Can Domestic Institutions Explain Exchange Rate Regime Choice?

The Political Economy of Monetary Institutions Reconsidered

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Abstract:

Recent research in *International Organization* and elsewhere has explored the role of domestic institutions in shaping exchange rate regime choice. These models abstract domestic institutions from the wider international context at a very high cost. We argue that the choice of exchange rate regimes in fact are much more strongly linked to external influences than domestic institutions. By replicating and reevaluating the work of William Bernhard, David Leblang, Mark Hallerberg and Lawrence Broz, we show that the effects attributed to domestic institutions are fragile or even reversible when actual (as opposed to declaratory) exchange rate policies are used as the dependent variable. Furthermore, through both replication and the development of a full (Markov) transition model, we show that governments are likely to learn from observing the performance of foreign exchange rate regimes. This Bayesian learning effect is potentially much more important than the effects of domestic institutions alone. We think it is therefore critical to re-situate the study of exchange rate regime choice in its broader global context.

Beyond Domestic Institutions: Policy Diffusion and Exchange Rate Regime Choice

I. Introduction

Research on the political economy of monetary institutions has advanced significantly over the past ten years. New scholarship on exchange rate regimes sees the choice as intimately linked to governments' broader monetary and fiscal policy goals and constraints. Exchange rate choice, this generation of research holds, is influenced by domestic institutional incentives and constraints associated with heightened capital mobility. The attraction of this comparative political economy approach is that, given these exogenous constraints, it is assumed we can analyze exchange rate regime choice like any other domestic monetary problem: as a function of partisanship, office holding, central banking institutions, veto players, and a host of domestic strategic interactions among national actors.

Articles appearing in *International Organization* have made notable contributions in this regard. David Leblang and William Bernhard have presented data to show that, in OECD countries, the choice of an exchange rate regime is conditioned by the nature of the electoral system.[1] In systems where the costs of electoral defeat is high and election timing is exogenous, governments will be reluctant to give up monetary policy independence and will tend to choose flexible exchange rate regimes; the endogeneity of elections and low costs of defeat are associated with fixing. In their model, governments choose to fix (giving up monetary policy autonomy) when they can manipulate the timing of elections and hope to retain influence even if they are relegated to the governing

[1] Bernhard and Leblang 1999.

opposition. In this analysis, the exchange rate regime choice is conditioned by domestic electoral institutions.

Exchange rate regime choice is also analyzed as a constrained form of national monetary policy-making in a special issue of *International Organization* devoted entirely to this specific problem. The domestic institutional landscape in this volume is increasingly complex, earning it the status of "second generation" exchange rate research by the volume's editors.[2] Two of the articles in the special issue deal squarely with the choice of exchange rate regime; both ask, how do highly constrained governments select exchange rate regimes to secure the monetary policy outcome they desire? Mark Hallerberg used Bernhard and Leblang's data to show that domestic veto players are important in the choice of exchange rate regime.[3] Under federalism, he finds, parties that constitute the central government have less control over fiscal policy; facing a raft of veto players in the fiscal policy realm, a central government prefers to retain monetary policy autonomy and so, by default, they opt for flexible exchange rates. In this formulation, the configuration of domestic institutions that can foil effective fiscal policy unwittingly are implicated in flexible exchange rate choice.

In the same volume, Lawrence Broz has argued that governments wanting to maintain price stability have the choice of "commitment technologies" to achieve price stability: they can either choose an independent central bank or peg the currency.[4] Non-democratic regimes have a commitment problem: they cannot credibly commit to price stability even if they have an "independent" central bank, for the general lack of transparency does not allow for much of a check on the bank's activities. Because they

[2] Bernhard, Broz and Clark 2002.
[3] Hallerberg 2002.
[4] Broz 2002.

are more transparent, Broz argues, democracies can engineer price stability through central bank independence alone. Democracies are therefore much more likely to choose flexible rates than are autocracies.

These studies advance our knowledge by considering exchange rate, fiscal policy, and central bank design jointly. They contribute to a growing literature that focuses on under-appreciated ways in which domestic institutional factors affect exchange rate regime choice. None of the scholars we cite in this note would claim that domestic institutional factors trump international influences, but we do believe that the orientation of this line of investigation is potentially fundamentally misleading. Despite its open economy assumptions, exchange rate regime choice is modeled as an *independent* national policy decision. In this paper we argue this domestic approach is unnecessarily limited and empirically biased. We present evidence that most of the results of the domesticists are fragile, and do not hold up when replicated using the best-available data on actual exchange rate practices. Moreover, the domestic arguments weaken considerably in the face of strong external influences on exchange rate regime choice.

II. Diffusion Accounts of the Choice of Exchange Rate Regime

A simple central contention motivates this paper: a domestic political economy of exchange rate regime choice can never be adequate. Exchange rates and by logical extension the choice of an exchange rate regime are *necessarily* influenced by the policies taken by foreign actors. We question the basic approach of the comparative political economy literature: that exchange rate policy can be conceptualized and modeled as a set of independent national choices. On the contrary, exchange rate regime

choice is conditioned on the choices of decisionmakers who are systematically excluded from these domestic models. Lawrence Broz and Jeffrey Frieden conclude that "Perhaps more than any other economic area, national exchange rate policies depend on those of other nations."[5]

In some sense, this point is obvious. Governments have coordinated their exchange rate regimes informally since the 19th century gold standard. Despite the breakdown of such coordination during the interwar period, one of the first multilateral projects of the post-world war two years was to reconstruct a system of stable exchange rates based on pegs to the dollar, which was in turn to be pegged to gold. This was a negotiated system, in which the International Monetary Fund was to be consulted before rates could be changed. The system broke down with the unilateral action of the United States in 1971, and a series of rump negotiations leading to the European snake and eventually to the European Monetary System partially reconstructed the fixed rate system. Governments may have domestically generated *preferences*, but *choices* have been the product of intense international interactions. These interactions are absent from domestic political economy models.

There is another sense in which external influences have an obvious effect on exchange rates and regime choice. Fundamentally, exchange rates are relative prices – the ratio of my money's value to that of another country or the rest of the world. As such it is affected in part by what other governments do to influence the value of their currency. Analyzing the purely domestic context of exchange rate regime choice – that is, as a commitment mechanism or a way to secure policy flexibility under electoral or fiscal

[5] Broz and Frieden 2001: 335.

constraints – ignores the external strategic relationships in which these choices are embedded.

Finally, the assumption of independent decision-making is untenable because of the strong network effects of fixed exchange rates.[6] The 19th century gold standard became a focal point for a wide range of countries because fixing to gold reduced transactions costs with other countries on gold. The benefits of fixing (better trade access, reduced exchange rate risk for investors) grew as more countries joined the regime.[7] The decision to fix, then, is in large part co-determined by the policies of foreign governments.

None of the scholars whose work we replicate here have denied the importance of external influences on exchange rate policy. Suppose, then, that we set these broadly acknowledged external strategic considerations aside and accept the premise of the domestic institutional scholars' inquiry: that economic policy makers view exchange rates within the context of domestic monetary policy-making. There are strong grounds even within this framework to view the decision to peg or float as systematically determined by the choices of other governments. This is because *governments and markets learn about the effects of pegging by observing the experiments of other governments.*[8] These foreign lessons are then incorporated into national exchange rate regime choice.

And why not? Governments face a good deal of uncertainty over precisely what causes inflation. They are also quite unsure exactly how particular exchange rate policies

[6] Lawrence Broz (2002) has partially controlled for this possibility by including a highly significant term for proportion of the world on fixed rates, "feasibility of fixing," in his study.
[7] See for example Meissner 2001.
[8] See for example Khamfula 1998.

will influence their domestic inflation rates. Most governments have a very limited set of experiences with differing exchange rate regimes (note that the dependent variable in these studies does not vary much over time in most cases). Thus it is unlikely that most governments would be able to draw very firm conclusions about the relationship between fixed rates and inflation based on their experience alone. In the extreme, how would they know about the effects of floating on domestic inflation if they had never tried it? Our answer is that they learn from the experiences of other countries. Rational governments will use information from whatever source may be available to update prior estimates of the probability, for example, that fixing is associated with price stability.

