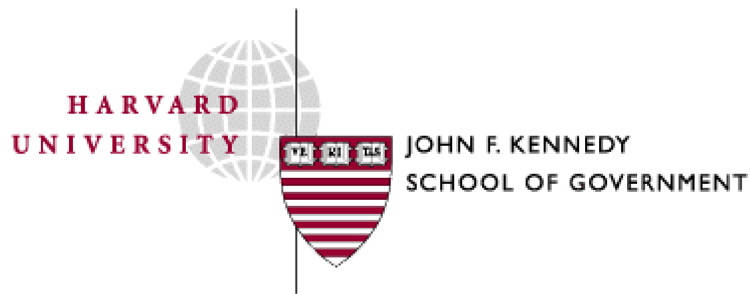

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On the Endogeneity of Exchange Rate Regimes

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On the Endogeneity of Exchange Rate Regimes

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ABSTRACT

The literature has identified three main approaches to account for the way exchange rate regimes are chosen: i) the optimal currency area theory; ii) the financial view, which highlights the consequences of international financial integration; and iii) the political view, which stresses the use of exchange rate anchors as credibility enhancers in politically challenged economies. Using de facto and de jure regime classifications, we test the empirical relevance of these approaches separately and jointly. We find overall empirical support for all of them, although the incidence of financial and political aspects varies substantially between industrial and non-industrial economies. Furthermore, we find that the link between de facto regimes and their underlying fundamentals has been surprisingly stable over the years, suggesting that the global trends often highlighted in the literature can be traced back to the evolution of their natural determinants, and that actual policies have been little influenced by the frequent twist and turns in the exchange rate regime debate.

JEL classification: F30; F33

Keywords: exchange rates, growth, impossible trinity, dollarization, capital flows

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I. Introduction

Much of the recent policy discussion on exchange rate regimes has tended to consider the regime choice as a policy decision largely independent from country-specific characteristics and the external environment[1]. After the early experiments with floats prompted by the collapse of Bretton Woods, we witnessed a regained popularity of pegs in the 80s and early 90s, to a large degree owing to their presumed beneficial effects on taming inflation. However, the stream of currency crises that started with the devaluation of the Mexican peso in 1994 cast doubt on their sustainability, and the ephemeral enthusiasm with "hard" pegs (particularly, currency boards) advocated by the bipolar view was debunked by the Argentine debacle. As a result, in recent years there has been a growing consensus in favor of flexible arrangements.

Yet such temporary fads, and the "one-size-fits-all" view of exchange rate arrangements that underlies them, seem at odds with both the casual evidence and the conventional wisdom that indicate that the regime choice is itself endogenous to the local and global economic contexts. This endogeneity of exchange rate regimes has not gone unnoticed in the economic literature. On the contrary, over the last forty years a large body of analytical work has provided key insights on the potential determinants of the regime choice.

This paper tests whether, and to what extent, the alternative approaches identified by the literature help explain the choice of exchange rate regimes. In addition, we explore whether the drivers underlying the choice of exchange rate regimes have changed over time, to assess to what extent, if any, the evolution in the intellectual debate on exchange rate arrangements influenced the policies actually chosen by policymakers.

[1] An exception is Frankel (1999).

Our results are quite revealing. We find that, once all contending hypotheses are considered jointly, the choice of exchange rate regimes can indeed be traced back to a few simple determinants that include a combination of trade, financial and political variables. Moreover, we find that the way countries choose their exchange rate regime in response to these basic determinants has not changed substantially over the last two decades, suggesting that the frequent twists and turns that characterized the exchange rate regime debate in academic and policy circles did not affect actual exchange rate policy to any significant degree.

To our knowledge, most of the empirical exploration of the determinants of the choice of regimes has been partial, focusing on a particular hypothesis without approaching the subject in a comprehensive model that encompasses all available candidate explanations –a concern given the frequently high correlation between the associated variables. Bayoumi and Eichengreen (1998), for example, center on the implications of the optimal currency area theory, Eichengreen et al (2002) on "original sin", and Stein and Frieden (2001) on political economy variables. The few attempts to tackle the issue from a broader perspective (for example, Edwards, 1996; Rizzo, 1998; and Juhn and Mauro, 2002) rendered disappointing results.[2]

In this light, the main contribution of this paper is twofold. First, it assesses the empirical support of alternative explanations based on an updated dataset, using by now standard de jure and de facto classifications of exchange rate regimes. Second, and more important, it nests the main theoretical views on the determinants of exchange rate

[2] See also Poirson (2001), and Collins (1996), for a sample of Latin American countries. In related work, Alesina and Wagner (2006) focus on the decision to renege on the announced regime (as captured by the difference between de jure and de facto regimes). The significant exception to these disappointing results has been the literature on the trade effect of currency unions started by Rose (2000). While not framed in terms of the choice of exchange rate regime, the estimated effects were so large that they would certainly have an impact on the choice of regime. See also Frankel and Rose (1997, 2002) and Frankel (2005) for a survey.

regimes in a common framework that allows us to test them jointly, unveiling the relative relevance of each one of them. These two contributions provide the basis for what we believe is a better and more comprehensive test of forty years of literature on exchange rate regimes.

More precisely, we simultaneously test what we believe are the three main competing approaches to explaining the choice of exchange rate regimes: i) the optimal currency area (OCA) theory pioneered by Mundell (1961), which relates the choice of regime to the country's trade links, size, openness and the characteristics of the shocks the economy is subject to;[3] ii) the financial view, which highlights the consequences of international financial integration;[4] and iii) and the political view, which regards the use of a peg (or, more generally, an exchange rate anchor) as a "policy crutch" for governments lacking (nominal and institutional) credibility.[5]

Our overall results using de facto regimes provide strong support for each of these views, although some of them have applied differently for industrial and non-industrial economies. In particular, we find that the implications of OCA theory carry through in both groups of countries. By contrast, whereas financial integration tends to foster flexible regimes among industrialized countries –in line with the impossible trinity view–, it increases the propensity to peg among non-industrial countries –something that we attribute to the fact that integration in those countries is strongly correlated with foreign currency-denominated external liabilities and larger currency mismatches, as documented by Eichengreen et al. (2003). We also find support for the political view, albeit in a qualified version: pegs are more likely if the country lacks a good institutional

[3] While this tradeoff is often associated with the traditional Mundell-Fleming framework, the view of exchange rates as real shock absorbers dates back to the work of Meade (1950).
[4] This approach comprises the impossible trinity argument that stresses the role of increased capital mobility as a factor limiting the effectiveness of pegs (see, i.e., Rose, 1994; Fischer, 2001; and Obstfeld and Taylor, 2002), and the currency mismatch argument that claims that exchange rate variability in financially dollarized emerging

track record, but less likely if the government is too weak to sustain them. Specifically, we find the choice of a peg to be negatively correlated with institutional quality –a result consistent with the policy crutch view–, but positively correlated with political strength –hinting at a sustainability problem. Indeed, we find that, although non-peg countries are more likely to adopt de jure (but not a de facto) peg in an inflationary context, most of these inflation-induced de jure pegs are ultimately short lived.

Finally, by recovering the time dummies estimated in our baseline specification, we can study whether the evolution of regimes over the last decades display any particular time pattern beyond and above that spanned by our set of basic controls. The estimation based on the IMF's de jure classification reveals a clear trend throughout the 80s and 90s: a strong "peg bias" in the early years that narrows steadily to virtually disappear by the end of the period. However, when we repeat the exercise based on de facto regimes, we find no discernible time pattern. From these findings, we conclude: i) that the trends often highlighted in the recent exchange rate regime debate, visible in the evolution of de jure regimes, are not reflected in de facto exchange rate policies, and ii) that actual regime choices can be traced back to the evolution of regime determinants rather than to a changing view of the relative merits of different arrangements: if anything, the debate can be credited for the increasing reluctance to adopt explicit exchange rate commitments at odds with underlying fundamentals.

II. The Theories of Exchange Rate Regimes Determination

Our exploration of the determinants of exchange rate regimes is centered on three main approaches that have long been part of the open economy macroeconomics toolkit: the

economies is contained due to their deleterious balance sheet effects (see, i.a., Calvo and Reinhart , 2002, and Eichengreen and Hausmann, 1999).

[5] See Drazen (2000) and references there.

theory of optimal currency areas (OCA), the financial integration approach (specifically, the incidence of the impossible trinity and balance sheet effects), and the political economy view of pegs as credibility enhancers.

In each case, a key aspect of the exercise consists in finding variables that capture, as close as possible, the factors highlighted by the theory. Since the choice of particular variables is bound to be controversial, we first evaluate a number of alternative controls for each of the three approaches separately. To do so, we run pooled logit regressions for an unbalanced panel data set of 183 countries over the post-Bretton Woods period (1974-1999) on each set of controls.[6] We then run a parsimonious specification that includes all controls selected in the partial tests, to assess the relative importance of each set of explanations.

