Democracy and Floating Exchange Rates

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International Political Science Review 2008 29: 73
DOI: 10.1177/0192512107083448

International Political Science Review (2008), Vol. 29, No. 1, 73–98

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Abstract. A number of articles in the past few years have found that democracies are more likely to commit to a floating exchange rate regime. I argue that we do not have a solid understanding of the causal mechanism that explains why democracies would float more often. I test a variety of hypotheses to explore exactly what features of democratic practice might account for the propensity to declare a float, using two different datasets. While the tests are not conclusive, they suggest that the number of veto players or the regular use of open, competitive elections may influence exchange rate commitments.

Keywords: • Democracy • Exchange rates • Exchange rate regimes • Transparency • Veto players

Introduction

A number of recent studies on the determination of exchange rate regimes have concluded that democracies are more likely to have de jure floating exchange rate regimes simply because they are democracies.[1] Even when controlling for economic conditions that may be associated with democracy, such as higher levels of GDP and exposure to trade, democracy seems to have an independent effect that encourages a government to declare that it is floating. In many of these tests, examined below, democracy is a control variable in studies of exchange rate regimes in which the data incorporate developing countries. This has led many scholars to neglect theorizing about why democracy matters to the choice of exchange rate regimes while they investigate other determinants. J. Lawrence Broz (2002), however, has articulated a theory as to why democracy promotes floating. In his view, the transparency of democratic institutions allows them to employ independent central banks to fight inflation, while more opaque dictatorships can only rely on fixed exchange rates to contain inflation. I argue below, though, that this proposition has difficulties in its theoretical logic and its application to the experiences of policymaking in developing countries.

DOI: 10.1177/0192512107083448
Sage Publications (Los Angeles, London, New Delhi, and Singapore)

If so, we are left with a puzzle. Why does democracy seem to make a government more likely to declare that it has a floating exchange rate? The question is important for understanding how domestic political institutions affect exchange rate regime choices. Recent political economy research has made significant strides in explaining how different forms of democratic institutions (majoritarian versus proportional representation [PR] and exogenously versus endogenously timed elections, for example) influence exchange rate regime choices in advanced industrial democracies.[2] We have less research, however, on understanding how domestic political institutions affect exchange rate regime choices among developing countries and what the differences in institutions are between developed and developing countries.

Moreover, democracy is clearly a crucial concept in political science and has been the subject of much research on how it affects international relations and policy outcomes. In many of these studies, a dichotomous indicator or an index of democracy is found to correlate with the dependent variable. Democracy, however, is a complex set of integrated rules and practices. Contemporary research rarely asks what specific democratic rules and practices account for the influences we observe. Consequently, we run the risk of using democracy as a vague explanation for a broad range of phenomena without understanding the specific underlying causal mechanisms. Even when the researcher specifies the causal mechanism in the development of the theory, current democracy indicators may not be specific enough to test whether it is the specific mechanism at work or some other. In this article, I explore what indicators of specific democratic rules and practices may account for why democracies float more often. This allows for more specific tests of Broz's theory and related theories of the effects of institutions on exchange rate regimes. In the process, we also learn more about why democracy has the policy effects that it has.

I present two sets of statistical tests on two different datasets, one of which employs Broz's original data. While the tests are not conclusive, some tests imply that governments with moderate numbers of veto players or several veto players may encourage floating. Some tests also imply that the mere presence of open, competitive elections may also encourage floating. The tests admittedly do not establish complete support for any particular causal mechanism, but they do establish the need for more theoretical development on this question and the most fruitful avenues for further research. In the next section, I argue that we have many examples of statistical correlations between democracy indicators and floating, but existing theories are not adequate to explain them. The third section outlines different theories and hypotheses that could plausibly explain what specific features of democracy are capable of encouraging a de jure float. The fourth and fifth sections display the results of tests on these hypotheses on two different datasets. The last section discusses what further research may be needed.

The Puzzle

As part of a project on the political economy of monetary institutions, Broz (2002) proposed that the transparency of political institutions influences the government's choice of exchange rate regime. The project sought to create a new generation of political economy research that examined how choices concerning central banking

and exchange rate regimes might be made simultaneously as coherent strategies to contain inflation (Bernhard et al., 2003). The project based its theories on the time-inconsistency problem, in which governments must avoid the temptation to stimulate growth in the short run in order to restrain inflation in the long run. Broz's theoretical contribution rested on four claims. First, states require some sort of monetary commitment strategy to fight inflation credibly. Second, states choose one of two rival anti-inflation strategies: central bank independence (CBI) or a fixed exchange rate. Third, people can monitor commitments to a fixed exchange rate more easily than commitments to delegate authority to a central bank. Fourth, central banks only have credible independence in a society with transparent decision-making. According to Broz, "transparency is the ease with which the public can monitor the government with respect to its commitments" (2002: 861). Broz consequently argues that only democracies, being more transparent, have the option of using credibly independent central banks to fight inflation, while dictatorships must rely more often on fixed exchange rates for a credible commitment that the public can monitor.

