow one to test propositions about both the conditional mean and the conditional variance of private investment, they may be biased to the extent they ignore parameter heterogeneity. Results of a Wald test suggest that the null hypothesis that the parameters for political institutions are constant across countries is in fact massively rejected. This is consistent with the theoretical discussion in section 2. In a dynamic panel data context, Robertson and Symons (1992) and Pesaran and Smith (1995) have shown that falsely assuming parameter homogeneity can bias estimates due to correlation of the error term with other right hand side variables. They propose when there is parameter heterogeneity across countries, consistent estimates of parameter averages can be estimated by taking mean values from individual country regressions. While this may generate consistent estimates, since there are relatively few observations per country in the panel used here (24 maximum), there is likely to be a massive loss of efficiency in performing country by country estimates, so dealing with this issue effectively may have to await the collection of longer time-series for private investment.

robustness of these results by using an alternative technique which does not impose a specific functional form on the variance of the error term.

Quantile regression as developed by Koenker and Bassett (1978) employs a least absolute deviation estimator which is often used to estimate a median regression line for the conditional distribution (rather than the mean line estimated by OLS). This estimation method can also be used to estimate percentiles of the conditional distribution other than the 50th. A test of the null hypothesis that slope parameters at different quantiles are equal can be used as a test for the presence of heteroskedasticity. Quantile regression can also be used to test specific propositions about the shape of the conditional distribution.

Most applications of quantile regression have been in microeconomics. In one example, Deaton (1996) has used quantile regression to show that while richer households in developing countries, on average, spend a smaller share of their income on food, it is also true that there is higher variance in the proportion of income spent on food among richer households. In other words, some rich households still spend significant shares of their income on food while others spend very little.[28]

The argument I have made about the effect of checks and balances can be tested in an analogous manner. If the variance of the conditional distribution of investment (conditional on checks and balances) is greater in countries with few checks and balances, then one should observe a pattern where, as the quantile one is estimating rises, the slope coefficients on *checks* and on *political constraints* should become less positive. This can be tested by estimating the same specification as used in the regressions in section 4, but at

[28] Deaton demonstrates this by showing that when income share spent on food (*YS*) is regressed on overall income (*Y*), while the coefficient on overall income (*Y*) is negative in a regression at the 10th percentile, in a regression at the 90th percentile it is actually positive. The fact that the estimated coefficient on *Y* is more positive at higher quantiles shows that there is greater variation in the conditional distribution (*YS* conditional on *Y*) at higher levels of overall income.

several different quantiles (such as the 25th, 50th, and 75th). Different coefficients are observed for each quantile and tested against the null that they are equal.

Table 5, below, reports results for coefficients on *checks* and *political constraints* from quantile regression estimates (complete results for these regressions are found in annex tables A1 and A2). As can be seen, both the short and long-run coefficients are more positive and more significant at lower quantiles. [29] Results of Wald tests provide further evidence against the null that the coefficients are equal in the case of *checks* although the null cannot be rejected in the case of *political constraints*

Table 6 repeats the exercise while including a set of dummies to control for unobserved country effects. Not surprisingly, given the correlation of the checks and balances measures with these country dummies, the magnitude of coefficients for *checks* and *political constraints* is considerably reduced, and they are no longer statistically significant at conventional levels. However, one still observes a pattern whereby the coefficients are less positive the higher the quantile estimated. Again in the case of *checks*, Wald tests favor rejecting the hypothesis that the coefficient at the 75th percentile is equal to the coefficient at the 25th percentile.

6. Robustness of the results

This final empirical section examines whether the above results may be influenced by (1) *checks* and *political constraints* proxying for more general features of

[29] Following other studies using quantile regression, I have used bootstrapped standard errors. This is necessitated by the fact that the density function for purposes of estimating the variance-covariance matrix is unknown. A common random number seed was used in all regressions in order to allow replication of the results. The procedure used for bootstrapping followed the standard approach of sampling from the joint distribution of observations. Buchinsky (1998) suggests that alternative methods of estimating bootstrapped standard errors are likely to be biased in this context.

countries such as the level of democracy or levels of income, (2) by the presence of outliers, and (3) by the endogeneity of the political system to levels of investment.[30]

With regard to the first of the above questions, checks and balances are not synonymous with democracy (witness countries like the UK which are democratic but have political institutions with few veto points). But checks and balances in general are likely to be highly correlated with other components of democracy, such as protection of human rights, which may themselves have a positive effect on levels of private investment. It is difficult to control for these other characteristics in my regressions, because panel data measures of other sub-components of democracy are not generally available.