As noted, an important novelty of this paper lies in its focus on de facto exchange rate regimes. More precisely, our empirical exercises use as dependent variable a peg dummy that takes a value of one if a country is classified as a de facto fixed exchange rate regime (and a value of zero otherwise) according to the de facto regime classification assembled by Levy Yeyati and Sturzenegger (2003, 2005).[7]

OCA theory

The first group of factors potentially underpinning the choice of regime is related to the geographical and trade aspects identified by the theory of optimal currency areas. This approach to the fix vs. float dilemma weighs the trade and welfare gains from a stable exchange rate vis à vis the rest of the world (or, more precisely, the country's main trade

[6] Appendix 1 lists the countries in our sample, grouped into the industrial and non-industrial categories.
[7] The robustness of the results to the standard IMF-based de jure classification, as well as Reinhart and Rogoff´s (2004) alternative de facto classification is addressed below.

partners) against the benefits of exchange rate flexibility as a shock adjuster in the presence of nominal rigidities.

According to this argument, country characteristics that favor a more stable (or fixed) exchange rate are openness (which enhances the trade gains derived from stable bilateral exchange rates)[8], smallness (indirectly through the higher propensity of small economies to trade internationally, and directly by limiting the scope for the use of a national unit of account), and the concentration of the country's trade with the peg currency country (which, again, increases the gains from reducing the bilateral exchange rate volatility). [9]

Regarding the incidence of real shocks, the traditional Mundell-Flemming framework argues that, in order to minimize output fluctuations, fixed (flexible) exchange rates are to be preferred if nominal (real) shocks are the main source of disturbance in the economy.[10] For example, high volatility of terms of trade would provide a rationale for a float. Moreover, to the extent that real shocks become increasingly important due to growing trade flows and capital market integration (alternatively, as monetary shocks or inflation concerns become less of a priority) one should expect to see a global trend towards more flexible arrangements.[11]

In order to test the optimal currency area hypothesis we use measures of size (the logarithm of the country's GDP in US dollars, Size), openness (the GDP share of the

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[8] An alternative explanation relies on Cavallo and Frankel (2004) who show that open economies are less prone to a sudden stop, thus reducing the benefits of flexibility.

[9] In the case of pegs to a basket of currencies, the reference currency is the main currency in the basket peg. For intermediate and floating regimes, the reference currency is assumed to be the one that exhibits the least volatility relative to the local currency among major international currencies and currencies of the main regional partners. See Levy Yeyati and Sturzenegger (2005) for details.

[10] Classic references on this are Fleming (1962) and Mundell (1963).

[11] Eichengreen et al (2002), Lane (1995), and Frieden et al (2000) provide empirical evidence suggesting that, contrary to the nominal-real story, terms of trade variability is positively related with the probability that a country selects a peg. Eichengreen et al (2002) propose the following explanation: "fixed exchange rate regimes should result in deeper financial markets, which should be particularly important in economies facing important terms of trade shocks". However, two recent papers by Broda (2004) and Edwards and Levy-Yeyati (2005) using de facto classifications of exchange rate regimes provide empirical evidence that pegs indeed exhibit larger output sensitivity to real shocks.

average of exports plus imports, Openness),[12] geographical concentration of trade (the share of exports to the reference currency country multiplied by openness, TradeConc), and, to capture the incidence of real shocks, terms of trade volatility (computed as the standard deviation of terms of trade changes over the previous five years weighted by the degree of openness, TOTShocks). We use lagged values (indicated by placing a 1 after the variable) where we believe endogeneity may be a concern.[13] All regressions in the paper include year dummies.

Table 1 reports the results from our partial test of the OCA approach. The expected sign is shown in the first column. As can be seen, the theory is strongly supported by the data. For non-industrial countries (column ii), all coefficients show the correct sign and are highly significant. For industrial countries the results remains virtually unchanged, with the exception of the trade concentration variable, which is statistically significant but of the wrong sign (column iii). However, closer examination reveals that this result is driven mostly by one significant outlier, Canada, which exhibits very concentrated trade (with the US) and yet runs a floating exchange rate regime. In fact, when we exclude Canada from the sample (column iv), the coefficient for trade concentration, while significant only at 14%, shows a positive coefficient.

The financial view

A key ingredient of the textbook Mundell-Fleming framework is the assumption of perfect capital mobility that implies international interest rate arbitrage across countries in the form of the uncovered interest parity. From this framework, it follows that monetary policies in open economies cannot be aimed both at maintaining stable

[12] The use of Frankel and Romer´s (1999) measure of openness to mitigate endogeneity problems yields similar results at the cost of fewer observations. We come back to the issue of endogeneity below.
[13] Appendix 2 presents a list of variables and sources, as well as a Table with summary descriptive statistics.

exchange rates and smoothing out cyclical output fluctuations due to real shocks. This is usually referred to as the "impossible trinity," namely, the fact that policymakers can choose at most two out the three vortexes of the trinity (capital mobility, monetary policy and a fixed exchange rate).

Obstfeld and Taylor (2002) link the evolution of exchange rate arrangements to the historical phases of financial globalization, based on this "impossible trinity" argument. They argue that, while capital mobility prevailed at a time when monetary policy was subordinated to exchange rate stability (as in the gold standard), as soon as countries attempted to use monetary policy to revive their economies in the interwar period, they had to impose controls to curtail capital movements.[14] Inverting their argument, it has been argued that, as financial globalization deepened in the last decades, monetary policy became increasingly at odds with fixed exchange rates. This argument underscores the so-called "bipolar view" of exchange rate regimes, according to which increased capital mobility has made intermediate regimes less viable in (financially open) industrial and emerging economies.[15] In addition, a rapid process of financial deepening and innovation (which typically has advanced pari passu with financial integration in international capital markets) has gradually reduced the effectiveness of capital controls, with the same consequences in terms of the monetary policy-exchange rate stability dilemma.

Recent literature has stressed that currency mismatches in financially dollarized economies may also be critical to the choice of exchange rate regimes. In particular, countries with important (private or public) foreign liabilities may be more prone to fix (either de jure or de facto) due to the deleterious impact of sharp nominal depreciation of

[14] On the same point, see Bordo and Flandreau (2001).
[15] See, e.g., Fischer (2001). The point had been raised earlier by Quirk (1994) and Eichengreen (1994), among others.

the currency on the solvency of balance sheets with currency mismatches.[16] Notice that this channel, if present, may undo the positive relationship between capital account openness and flexible regimes suggested by the impossible trinity argument. To the extent that financial openness induces large swings in capital flows that in turn lead to large changes in the value of the exchange rate, financially dollarized countries may find it more convenient to fix rather than float merely for prudential reasons, a phenomenon that has been dubbed "fear of floating" by Calvo and Reinhart (2002). As the authors argue, "defaults and general debt servicing difficulties mount if the exchange rate is allowed to slide," which "may help explain why, at least historically, there has been a marked tendency in most countries to confine exchange rate movements to relatively narrow bands..."

Thus, disentangling empirically these two countervailing aspects –impossible trinity and currency mismatches– is particularly challenging in the case of non-industrial economies, many of which tend to exhibit important levels of financial dollarization. Moreover, in most cases, financial integration is likely to be correlated with larger stocks of foreign liabilities denominated in a foreign currency, rendering balance sheet considerations relatively more pressing. In this regard, it is important to note that, as holders of foreign assets and liabilities differ (at least from a legal point of view), a sudden devaluation is likely to hurt dollar debtors irrespective of the amount of foreign assets owned by the country's residents; hence, our focus on gross (as opposed to net) foreign liabilities as a measure of the currency mismatch.[17]

[16] See, e.g., Eichengreen and Hausmann (2003). It has to be noted that, while the real exchange rate adjustment in the event of a negative external shock cannot be avoided by sustaining a peg, the downward rigidity of prices may postpone the process over time, preventing a financial collapse. In addition, a nominal adjustment of the exchange rate is usually accompanied by an exchange rate overshooting that can only reinforce the negative financial implications.

[17] See Levy Yeyati (2004) for a discussion along these lines.