Broz then presents ordinal probit tests on a dataset with annual data on 152 developed and developing countries during the period from 1973 to 1995. As his indicator for democracy, he uses the POLITY2 score developed for the Polity IV database, which measures regime type on an index ranging from –10 (the least democratic) to 10 (the most democratic). He finds that democracy correlates with a greater chance of a de jure float and that the indicator is significant at the 0.01 level. This finding is consistent with other research on exchange rate regime choice. Jeffry Frieden, Piero Ghezzi, and Ernesto Stein (2001) run cross-sectional ordinal logit tests on Latin American data and also find significance for a dummy variable based on Polity indicators. George Shambaugh (2004) runs cross-sectional, time-series logit tests on different annual data on developing countries and also finds significance for the Polity indicator. Using quarterly data on a dataset of 65 developing countries, I ran cross-section, time-series logit tests (Hall, 2006) and found the Polity indicator is associated with a lower likelihood of pegging. Frieden et al. (2001), Shambaugh (2004), and Hall (2006) use regime type as a control variable in these tests, however, and do not venture to explain why democracy has this effect. The effect of the Polity democracy indicators on de jure exchange rate regimes, however, is robust with respect to different forms of data, modeling, case selection, and control variables.[3]

Some recent research argues that democracy also has an influence on de facto exchange rate regimes (which reflect the actual behavior of exchange rate movements), but less research has been performed on this question and there is less consensus on it. Beth Simmons and Jens Hainmueller (2004) have tested the effect of democracy and other domestic politics variables on de facto exchange rate regimes and found weak or nonexistent influences. David Bearce and Mark Hallerberg (2006), however, perform tests using Broz's (2002) data and find that the POLITY2 indicator does correlate with de facto floating. What implications this has for the relationship between democracy and de jure exchange rate regimes is not yet clear.

We only have Broz's transparency theory, then, to explain the relationship between democracy and de jure floating. Broz's theoretical assumptions, however, have three problems when applied to actual cases in developing states, problems that imply that Broz may have identified the wrong causal mechanism. First, the

theory assumes that all governments seek to reduce inflation over the long run. In several cases, developing countries have instead focused on short-run economic objectives, such as the use of an inflation tax to compensate for an inefficient tax system (Vegh, 1989), the reduction of debt service costs (Calvo, 1992), or the redistribution of income to a populist political base (Dornbusch and Edwards, 1991). The result has often been periods of hyperinflation (as in Argentina, Brazil, Peru, Romania, or Russia) or longer periods of chronic inflation (as in Colombia or Turkey). In such cases, both exchange rate pronouncements and the central bank may simultaneously lack credibility. Over the long run, governments may introduce austerity measures to fix inflationary or current account crises, but it may take years for a government to do so. In the meantime, other factors besides anti-inflation credibility determine exchange rate and monetary policy choices.

Second, it is not clear that governments do treat fixed exchange rates and independent central banks as substitutes for fighting inflation. When Bernhard et al. (2003) introduce the rationale for the time-inconsistency approach, they examine the stylized facts on exchange rate regimes and central banking for a large sample of developed and developing countries. Just in excess of 26 percent of their sample includes states that both employed a fixed exchange rate for longer than average and granted relative independence to their central banks simultaneously. Nor are fixed exchange rates and CBI mutually exclusive in theory. According to the Mundell-Fleming model, governments exposed to capital mobility with a fixed exchange rate can still manage the economy with fiscal policy, while a central bank, if it is politically independent of the government, would simply adapt its interest rates to transnational capital flows (Goodman, 1992). In short, states are not limited to a choice of either central bank independence or fixed exchange rates. They may choose both or, in the case of some high-inflation countries, neither.

Third, income policy represents a third strategy for fighting inflation in some developing countries. The Mexican government under the Salinas administration, for example, negotiated a pact in which the government promised business and labor representatives to adhere to a credible crawling peg in exchange for restraint on wage and price increases (Gould, 1996: 25; Lustig, 1998: 51). Such tripartite agreements are common ingredients in stabilization programs in Latin America and other developing countries, although they have not seen as much success in Eastern Europe (International Labour Organization, 1997).

Aside from Broz, not many have attempted to theorize about why democracies float. It is not clear that Broz's theory has adequately captured the causal mechanism responsible, however. We have several tests to confirm that democracy encourages de jure floating, but not necessarily a clear idea why. Part of the problem is that "democracy" is a complex set of integrated rules and practices, with several features that might plausibly have an effect on exchange rate regime choice. The transparency of democratic government may not be the primary reason for the correlation we observe. What follows is an attempt to narrow down what it is about democracy that may encourage floating in order to specify what the next steps in research should be. The next section identifies different theories emphasizing different, specific features of democracy that might encourage floating. The following sections test the subsequent hypotheses of those different theories on two different datasets. One dataset is Broz's, in order to see how indicators of specific features of democracy perform when substituted for POLITY2 in Broz's

original tests. Another dataset uses quarterly data on developing countries only, with a different set of control variables, to provide some indication of how robust the results are.

Competing Theories and Hypotheses

I test three theoretical approaches as to why democracies float more often. The first theory is Broz's, which emphasizes the transparent nature of democratic decision-making. Broz uses two proxy indicators for political system transparency in his research, the POLITY2 indicator and the Freedom House index for civil liberties. The civil liberties indicator represents a judgment by experts of how free expression and media are in a society, measured on a scale from one (highest) to seven (lowest). This captures, in part, the ability of citizens to monitor government behavior and use that information in deciding their economic reactions. Broz himself states that "the civil liberties index is slightly closer than [POLITY2] to my conception of political transparency" (2002: 877). The POLITY2 index and its components capture an element of how well political opposition can question the government's treatment of the central bank (Broz, 2002: 877), but mostly POLITY2 is a measure of political institutionalization that is statistically correlated with transparency, but not identical to it. Broz only used the civil liberties indicator, however, in tests designed to measure the effectiveness of CBI on inflation rates, not on tests of exchange rate regime choice. Here, I use the indicator to test the transparency hypothesis on exchange rate regime choice. Since the values of the indicator decrease as the political system becomes more transparent, the parameter estimates should have a positive sign.