The learning process we envision is depicted in Figure 1.[9] Suppose the hypothesis (h) under evaluation is that a fixed exchange rate reduces inflation. The three curves in the figure represent the estimated probability that a range of relationships are possible given various information environments. Suppose P(h/D) represents the strength of a government's prior beliefs given only domestic experience. The relatively flat curve reflects the government's belief that the information available is consistent with a broad range of relationships, with fairly equal probability. As the amount and quality of confirming information (new data) increases (linked, we suggest to the experience with fixing versus floating in other countries) probability estimates on this relationship begin to rise, as illustrated by the curve P(h/D1). If many other cases provide corroborating evidence for the hypothesized relationship, we would expect probability estimates to peak, as they do in curve P(h/D2). The curve P(h/D2) represents a strong belief that inflation can be reduced by fixing the exchange rate. The peaking is driven, we suggest, by new information gathered from a wider set of foreign "natural experiments." The new

[9] For a good review of the learning literature as used in foreign policy making, see Levy 1994.

data from these experiments influences beliefs, and these updated beliefs ultimately influence policy choice.

[FIGURE 1 ABOUT HERE]

The theory we have proposed here is simply a logical extension of the problem of exchange rate regime choice as framed by the domestic political economy models discussed above. It is still essentially a story about exchange rate regime choice as a part of monetary policy choice writ large. Our point is that even if we accept that governments are worried about domestic price stability, we should see strong diffusion effects based on Bayesian learning. Indeed, it would be *irrational* for policymakers to ignore externally available evidence about how pegging influences inflation, especially when theory is contested and domestic experience is limited. The empirical implications of such a social learning model are straightforward: if diffusion of exchange rate regimes is influenced by new information about fixed rates' ability to reduce inflation, then the probability of switching from a float to a fixed rate system should correlate with good inflation performance among fixers.

III. Empirical Tests: Replication and Robustness of the Domestic Results

We test the learning hypothesis first for OECD countries (replicating and re-evaluating Bernhard and Leblang 1999 and Hallerberg 2002) and then for the world (replicating, re-evaluating, and remodeling Broz 2002).[10]

[10] Due to lack of variation (i.e. co-linearity between the numerous time invariant variables) a full (Markov) transition model, which we use in the later part of our test for the global sample is unfortunately not

Exchange rate choice in the OECD

Bernhard and Leblang (1999) and Hallerberg (2002) use the same data and methods. Their dependent variable is a binary indicator[11] taken from declaratory policies reported to the International Monetary Fund about whether the currency is fixed or floats.[12] They differ, however, with important consequences for their analysis, on how to code Germany and Italy on the dependent variable.[13] Our strategy is to (1) replicate their work as precisely as possible, (2) retest it using a measure of the dependent variable that captures *actual* rather than *declaratory* policies, and (3) re-estimate their model taking foreign macroeconomic learning into account. The second step is justified because the arguments advanced by these scholars relates to *actual* policy, not to claims about policy reported to the IMF.[14] If the domestic institutional configurations really do have

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identified in the OECD sample. We cannot retest the Hallerberg and B&L claims in the global sample either, because data on their central institutional proxies are not available.

[11] Bernhard and Leblang also present a constrained multinomial logit model yielding "broadly similar results" as their binominal logit regressions. This is why Hallerberg tests his claims using their binomial specification only. Here, we focus exclusively on these binominal models.

[12] These measures are based on data taken from the IMF's "Exchange Rate Arrangements and Exchange Restrictions Annual Reports" (*ERAR)*, which for many decades recorded countries' notifications about their exchange rate arrangements to the Fund.

[13] While Bernhard and Leblang's dependent variable directly follows the official IMF *ERAR* classification (broken down to a binary proxy), Hallerberg's measure slightly deviates from their coding as he considers Germany to be floating over the whole sample period. He also recodes Italy 1993 to a float as it "was forced out of the European Monetary System in Fall 1992." Hallerberg 2002: 798. We are grateful to William Bernhard and David Leblang as well as Mark Hallerberg for generously providing us with their data.

[14] The distinction between de facto and de jure measures of exchange rate regimes is now commonplace in the economics debate, with the *ERAR* classification clearly falling in the latter category. In fact, recognizing the substantial divergence between actual and declaratory monetary policy, the IMF itself in 1997 changed its reporting criteria to take into account the actual functioning of a country's regime. Cf. IMF 1999. Section IV and IMF 2003. We use the "Natural Exchange Rate Classification" as complied by Reinhart and Rogoff 2004 as our measure of *de facto* regimes. This measure, which makes use of "parallel" market exchange rates as well as extensive country chronologies to determine the actual operation of an exchange rate regime, provides the current state of the art among economists for the measurement of actual exchange rate policies. The R&R classification (*mcode*) is based on a 15 point scale, which we dichotomize with a 10 point cutoff. Our cut-off includes the following as a float: de facto crawling band that is narrower than or equal to +/-5%; moving band that is narrower than or equal to +/-2%; managed floating, and freely floating. Our cut-off includes the following as fixed: no separate legal tender; pre-announced peg or

implications for exchange rate policy choice (as opposed to mere policy declarations) then it should be possible to replicate the findings of these studies using data on actual exchange rate policies.[15] The third step allows us to compare domestic arguments to learning based on foreign examples. Summary statistics of all dependent variables are displayed in panel A of Table 1. As it turns out, fewer countries actually float than claim to do so.[16]

[Table 1 about here]

To replicate Bernhard and Leblang (1999) and Hallerberg (2000), we run ordinary binary logit models estimated with a lagged dependent variable[17] and robust standard errors adjusted for potential within-country clustering, using all of their original explanatory variables. The results appear in the first two columns of Table 2.

[Table 2 about here]

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currency board arrangement; pre-announced horizontal band that is narrower than or equal to +/-2%; De facto peg; pre-announced crawling peg; pre-announced crawling band that is narrower than or equal to +/-2%; de factor crawling peg; de facto crawling band that is narrower than or equal to +/-2%; pre-announced crawling band that is wider than or equal to +/-2%. We discard observations classified as "freely falling" (mostly years of currency crisis and hyperinflation) and those for which parallel market data is missing. As R&R confirm, "given the distortions associated with very high inflation, any fixed versus flexible exchange rate regime comparisons that do not break out the freely falling episodes are meaningless." Reinhart and Rogoff 2004:4. As a robustness test we have also estimated all our models using a higher cut-off point of 12 and the results are similar and available upon request. For extensive reviews of the debate on exchange rate regimes classifications see Edwards and Savastano 2000; Ghosh, Gulde, Ostry and Wolf 1997; Ghosh, Gulde and Wolf 2003; Levy-Yeyati and Sturznegger 2003; Rogoff, Husain, Mody, Brooks and Oomes 2003. We prefer the R&R measure to other de facto measures such as Levy Yeyati and Sturzenegger 2003 or Gosh et al. 2003 because it is most widely available and focuses explicitly on the identification of longer-term "regimes" rather than shorter-term "spells" within a regime. Cf. Rogoff et al. 2003:10.

[15] Most of the de facto measures available today including the one we employ here have been developed just recently, and were not available when the original studies were written.

[16] See Calvo and Reinhart 2002. In fact the R&R natural classification algorithm "rejects about half of the official pegs as true pegs." Reinhart and Rogoff 2004:30.

[17] A lagged dependent variable is used to replicate the original findings. We do not advocate this as an appropriate way to model time series data with a dichotomous dependent variable. This is why we develop a full (Markov) transition model (see below).