With this in mind, we examine the following five alternative variables to capture the influence of financial linkages on the choice of regime:

i) Chinn and Ito´s (2002) measure of de jure capital account openness (KAOpen). The measure, available for 105 countries since 1977, is based on four binary dummy variables reported in the IMF´s Annual Report on Exchange Rates and Exchange Restrictions with a higher number indicating a lower overall level of restrictions.[18]

ii) The sum of the absolute value of inward and outward flows of portfolio investments and financial derivatives (sourced from the International Financial Statistics) as a share of GDP (Portfolio), a measure of de facto capital account openness.[19]

iii) A financial development dummy (FinDev), equal to one when the country belongs to the industrial group or when it is included in the JP Morgan's EMBI Global index.[20]

iv) The country's gross stock of foreign assets over GDP (CumLoans), an alternative measure of de facto capital account openness, where the asset stock (computed by Lane and Milesi-Ferreti, 2001) is measured as the cumulative flows of portfolio debt assets, other assets and net errors and omissions.

v) The ratio of foreign liabilities in the domestic financial sector relative to money stocks (FLM), a measure of liability dollarization to proxy for the presence of currency mismatches.[21]

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[18] Kaminsky and Schmukler's (2001) capital controls index, an alternative candidate, failed to be significantly correlated with the regime choice, possibly due to the fact that it covers only 28 countries.

[19] This is a modified version of Juhn and Mauro's (2002) portfolio openness measure, computed as the sum of the absolute value of the change in financial assets and liabilities, which includes (longer term) FDI and (presumably countercyclical) official flows. The latter are arguably less relevant to the hypotheses that we want to test and are therefore excluded from our measure. The distinction between de jure and de facto capital account openness (more precisely, between restrictions on capital flows and realized capital flows) is highlighted by Prassad et al. (2003).

[20] The use of the EMBI index as an indicator of financial development, also borrowed from Mauro and Juhn (2002), is motivated by the fact that non-industrial countries make it to the EMBI Global portfolio if their (typically external) sovereign bond issues have sufficient liquidity (JPMorgan, 1999). Furthermore, EMBI indexing tends to

Table 2 shows the correlations between these different measures of financial linkages. The first four are all strongly (positively) correlated, and virtually uncorrelated with our measure of liability dollarization, with the exception of the stock of foreign assets. As the latter is available only for a limited set of countries and periods, we drop it from our empirical tests below.[22] Since FinDev may be correlated with the size of the economy for the non-industrial sample due to the minimum trading volume required to be in the EMBI index, we include size as an additional control in the regressions to partial out this effect.

Table 3 shows the results of our partial test of the financial linkages approach, for the whole sample as well as for the industrial and non-industrial subsamples. As before, our dependent variable is the de facto peg dummy. According to the currency mismatch hypothesis, we should expect higher liability dollarization to be positively associated with the propensity to peg, something that bears out in the two groups as identified by the positive and significant coefficient of the foreign liability variable. The capital integration variables, on the other hand, exhibit strikingly different results for each of the two groups of countries. For the non-industrial sample (column ii), the coefficients are positive and significant, suggesting that the implications of impossible trinity should be qualified by the presence of currency mismatches that appear to prevail in the choice of exchange rate policy. By contrast, for (non-financially dollarized) industrial economies with limited or no currency mismatches (column iii), the regime choice is consistent with the impossible trinity argument, as indicated by the negative correlation between capital openness and the propensity to peg. Notice that these results are also consistent with Aghion et al (2006) who show that exchange rate volatility enhances growth for financially developed

fuel international investors interest and, through this channel, helps strengthening the country's financial links with international markets.

[21] Alternative candidates such as Eichengreen et al.´s (2002) "ability to pay" measures and Levy Yeyati's (2004) deposit dollarization ratios are available only for a limited number of countries or for recent years.

[22] Its inclusion yields results that are comparable with those for the other capital account openness controls, at the expense of an important reduction in the size of the sample.

economies. While we have not cut our sample on the basis of financial development, to the extent that developed economies are more developed financially, the fact that portfolio flows or capital account openness leads to floating regimes for developed economies but not so for less developed economies may be signaling the different impact of exchange rate volatility for the two groups.

The political economy view

A large strand of literature has studied the use of the exchange rate as a nominal anchor to reduce inflation. In particular, it has been argued that governments with a preference for low inflation but handicapped by low institutional credibility, facing the uphill task of convincing the public of their commitment to nominal stability, may adopt a peg as a "policy crutch" to tame inflationary expectations. It follows that countries with a poor institutional track record may be more prone to rely on fixed exchange rate arrangements as a second best solution to a commitment problem. As the argument goes, weak governments that are more vulnerable to "expansionary pressures" or "fiscal voracity" (i.e., pressures from interest groups with the power to extract fiscal transfers) may choose to use a peg as a way of fending off these pressures. [23]

To be sure, the literature does not provide an unambiguous answer regarding the sign of the link between political strength and exchange rate regimes. Indeed, the policy crutch effect can be easily reversed: weak governments could be associated with larger deficits (or lower ability to reduce them, if needed) that makes the peg more difficult to sustain. This is particularly true in the presence of wars or social unrest, but could be extended to episodes of political turmoil or even to tranquil times to the extent that anemic governments become more vulnerable to the political pressure from interest groups.

More in general, a “sustainability hypothesis” that links weak governments with either the collapse of existing pegs or the inability to launch a credible one would entail a positive correlation between political strength and pegs, rather than the other way around as the policy crutch argument would imply.

Testing these views, in turn, is challenging due to the difficulty of capturing in a single, or even a few observable variables the concepts of credibility and sustainability. Thus, we adopt a fairly candid view to examine a diverse set of variables used in the economic and political science literature to reflect political and institutional characteristics, and to assess whether they could be interpreted as indicators of political strength. The first three variables, taken from the World Bank’s Database of Political Institutions (V.2.0), include the number of years that the incumbent administration has been in office (YearsinOffice), a Herfindahl index of congressional politics (Herfindahl), and a legislative index of electoral competitiveness (LegComp).

Since our data covers all countries in the world, long tenures may indicate both relatively successful governments, as well as long-lasting (possibly totalitarian) regimes with a high degree of control of the local political process. If so, years in office would represents a measure of government strength, possibly subject to a diminishing effect as the clout of longer governments eventually wears out. The Herfindahl index is defined as the sum of the squared seat-shares of all parties in the government. As discussed early on by Olson (1982, 1993) the atomization of political players is associated with the deepening of common pool problems, leading to greater incentives to extract from the common resources and mounting fiscal pressure. Thus, a larger value of the Herfindahl index would be associated with political strength. Finally, the legislative index of electoral competitiveness increases as the legislatures become more competitive. Ranging from a

[23] See Drazen (2000), for an extensive review, including a careful discussion of Giavazzi and Pagano (1988) where

value of 1, when there is no legislature, to a value of 7 when the largest party in congress holds less than 75% of the seats, this index would be correlated with political weakness.

We also test a fourth political variable, namely, the number of veto points in the political system (VetoPoints) as reported in Henisz´s Polcon Database (2002), which measures directly the difficulties or steps required by a government to push its agenda. Accordingly, we expect it to represent a measure of government weakness.

Finally, we examine two alternative institutional quality indicators. First, we use the operations risk index (OperationsRisk), a survey that gauges the domestic environment for the operation of foreign businesses assembled for 53 countries by Business Environment Risk Intelligence S.A, where a higher value indicates a better environment. Second, we evaluate the World Bank's Country Policy and Institutional Assessment Rating System (CPIA). These ratings are based on the assessment of each country's governance as well as its economic, structural, social, and public reform policies prepared by the World Bank's country economists; again, a higher value would indicate higher institutional quality.[24] In both cases, we would expect to observe a negative link between the quality of institutions and the propensity to peg.

The correlations matrix reported in Table 4 confirms our priors concerning the political variables: measures of political strength (Herfindahl and years in office) and political weakness (veto points and legislative competitiveness) are positively correlated with each other, and negatively correlated with the other group. In addition, the two institutional variables (operation risk and the CPIA) are also strongly correlated with each other, as expected.

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the idea was first developed. On the fiscal voracity effect, see Tornell and Lane (1999).

[24] See http://info.worldbank.org/governance/kkz2002/notes.html for a more detailed description. These ratings, prepared for a broad set of developing economies, cover the longer period (1977-1999) required by our empirical tests, unlike other governance indicators assembled by The World Bank that are available only since the mid-90s.

This allows us to build a parsimonious specification that avoids excessive multicollinearity for the partial tests of the political approach. More precisely, we select three variables that capture, political strength (YearsinOffice, which is also included squared to test for the potential diminishing effect of long tenures noted above), political weakness (VetoPoints), and institutional quality (CPIA, available for the non-industrial sample).[25] For the latter, the absence of an institutional control for the industrial sample should not be a concern to the extent that it is reasonable to assume that the differential influence of institutional quality on the choice of regime among developed countries, with comparably high standards, should be, at best, very minor.

As the regressions results show (Table 5), the propensity to peg is negatively correlated with the quality of institutions, as the policy crutch view would indicate. However, the results also show that pegs are positively related to political strength, in line with the sustainability hypothesis: weak governments appear to be less prone to implement (and sustain) pegs, although the result appears to be statistically significant only for developing economies.