The second theory emphasizes the open, competitive nature of acquiring office in a democracy. Susan Collins (1996: 120) notes that with a fixed exchange rate any devaluation of the currency is identifiable as a government action. With a floating currency, depreciation could cause a loss of purchasing power, but it is more difficult for voters to identify whether currency market activity or government policy is responsible. Shambaugh (2004: 287–8) argues that democratic governments would have an incentive to opt for floating on the grounds that it gives voters less opportunity to blame the government for the loss of purchasing power. This argument is similar to Broz's in that it depends on how well voters can monitor the commitments and behavior of governments. Note three differences between the arguments, however. First, Shambaugh's theory is only concerned with a government's commitments to the purchasing power of domestic currency relative to foreign currencies, not its commitment against inflation. Consequently, what distinguishes democracies from dictatorships in this argument is not the government's ability to commit to the independence of the central bank. Third, what does distinguish democratic decision-makers here is their exposure to voter disapproval. For this argument, then, the ability of voters to monitor government relations to the central bank through open debate and free media is not the crucial indicator. Instead, what is crucial is the degree to which decision-makers are exposed to open, competitive electoral competition. The theory yields two hypotheses on what encourages floating:

The Openness Hypothesis: The more opportunity that all citizens have, in principle, for becoming the executive, the more likely a government is to float.

The Competitiveness Hypothesis: The more that a system uses competitive elections to select an executive, the more likely a government is to float.

The POLITY2 index contains indicators of how open and competitive the selection of decision-makers is. The approach of the Polity IV database is to meas-ure democracy as an index of four components: XROPEN (the openness of the executive recruitment process), XRCOMP (the competitiveness of the executive recruitment process), XCONST (constraints on the executive), and PARCOMP (how open the system is to participation from citizen groups). Each component is given a score on a different scale representing the judgment of coding experts (see Marshall and Jaggers, 2005). The four components are added together to yield a total democracy score ranging from zero to 10. A separate 0–10 score is also calculated for the degree of autocracy in each country, which is a composite index of all of the variables listed above plus another variable representing the degree to which political participation is regulated. POLITY2 is simply the democracy score minus the autocracy score.

One can test Shambaugh's theory by using XROPEN and XRCOMP as indicators of the extent to which decision-makers must be concerned with voter disapproval. Each indicator becomes higher in value the more open or competitive it is. Since the dependent variables in the tests are assigned higher values with greater degrees of exchange rate fixity, the parameter estimates for the indicators should have negative signs if the hypotheses are correct. Testing the XROPEN and XRCOMP indicators has the added benefit of helping us investigate what parts of the POLITY2 indicator might also be responsible for the correlations between POLITY2 and floating that we see in so many studies. That is why the results for tests on all four of the components of POLITY2 are reported in tables below.

The third approach to hypothesis testing is based on recent theories of what effect the number of veto players in a political system has on exchange rate regimes, exchange rate volatility, and speculative attacks. Democracies often have more veto players than dictatorships and so the literature on veto players may provide additional insights. Scholars, however, make diverse claims about the effects of veto players. One can posit a variety of hypotheses about how many veto players encourage floating, or whether veto players condition exchange rate regime choices at all, such as the following:

The Diffusion Hypothesis: More veto players make a government more likely to float.

The Concentration Hypothesis: Fewer veto players make a government more likely to float.

The "Goldilocks" Hypothesis: A moderate number of veto players, as opposed to relatively more extreme numbers will make a government more likely to float.

The Null Hypothesis: Veto players have no effect on exchange rate regimes.

Philip Keefer and David Stasavage (2002: 757–8) argue for the null hypothesis on the grounds that the number of veto players will not lend any anti-inflation credibility to fixed exchange rate commitments. Veto players will not improve the credibility of a peg if the inflation rate of the anchor currency is lower than any of the domestic veto players prefer, if the executive sets exchange rate policy without input from legislative veto players, or if veto players make it more difficult to respond to economic shocks that threaten the peg. Since few cases avoid all

of these conditions, they predict that tests will show no correlation between the number of veto players and fixed exchange rates.

These arguments apply if one assumes the government is primarily concerned with anti-inflation credibility. Governments may also be concerned with exchange market volatility and investor confidence, however. One could start with the argument that a political system with many, diffuse veto players could make a government's commitments to property rights, contract enforcement, and liberal regulatory policies more credible (Henisz, 2000b). Credibility for maintaining a friendly business environment would presumably encourage foreign investment and reduce volatility in currency markets regardless of which exchange rate regime the government followed. To the extent that the political decision-makers face little resistance in their ability to alter the business environment, however, investors may attribute greater risk to the country and its currency. If a greater number of veto players reduces risk and volatility in currency markets, then democracies, which tend to have more veto players, would have less reason to exhibit "fear of floating" (Calvo and Reinhart, 2002) and less reason to resort to fixed exchange rates to reduce volatility.

Despite this latter argument, however, most of the recent scholarship argues or implies that it is the concentration, not the diffusion, of veto players that is more likely to encourage floating. Hallerberg (2002) argues that parliamentary governments choose exchange rate regimes based on how well voters can identify specific political parties as being in control of the policies that provide benefits. Single-party governments will favor floating so that voters can reward them for stimulating monetary policy. Governments run by party coalitions will favor fixed exchange rates as voters will find it easier to identify which party provided which benefits through fiscal policy instead. Bernhard and Leblang (1999) argue that politicians choose exchange rate regimes with an eye toward re-election. In single-party majoritarian governments, politicians would favor floating to stimulate monetary policy before elections. In multiparty proportional representation systems, however, parties are more likely to focus on a transparent fixed exchange rate to reduce conflicts within the coalition and allow all parties in the coalition a chance to monitor exchange rate decisions.

Hallerberg, Bernhard, and Leblang are mostly concerned with parliamentary democracies, which cannot be taken for granted in non-European polities. David Leblang and Shanker Satyanath (2006), however, develop an argument concerning the effects of veto players on exchange rate crises that is more applicable to a wide range of political systems. They argue that in systems with more veto players, different power centers are more likely to propose divergent forecasts of economic fundamentals, eroding the credibility of government signals and creating more uncertainty among investors, which makes the country more susceptible to speculative attack. According to tests on two of their three different datasets, divided democracies experienced speculative attacks more often. These results challenge the notion that a larger number of veto players will improve credibility with investors and reduce fear of floating. If it is true that a single veto player is more likely to float, however, this makes the correlation between democracies, which often have more than one veto player, and floating even more puzzling.