Given this limitation, an alternative route is to include a more general measure of democracy, such as the Polity III democracy index (*democracy*), as a control in the Table 1I regressions (Gurr, Jaggers, and Moore 1998) When regressions 3 and 4 were re-estimated including this variable, the checks and balances measures remained significant while the indicator for democracy was not significant.[31] This result may be influenced by multicollinearity between the checks and balances measures and *democracy*, however, so I also conducted a non-nested test comparing the explanatory power of a model including checks and balances (but not *democracy*) and a model including *democracy* (but not checks).[32] This followed the J-test methodology for non-nested tests proposed by

[30] Another potential robustness issue involves autocorrelation. While autocorrelation is a common problem in panel data, use of a specification that includes a lagged dependent variable generally provides a means of reducing any serial correlation of error terms. Results of Lagrange multiplier tests adapted for panel data show that the null hypothesis of no serial correlation cannot be rejected in any of the results of the pooled regressions in Table 2. These tests were based on a variant of the Breusch-Godfrey test for serial correlation. In all cases p-values for the tests against the null of no serial correlation were P>0.99.

[31] Coefficients and standard errors for *checks* and *political constraints* were .025 (.009) and .061 (.029), respectively. Coefficients and standard errors for democracy were .002 (.0015) and .0001 (.001).

[32] The simple correlation between *democracy* and *political constraints* is 0.78. The simple correlation between *democracy* and *checks* is 0.45.

Davidson and MacKinnon (1981). These test results were consistent with the earlier regressions in favoring the checks and balances specification.[33]

The two variables *checks* and *political constraints* might also be proxying for overall levels of income, raising the question whether establishing checks and balances is likely to have as much of an impact on private investment in a poor country as in a rich country. While it is true as a general statement that countries with checks and balances in their political system tend to be wealthier than those without, this same conclusion does not apply as strongly in the data set which I have used for this study, because it is restricted to developing countries. In my developing country data set the correlation of *checks* and *political constraints* with real GDP per capita is positive but not extremely high (0.13 and 0.30 respectively). When I regressed each political variable on GDP per capita, and then used the residual from this auxiliary regression in specifications 3 and 4 from Table 1I (instead of *checks* and *political constraints*), the results remained robust.

A second robustness issue involves the possibility that the results reported above are influenced by outlying observations. To examine this possibility I re-estimated regressions 3 and 4 from Table 1I after excluding observations which might have a particularly large effect on the coefficients for *checks* and *political constraints* based on the dfbeta criterion. The results of these new estimates were similar to those in the original regressions.[34] Outliers should be less of a concern for the quantile regression results in

[33] These tests involved estimating the two alternative specifications and then re-estimating each specification while including the fitted values from the alternative model as a parameter. The *t*-statistic on the fitted values can be interpreted as a test of the null that the alternative specification *would not* add explanatory power to the existing model. In the case of the regression using *checks* and the regression using *political constraints* the null could not be rejected. In both alternative specifications using *democracy* the null was rejected.

[34] The dfbeta statistic measures the relative influence of each observation on the estimated coefficient of a particular variable, making it the most relevant test of outliers for the inferences in this paper. Following standard practice, observations with an absolute dfbeta value>2/sqrt(N) were dropped, where N is the number of observations. Coefficients and standard errors for *checks* and *political constraints* were 0.022 (0.0088) and 0.047 (0.021) respectively.

Tables 3 and 4, because these derive from a least absolute deviation (rather than least squares) estimator which should logically be less influenced by outliers.

A third robustness issue involve the possibility of joint endogeneity between levels of private investment and checks and balances. To the extent that levels of checks and balances derive from features of a country's political system which change only very infrequently (such as electoral rules), there may be less reason to believe they are endogenous to investment. On the other hand, one might observe a phenomenon whereby increased growth and investment prompt rulers to relax restrictions on political competition. This change would, in some countries, lead to the emergence of multiple political parties in government, and thus joint endogeneity would be a concern.