How can we reconcile this second finding with the long-dated debate about the use of pegs as nominal anchors in inflationary economies –and the many historical experiences in this direction? To address this issue, it is useful to focus more narrowly on the incidence of inflation on the decision to move to a peg from a non-peg regime. Following Vegh (1992), Calvo and Vegh (1999) and Frieden et al (2000), it could be argued that countries with moderate to high inflation have incentives to use the exchange rate as an

[25] We drop Herfindahl due to relatively fewer observations, Operations Risk because of its very limited coverage, and Leg.Comp because, as opposed to VetoPoints, it has the same value for all industrial countries in our sample. The inclusion of the whole set of variables yields comparable results at the cost of severely limiting sample size.

anchor.[26] However, persistent high inflation also creates pressures on the exchange rate market that may force monetary authorities to float (either voluntarily or as a consequence of a currency crisis). Thus, the correlation between inflation and exchange rate rigidity may reflect both credibility and sustainability aspects. To disentangle these two effects, and bearing in mind that most exchange rate-based stabilizations in the past were preceded by hyperinflations bursts, it is useful to differentiate moderate from high inflation (and hyperinflation) episodes known to elicit rapid policy reactions.

To that end, in the last two columns of Table 5, we restrict our sample to countries that in the preceding period are not classified as pegs, and test the effect associated to a dummy for high inflation (High250) that is equal to one whenever the inflation rate in the previous year exceeds 250%, which we believe reflects more accurately the need for a quick credibility enhancement. Using a de facto classification, the results indicate that the probability of adopting a peg is not significantly higher in countries coming from high inflation (column iv).

One could argue, however, that the use of an exchange rate anchor is intimately related to an explicit commitment to a peg. If so, we would expect inflation-challenged governments to peg de jure (rather than de facto). This is indeed what the evidence seems to indicate (column v). When we use the de jure regime classification, the high inflation dummy becomes significant and with the expected positive sign: high inflation tends to induce the de jure adoption of an exchange rate anchor.[27]

These results can be illustrated by a cursory look at the data. Out of the 37 non-peg high inflation country year observations included in the regression, 16 moved to a de jure peg

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[26] There is empirical evidence that (long-lasting) pegs have been successful at reducing inflation. See, among others, Ghosh et al. (1997) and Levy Yeyati and Sturzenegger (2001).

the following year.[28] But of these, only 3 qualify as de facto pegs. This suggests that, whereas weak governments tend to implement explicit (de jure) exchange rate commitments as a policy crutch against inflation, these attempts tend to be short-lived as the peg becomes ultimately unsustainable.

Thus, our preliminary findings provide a nuanced support for the policy crutch approach. On the one hand, institutional quality is inversely related to the propensity to peg, as countries rely on an exchange rate anchor to compensate for low institutional credibility. On the other, the fact that weak governments are likely to exhibit more flexible exchange rate arrangements points at sustainability as the main driving aspect. Again, this does not contradict the view of pegs as policy crutches: it provides evidence that hard-pressed governments are prone to resort to a jure peg as a deflationary device, despite the fact that they are later on unable to sustain it.

III. Putting it all together

The previous section showed that the factors identified by the literature, taken separately, exhibit significant links to the choice of exchange rate regimes. We are ready to tackle the main objective of the paper, namely, to test the alternative hypotheses simultaneously in a framework that allows us to assess their relative importance –a crucial step given the fact that the three groups of controls are likely to be correlated with each other, potentially biasing the result of our partial tests.

[27] The choice of 250% is not arbitrary. Lower levels did not deliver significant results, indicating that the credibility effect at lower inflation rates is not sufficient to induce the choice of a peg.
[28]Note that this amounts to 43% of the high inflation observations, significantly above the 25% probability of switching from non-peg to peg for the whole sample.

To ensure consistency and comparability of the results, we put the three approaches to test by pooling the selected controls in a baseline specification and testing them jointly.[29] However, since our primary interest is to examine the relevance of different analytical approaches to the regime choice problem rather than the significance of individual variables we also report joint significance tests for each group of variables.

The results of our baseline regression, shown in Table 6, are in line with our previous findings. As can be seen, the estimated coefficients for the whole sample (column i) reflect closely those for the non-industrial group (column ii). The propensity to peg is higher in small open economies with high levels of foreign liabilities and financial integration, poor institutional quality and strong governments. At the bottom of the table we report the Wald test of joint significance for all the coefficients corresponding to each approach –all of them strongly statistically significant.

Column (iii) replicates the baseline for industrial countries. OCA variables remain strong, or even stronger, predictors of exchange rate choice. However, capital account openness controls now show negative coefficients –like liability dollarization, which is no longer significant– supporting the impossible trinity hypothesis. Moreover, the coefficients of political variables display a less clear pattern. These results are, again, in line with our partial tests, suggesting that exchange rate policy in industrial countries, relatively free from political or financial constraints, enjoys a higher degree of independence and respond essentially to standard economic factors.

Overall, the model displays good levels of predictive accuracy. For the non-industrial sample, the baseline specification correctly identifies 74% of pegs and 78% of non-pegs,

[29] Due to its more limited sample coverage and its strong correlation with openness, we dropped the trade concentration variable from our baseline specification. Results for the complete specification are similar and available upon request.

showing significant in-sample predictive power. The numbers are even better for the industrial sample: 89% of pegs and 90% of non-pegs are correctly identified.

For non-industrial sample, the predictive power is also quite good out of sample. The predictions for year 1999, based on the baseline estimated for the period 1974-1998, indicate that the model has a success of about 76%, correctly predicting 34 cases out of a total of 45.

Interestingly, a similar exercise for industrial countries predicts unambiguously a floating rate, despite the fact that most European countries in the sample are classified as de facto pegs, indicating that the rationale for the exchange rate constraints imposed by the convergence to the Euro may go beyond the natural determinants captured by the present model.

What are the economic relevance of these results? Figure 1 addresses this point by computing the change in the estimated probability of choosing a peg as we span the support of each explanatory variable (while others are kept fixed at their mean values). To visualize the relative significance of each variable, we set the vertical axis to the [0,1] range for all variables. The exercise is conducted for non-industrial and industrial countries (based on the estimates reported in Table 6, col ii and iii, respectively) to highlight the differences between the two groups.

The results are illuminating. As can be inferred from the figure, size appears to be critical as a determinant of exchange regime: very small (large) countries choose a peg (float) almost with probability one. Openness also plays a significant role. A developing economy with a trade to GDP ratio equal to 1 is 45% more likely to choose a fixed regime than a fully closed economy. Trade openness is even more strongly associated with the

propensity to peg in industrial economies, where a 50% trade to GDP ratio virtually implies a peg. The volatility of terms of trade, important for industrials, is almost negligible for non-industrials.

The divergence of the impact of financial variables across the two groups of countries is clearly illustrated in the figure. Capital account openness has a strong negative influence on the propensity to peg for developed economies (roughly a 70% decline over the whole range), as opposed to a minor positive effect for developing ones (about 10% increase). Similarly, the slightly smaller negative effect of portfolio flows for the industrial sample mirrors what is found for non-industrials. In particular, a developing economy with flows of about 10% of GDP has a 30% higher propensity to peg than one with no flows. Moreover, as the graph indicates, virtually no developing economy with flows of 25% of GDP or higher allows its currency to float. Finally, increasing the liability dollarization ratio from zero to twice the money base raises the preference for a peg among developing economies by roughly 25%, and reduces it by about 10% in developed ones.

Finally, regarding political variables, tenure in office appears to be more relevant among non-industrials (it increases the probability of choosing a peg throughout the initial twenty years), while the number of veto points has a sizable effect for industrials (close to a 70% variation over the whole range). Finally, governments' preference for a peg declines by 40% as countries go from low to high institutional quality.

Alternative regime classifications

Reinhart and Rogoff (2004; RR) build an alternative de facto classification based on the "verification" of the de jure regime, reclassifying the regimes where the exchange rate behavior does not match what is expected from the stated policy. Compared with the LYS

classification used here, RR offers the advantage that it corrects for multiple exchange rates, a practice that, while common among developing countries until the early 1970s, diminished steadily to less than 10 percent of cases during the post-Bretton Woods period. However, it is silent about the degree of exchange rate intervention, an aspect that is essential to characterize exchange rate policy, particularly when it comes to regime choice, to the extent that this choice is likely to depend critically on the policy constraints imposed by each regime.[30]

It is reassuring to see that our findings are broadly preserved when we rerun our baseline specification using RR (Table 7). For the sake of comparison, the sample used in the regressions includes only those observations that are also classified under the LYS methodology (columns (i) and (ii) reproduce the results using LYS).[31] As can be seen, the results for non-industrial countries (column iii) are comparable, providing strong support for the OCA, the currency mismatch and (to a lesser extent) the sustainability hypotheses. The same can be said for industrials countries (column iv), with the notable exception of financial linkages variables (the impossible trinity effect ceases to be significant while liability dollarization appears to be positively related to pegs).