It is theoretically possible to reconcile the divergent views of how veto players affect exchange rate volatility and speculative attacks, however. The number of veto players may simply not have a linear effect on exchange rate regime choices.

Andrew MacIntyre's (2003) case studies on Southeast Asia suggest a third possibility, that the relationship between veto players and economic policy is, instead, curvilinear. In his view, political systems with power concentrated in relatively few veto players lack credibility in policy commitments. When power is diffused among relatively many veto players, the system is too rigid to respond to policy problems or crises. States with moderate numbers of veto players are more likely to provide sound economic governance. Jude Hays, John Freeman, and Hans Nesseth (2003) analyze Markov switching models applied to daily exchange rate data in four Southeast Asian countries. They argue that in democratic states, news concerning elections and coalition politics is likely to produce contagion in financial markets. Their results are consistent with MacIntyre's thesis, in that young democracies are prone to having relatively few or relatively many veto players, and the resulting fluidity of election and coalition politics in young democracies is more likely to produce exchange rate volatility.

The concept of curvilinearity could be extended to the theory of exchange rate regime choice to form a "Goldilocks" hypothesis. If a state has few veto players, it loses credibility in its commitments to maintaining a favorable business climate. If a state has several veto players, it loses credibility in its economic forecasts and the ability to respond decisively to exchange rate misalignments. Both cases could be prone to exchange rate volatility. States with moderate numbers of veto players, on the other hand, should provide the most favorable governance and experience the least volatility in currency markets. This reduced volatility should lead states with moderate numbers of veto players to "fear floating" the least. Consequently, relatively more mature and stable democracies should have the most incentive to float.

The tests reported here measure the number of veto players in a political system using the logarithm of the CHECKS1 indicator in the World Bank's *Database of Political Indicators*, Witold Henisz's (2000a, 2000b) POLCONV indicator, and the XCONST indicator from the Polity IV database.[4] CHECKS1 represents the number of veto players in the system. The tests use the logarithm of CHECKS1 as the effects of a veto player diminish with the addition of each veto player. Tests for the concentration and diffusion hypotheses use these indicators as monotonic (or linear) terms. If the log of CHECKS1 is positive, it supports the concentration hypothesis, while if the log of CHECKS1 is negative, it supports the diffusion hypothesis. XCONST, another measure of checks on the executive, should behave similarly to the log of CHECKS1 in tests. If the coefficient of XCONST is positive, it supports the concentration hypothesis and if the coefficient of XCONST is negative, it supports the diffusion hypothesis. The POLCONV indicator also yields similar predictions since the indicator is zero if the executive has complete discretion and one if the executive faces complete constraint. For these tests, however, Henisz's POLCONV indicator was adjusted by adding one to all of the scores, yielding scores ranging from one to two. When calculating his index, Henisz takes the divergence of preferences of governing groups into account as well as the number of groups with veto abilities, and so his index is arguably closer to George Tsebelis' (2002) original theory of veto players than CHECKS1 or XCONST are. Tests for the Goldilocks hypothesis use equations with the log of CHECKS1 and POLCONV indicators in quadratic form, combining squared and unsquared terms for the same indicator. If the Goldilocks hypothesis is correct and states with moderate numbers of veto players are more likely to float, the

squared CHECKS1 indicator should be positive, while the unsquared CHECKS1 indicator in the same equation should be negative, yielding a U-shaped curve. With an adjusted POLCONV indicator, such tests should yield similar results.

Broz's Data and Results

The first set of tests uses the same data and methods that Broz used in his original article, only substituting the indicators listed above for his POLITY2 indicator. Broz's data are cross-sectional, annual, time-series data covering 152 countries from the period 1973–95. The dependent variable is coded as an ordered categorical variable, in which four equals fixed, three equals limited flexibility, two equals managed floating, and one equals independent floating. Broz tests his hypothesis with ordinal probit equations using robust standard errors. His control variables include a lagged dependent variable, GDP per capita, the log of GDP, trade openness (export plus imports divided by GDP), the differential between a country's inflation rate and the world rate, and the number of months foreign exchange reserves can purchase imports. Dennis Quinn's (1997) 14-point index of capital controls measures financial openness. Political controls include "feasibility" (the percentage of countries that use a pegged exchange rate) and a count of government crises that could bring the downfall of the current government (see Banks, 1994). Using Broz's data and methods I was able to replicate the findings for the three tests he reports in his article.

I then substitute my alternative indicators for his POLITY2 indicator. Table 1 lists the indicators used in the tests and how the units are measured for each indicator. Table 1 also reports the expected signs for each indicator and whether the tests using Broz's data support these expectations. Tables 2 and 3 report the findings. In Table 2, equation 1-A uses Freedom House's civil liberties indicator, which tests Broz's transparency hypothesis. Equations 2-A and 3-A test the veto players hypotheses with the log of the CHECKS1 indicator, while equations 4-A and 5-A test the same hypotheses with POLCONV. Equations 2-A and 4-A test the concentration and diffusion hypotheses, while equations 3-A and 5-A test the Goldilocks hypothesis. Equations 6-A and 7-A test the civil liberties and unsquared and squared veto players indicators in the same equation to compare the efficacy of the indicators against each other. The correlations between civil liberties and the veto players indicators are relatively lower than correlations involving the components of Polity IV (see Table 6), yielding less trouble with multicollinearity than would be the case if other combinations of democracy indicators were used.