I considered the potential endogeneity of the checks and balances variables in my sample by first running Granger causality tests. An initial variant involved regressing each checks and balances variable on lagged values of itself and on lagged private investment. Individual regressions were estimated for each country. In only two countries for *checks* and three countries for *political constraints* was the coefficient on lagged investment significant at conventional levels. A second variant of these tests involved regressing each checks and balances variable on its own lag and on a variable measuring the net change in the level of private investment over the previous four years. This produced similar results. While the number of observations in each of these individual country regressions was not large, this is nonetheless fairly convincing evidence that, on average, neither *checks* nor *political constraints* is Granger-caused by investment.

As a further test of the potential endogeneity of checks and balances, I considered whether there might be a simultaneity bias to the extent that checks and balances and private investment might be jointly influenced by certain political and economic shocks. One way to deal with this issue is to repeat regressions 3 and 4 from

Table 1I while instrumenting for *checks* and *political constraints* with their respective lags. A Hausman specification test was then used to compare the OLS and IV estimates and so test for the consistency of the OLS estimates. In both the case of *political constraints* and *checks* the null of the consistency of the OLS estimates could not be rejected.[35] In sum, while there is no doubting the fact that political and economic variables are often jointly endogenous, in the case of political checks and balances, it appears feasible to consider them as being exogenous to levels of private investment.

8. Conclusion

There are a number of mechanisms which might aid governments in committing not to make opportunistic changes in taxes and regulations once private firms have made irreversible investments. This paper has investigated the extent to which checks and balances in government might facilitate credible commitment. I have argued that on average, checks and balances should be higher levels of private investment, but because they are not a necessary condition for credible commitment, we should also observe greater variability in levels of investment within the group of country-years where checks and balances are absent. Results from heteroskedastic regression and quantile regression estimates support both of these propositions.

[35] P-values in each case were P>0.99.

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Table 1: Summary Statistics

	Mean	Std. Dev.	Min.	Max.
Private investment%GDP	11.6	6.7	0.14	44.9
Lagged investment %GDP	11.6	6.7	0.14	44.0
GDP growth %	0.01	0.06	-0.48	0.27
Private sector credit %GDP	24.9	17.8	0.11	128.4
Inflation (stdev)	0.70	4.54	0.001	53.4
% oil & mineral exports	0.23	0.29	0	0.96
Coups	0.07	0.26	0	1
terms of trade shock	-0.02	0.09	-0.40	1.19
Legislative turnover	0.04	0.16	0	2.24
Executive turnover	0.11	0.31	0	1
Checks	2.43	1.43	1	16
Checks (log)	0.70	0.56	0	2.20
Political constraints	0.12	0.19	0	0.74

Table 2: Panel Estimates of Private Investment

Dependent variable: Private Investment	(1) OLS	(2) OLS	(3) GLS	(4) GLS	(5) GLS	(6) GLS
country dummies?	no	no	no	no	yes	yes
Checks (short-run coeff)	**.025**		**.027**		**.013**	
	(.017)		**(.009)**		**(.012)**	
Checks (long run coeff)	**.226**		**.271**		**.054**	
	(.144)		**(.087)**		**(.052)**	
Pol Constraints(short run)		**.098**		**.076**		**.023**
		(.031)		**(.023)**		**(.029)**
Pol Constraints (long run)		**.883**		**.778**		**.104**
		(.305)		**(.237)**		**(.134)**
Lagged investment	.889	.888	.896	.901	.744	.765
	(.021)	(.018)	(.012)	(.011)	(.020)	(.017)
GDP growth %	.859	.673	1.10	.978	1.07	.902
	(.207)	(.193)	(0.13)	(.122)	(.141)	(.125)
Private sector credit	.029	.027	.013	.011	.028	.023
	(.016)	(.016)	(.007)	(.006)	(.017)	(.013)
Inflation (stdev)	-.001	-.002	-.00006	-.0006	-.001	-.002
	(.003)	(.003)	(.002)	(.002)	(.002)	(.002)
Coups	-.058	-.058	-.044	-.026	-.068	-.047
	(.037)	(.034)	(.022)	(.021)	(.021)	(.019)
Legislative turnover	.062	.031	.018	.013	.019	.014
	(.031)	(.027)	(.014)	(.014)	(.016)	(.015)
Executive turnover	-.009	-.012	-.017	.015	.011	.015
	(.023)	(.022)	(.013)	(.012	(.013)	(.013)
% Oil and mineral exports	.002	.001	.006	.012		
	(.034)	(.031)	(.020)	(.020)		
Terms of trade shock	.201	-.037	.193	.031	.424	.270
	(.140)	(.106)	(.084)	(.077)	(.103)	(.081)
N=	1376	1575	1376	1575	1376	1575
R^2	.83	.83				
Log likelihood			175.09	210.04	249.00	287.38

All estimates use White's heteroskedastic consistent standard errors (in parentheses).