While we have been mostly concerned with the determinants of actual exchange rate policies as reflected in de facto regimes, it is interesting to examine how the determinants identified above influence the choice of de jure arrangements. With this view, columns (v) and (vi) estimate the baseline using the IMF-based de jure classification.[32] Once again, the results compare surprisingly well with those for the de

[30] For example, due to the presence of multiple exchange rates, RR classify as intermediates or floats about 250 observations (or 7.5% of their database) for which the official exchange rate does not move, despite the fact that these cases display an average monthly exchange rate intervention of 4% of the monetary base, indicating that the objective of keeping the official rate constant was an effective policy constraint.
[31] Note that, while RR goes farther back in time than LYS, sample coverage is not an issue for the post-Bretton Woods period studied in this paper, for which the number of classified cases is virtually the same: 3435 under LYS and 3345 under RR.
[32] The data on de jure regimes is taken from Ghosh et al. (2003).

facto classification. The regression in column (v), for non-industrial countries, provides evidence in favor of OCA and for the currency mismatch hypotheses. However, it delivers contradictory results for the political variables. For industrial countries the de jure classification finds an effect of balance sheet exposure and provides support for the impossible trinity hypothesis.

In sum, our main findings are robust to the classification criteria, confirming the overall relevance of the OCA approach, the support for the impossible trinity view in the case of industrial countries, and the incidence of currency mismatch considerations as an incentive to peg in non-industrial countries. Additionally, our findings are consistent with the view that countries with poor institutional quality tend to resort to pegs as policy crutches, subject to the constraints imposed by their political ability to sustain them.

Robustness

While most of the key explanatory variables that we used can be reasonably assumed to be exogenous to the regime decision, not all are free from reverse causality concerns. For example, a peg, by reducing bilateral exchange rate volatility, may foster trade with other users of the peg currency and, in turn, may increase openness.[33] Financial depth has also been associated to exchange rate regimes, as a fixed exchange rate reduces exchange rate volatility, stimulating capital flows.[34] Others have argued that a peg, by creating the perception of an implicit exchange rate guarantee, fosters financial dollarization. In all three cases, the reverse link, from regime choice to openness, portfolio flows and liability dollarization, has the same positive sign as the one discussed above; hence, the potential endogeneity cannot be easily dismissed.

While in previous tests we used lagged values to attenuate the potential endogeneity, here we approach the problem more directly in two different ways. We first replicate the baseline specification (column i of Table 6) using initial values for openness and liability dollarization (these variables are identified with an i at the end), as well as the average of portfolio flows over the previous five years (Portfolio5), all variables that, as noted, may be affected by reverse causality. The results are reported in column (i) of Table 8: the coefficients on initial values all have the same sign as those obtained in Table 6, and in most cases are similar in size.

Correcting for endogeneity by replacing the variables with their initial values may be a highly imperfect way of dealing with the problem, particularly if some variables show country-specific or global trends that may alter them significantly over long periods of time, rendering the initial value relatively uninformative. This may be the case for financial variables such as liability dollarization and portfolio flows. To address this

[33] This point is made forcefully in Frankel and Rose (1997). See, among others, Rose and Van Wincoop (2001) and Micco, Stein and Ordonez (2003).
[34] Hausmann et al (2000).

shortcoming, we resort to instrumental variable (IV) estimation. Standard econometric packages do not carry a routine for IV estimation in logit models; thus, for proper computation of standard errors we used a bootstrapping technique. Specifically, we replicate the equation for the prediction of the endogenous variable 1000 times by bootstrapping the errors. We then replaced the instrumented variable in the exchange rate regime equation, which gave us a set of 1000 coefficients for each variable from which consistent estimators of the standard errors could be obtained. We used these standard errors to test hypotheses in the original specification.

As predictors of portfolio flows we use income per capita (strongly correlated with portfolio flows, with higher income countries showing much larger degrees of financial integration) and the overnight LIBOR, based on the evidence that lower rates in industrial countries tend to foster capital flows, particularly towards developing countries.[35] For liability dollarization, we use its first lag, as well as Kauffman et al.'s (2002) Rule of Law index (ROL, averaged for each country), under the assumption that higher contractual risk tends to induce more rigid contracts (of which dollarization is one common example).[36] The results, reported in column (ii), confirm our previous findings.

One additional source of concern is the omission of relevant variables that may be correlated with (and spuriously captured by) the included regressors. The most general way to control for (country-specific, time-invariant) omitted variables is through the use of country fixed effects. Unfortunately, the introduction of fixed effects suffers in our case from an important drawback. By restricting information to within-country variability, it limits the scope of the exercise as some of the hypotheses tested here (most notable, OCA) involve slow moving variables. With this caveat in mind, we report in

[35] On this, see, for example Calvo et al. (1992).

[36] De Nicoló et al. (2003) and Rajan (2004) provide empirical evidence along these lines. It has to be noted, however, that we implicitly assume ROL remains relatively stable over time, since the variable is available only since 1996.

column (iii) the estimates from a fixed effect estimation of the baseline specification. As can be seen, most results remain unchanged, with the only (expected) exception of the OCA variables that, showing little within volatility, become insignificant.

Global Trends and sensitivity analysis

Our empirical specification also allows us to test the presence of global trends in the choice of exchange rate regimes beyond and above those driven by the evolution of their underlying determinants. These trends can be recovered from the coefficients of the year dummies in any of the previous regressions. The value of these dummies can be interpreted as the impact of common global trends not captured by the fundamentals included in the specification as well as a bias reflecting the preferences of policy makers as a result of the evolving economic debate (or, possibly, transitory fads).

Figure 2 shows the difference in the change in the probability of choosing a peg associated to each year dummy in the baseline specification (Table 6, col. i) evaluated at the mean values of the remaining controls. In short, these values indicate by how much the probability of choosing a peg was affected by factors beyond those captured by our fundamentals. For comparison, we replicate the exercise using the de jure regime classification.

As can be seen, year dummies obtained from de facto regimes do not display any significant trend or systematic bias over the last twenty years (i.e. since the mid eighties). On the other hand, de jure regimes display a strong pro-fix bias that decreases steadily throughout the period. Initially countries exhibited a probability of choosing a peg that was 40% higher than warranted by fundamentals. By the end of the period this bias had fallen to 5%. These findings may be a reflection of the Bretton Woods inheritance: a

prevalence of de jure pegs that were not justified by fundamentals –and, possibly because of this, plagued by frequent realignments or sudden collapses.

If so, the results may be indicating that a welcome influence of the intellectual debate on exchange rate policies (and perhaps the only one) lies in the increasing reluctance to embrace explicit exchange rate commitments when underlying fundamentals and sustainability concerns recommend otherwise. By contrast, it appears that the way in which de facto regimes relate to underlying fundamentals has remained relatively stable despite the heated discussions and frequent changes of heart that have characterized the exchange rate debate.

IV. Conclusions

The evidence presented in this paper indicates that the choice of exchange rate regimes can be traced back to a group of geographical, financial and political variables in line with the existing theories of regime determination. While the relevance of these theories naturally depends on the particular characteristics of individual countries, our tests reveal some general patterns. More precisely, we find that, while OCA considerations affect similarly industrial and non-industrial countries, the influence of financial linkages differs across the two groups: whereas the impossibly trinity view prevails for the former, currency mismatch concerns appear to dominate in the case of the latter. Similarly, institutional quality and sustainability aspects appear to play a substantive role only in the case of non-industrials economies.

Understanding the role played by country-specific factors in the determination of exchange rate regimes is essential to assess the convenience and the ultimate success of any attempt to induce a country to adopt a particular regime, as well as to evaluate the

chances of a regime reversal down the road. The application of the "one size fits all" approach seems grossly at odds with the importance of key and identifiable fundamentals for the choice of exchange rate regimes.

To the extent that the underpinnings of the choice of exchange rate regimes has not changed in a visible way over the last two decades, one can also conclude that the intellectual discussion has been in this case far less influential than previously thought. Far from the fireworks of the academic fads, policy makers that have to choose actual exchange rate arrangements continue to balance a few simple tradeoffs.