Interestingly, the civil liberties indicator is positive as expected, but is not significant, providing no confirmation for the transparency hypothesis. When tested as a linear function, the veto players indicator in equation 2-A is negative, but not significant. The veto players indicator is negative in equation 4-A and significant at the 0.05 level there. In equation 4-A, a shift from the 25th to the 75th percentile of POLCONV values increases the likelihood of managed floating (dv = 2) by an average of 8.3 percent and reduces the likelihood of fixing (dv = 4) by an average of 9.6 percent.[5] The squared CHECKS1 indicator in equation 3-A, however, is significant at the 0.05 level and is positive as expected, while the unsquared indicator is negative as expected and also significant.[6] Figure 1 displays the predicted probabilities of a managed float for the CHECKS1 indicator in

TABLE 1. *Indicators*

Indicator	Unit of measurement	Expected sign	Support from Broz's data?	Support from author's data?
Civil liberties	Freedom House's index from 1–7	+	No	No
Veto players	Log of World Bank's count of "checks" in political institutions	+ or –	No	No
	Henisz's index of political constraints from 0–1	+ or –	Yes (–)	Yes (–)
Veto players squared	Square of World Bank's logged count of "checks" in political institutions	+ (sq) – (unsq)	Yes	Yes
	Square of Henisz's index of political constraints from 0–1	+ (sq) – (unsq)	No	No
XRCOMP	Polity IV's index from 0–3	–	Yes	No
XROPEN	Polity IV's index from 0–4	–	Yes	No
XCONST	Polity IV's index from 1–7	[+ or –]	[Yes(–)]	[Yes (–)]
Broz's control variables				
GDP per capita	Unlogged GDP per capita in thousands of constant US dollars	–	Yes (+)	
Log of GDP	Log of GDP in constant US dollars	–	Yes	
Trade openness	Exports plus imports as a percentage share of GDP	+	Yes	
Inflation differential	Lagged and logged absolute difference between domestic and world inflation rates	– (OCA)	No	
Financial openness	Quinn's (1997) index of capital controls from 0–14	+	Yes (–)	
International reserves	Reserves of foreign exchange in months of imports	+	Yes	
Feasibility	Percentage of states in the world with pegs	+	Yes	
Government crises	Banks' (1994) count of any situation that threatens to bring down the present regime	–	No	

Author's control variables			
Log of GDP	Log of GDP in constant US dollars	–	Yes
Log of GDP per capita	Logged GDP per capita in thousands of constant US dollars	–	No
Trade exposure	Exports plus imports as a percentage share of GDP	+	No
International reserves	Reserves of foreign exchange in months of imports	+	No
Log of inflation	Log of one plus the percentage change in the Consumer Price Index over the past four quarters	– (OCA)	No
Capital controls	Index of capital controls from 0–4	+	No
Manufacturing sector	Percentage of GDP attributed to the manufacturing sector	–	No
Public-sector debt	Publicly guaranteed debt as a percentage of GDP	–	Yes
Private-sector debt	Private, non-guaranteed debt as a percentage of GDP	–	No
Foreign liabilities	Foreign liabilities as a percentage of M2	+	Yes
Elections	Dummy variable for presence of an election	+	No

TABLE 2. *Tests on Broz's Data*

	1-A	2-A	3-A	4-A	5-A	6-A	7-A
	Transparency	Veto players (linear)	Veto players (quadratic)	Veto players (linear)	Veto players (quadratic)	Combined	Combined
	Freedom House	World Bank	World Bank	Henisz	Henisz	World Bank	Henisz
Lagged dv	1.2969***	1.2985***	1.2919***	1.2831***	1.2818***	1.2897***	1.2805***
	(0.0711)	(0.0706)	(0.0708)	(0.0715)	(0.0716)	(0.0711)	(0.0718)
Civil liberties	0.0302					0.0128	0.0075
	(0.0254)					(0.0294)	(0.0358)
Veto players		–0.0655	–0.4237***	–0.3241**	–1.9028	–0.3947**	–1.7859
		(0.0625)	(0.1569)	(0.1477)	(1.7805)	(0.1764)	(1.8053)
Veto players squared			0.2075***		0.5788	0.1978**	0.5470
			(0.7412)		(0.6475)	(0.0795)	(0.6493)
GDP per capita	0.0218**	0.0183**	0.0173**	0.0236***	0.0206**	0.0190**	0.0212**
	(0.0965)	(0.0083)	(0.0081)	(0.0091)	(0.0092)	(0.0094)	(0.0098)
Log of GDP	–0.2648***	–0.2334***	–0.2396***	–0.2258***	–0.2251***	–0.2428***	–0.2280***
	(0.0522)	(0.0542)	(0.0544)	(0.0594)	(0.0595)	(0.0548)	(0.0602)
Trade openness	0.1768*	0.1892**	0.1813**	0.1856**	0.1898**	0.1775*	0.1862*
	(0.0950)	(0.0932)	(0.0920)	(0.0943)	(0.0937)	(0.0939)	(0.0965)
Inflation	–0.2279	–0.2374	–0.2321	–0.2157	–0.2029	–0.2246	–0.2013
	(0.2694)	(0.2685)	(0.2674)	(0.2647)	(0.2624)	(0.2675)	(0.2623)

Financial openness	–0.0525** (0.0231)	–0.0620*** (0.0240)	–0.0598** (0.0238)	–0.0528** (0.0234)	–0.0520** (0.0234)	–0.0573** (0.0236)	–0.0512** (0.0233)
International reserves	0.0340** (0.0134)	0.0304** (0.0130)	0.0298** (0.0129)	0.0359*** (0.0136)	0.0354*** (0.0137)	0.0313** (0.0132)	0.0357*** (0.0137)
Feasibility	1.0760*** (0.3608)	0.9949*** (0.3643)	0.9962*** (0.3647)	0.8985** (0.3704)	0.9158** (0.3713)	1.0297*** (0.3692)	0.9481** (0.3852)
Government crises	0.0132 (0.0885)	0.0124 (0.0881)	0.0035 (0.0892)	0.0130 (0.0883)	0.0157 (0.0886)	0.0056 (0.0898)	0.0168 (0.0890)
Obs (n)	1827	1768	1768	1701	1701	1765	1698
Wald Chi^2	648.44	632.24	634.17	604.01	604.38	641.16	608.76
Prob > Chi^2	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000

Notes: Standard errors in parentheses.
* = Significant at the 0.10 level.
** = Significant at the 0.05 level.
*** = Significant at the 0.01 level.