The estimates in the first column are almost identical to those presented in Bernhard and Leblang (1999; their original Table 4). However, in attempting to replicate Hallerberg (2002; his original table 6) we found statistically insignificant effects for party players in federal systems[18] as well as for the decisiveness of the electoral system. As it turns out, the findings are extremely sensitive to how one codes Germany.[19]

Neither study's main findings survive a test using actual (de facto) rather than declared (de jure) exchange rate policies (columns 3 & 4). Bernhard and Leblang's argument about the impact of the decisiveness of the electoral system is almost turned on its head. Systems with low opposition influence (both proportional and majoritarian) seem, if anything, to be *more* likely than their high opposition influence counterparts (the reference category) to adopt a *de facto* fixed exchange rate regime. The electoral timing dummy shrinks significantly in magnitude, and is sensitive to the cut-point used to distinguish fixed from floating regimes. Hallerberg's claim about the effect of multiple veto players in a unitary system survives, but his claim about the negative effect of veto players in federal systems receives little support.

Now suppose we take learning from foreign experience into account. Our rational theory of Bayesian updating expects governments to update their priors about the value of fixed rates based on their observed ability more reliably to deliver price stability than

[18] Note that the multiplicative term of federalism and multiple party VP has the predicted negative sign and is significant, but the conditional coefficient for party players in federal systems, which is the combined effect of the multiplicative term and its lower order component is insignificant. The variance of the combined effect is computed $\text{Var}(\beta_{\text{combined effect}}) = \text{Var}(\beta_{\text{party VP}}) + \text{Var}(\beta_{\text{party VP*federalism}}) + 2\text{Cov}(\beta_{\text{party VP}}, \beta_{\text{party VP*federalism}})$. The standard error is just the square root of the variance. A potential reason for this is that according to Hallerberg, the institutional proxies he send us are a slightly updated version of the one used in the original article.

[19] Recall that Bernhard and Leblang code Germany as a fix, and Hallerberg as a float over the whole sample period.

do floating rate regimes. We need a measure that captures clear evidence that fixed exchange rates work. One way to do this is to benchmark the inflation performance of fixers compared to floaters. We compute a *Fixed Rate Inflation Advantage* indicator as follows:

$$FIA = [\text{Mean } (LN(INF))_{\text{countries with floating ER, t-1}}] - [\text{Mean}(LN(INF))_{\text{countries with fixed ER, t-1}}]$$ [20]

This measure aims explicitly to cover the premium in average inflation performance of fixed versus floating countries. It reflects our central claim that learning among policymakers takes place primarily *by comparing the average inflation performance of countries with different exchange rate regimes*. The better the average relative effectiveness of fixed compared to floating regimes, the more likely it is to attract adopters. Since our dependent variables are all coded Fix=1 and Float=0, this learning indicator should have a positive coefficient.

The results are displayed in columns 5, 6 and 7 of Table 2. Now virtually all of the domestic institutional variables drop to insignificance in the models of actual exchange rate policies (column 7). Part of this is due to the fact that we have had to drop the time dummies from the model, since they are perfectly collinear with our measure of the fixed rate inflation advantage.[21] Learning effects are generally supported by these

[20] QQ.normal plots revealed substantial leptokurtosis in the upper tail of the inflation rates distribution. Therefore, we log transformed the inflation distribution using a scant of plus one [LN(INF) = LN(1+|INF|) if INF>0 and -|LN(1+|INF|)| if INF<0] to prevent skew in the calculation of our differentials. This transformation yielded the best results in terms of approximating a normal distribution compared to other transformations down and up the ladder of powers.

[21] Note that, since our diffusion proxy Fixed Inflation Advantage (FIA) is invariant within each year, we have to drop the year dummies from the specification in order to identify the model. We think it is better to provide substantive rather than dummy variables where it is possible to do so. At the same time, plotting our Fixed Inflation Advantage measure against year demonstrates that it does not simply pick up a time

tests. Importantly, the magnitude of these effects is greatest in models in which measures of *actual* exchange rate regimes are used as the dependent variable. For example, based on the model estimates displayed in column 7, when the inflation advantage of fixed rates goes from its lowest to its highest level, the predicted probability of opting for a *de facto* fixed regime increases by 0.25, holding all other covariates at the sample mean. This is a rather big effect in substantive terms given that the average logged inflation differentials computed by FIA vary from only -.36 to .19. By comparison, in the same model the statistically insignificant effect of going from endogenous to exogenous election timing is estimated to decrease the probability of pegging by only 0.17.

Taken together these findings imply that there are likely to be strong external learning dynamics involved in choosing an exchange rate regime, and that these tend to swamp the initial domestic findings. If anything, the domestic institutions considered seem to affect declaratory policy, though not actual exchange rate policies.

A serious problem with testing the domestic theories is the general lack of variation in many of the independent and, most troubling, the dependent variables in this small OECD sample. Using Bernhard and Leblang's coding for example, in 95% of the cases a float is followed by a float or a fix is followed by a fix in the subsequent year. Clearly, this should make us wary of drawing any firm conclusion about the determinants of exchange rate choice from this data. Therefore, we now turn to an examination of exchange rate choice in the global sample.

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trend. Most importantly, the results we present in Table 2 are also robust (with almost identical magnitude and significance) to the inclusion of 5-year "period dummies," increasing our confidence that these results do not simply reflect period effects. Results available upon request.

Exchange Rate Choice in the Global Sample

In this section, we replicate, retest and reanalyze Broz's (2002) argument that democracies are less likely to choose fixed exchange rates because their independent central banks are credibly able to commit to price stability. We check for robustness of his results using data on actual policies, and then test our learning argument using Broz's original methodology.[22] In addition, thanks to the increase in variation provided by the global sample, we are also able to present estimates from a full (Markov) transition model that in our opinion provides a more accurate way of modeling the dynamics of exchange rate choice.

We have replicated Broz's 2002 results,[23] using both his original 4 point scale and a dichotomized version of the same data (Table 3, models 1 and 3).[24]

[Table 3 about here]

The results are virtually identical to those reported in his original Table 2. Most importantly, the democracy variable enters with the negative sign and is highly statistically significant indicating that as countries become more democratic they are less likely to peg their exchange rate. The adjacent columns (2 and 4) present the retests of

[22] See Broz 2002. for further details about the sample and variables. We thank Lawrence J. Broz for generously providing us with his data.

[23] We run an ordinary ordered probit model estimated with a lagged dependent variable and robust standard errors. Additionally, we also estimate the exact same specification just using our binary measure in lieu of the categorical exchange rate regime measures. In these binominal probit models we also (in contrast to Broz) adjust our standard errors for potential within-country clustering and add a full set of regional fixed effects to the specification for obvious reasons. The latter most often turn up jointly highly significant and thus belong into the model.

[24] Note also that Broz explicitly states that his results are very similar when a binary exchange rate measure is used, which is what we find. His four point and two point scale measures are very highly correlated (0.93***) and thus very little is lost by employing a binary dependent variable in lieu of his four point scale. We use these binary measures to facilitate construction of the empirical proxies of social learning that constitute the central independent variables in our analysis. Summary statistics of all dependent variables used in the part of our test are displayed in panel B of table 1.

Broz's argument using the R&R measures of actual exchange rate policies (using a four point scale derived from R&R's data).[25] The results indicate that Broz's argument does not hold up when reevaluated with *de facto* measures: the democracy coefficient becomes positive (though insignificant) with his full set of controls.[26]

Once we enter our learning proxy to the models (columns 5 and 6) we again find substantial evidence of learning. Using both Broz's declaratory measure as well as de facto policies, the coefficient on Fixed Rate Inflation Advantage has the right sign, and is highly significant for the latter. More importantly, the learning effect is substantially bigger than the estimated democracy effect. For example, based on model 6, going from the lowest to the highest level for Fixed Rate Inflation Advantage (when holding all other covariates at the sample mean) increases the estimated probability of pegging in the next period by 0.22. Given that the average logged inflation differentials computed by FIA vary from only -.19 to 1.27 in this sub-sample this is an economically highly significant effect. By comparison, a move from the most autocratic to the most democratic regime type is associated with a decrease in the probability of pegging of merely 0.06.