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Table 1. Propensity to Peg and OCA Theory

	Expected Sign	Full Sample	Non-Ind.	Ind.	Ind. †
		i	ii	iii	iv
Size1	-	-0.360	-0.565	-0.344	-0.381
		(0.026)***	(0.045)***	(0.128)***	(0.119)***
Openness1	+	3.075	2.000	13.756	8.776
		(0.439)***	(0.483)***	(2.123)***	(2.345)***
TradeConc1	+	1.428	3.100	-9.579	7.890
		(0.919)	(1.066)***	(2.574)***	(5.388)
TOTshocks	-	-4.395	-3.379	-28.421	-14.968
		(1.467)***	(1.565)**	(11.328)**	(11.434)
Observations		2012	1576	436	410
Pseudo R^2		0.186	0.239	0.334	0.333

All regressions include year dummies.
Lagged values of variable x are denoted x1.
Robust standard errors in parentheses.
*significant at 10%; ** significant at 5%; *** significant at 1%.
† Excludes Canada.

Table 2. The Financial View - Correlations

	KAOpen	Portfolio	FinDev	CumLoans	FLM
KAOpen	1				
	2357				
Portfolio	0.4424†	1			
	1888	2371			
FinDev	0.4136†	0.2591†	1		
	2357	2371	4941		
CumLoans	0.3436†	0.3462†	0.0741†	1	
	1350	1352	1625	1625	
FLM	0.0397‡	0.054†	0.0709†	0.5355†	1
	2271	2294	3767	1544	3767

‡ significant at 5%; † significant at 1%.

Table 3. Propensity to Peg and the Financial View

	Expected Sign		Full Sample	Non-Ind.	Ind.
	Impossible Trinity	Currency Mismatch	i	ii	iii
KAOpen1	-	+	0.139	0.159	-0.583
			(0.051)***	(0.056)***	(0.209)***
Portfolio1	-	+	10.543	10.911	-5.247
			(2.720)***	(3.108)***	(8.123)
FinDev1	-	+	1.192	0.865	
			(0.227)***	(0.317)***	
FLM1		+	0.365	0.109	1.264
			(0.127)***	(0.041)***	(0.448)***
Size1	-	-	-0.673	-0.631	-1.042
			(0.047)***	(0.050)***	(0.170)***
Observations			1541	1247	294
Pseudo R^2			0.189	0.187	0.411

All regressions include year dummies.
Lagged values of variable x are denoted x1.
Robust standard errors in parentheses.
* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 4. The Political View - Correlations

	Herfindahl	YearsinOffice	VetoPoints	LegComp	OperationsRisk	CPIA
Herfindahl	1					
	3193					
YearsinOffice	0.3025†	1				
	3193	3915				
VetoPoints	-0.511†	-0.376†	1			
	2675	3258	3490			
LegComp	-0.59†	-0.187†	0.6663†	1		
	3193	3910	3254	3916		
OperationsRisk	-0.217†	-0.105†	0.457†	0.3255†		
	913	993	937	993	1029	
CPIA	-0.156†	0.079†	0.306†	0.2409†	0.4693†	1
	2002	2428	2159	2428	444	2685

† significant at 1%.

Table 5. Propensity to Peg and the Political View

	Expected Sign		Full Sample	Non-Ind.	Ind.	Adopting a Peg†	
						LYS	IMF
	Policy crutch	Sustainability	i	ii	iii	iv	v
CPIA1	-			-0.349			
				(0.076)***			
YearsinOffice	-	+	0.110	0.080	0.136	0.096	-0.081
			(0.018)***	(0.022)***	(0.109)	(0.037)***	(0.031)**
YearsinOffice^2	+	-	-0.003	-0.002	-0.013	-0.002	0.003
			(0.001)***	(0.001)***	(0.009)	(0.001)**	(0.001)***
VetoPoints1	+	-	-0.258	-0.242	-0.121	-0.071	-0.193
			(0.020)***	(0.029)***	(0.195)	(0.039)*	(0.035)***
High250	+	-				-0.442	0.684
						(0.557)	(0.345)**
Observations			2588	1607	482	1132	1132
Pseudo R^2			0.121	0.136	0.089	0.051	0.093

All regressions include year dummies.
Lagged values of variable x are denoted x1.
Robust standard errors in parentheses.
* significant at 10%; ** significant at 5%; *** significant at 1%.
† The sample includes countries that are classified as non-pegs in the previous year.

Table 6. Propensity to Peg – Baseline Specification

	FULL SAMPLE	NON-IND.	IND.
	i	ii	iii
Size1	-0.616	-0.672	-1.417
	(0.060)***	(0.078)***	(0.266)***
Openness1	1.863	1.857	23.595
	(0.562)***	(0.694)***	(3.919)***
TOTshocks	-3.070	-2.393	-98.823
	(2.303)	(2.588)	(21.103)***
KAOpen1	0.086	0.138	-0.737
	(0.057)	(0.074)*	(0.268)***
Portfolio1	10.616	15.095	-7.896
	(3.279)***	(5.506)***	(8.558)
FinDev1	1.771	1.081	
	(0.266)***	(0.373)***	
FLM1	0.538	0.687	-0.249
	(0.177)***	(0.334)**	(0.320)
CPIA		-0.334	
		(0.122)***	
YearsinOffice	0.081	0.104	-0.286
	(0.025)***	(0.030)***	(0.247)
YearsinOffice^2	-0.002	-0.003	0.033
	(0.001)***	(0.001)***	(0.019)*
VetoPoints1	-0.245	-0.234	-2.127
	(0.035)***	(0.038)***	(0.492)***
Observations	1380	1013	293
Pseudo R^2	0.244	0.279	0.563
Joint test†			
OCA	149.13***	98.24***	66.62***
Financial	103.78***	29.35***	12.96***
Political	83.12***	79.72***	21.30***

All regressions include year dummies.
Lagged values of variable x are denoted x1.
Robust standard errors in parentheses.
Statistics are chi-square distributed.
* significant at 10%; ** significant at 5%; *** significant at 1%.
† Reports the χ^2 corresponding to the Wald test of the variables associated with each group of explanations.

Table 7. Alternative Regime Classifications

	LYS	LYS	RR	RR	IMF	IMF
	NON-IND.	IND.	NON-IND.	IND.	NON-IND.	IND.
	i	ii	iii	iv	v	vi
Size1	-0.672	-1.417	-0.797	0.164	-0.750	-0.950
	(0.078)***	(0.266)***	(0.090)***	(0.195)	(0.073)***	(0.278)***
Openness1	1.857	23.595	1.641	11.285	3.185	-6.459
	(0.694)***	(3.919)***	(0.712)**	(2.736)***	(0.823)***	(4.613)
TOTshocks	-2.393	-98.823	-1.687	-44.599	-3.050	-38.069
	(2.588)	(21.103)***	(2.510)	(16.279)***	(2.979)	(28.412)
KAOpen1	0.138	-0.737	0.320	-0.015	0.016	-0.812
	(0.074)*	(0.268)***	(0.072)***	(0.222)	(0.076)	(0.294)***
Portfolio1	15.095	-7.896	10.314	0.226	8.246	2.039
	(5.506)***	(8.558)	(6.150)*	(6.693)	(7.517)	(12.692)
FinDev1	1.081		0.860		1.035	
	(0.373)***		(0.434)**		(0.447)**	
FLM1	0.687	-0.249	1.359	0.764	1.771	1.789
	(0.334)**	(0.320)	(0.435)***	(0.359)**	(0.418)***	(0.846)**
CPIA	-0.334		0.052		-0.657	
	(0.122)***		(0.125)		(0.142)***	
YearsinOffice	0.104	-0.286	-0.009	-0.599	-0.050	-0.108
	(0.030)***	(0.247)	(0.029)	(0.222)***	(0.034)	(0.269)
YearsinOffice^2	-0.003	0.033	-0.000	0.061	0.002	0.010
	(0.001)***	(0.019)*	(0.001)	(0.018)***	(0.001)**	(0.024)
VetoPoints1	-0.234	-2.127	-0.353	-1.261	-0.236	14.377
	(0.038)***	(0.492)***	(0.051)***	(0.347)***	(0.043)***	(0.538)***
Observations	1013	293	941	293	1013	293
Pseudo R^2	0.279	0.563	0.284	0.352	0.367	0.368
Joint test†						
OCA	98.24***	66.62***	106.79***	18.83***	144.06***	12.51***
Financial	29.35***	12.96***	37.42***	5.62	27.71***	8.31**
Political	79.72***	21.30***	56.42***	26.20***	63.62***	736.78***

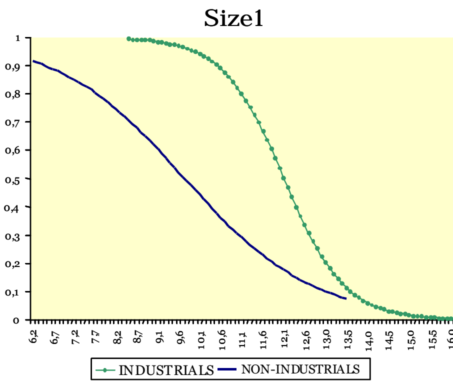
All regressions include year dummies.
Lagged values of variable x are denoted x1.
Robust standard errors in parentheses.
Statistics are chi-square distributed.
* significant at 10%; ** significant at 5%; *** significant at 1%.
† Reports the χ^2 corresponding to the Wald test of the variables associated with each group of explanations.