Table 3: Estimates of conditional variance of investment

Dependent variable: ln(var[¶])	(3)	(4)	(5)	(6)
country dummies?	yes	yes	yes	yes
Checks	-.159 (.119)		-.242 (.133)	
Political constraints		-.982 (.329)		-.910 (.358)

Standard errors in parentheses. Coefficients from country dummies not reported

Table 4: Estimated effect of one standard deviation increase (*checks* and *political constraints*)

	GLS		GLS individual country intercepts	
	▤investment	▤variance	▤investment	▤variance
Checks	**+16%**	**- 9%**	**+3%**	**- 13%**
Political constraints	**+16%**	**- 17%**	**+2%**	**- 16%**

Table 5: Quantile Regression Estimates
(without country dummies)

	Checks		Political constraints	
Percentile	**Short run**	**Long run**	**Short run**	**Long run**
25th	**.044** (.015)	**.800** (.355)	**.090** (.041)	**1.25** (.617)
50th	**.023** (.011)	**.295** (.144)	**.082** (.025)	**1.23** (.402)
75th	**.006** (.013)	**.044** (.102)	**.040** (.036)	**.305** (.269)
25th=75th	P=.03	P=.03	P=.24	P=.61

Bootstrapped standard errors in parentheses. (100 replications) *Checks* regressions, N=1376. *Political constraints* regressions, N=1575.

Table 6: Quantile Regression Estimates
(with country dummies)

	Checks		Political constraints	
Percentile	**Short run**	**Long run**	**Short run**	**Long run**
25th	**.037** (.023)	**.144** (.087)	**.086** (.051)	**.445** (.290)
50th	**.020** (.019)	**.082** (.079)	**.067** (.048)	**.279** (.208)
75th	**-.016** (.019)	**-.052** (.062)	**-.004** (.047)	**.015** (.179)
25th=75th	P=.02	P=.03	P=.14	P=.13

Bootstrapped standard errors in parentheses. (100 replications) *Checks regressions*, N=1376. *Political constraints* regressions, N=1575.

Figure 1: Private Investment and Checks and Balances

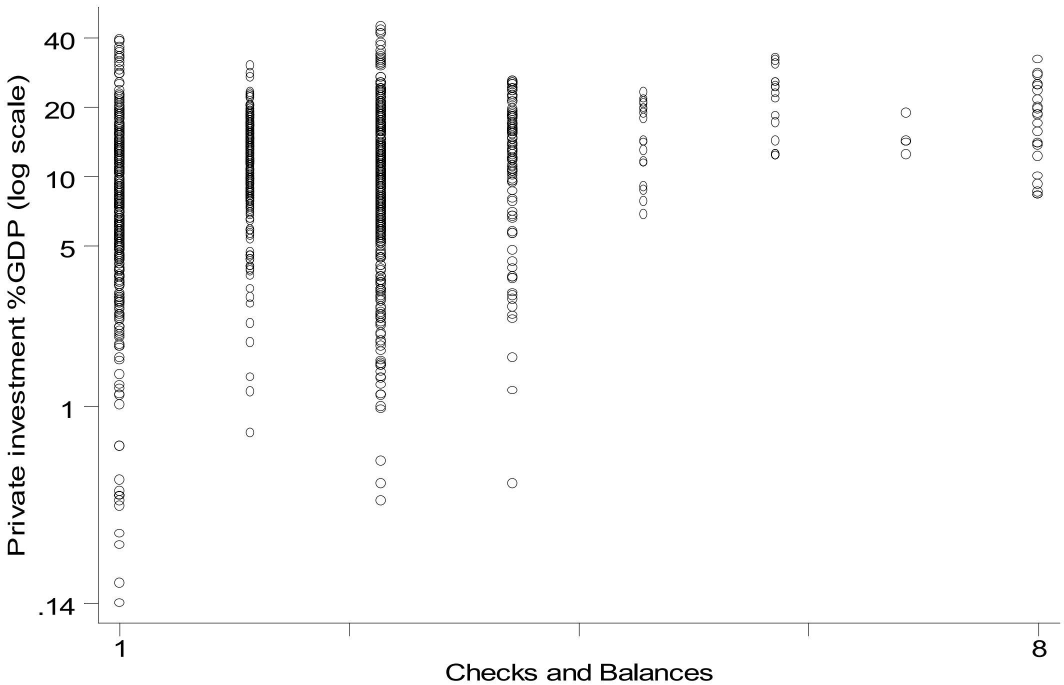

Table A1: Full results of quantile regressions (specification using checks)