Taken together, these findings from the global sample reinforce those obtained for the developed countries. The effect of democracy on exchange rate regime choice dissolves when measures of actual rather than declaratory policies are used. But the evidence that governments learn from and respond to actual policy success is relatively strong.

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[25] We reduced R&R's original 6 categories to 4 by dropping cases where data are missing and cases of currency free falling.

[26] Due to missing and discarded data for the R&R proxy (see above), the models of de facto policies are based on smaller sample sizes. However, if we rerun the original Broz models on the exact same sub-samples as available for the R&R proxies, the polity coefficient always gains in magnitude and significance. This is clear evidence that what really "eliminates" the democracy finding is the change from measures of declaratory to de facto regime measures and not the reduction in sample size associated with it. Results of these tests are available upon request.

IV. Re-evaluation: A Full Markov Transition Model of Exchange Rate Choice

The preceding tests used the methods employed by Bernhard and Leblang, Hallerberg, and Broz to facilitate direct comparisons of results when external learning is taken into account. Our measure of learning performed well in these models: results were positive, relatively consistent using different measures and specifications, and learning was associated with much greater substantive effects than were the key domestic arguments under examination.

Ordinary probit or logit models are not, however, the ideal way to make this point. In particular, simply adding a lagged dependent variable to the specification most often does not adequately address the various econometric challenges associated with modeling binary time series cross sectional (BTSCS) data.[27] A more appropriate strategy is to estimate a "full" transition model based on first order Markov assumptions.[28]

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[27] Note that the common approach of simply including a lagged *y* in an ordinary probit/logit specification (i.e. what Beck et al. label the "restricted transition model"), "is NOT the natural analog to the continuous dependent variable times series model with a lagged dependent variable. The right way to think about binary time series analogies of their continuous cousins is to write the time series model in terms of a continuous latent variable and then just take each period's realization of a zero or a one as arising from a draw of the underlying normal distribution." Beck et al. 2002 : 3. The latter approach leads to a model that includes a lagged latent *y**, not a lagged realized *y* in the specification. In our context, the only justification for including a lagged realized *y* in an ordinary probit/logit would be to assume that a fix (float) at *t-1* itself causes the fix (float) in the current period. This is much different from the standard time series assumption that the underlying latent *y** (i.e. the latent propensity to peg) shows persistence over time. Note that even if we assume that past regimes indeed cause current regimes, the "restricted transition model" is still "an odd choice" because it assumes that the only parameter that contrasts *transitions* (fix_{t-1} to $float_t$) from *continuation processes* (fix_{t-1} to fix_t) is the constant. This assumption is soundly rejected by the evidence; the "full" transition model we estimate below clearly shows that the effect of many covariates is dramatically different in these two processes and thus they need to be modeled as two separate dynamics. In this regard, the "restricted transition model" is a special and much too restricted case of the "full" transition model, that allows for asymmetry in the effect of each covariate. See below and the discussion in Beck et al. 2002: 6-9.

[28] Beck, Epstein, O'Halloran and Jackman 2002. Their test is based on an empirical model of state failure and to our knowledge currently provides the most comprehensive treatment of the relative performance of BTSCS models for political science applications.

Apart from adequately dealing with serial correlation dynamics, such a specification has other key advantages over an ordinary logit or probit specification. In line with our theoretical expectation the full transition model allows us simultaneously to (1) condition today's exchange rate choice on a country's institutional history and (2) model the asymmetry in the effect of our contemporaneously measured (or lagged) independent variables ($\boldsymbol{\beta}_0$ or $\boldsymbol{\beta}_1$) on the conditional probability of pegging at time *t* ($\Pr(\mathbf{y}_t = 1 \mid \mathbf{y}_{t-1} = 1)$ or $\Pr(\mathbf{y}_t = 1 \mid \mathbf{y}_{t-1} = 0)$).[29] This model is a good fit with our assumption that policy makers know the (immediate) history of their country and condition their exchange rate choice today on *how well their country performed relative to those countries with a different exchange rate regime in place over this time horizon.*

The transition model is constructed by interacting all independent variables with the lagged value of the dependent variable in a first order Markov chain. We estimate this model using ordinary binary probit, which produces consistent estimates of both $\boldsymbol{\beta}_0$ and $\boldsymbol{\beta}_1$ if the first order Markov specification is correct.[30] Formally, we estimate the following equations:

$$(1)\ \Pr(\mathbf{y}_{i,t} = 1 \mid \mathbf{y}_{i,t-1} = 0\ ) = \text{Probit}(\mathbf{x}_{i,t}\boldsymbol{\beta}_0)$$

$$(2)\ \Pr(\mathbf{y}_{i,t} = 1 \mid \mathbf{y}_{i,t-1} = 1\ ) = \text{Probit}(\mathbf{x}_{i,t}\boldsymbol{\beta}_1)$$

These can be combined to

$$(3)\ \Pr(\mathbf{y}_{i,t} = 1 \mid \mathbf{y}_{i,t-1}\ ) = \text{Probit}(\mathbf{x}_{i,t}\boldsymbol{\beta}_0 + y_{i,t-1}\mathbf{x}_{i,t}\boldsymbol{\gamma})$$

where $\boldsymbol{\beta}_0$ denotes a vector of coefficient estimates for the effect of $\mathbf{x}_i$ on the conditional probability of pegging in the next period for countries that had a floating exchange rate at

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[29] Jackman 2000.and Beck et al. 2002 provide extensive treatment of the "full" transition model. Our model draws on this work. For the seminal work using a Markov chain model in the context of exchange rate transitions, see Masson 2001.

[30] One could similarly use a logit here. Amemiya 1985.ch11 proves consistency of the MLE for both $\boldsymbol{\beta}_0$ and $\boldsymbol{\beta}_1$.

t-1, and $\boldsymbol{\beta}_1 = \boldsymbol{\beta}_0 + \boldsymbol{\gamma}$, denotes the respective vector of coefficient estimates for countries that had a fixed exchange rate at t-1 (since when $y_{i,t-1} = 0$, $\mathbf{x}_{i,t}\boldsymbol{\gamma}$ zeros out of the equation). Accordingly, we can simply use $\boldsymbol{\gamma} = 0$ as the null hypothesis to test whether the effect of $\mathbf{x}_i$ is significantly different for *transition* ($float_{t-1}$ to fix_t) as compared to *continuation* processes (fix_{t-1} to fix_t).

Thus, the full transition model allows us not only to deal with the issue of serial correlation in the data, but also to model both *transitions* and *continuations* within a single equation while allowing for different parameter estimates of the effect of our independent variables in these two distinct processes.[31] Note also that for both *transition* and *continuation* processes this specification allows for a different parameter estimate for all covariates not just the learning indicator. Therefore our model also subjects the political economy hypotheses (e.g., Broz's hypothesis that democracies are less likely to peg) to further scrutiny, since it tests for the effect of democracy conditional on the exchange rate regime in place at *t-1*.[32]

Since the Markov transition model allows us to distinguish transitions from continuations, we develop a more nuanced indicator of learning that takes advantage of the model's special capabilities. *Contextual Social Learning (CSL)* explicitly focuses on the asymmetry we expect in the effect of new information for fixed as compared to floating countries. We calculate this indicator as follows:

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[31] Jackman 2000: 8. As Beck et al. 2002:8 note: "in the epidemiology world, this distinction is between modeling incidence and prevalence." Jackman 2000 uses this property of the transition model to show that the effect of dyadic democracy on the outbreak of war is much different than on war prevalence. The full transition model is regularly used in biostatistics and econometrics, and has been successfully applied to various topics in sociology, economics, as well as political science. See the numerous examples cited in Jackman 2000: 8 and Beck et al. 2002: 4.