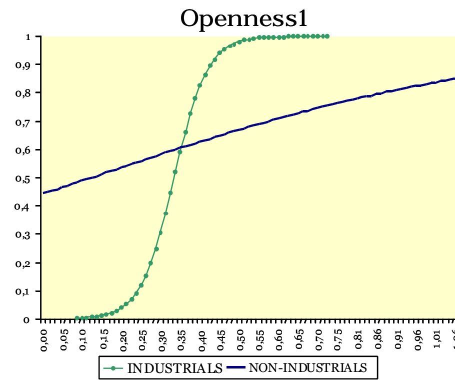
Table 8. Additional Robustness Checks

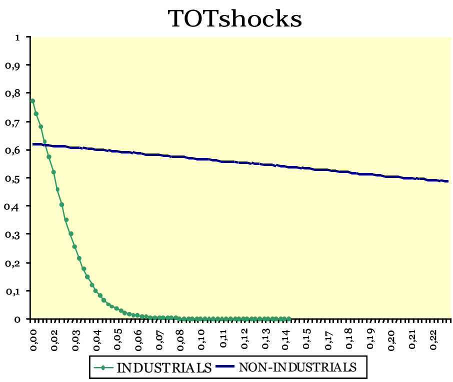
	INITIAL VALUES	IV†	FE
	i	ii	iii
Size1	-0.537	-0.695	0.367
	(0.074)***	(0110)***	(0.728)
Openness1		1.811	2.667
		(0.971)*	(1.519)*
Opennessi	5.012		
	(0.766)***		
TOTShocks	-3.418	-2.079	-4.759
	(2.470)	(4.036)	(3.566)
KaOpen1	0.058	0.046	0.265
	(0.069)	(0.102)	(0.116)**
Portfolio1		40.016	21.493
		(15.628)**	(5.394)***
Portfolio5	17.507		
	(5.300)***		
FinDev1	2.052	1.633	0.877
	(0.297)***	(0.457)***	(0.428)**
FLM1		0.546	0.863
		(0.288)***	(0.385)**
FLMi	0.060		
	(0.023)***		
YearsinOffice	0.069	0.089	0.136
	(0.030)**	(.045)**	(0.040)***
$YearsinOffice^2$	-0.002	-0.003	-0.003
	(0.001)**	(0.001)**	(0.001)***
VetoPoints 1	-0.307	-0.291	-0.145
	(0.045)***	(0.062)***	(0.076)*
Observations	1091	1377	1040
Pseudo R^2	0.236		0.184

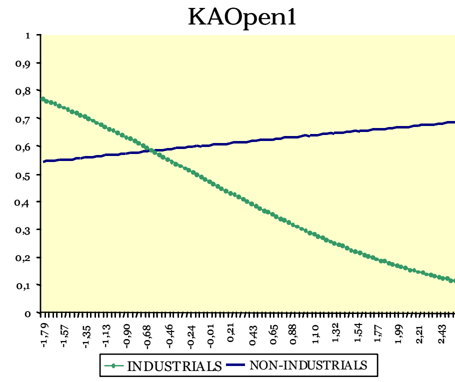
All regressions include year dummies.
Lagged values of variable x are denoted x1. Initial values of variable x are denoted xi.
† IV: instruments for Portfolio1: Libor and per capita GDP as a percentage of US per capita GDP; instruments for FLM1: Rule of Law and lagged value of FLM1 (FLM2).
Standard errors in parentheses.
* significant at 10%; ** significant at 5%; *** significant at 1%.

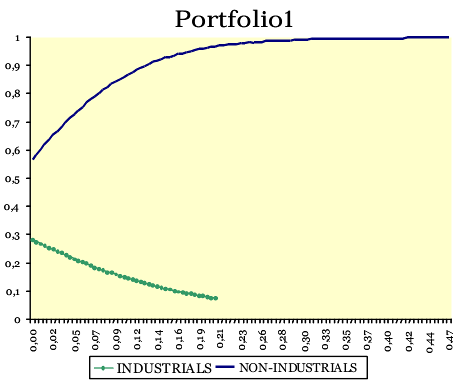
Figure 1

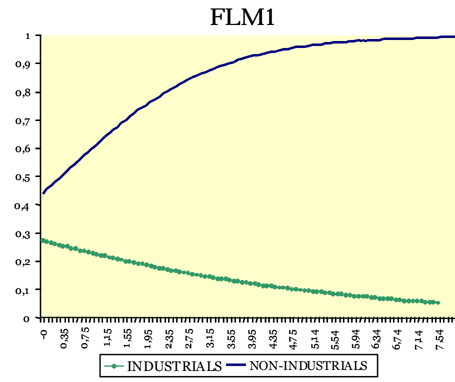


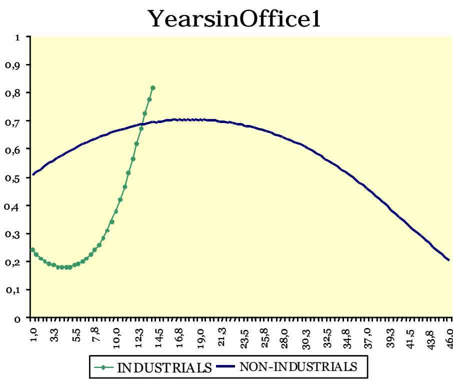


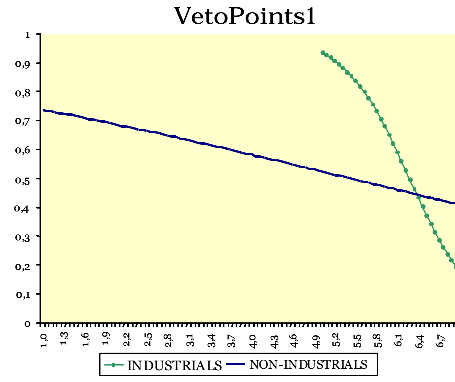












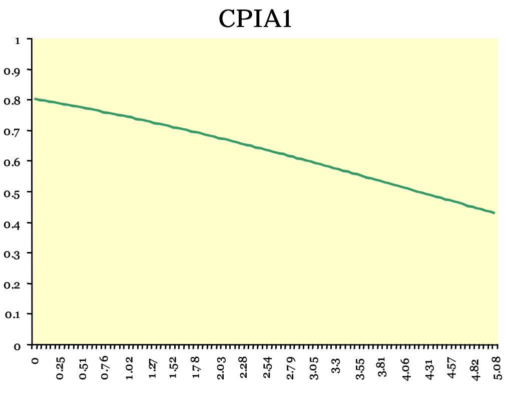
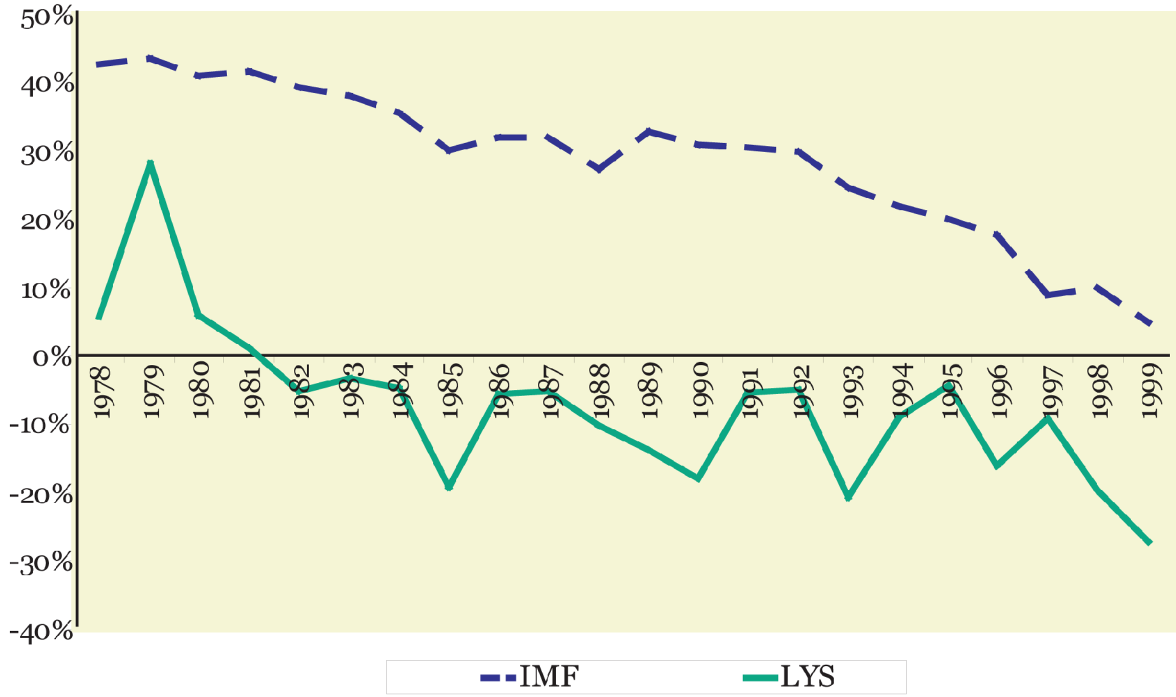