	Percentile estimated		
	25th	50th	75th
Checks	.044	.024	.006
	(.015)	(.011)	(.014)
Lagged investment	.945	.919	.863
	(.020)	(.016)	(.020)
GDP growth %	1.15	.882	.750
	(0.22)	(.193)	(.173)
Private sector credit	.048	.008	.003
	(.016)	(.008)	(.011)
Inflation (stdev)	-.008	-.00007	.003
	(.004)	(.005)	(.004)
Coups	-.071	-.030	-.041
	(.046)	(.024)	(.025)
Legislative turnover	.020	.024	.017
	(.027)	(.031)	(.030)
Executive turnover	.024	.014	.007
	(.024)	(.013)	(.015)
% Oil and mineral exports	-.090	-.035	.042
	(.030)	(.024)	(.033)
Terms of trade shock	.057	.178	.273
	(.143)	(.123)	(.119)

Bootstrapped standard errors in parentheses. (100 replications) N=1376.

Table A2: Full results of quantile regressions (specification using political constraints)

Specification using Political constraints	Percentile estimated		
	25th	50th	75th
Political constraints	.090	.082	.040
	(.041)	(.025)	(.036)
Lagged investment	.929	.934	.869
	(.017)	(.015)	(.022)
GDP growth %	.922	.658	.587
	(.168)	(.185)	(.151)
Private sector credit	.048	.007	.003
	(.016)	(.009)	(.011)
Inflation (stdev)	-.008	-.0007	.003
	(.004)	(.004)	(.003)
Coups	-.052	-.019	-.038
	(.034)	(.022)	(.023)
Legislative turnover	.036	.013	-.010
	(.026)	(.024)	(.031)
Executive turnover	.030	.011	.003
	(.025)	(.013)	(.017)
% Oil and mineral exports	-.098	-.022	.044
	(.030)	(.022)	(.032)
Terms of trade shock	-.069	-.119	.147
	(.102)	(.123)	(.134)

Bootstrapped standard errors in parentheses. (100 replications) N=1575

Table A3: Country-years included in the private investment dataset

Argentina 1971-1994 Bangladesh 1976-1994 Benin1971-1994 Bolivia1971-1994 Brazil1971-1994 Burundi 1974-1994 Cameroon1971-1994 Central African Republic Chad 1975-1994 Chile1971-1994 China 1977-1994 Colombia1971-1994 Comoros 1982-1994 Congo, DR 1971-1992 Costa Rica1971-1994 Cote d'Ivoire1971-1994 Dominican Republic1971-1994 Ecuador1971-1994 Egypt 1976-1994 El Salvador1971-1994 Ethiopia 1985-1994 Gabon1971-1994 Gambia, The1971-1994 Ghana 1971-1990 Guatemala1971-1994 Guinea 1991-1994 Guinea-Bissau1971-1994 Guyana1971-1994 Haiti1971-1994 Honduras1971-1994 India1971-1994 Indonesia1971-1994 Iran 1979-1994 Jamaica1971-1994 Jordan 1984-1994 Kenya1971-1994 Lesotho 1973-1994 Liberia 1974-1987	Madagascar1971-1994 Malawi1971-1994 Malaysia1971-1994 Mali1971-1994 Mauritania 1981-1994 Mauritius 1977-1994 Mexico1971-1994 Nepal1971-1994 Nicaragua1971-1994 Niger1971-1994 Nigeria1971-1994 Oman 1972-1994 Pakistan1971-1994 Panama1971-1994 Papua New Guinea 1980-1994 Paraguay1971-1994 Peru1971-1994 Philippines1971-1994 Rwanda1971-1994 Senegal1971-1994 Sierra Leone1971-1994 Somalia1971-1994 South Africa1971-1994 Sri Lanka1971-1994 Sudan1971-1994 Swaziland 1973-1994 Syria1971-1994 Thailand1971-1994 Togo1971-1994 Trinidad&Tobago 1971-1994 Tunisia1971-1994 Turkey1971-1994 Uruguay1971-1994 Venezuela1971-1994 Zambia1971-1994 Zimbabwe 1975-1994