[32] As with our ordinary logit specification, we estimate the full transition model with robust standard errors adjusted for potential within country clustering. In each specification we also include a full set of regional dummies.

Contextual Social Learning = [weighted mean $(LN(INF))_{\text{countries with a different ER, t-1}}$] - [$Ln(INF)_{\text{country i, t-1}}$]

This measure captures learning effects based on a comparison of policymakers' *own country performance against the average performance of those countries with a different exchange rate regime*. The better a country's own exchange rate regime at controlling inflation relative to the inflation performance of the alternative regime, the less likely a government is to switch regimes. But where the domestic regime clearly underperforms relative to the alternative, governments increasingly believe switching would be worthwhile. We weight this measure by the GDP of each economy observed, since it is likely that governments learn more from the experiences of large countries.[33] We also calculate this variable by region,[34] since lessons within region may be more salient than those from the other side of the world.[35] Since our dependent variable is coded Fix=1 and Float=0, *Contextual Social Learning* should have a *negative* effect for floating and a *positive* or *no effect* for countries with a fixed rate.

The results for the full transition model are displayed in Table 4. The left panel contains the estimates for the global and the right panel the estimates for the regional reference group. For each model we present three columns, where $\boldsymbol{\beta}_0$ displays the effect of each covariate for those countries on a float, $\boldsymbol{\beta}_1$ displays the effect of each covariate for

[33] Using an unweighted version of *Contextual Social Learning* yields results that are substantively identical to the estimates presented here. Results are available from the authors.

[34] For the *global* version of *Contextual Social Learning*, the inflation performance gap is computed against the (weighted) global average of those countries with a different exchange rate, while for the *regional* variant of *Contextual Social Learning* we compute the (weighed) average of those countries with a different exchange rate regime within the same region. The interpretation of the latter variable is identical to its global counterpart, except for the fact that now we assume policy makers to primarily orient themselves within their own region

[35] Sociologists have noted that learning is influenced by an "availability heuristic," meaning that learning is likely to take place based on examples that are most readily available. See Gale and Kariv 2003; Kahneman, Slovic and Tversky 1982.

fixed rate countries, and the parameter $\boldsymbol{\gamma}$ contrasts the effect of each covariate across states (note that only $\boldsymbol{\beta}_0$ and $\boldsymbol{\gamma}$ are estimated).

First, take a look at the very bottom row of the table where we present the estimates of the joint test that the effects of $\mathbf{x}_i$ are significantly different for *transition* ($float_{t-1}$ to fix_t) as compared to *continuation* processes (fix_{t-1} to fix_t). Across models, the null of no asymmetry is soundly rejected suggesting that the transition model is the more accurate specification than the ordinary (constrained) probit models presented above. Furthermore, judging by the significance of the coefficients reported in column $\boldsymbol{\gamma}$, the effects of both democracy but especially *contextual social learning* on actual exchange rate polices are fundamentally different depending on the country's immediate institutional history (i.e. whether it is on a float or a fixed regime at t-1).

That governments learn from the inflation performance of other countries seems clear from the results. For the floating countries, the coefficient for *contextual social learning* has the expected negative sign and is significant across regime proxies and reference groups. The only exception is the borderline insignificant (p=0.11) coefficient for the Broz measure of declaratory exchange rate polices in the global reference group, which is arguably the least relevant model for testing our theory. For countries with a fixed exchange rate, *contextual social learning* enters with the expected positive sign across models, and in three of the four models is statistically significant.[36]

[36] As a robustness test, we also ran this model with a full set of year dummies to absorb various commonly experienced global shocks. In these models we have to drop the feasibility measure, which is invariant in a given year; we also lose some observations that are perfectly predicted due to collinearity. The findings for *contextual social learning* were basically robust, with two exceptions: the coefficient for floaters in model 1 (using de jure exchange rates) became statistically insignificant, and the coefficient for fixers in the global reference group (model 2), became only borderline statistically significant.

Third, support for Broz's hypothesis about the effect of democracy on the probability of fixing one's exchange rate is mixed at best. Consistent with his original claim, for countries with a fixed rate, democracy may have a negative effect on the probability of maintaining the pegging in the next period. For floating countries, however, support for the Broz hypothesis is measure dependent. For floaters, democracy has a significant negative effect on the probability of *declaring* a pegged exchange rate regime, but a significant positive (and bigger) effect on the probability of *actually* fixing rates. This speaks against the claim that countries generally become more likely actually to float as they become more democratic. Apparently, even democracies consistently exhibit some fear of floating.[37]

The learning effect for floating countries swamps the democracy effect in terms of magnitude. Figure 2 plots the predicted probability of the floaters switching to a peg at different values of democracy and *contextual social learning* (holding all other covariates at the sample means). The differences in magnitudes are stark. While a move from the most autocratic to the most democratic regime type shifts the probability of pegging by merely 0.07, the respective increase when going from the lowest to the highest values of *contextual social learning* is about 0.5, or greater by a factor of more than seven.

As the shape of the curve of predicted probabilities makes clear, the effect of social learning neatly follows our theoretical expectations of Bayesian updating. For positive values of *contextual social learning* (that is, when pegged countries perform below the fixed regime benchmark inflation performance), we hardly see any effect on the probability of switching. But once floating countries start to perform worse than the fixed benchmark (for negative values of *contextual social learning*) the marginal effect

[37] Calvo and Reinhardt 2002.

on the probability of switching rises exponentially. For example, at a value of *contextual social learning* of zero (when the floating country is exactly on par with the fixed average) the point estimate of the predicted probability of switching to a peg is only 0.03. This probability rises to 0.06 at a value of *contextual social learning* of minus 1 (i.e. when the floating country is about 1 percentage points below the fixed average in terms of relative logged inflation performance) and subsequently rises exponentially as it becomes more and more evident that the floating exchange rate arrangement is ineffective in fighting inflation. Finally, at a value of *contextual social learning* of minus 4 (i.e. when the floating country is about 4 percentage points below the fixed average in terms of relative logged inflation performance) the probability of switching to a peg is as high as 0.46.

V. Conclusions

Scholarship on the political economy of exchange rate choice has advanced significantly over the past several years. New models (several of which have been published in *International Organization)* demonstrate the value of thinking about how monetary and fiscal choices interact with domestic institutional conditions to influence exchange rate regime choice. There is much merit in studying the effects of domestic institutions on exchange rate choice. But something crucially important is missing from these increasingly sophisticated domestic models: the influence of the outside world. In principle, our findings are complementary to the domestic research agenda. But since these external influences tend to be very large indeed compared to those flowing from domestic institutions alone, continuing to research the exchange rate regime effects of

local institutions in isolation is quite misleading. External influences are potentially of many different kinds,[38] but in this paper we make the case for the importance of one channel of influence in particular: Bayesian learning based on compelling foreign evidence of policy success.

We do not claim to have fully modeled diffusion processes in this note. But it has been possible to demonstrate that domestic explanations do not account well for actual (as opposed to declaratory) exchange rate policies. Lesson-drawing from policy examples seems much better able to do so. Future work should not only develop and hopefully improve upon the Bayesian learning explanation advanced here, but should continue to think creatively about exactly how and why national policy choices are likely to be interdependent. This is a project that will take political economy well beyond its current domesticism and require further attention to how governments incorporate influences of the broader global polity.

[38] These include commonly experienced shocks as well as various forms of policy diffusion. For a review of international diffusion processes see Simmons, Dobbin and Garrett 2004.