Figure 2



APPENDIX 1

List of countries

Industrials

AUSTRALIA
AUSTRIA
BELGIUM
CANADA
DENMARK
FINLAND
FRANCE
GERMANY
GREECE
ICELAND
IRELAND
ITALY
JAPAN
LUXEMBOURG
NETHERLANDS
NEW ZEALAND
NORWAY
PORTUGAL
SPAIN
SWEDEN
SWITZERLAND
UNITED KINGDOM
UNITED STATES

Non-Industrials

AFGHANISTAN, I.S. OF
ALBANIA
ALGERIA
ANGOLA
ANTIGUA AND BARBUDA
ARGENTINA
ARMENIA
ARUBA
AZERBAIJAN
BAHAMAS, THE
BAHRAIN
BANGLADESH
BARBADOS
BELARUS
BELIZE
BENIN
BHUTAN
BOLIVIA
BOSNIA AND HERZEGOVINA
BOTSWANA
BRAZIL
BRUNEI DARUSSALAM
BULGARIA
BURKINA FASO
BURUNDI
CAMBODIA
CAMEROON
CAPE VERDE
CENTRAL AFRICAN REP.
COLOMBIA
COMOROS
CONGO, DEM. REP. OF
CONGO, REPUBLIC OF
COSTA RICA
COTE D IVOIRE
CROATIA
CYPRUS
CZECH REPUBLIC
CHAD
CHILE
CHINA,P.R.: MAINLAND
CHINA,P.R.:HONG KONG
DJIBOUTI
DOMINICA
DOMINICAN REPUBLIC
ECUADOR
EGYPT
EL SALVADOR
EQUATORIAL GUINEA
ESTONIA
ETHIOPIA
FIJI
GABON
GAMBIA, THE
GEORGIA
GHANA
GRENADA
GUATEMALA
GUINEA
GUINEA-BISSAU
GUYANA
HAITI
HONDURAS
HUNGARY
INDIA
INDONESIA
IRAN, I.R. OF
IRAQ
ISRAEL
JAMAICA
JORDAN
KAZAKHSTAN
KENYA
KIRIBATI
KOREA
KUWAIT
KYRGYZ REPUBLIC
LAO PEOPLE'S DEM.REP
LATVIA
LEBANON
LESOTHO
LIBERIA
LIBYA
LITHUANIA
MACEDONIA, FYR
MADAGASCAR
MALAWI
MALAYSIA
MALDIVES
MALI
MALTA
MARSHALL ISLANDS
MAURITANIA
MAURITIUS
MEXICO
MICRONESIA, FED.STS.
MOLDOVA
MONGOLIA
MOROCCO
MOZAMBIQUE
MYANMAR
NAMIBIA
NEPAL
NETHERLANDS ANTILLES
NICARAGUA
NIGER
NIGERIA
OMAN
PAKISTAN
PALAU
PANAMA
PAPUA NEW GUINEA
PARAGUAY
PERU
PHILIPPINES
POLAND
QATAR
ROMANIA
RUSSIA
RWANDA
SAMOA
SAO TOME & PRINCIPE
SAUDI ARABIA
SENEGAL
SEYCHELLES
SIERRA LEONE
SINGAPORE
SLOVAK REPUBLIC
SLOVENIA
SOLOMON ISLANDS
SOMALIA
SOUTH AFRICA
SRI LANKA
ST. KITTS AND NEVIS
ST. LUCIA
ST. VINCENT & GRENS.
SUDAN
SURINAME
SWAZILAND
SYRIAN ARAB REPUBLIC
TAJIKISTAN
TANZANIA
THAILAND
TOGO
TONGA
TRINIDAD AND TOBAGO
TUNISIA
TURKEY
TURKMENISTAN
UGANDA
UKRAINE
UNITED ARAB EMIRATES
URUGUAY
VANUATU
VENEZUELA, REP. BOL.
VIETNAM
YEMEN, REPUBLIC OF
ZAMBIA
ZIMBABWE

APPENDIX 2

Variables	Definition and Sources
Size	Logarithm of GDP in dollars. (Source: World Economic Outlook-IMF).
Openness	Ratio of [export + import]/2 to GDP (Source: IFS (line 90c+line 98c)/2/ line 99b).
TradeConc	Geographical concentration of trade, the share of exports to the reference currency country multiplied by openness. (Source: Department of Trade Statistics- IMF).
TOTShocks	Standard deviation of the logarithm of terms of trade over the previous five years adjusted by average openness in the 5 previous years. (Source: WDI Series Code: NY.EXP.CAPM.KN).
KAOpen	Measure of capital openness provided by Chinn and Ito (2002), based four binary dummy variables reported in the IMF´s Annual Report on Exchange Rates and Exchange Restrictions with a higher number indicating capital account liberalization.
Portfolio	Sum of the absolute value of inward and outward flows of portfolio investments and financial derivatives as a ratio of GDP. (Source: IFS.(|line 78bfd|+|line 78bgd|+|line 78bwd|+|line 78bxd|) /GDP).
FinDev	Dummy indicating that the country made it into the EMBI global Index or belongs to industrial group.
CumLoans	Sum of cumulative flows of portfolio debt assets, other assets and net errors and omissions. (Source:External Wealth of Nations Data Set).
FLM	Ratio of Foreign Liabilities to Money. (Source: IFS line 26C/ (line 14 + line 24).
Herfindahl	The sum of the squared seat shares of all parties in the government. (Source: Database of Political Institutions 2000, Thorsten et al. (2001)).
YearsinOffice	Years the incumbent administration has been in office. (Source: Database of Political Institutions 2000, Thorsten et al. (2001)).
VetoPoints	Variable referred to the extent of institutionalized constraints on the decision-making powers of chief executives, whether individuals or collectivities. (Polcon_2002 Database).
LegComp	Legislative index of electoral competitiveness. (Source: World Bank´s Database of Political Institutions (V.2.0) ,Thorsten et al. (2001)).
OperationsRisk	Survey to gauge the operations climate for foreign businesses, a higher value indicating a better environment. (Source: Business Environment Risk Intelligence S.A.).
CPIA	World Bank´s Country Policy and Institutional Assessment Rating System. This measure is composed of 20 different components covering macroeconomic and sectoral policies, as well as issues such as the rule of law and corruption. Each of the twenty components is rated ordinally by country specialists, on a scale of 1-6, using standardized criteria.
Inflation	Logarithm of one plus the annual percentage change in Consumer Price Index. (Source: IFS line 64)
High250	Dummy variable for High Inflation. (Inflation greater than 250% in the previous year).
Instrument 1 (for Portfolio)	Libor. Overnight inter-bank interest rate. (Source: IFS line 11260B)
Instrument 2 (for Portfolio)	Per capita GDP as a percentage of US per capita GDP. (Source: World Economic Outlook-IMF)
Instrument 3 (for FLM)	Rule of law, includes several indicators that measure the extent to which agents have confidence in and abide by the rules of society. (Source: Kaufmann, Daniel, Kraay, Aart and Pablo Zoido-Lobaton(2002)).
Variable1	Lagged value of Variable
Variablei	Initial value of Variable

Summary statistics (baseline sample)

Variable	Observations	Mean	Std. Dev.	Min	Max
Size	1380	10.06713	2.200548	6.214608	15.97254
Openness	1380	0.323407	0.182723	1.11E-06	1.258302
TOTShocks	1380	0.045917	0.037058	0.001455	0.232774
KAOpen	1380	0.075929	1.496093	-1.79269	2.656628
Portfolio	1380	0.014228	0.031004	0	0.468185
FinDev	1380	0.272464	0.445389	0	1
FLM	1380	0.87711	5.114038	-0.04733	81.58941
YearsinOffice	1380	7.078261	7.735922	1	46
VetoPoints	1380	4.515217	2.441584	1	7
CPIA	1029	3.107178	0.760019	0	5.075