TABLE 3. *Tests on Broz's Data*

	8-A	9-A	10-A	11-A
	Competitiveness	Openness	Constraints	Participation
Lagged dv	1.281686***	1.291706***	1.296585***	1.300948***
	(0.748946)	(.0744391)	(.0755772)	(.0754433)
XRCOMP	– .1136558***			
	(.0402745)			
XROPEN		–.0830667***		
		(.0322698)		
XCONST			–.0450337**	
			(.0180132)	
PARCOMP				–.0555556*
				(.031011)
GDP per capita	.0228858**	.0173772*	.0226111**	.022246**
	(.0103864)	(.0100275)	(.0104171)	(.010613)
Log of GDP	–.2321853***	–.2141933***	–.2378103***	–.237575***
	(.0648223)	(.0659595)	(.0651626)	(.0652097)
Trade openness	.1675852*	.1880378**	.1386825	.1368819
	(.0946102)	(.0944365)	(.0961066)	(.096732)
Inflation	–.1625473	–.1878753	–.2070846	–.2244011
differential	(.2690676)	(.2720009)	(.2680236)	(.2688638)
Financial	–.0519961**	–.057942**	–.0534455**	–.0518018*
openness	(.0254016)	(.0258111)	(.0265112)	(.0267663)
International	.0407036***	.035036***	.0439403***	.0425443***
reserves	(.0124662)	(.01239)	(.0124899)	(.0125163)
Feasibility	1.107786***	1.11948***	.9356938***	.9569522***
	(.3648357)	(.3651288)	(.3580467)	(.3564009)
Government	.0157861	–.0085094	.0021394	–.0113668
crises	(.099102)	(.0879125)	(.0901317)	(.0882655)
Obs (*n*)	1613	1613	1616	1616
Wald Chi^2	586.02	562.20	568.12	553.32
Prob > Chi^2	0.0000	0.0000	0.0000	0.0000

Notes: Standard errors in parentheses.
* = Significant at the 0.10 level.
** = Significant at the 0.05 level.
*** = Significant at the 0.01 level.

this equation. The probability of a managed float is highest when a state has two to four veto players. In equation 5-A, the squared and unsquared POLCONV indicators are also the correct signs. Both veto players indicators fail to achieve significance individually in equation 5-A.[7] These tests provide no confirmation of the concentration hypothesis. Tests with the CHECKS1 indicator only confirm the Goldilocks hypothesis and tests with the POLCONV indicator only confirm the diffusion hypothesis. Tests in equations 6-A and 7-A confirm these results, showing no significance for civil liberties and significance for the squared and unsquared veto players terms only when using the CHECKS1 indicators.

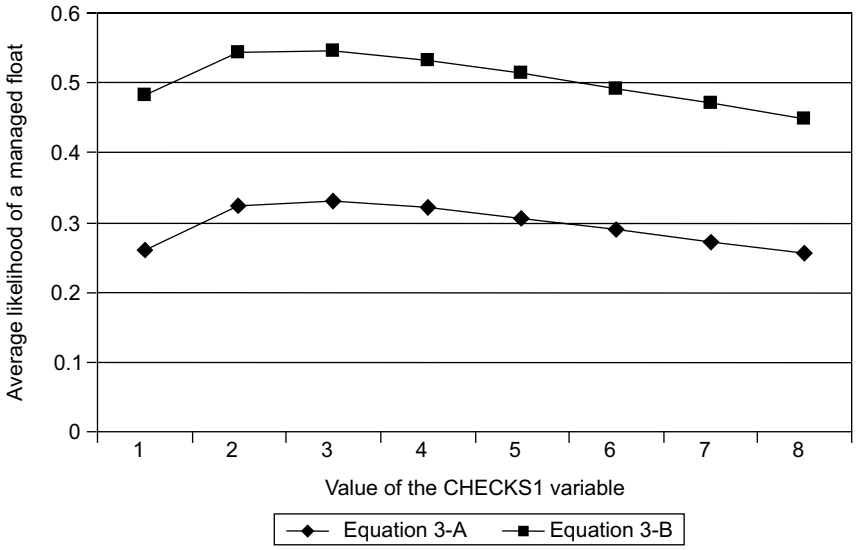

FIGURE 1. *Predicted Probabilities for Tests of the Goldilocks Hypothesis*

The control variables largely maintain the significance levels they did in Broz's original test using all of the control variables.

Table 3 reports the results of the tests using the components of the POLITY2 indicator. Equation 8-A tests the competitiveness hypothesis with XRCOMP; equation 9-A tests the openness hypothesis with XROPEN; equation 10-A tests the concentration and diffusion hypotheses with XCONST; and equation 11-A uses the indicator PARCOMP. XRCOMP and XROPEN are both significant at the 0.01 level and are negative as predicted. XCONST is significant at the 0.05 level and also negative. PARCOMP is only significant at the 0.10 level and is negative. These results imply that most of the variation in the POLITY2 indicator that is responsible for correlation with floating concerns open, competitive elections and constraints on executive power. Citizen participation is not as responsible for the correlations. The results for XROPEN and XRCOMP support the openness and competitiveness hypotheses, while the results for XCONST provide additional support for the diffusion hypothesis. Figure 2 displays the predicted probabilities using these equations. For every unit of change in XROPEN and XRCOMP, the predicted probability of a state choosing managed floating increases by an average of 2–3 percent. Every unit of change in XCONST increases the likelihood by an average of 1.5 percent. Once again, most of the control variables keep their levels of significance in all of these tests.