Figure 1: Bayesian updating

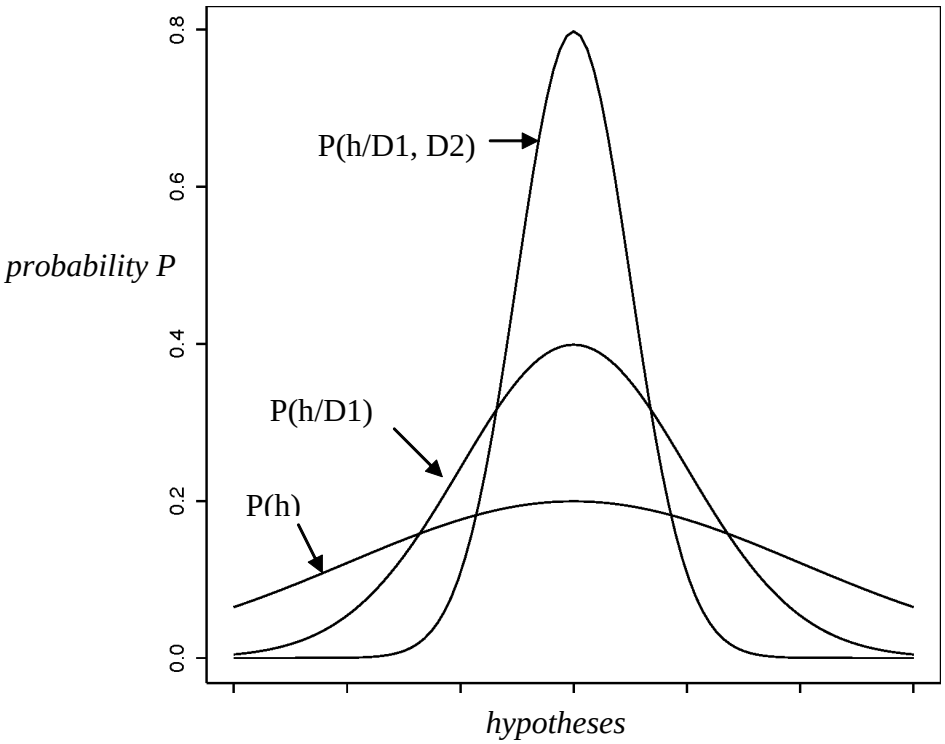
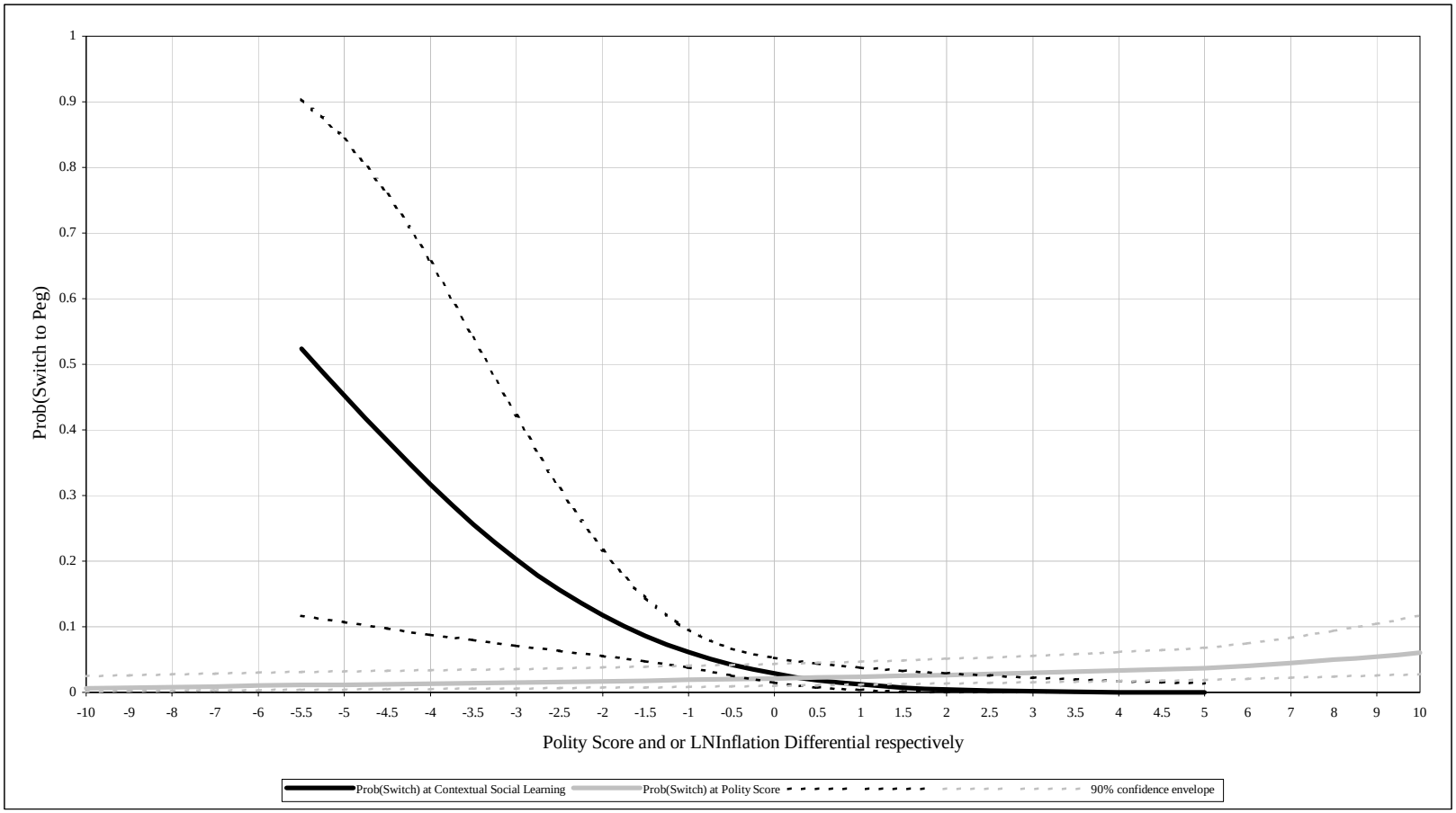


Figure 2 :
The Likelihood of Pegging for Floating Countries: Democracy vs. Learning Effect[1]
(90 % dashed confidence envelope around solid line of point estimates)



[1] Based on estimates of model 2, table 4 (global reference group, R&R measure of actual ER-regimes)

Table 1:
Different Classifications of Exchange Rate Regimes: *De jure* and *de facto* measures

Panel A: The OECD Sample[1]			
ER-Regime Proxy	Obs	Mean	SD
De jure :			
B&L 1999 * (binary measure)	460	0.56	0.50
Hallerberg 2002 * (binary measure)	460	0.51	0.50
De facto:			
Reinhard & Rogoff 2004 ** (binary measure)	439	0.66	0.48
Correlations:	B&L 1999	Hallerberg 2002	R&R No1 (cutoff 10)
Hallerberg 2002	0.90	1	
Reinhard & Rogoff 2004	0.40	0.53	1
Panel B: The GLOBAL Sample[2]			
ER-Regime Proxy	Obs	Mean	SD
De jure :			
Broz 2002 * (four point scale)	3357	3.29	1.05
Broz 2002 * (binary measure)	3357	0.72	0.45
De facto:			
Reinhard & Rogoff 2004 ** (four point scale)	2315	3.05	0.91
Reinhard & Rogoff 2004 ** (binary measure)	2315	0.70	0.46
Correlations:	Broz 2002 (four point scale)	Broz 2002 (binary measure)	Reinhard & Rogoff 2004 (four point scale)
Broz 2002 (binary measure)	0.93	1	
Reinhard & Rogoff 2004 (four point scale)	0.31	0.28	1
Reinhard & Rogoff 2004 (binary measure)	0.10	0.08	0.86

[1] Dataset taken from B&L 1999.
[2] Dataset taken from Broz 2002.
* measure based on the (former) IMF's AREAER classification.
** measure based on the "Natural Classification" proposed by Reinhart and Rogoff 2004.