The tests presented here were also run with the data from OECD countries dropped from the dataset, to check if data from industrialized democracies were driving the correlations presented here. Industrial democracies do not solely drive the results. Tests using only data from developing countries recapitulate most of the results shown here. The only exception is that when the civil liberties indicator was tested only with the control variables, it was weakly significant at the 0.10 level. When the squared and unsquared veto players indicators were included with civil liberties, however, the civil liberties indicator was not significant.

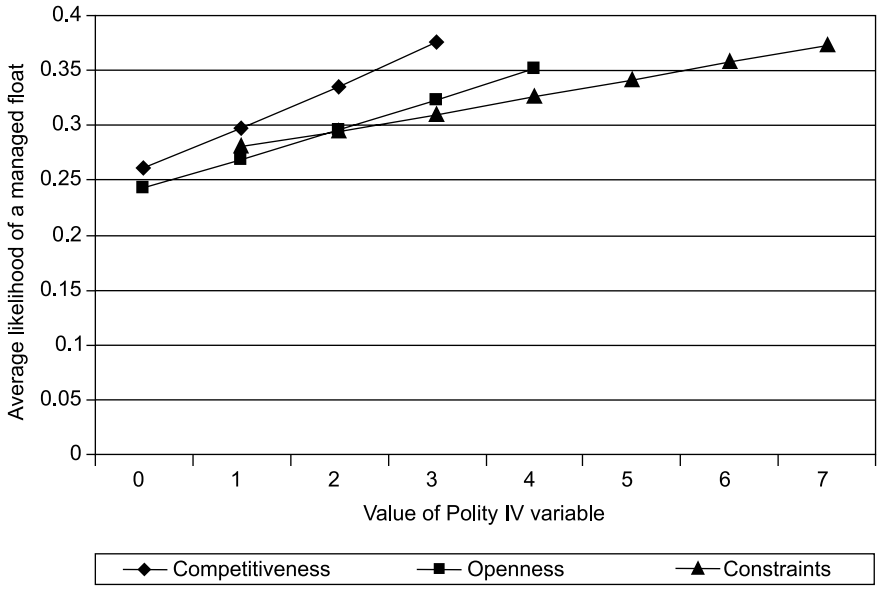

FIGURE 2. *Predicted Probabilities of Polity IV Variables using Broz's Data*

Tests on Developing-Country Data

The second set of tests applies the hypotheses to tests and data with different properties. While the datasets are not easily comparable, significance of results in both sets of tests demonstrates robustness. The second dataset contains quarterly data on 65 developing countries for the period from 1977 to 1998.[8] Using only developing countries allows us to determine if the hypotheses continue to hold even if advanced industrial democracies are not included. I continue to perform ordinal probit tests with robust standard errors to maintain some consistency in methods. In these tests, however, I adjust the standard errors to cluster the estimations around individual countries, to guard against heteroskedasticity and country effects.[9]

The dependent variable employs Broz's indicator for exchange rate regimes, but here it is converted into three ordinal categories. Since the category of limited flexibility, which applied to participation in the European Monetary System, is not applicable here, that category is dropped and fixed exchange rates are simply recoded as three. Managed and independent floating retain their codings as two and one, respectively.

The tests also employ a different set of control variables to account for some variables that the other tests did not. Economic controls include the log of GDP, GDP per capita, the level of trade exposure, the log of the inflation rate (rather than the inflation differential), and the level of foreign exchange reserves (in terms of months of imports). Financial openness is measured here as a four-point index of capital controls.[10] Political controls include measures of the manufacturing sector as a share of GDP (Frieden et al., 2001) to control for the preferences of manufacturing interests. They also include measures of public-sector debt

and private, non-guaranteed debt as a share of GDP (Shambaugh, 2004), plus foreign liabilities as a share of M2, to control for the effects of specific forms of capital flows. An indicator for elections is coded as one in the two quarters prior and up to an election, minus one in the quarter following an election, and zero otherwise. When facing an upcoming election, governments face pressure to maintain a fixed exchange rate and avoid devaluation (Blomberg et al., 2005; Leblang, 2003).[11]

Tables 4 and 5 report the results of these tests. The "B" equations in Tables 4 and 5 test the same hypotheses as the "A" equations that use Broz's dataset, with the same numbers corresponding to the same hypothesis tests given above. Once again, the tests provide no support for the transparency and concentration hypotheses. The diffusion and Goldilocks hypotheses find support depending on how veto players are measured and how the equation is specified. Equation 1-B shows no significance for the civil liberties indicator. Nor does the civil liberties indicator show significance in equations 6-B and 7-B, when they control for the effects of veto players. Equation 2-B displays no significance for the CHECKS1 indicator, but equation 3-B displays significance for both the squared and unsquared CHECKS1 indicators. The reverse is true in equations 4-B and 5-B, where Henisz's POLCONV indicator is negative and significant when unsquared and alone, but the squared term is not significant.[12] In equations 6-B and 7-B, the squared veto players terms have some weak significance at the 0.10 level, however, regardless of which indicator is used. Once again, the CHECKS1 indicator gives more support to the Goldilocks hypothesis, while the POLCONV indicators only support the diffusion hypothesis. Figure 1 shows that the predicted probability of a state choosing managed floating is highest when the state has two to four veto players when using equation 3-B, just as it did with equation 3-A. Using equation 4-B, a shift from the 25th to the 75th percentile of POLCONV values increases the likelihood of managed floating by an average of 6.2 percent, and reduces the likelihood of fixing by the same amount.