Table 2:
Exchange Rate Choice in the OECD sample: Domestic Institutions vs. Policy Diffusion

Dependent Variable		*Binary ER-Regime measure: 0=float 1=fix*						
Source		Bernhard & Leblang 1999	Hallerberg 2002	Reinhard & Rogoff 2004	Reinhard & Rogoff 2004	Bernhard & Leblang 1999	Hallerberg 2002	Reinhard & Rogoff 2004
Model No.[1]		1	2	3	4	5	6	7
Independent Variables:	Exp. Sign:							
Domestic Institutions								
Majoritarian-Low Opposition Influence (B&L 1999)	Negative	-3.546**	0.405	3.420**	5.778	-1.958**	-0.385	1.832
		(1.510)	(1.268)	(1.635)	(3.991)	(0.972)	(0.879)	(1.435)
Proportional-Low Opposition Influence (B&L 1999)	Negative	-3.168**	0.398	2.344	6.287	-2.064**	-0.550	1.672
		(1.560)	(1.386)	(2.229)	(8.533)	(0.934)	(0.728)	(1.667)
Electoral Timing (B&L 1999)	Negative	-3.930***	-2.925**	-2.138***	-2.411***	-2.007**	-1.435**	-1.147
		(1.345)	(1.397)	(0.825)	(0.663)	(0.786)	(0.597)	(0.960)
Federal Dummy (Hallerberg 2002)	Insignificant		0.709		3.540	0.847	0.541	2.393
			(0.639)		(4.198)	(0.692)	(0.631)	(1.758)
Multiple Party Veto Dummy (Hallerberg 2002)	Positive		2.739***		2.940*	1.415**	1.607***	1.521
			(0.654)		(1.687)	(0.553)	(0.469)	(1.467)
Federal Dummy * Multiple Party Veto Dummy (Hallerberg 2002)			-4.168**		-5.632	-1.080	-2.526**	-2.622
			(1.763)		(4.811)	(1.047)	(1.032)	(2.263)
Conditional Coefficient: Multiple Party when Federalism = 1 (Hallerberg 2002)	Negative		-1.428		-2.691	.334	-.918	-1.101
			(1.927)		(3.627)	(.997)	(1.026)	(2.358)
Learning[2]								
Fixed Inflation Advantage (constructed using B&L ER-coding)	Positive					2.681*		
						(1.608)		
Fixed Inflation Advantage (constructed using Hallerberg ER-coding)	Positive						1.901	
							(1.506)	
Fixed Inflation Advantage (constructed using R&R ER-coding)	Positive							3.513**
								(1.640)
Covariates: (all from B&L 1999)								
Openness		7.438***	9.816***	9.988*	17.500	4.190***	5.157***	6.204**
		(2.576)	(2.512)	(5.301)	(17.123)	(1.377)	(1.357)	(2.954)
Domestic Credit Shock		-0.003	-0.004**	0.075***	0.058**	-0.002	-0.000	0.022
		(0.010)	(0.002)	(0.027)	(0.027)	(0.001)	(0.003)	(0.023)
Capital Controls		3.134***	1.904**	0.060	-1.021	2.062***	1.587**	0.644
		(0.912)	(0.828)	(1.113)	(2.590)	(0.694)	(0.624)	(1.341)
International Capital Mobility		-0.000	-0.000	-0.000**	-0.000	-0.000	-0.000	-0.000**
		(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Economic Growth		-182.677***	-198.796***	-871.399***	-1,204.062	-165.333***	-145.449**	-274.481
		(50.857)	(76.888)	(334.626)	(738.740)	(57.793)	(62.208)	(361.382)
Partisanship		0.450	1.854**	-0.532	-0.753	1.475**	1.635***	-0.575
		(0.537)	(0.904)	(0.627)	(1.098)	(0.673)	(0.593)	(1.434)
Election Year		-0.003	0.595	0.776	1.280	-0.646	-0.233	-0.074
		(0.505)	(0.473)	(1.125)	(1.497)	(0.508)	(0.402)	(0.476)
Europe		0.984	3.276***	4.388*	6.186	0.123	0.911	2.742**
		(0.840)	(1.107)	(2.255)	(3.919)	(0.701)	(0.712)	(1.232)
EC membership		2.536*	-0.114	-1.841	-2.634***	2.437***	1.065	-0.895
		(1.539)	(1.443)	(1.121)	(1.011)	(0.831)	(0.733)	(0.978)
Respective Lagged DV[3]		8.225***	8.347***	14.341***	16.465**	5.705***	5.353***	8.163***
		(2.156)	(2.031)	(3.401)	(8.285)	(1.017)	(0.772)	(1.379)
Intercept		-7.226**	-11.869***	-8.494	-15.112	-5.688**	-6.848***	-7.438**
		(3.081)	(3.327)	(6.172)	(15.020)	(2.372)	(2.125)	(3.396)
Observations		433	432	420	419	432	432	419
Log likelihood		-37.72	-35.92	-26.05	-23.35	-50.42	-55.65	-37.05
Pseudo R-squared		0.87	0.88	0.90	0.91	0.83	0.81	0.86

[1] Logit estimations: Logit coefficients shown with robust standard errors, adjusted for potential within country clustering, in parentheses. Models 1-4 include a full set of year dummies (coefficients not shown). * significant at 10%; ** significant at 5%; *** significant at 1%. For detailed definitions of all variables apart from the diffusion proxies cf. Bernhard and Leblang 1999 and Hallerberg 2002.

[2] Fixed Inflation Advantage: *FIA* = [Mean (LN(INF))$_{\text{countries with floating ER, t-1}}$] - [Mean(LN(INF))$_{\text{countries with fixed ER, t-1}}$]. Models include different FIA measures depending on which ER coding is used as the model's dependent variable.

[3] Models include different lagged dependent variables depending on which ER coding is used as the model's dependent variable. Estimates are presented in a single row to economize on space.

Table 3:
Probit Model: Exchange Rate Choice in the Global Sample: Domestic Institutions vs. Policy Diffusion

Dependent Variable		Four point scale ER-Regime Measure: 1=float 4=fix		Binary ER-Regime Measure: 0=float 1=fix			
Source		Broz 2002	Reinhard & Rogoff 2004	Broz 2002	Reinhard & Rogoff 2004	Broz 2002	Reinhard & Rogoff 2004
Model No.[1]		1	2	3	4	5	6
Independent Variable	Exp. Sign						
Domestic Institutions							
Polity score (Broz)	negative	-0.016***	0.001	-0.017**	-0.009	-0.019**	-0.012
		(0.006)	(0.007)	(0.009)	(0.012)	(0.009)	(0.012)
Learning[2]							
Fixed Inflation Advantage (FIA) (constructed using Broz's ER-coding)	positive					0.023	
						(0.236)	
Fixed Inflation Advantage (FIA) (constructed using R&R ER-coding)	positive						0.583**
							(0.258)
Covariates (all from Broz 2002)							
Wealth (per capita GDP)		0.024***	-0.017	0.007	-0.060**	0.002	-0.057**
		(0.009)	(0.010)	(0.019)	(0.026)	(0.020)	(0.027)
Size (LN GDP)		-0.257***	-0.113	-0.302**	-0.063	-0.309**	-0.033
		(0.063)	(0.078)	(0.118)	(0.168)	(0.123)	(0.167)
Trade Openness ((X+M)/GDP)		0.121	0.101	0.077	1.123***	0.114	1.136***
		(0.097)	(0.085)	(0.124)	(0.416)	(0.138)	(0.430)
Financial Openness (0=low to 14=high)		-0.054**	0.048	-0.005	0.061	0.007	0.040
		(0.026)	(0.035)	(0.037)	(0.049)	(0.036)	(0.049)
International Reserves (in month of imports)		0.041***	0.017	0.032*	0.025	0.032*	0.026
		(0.014)	(0.017)	(0.018)	(0.031)	(0.018)	(0.032)
Feasibility (Global % of countries fixed)[3]		1.211***	1.530	1.344**	0.919	1.351*	5.973*
		(0.437)	(1.134)	(0.588)	(2.009)	(0.754)	(3.229)
Government Crisis (Count)		0.032	-0.038	-0.078	0.013	-0.072	0.005
		(0.093)	(0.084)	(0.120)	(0.097)	(0.120)	(0.108)
Inflation differential (Country-World, logged and lagged)		-0.213	0.448	-0.413	0.767		
		(0.261)	(0.590)	(0.347)	(0.978)		
Respective Lagged DV[4]		1.236***	2.764***	2.639***	3.483***	2.727***	3.492***
		(0.072)	(0.162)	(0.151)	(0.164)	(0.142)	(0.164)
Constant				-1.399*	-3.005*	-1.583*	-6.678***
				(0.759)	(1.813)	(0.941)	(2.515)
Observations		1531	1114	1497	1114	1456	1090
Log likelihood		-896.72	-432.79	-364.50	-160.58	-340.85	-157.35
Pseudo R-squared		0.46	0.69	0.64	0.77	0.65	0.77
Regional Fixed Effects				x	x	x	x

[1] Models 1-2 ordered and 3-6 binary logit coefficients shown; robust standard errors, adjusted for potential within country clustering, in parentheses. Following Broz's original specification, model's 1-2 are estimated using ordinary robust (i.e. unclustered) standard errors only. Models 3-6 include a full set of regional dummies according to the official world bank classification (coefficients not shown). * significant at 10%; ** significant at 5%; *** significant at 1%. For detailed definitions of all variables apart from the diffusion proxies cf. Broz 2002.

[2] Fixed Inflation Advantage: $FIA = [\text{Mean}(LN(INF))_{\text{countries with floating ER, t-1}}] - [\text{Mean}(LN(INF))_{\text{countries with fixed ER, t-1}}]$. Models include different FIA measures depending on which ER coding is used as the model's dependent variable (cf. table 2).

[3] Models include different feasibility measures depending on which ER coding is used as the model's dependent variable. Estimates are presented in a single row to economize on space.

[4] Models include different lagged dependent variables depending on which ER coding is used as the model's dependent variable. Estimates are presented in a single row to economize on space.

Table 4: The Full Transition Model: Exchange Rate Choice in the Global Sample: Domestic Institutions vs. Policy Diffusion

Dependent Variable	**Reference Group: Global (weighted)**						**Reference Group: Region (weighted)**					
	Binary ER-Regime Measure:0=float 1=fix						*Binary ER-Regime Measure:0=float 1=fix*					
Source	Broz 2002			Reinhard & Rogoff 2004			Broz 2002			Reinhard & Rogoff 2004		
Model No.[1]	1			2			3			4		
ER-Regime at y_{t-1}	Float		Fix	Float		Fix	Float		Fix	Float		Fix
Parameter estimate	β_0	γ	β_1	β_0	γ	β_1	β_0	γ	β_1	β_0	γ	β_1
Independent Variables:												
Domestic Institutions												
Polity score (Broz)	-0.030**	0.016	-0.014	0.048**	-0.086***	-.038**	-0.024*	0.004	-.020	0.056***	-0.110***	-.054***
	(0.013)	(0.018)	(.013)	(0.020)	(0.020)	(.015)	(0.014)	(0.019)	(.013)	(0.021)	(0.022)	(.020)
Learning[4]												
Contextual Social Learning Global	-0.124	0.442***	.318***	-0.357**	0.445***	.088						
	(0.080)	(0.118)	(.090)	(0.155)	(0.162)	(.058)						
Contextual Social Learning Regional							-0.167**	0.449***	.282***	-0.320*	0.465**	.145**
							(0.084)	(0.110)	(.066)	(0.175)	(0.200)	(.066)
Covariates:												
Wealth (per capita GDP)	-0.049**	0.085***	.036	-0.091**	0.043	-.048	-0.067***	0.121***	.054*	-0.119**	0.084*	-.035
	(0.020)	(0.031)	(.030)	(0.043)	(0.040)	(.033)	(0.022)	(0.032)	(.031)	(0.047)	(0.044)	(.036)
Size (LN GDP)	-0.196	-0.303	-.499	-0.140	0.227	.087	-0.242	-0.244	-.486***	-0.073	-0.087	-.160
	(0.149)	(0.220)	(.166)***	(0.247)	(0.340)	(.240)	(0.156)	(0.223)	(.165)	(0.274)	(0.319)	(.245)
Trade Openness ((X+M)/GDP)	-0.144	0.214	.070	0.807*	0.520	1.327*	-0.266	0.365	.099	0.972**	0.028	1.000
	(0.327)	(0.400)	(.179)	(0.431)	(0.884)	(.786)	(0.392)	(0.472)	(.198)	(0.442)	(0.870)	(.830)
Financial Openness (0=low to 14=high)	0.069	-0.119	-.050	0.026	-0.036	-.010	0.091	-0.143*	-.052	0.016	-0.027	-.011
	(0.068)	(0.079)	(.044)	(0.070)	(0.113)	(.076)	(0.074)	(0.086)	(.044)	(0.077)	(0.124)	(.084)
International Reserves (in month of imports)	-0.028	0.084	.056*	0.069	-0.060	.009	-0.020	0.079	.059*	0.047	0.005	.052
	(0.035)	(0.054)	(.032)	(0.044)	(0.063)	(.038)	(0.035)	(0.054)	(.032)	(0.050)	(0.063)	(.040)
Feasibility (Global % of countries fixed)[2]	0.688	1.963*	2.651***	7.729**	-7.255	.474	0.617	1.793	2.41***	4.857	-1.147	3.710
	(1.007)	(1.191)	(.729)	(3.573)	(4.557)	(2.85)	(1.021)	(1.220)	(.717)	(3.343)	(4.184)	(2.85)
Government Crisis (Count)	0.283	-0.496**	-.213*	-0.885**	1.294**	.409	0.288	-0.485**	-.197	-0.765*	1.127**	.362
	(0.195)	(0.241)	(.127)	(0.427)	(0.546)	(.319)	(0.191)	(0.238)	(.130)	(0.427)	(0.554)	(.312)
Constant	3.182**	-1.823*	1.359	7.749***	-7.095***	.654	3.019**	-1.420	1.599*	4.938	-5.154**	-.216
	(1.310)	(1.047)	(.989)	(2.875)	(2.574)	(2.19)	(1.381)	(1.098)	(.995)	(3.122)	(2.542)	(2.58)
Observations		1456			1090			1453			1026	
Log likelihood		-311.79			-147.42			-312.56			-130.87	
Pseudo R-squared		0.68			0.78			0.68			0.80	
Regional fixed effects		X			X			X			X	
Joint Test of Null of Equal Slopes (H_0: γ=0)												
χ^2 (9 df)		66.36			37.67			63.83			47.39	
Prob > χ^2		0.000			0.000			0.000			0.000	

[1] Markov Transition Model estimated with a probit specification. Probit coefficients shown; robust standard errors, adjusted for potential within country clustering, in parenthesis. The parameter estimate for γ contrasts the effects of each covariate across states: probit[Pr($\mathbf{y}_{it}$ =1) | $\mathbf{y}_{i,t-1}$ = j] = $\mathbf{x}_{it}$ $\boldsymbol{\beta}_j$ with $\boldsymbol{\beta}_1 = \boldsymbol{\beta}_0 + \boldsymbol{\gamma}$. All models include a full set of regional dummies according to the official world bank classification (coefficients not shown) * significant at 10%; ** significant at 5%; *** significant at 1%. For detailed definitions of all variables apart from the diffusion proxies cf. Broz 2002.

[2] *Contextual Social Learning* = [weighted mean $(LN(INF))_{\text{countries with a different ER, t-1}}$] - [$LN(INF)_{\text{country i, t-1}}$]. Models include different CSL measures depending on which ER coding is used as the model's dependent variable.

[3] Models include different feasibility measures depending on which ER coding is used as the model's dependent variable. Estimates are presented in a single row to economize on space.

[4] Models include different lagged dependent variables depending on which ER coding is used as the model's dependent variable. Estimates are presented in a single row to economize on space.

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