Table 5 reports the results for tests using the components of the POLITY2 indicator. Here, only XCONST is significant at the 0.05 level. Since XCONST is negative, its test also supports the diffusion hypothesis. XRCOMP, XROPEN, and PARCOMP do not achieve significance in these tests. While the tests on the Broz dataset provide support for the openness and competitiveness hypotheses, the tests on this dataset do not.

Implications and Conclusions

The transparency hypothesis receives no support from the tests given here. Given the lack of support in these tests and the questionable applicability of the theory's assumptions, we have reason to question whether the correlation between democracy and floating is actually due to governments of varying transparency weighing the requirements of fixed exchange rates and independent central banks. The tests shown here do not, of course, decisively solve the puzzle of what is causing a correlation between democracy and floating, but do demonstrate how much difficulty current theoretical approaches have in yielding robust confirmation on this question and why further research is necessary. The tests also provide some insights as to how to proceed with further research. Approaches based on the open and competitive nature of elite selection and on veto players deserve

TABLE 4. *Tests on Author's Data*

	1-B	2-B	3-B	4-B	5-B	6-B	7-B
	Transparency	Veto players (linear)	Veto players (quadratic)	Veto players (linear)	Veto players (quadratic)	Combined	Combined
	Freedom House	World Bank	World Bank	Henisz	Henisz	World Bank	Henisz
Lagged dv	3.5522***	3.5191***	3.5183***	3.5226***	3.5211***	3.5389***	3.5434***
	(0.1463)	(0.1451)	(0.1457)	(0.1441)	(0.1436)	(0.1483)	(0.1471)
Civil liberties	0.0295					0.0126	–0.0200
	(0.0306)					(0.0325)	(0.0381)
Veto players		–0.0603	–0.3597**	–0.2753**	–2.2693*	–0.3042*	–2.7648**
		(0.0653)	(0.1459)	(0.1088)	(1.2799)	(0.1635)	(1.3242)
Veto players squared			0.1923***		0.7284	0.1643*	0.8807*
			(0.0750)		(0.4587)	(0.0848)	(0.4693)
Log of GDP	–0.0787**	–0.0732**	–0.0806**	–0.0746**	–0.0764**	–0.0740**	–0.0606*
	(0.0329)	(0.0348)	(0.0344)	(0.0333)	(0.0333)	(0.0349)	(0.0337)
Log of GDP per capita	0.0264	0.0031	0.0238	0.0449	0.0448	0.0135	0.0175
	(0.0807)	(0.0713)	(0.0680)	(0.0734)	(0.0737)	(0.0780)	(0.0804)
Trade exposure	–0.0003	–0.0003	–0.0010	–0.0003	–0.0003	–0.0004	0.0005
	(0.0015)	(0.0016)	(0.0017)	(0.0015)	(0.0015)	(0.0017)	(0.0014)
Log of inflation	–0.0752	–0.0801	–0.0879	–0.0841	–0.0830	–0.0797	–0.0790
	(0.0810)	(0.0787)	(0.0781)	(0.0804)	(0.0734)	(0.7926)	(0.0717)
International reserves	–4.15e–06	–5.83e–06	–2.14e–06	1.70e–06	1.29e–06	–1.88e–06	1.89e–06
	(6.10e–06)	(6.78e–06)	(7.26e–06)	(7.40e–06)	(7.31e–06)	(7.05e–06)	(7.26e–06)
Capital controls	–0.0338	–0.0381	–0.0348	–0.0383	–0.0361	–0.0318	–0.0340
	(0.0269)	(0.0274)	(0.0273)	(0.0274)	(0.0272)	(0.0271)	(0.0282)

Manufacturing	–0.0073	–0.0084	–0.0082	–0.0077	–0.0061	–0.0079	–0.0066
sector	(0.0068)	(0.0068)	(0.0068)	(0.0062)	(0.0063)	(0.0071)	(0.0066)
Public-sector debt	–0.0021**	–0.0023**	–0.0021**	–0.0022**	–0.0021*	–0.0020**	–0.0019*
	(0.0011)	(0.0011)	(0.0010)	(0.0011)	(0.0011)	(0.0010)	(0.0011)
Private-sector debt	–0.0027	–0.0023	–0.0033	–0.0023	–0.0031	–0.0035	–0.0039
	(0.0039)	(0.0039)	(0.0038)	(0.0041)	(0.0042)	(0.0037)	(0.0041)
Foreign liabilities	0.0010***	0.0011***	0.0011***	0.0011***	0.0011***	0.0011***	0.0011***
	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0004)	(0.0003)	(0.0004)
Elections	0.0132	0.0086	0.0029	0.0209	0.0201	0.0062	0.0175
	(0.1251)	(0.1241)	(0.1243)	(0.1222)	(0.1241)	(0.1277)	(0.1286)
Obs (*n*)	4237	4202	4202	4240	4240	4176	4213
Wald Chi^2	764.69	800.74	817.78	771.11	779.51	798.63	761.00
Prob > Chi^2	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000

Notes: Standard errors in parentheses.
* = Significant at the 0.10 level.
** = Significant at the 0.05 level.
*** = Significant at the 0.01 level.
e = exponent.

TABLE 5. *Tests on Author's Data*

	8-B	9-B	10-B	11-B
	Competitiveness	Openness	Constraints	Participation
Lagged dv	3.5196***	3.5264***	3.5105***	3.5168***
	(0.1476)	(0.1485)	(0.1471)	(0.1461)
XRCOMP	–0.0557			
	(0.0351)			
XROPEN		–0.0274		
		(0.0271)		
XCONST			–0.0356**	
			(0.0158)	
PARCOMP				–0.0511
				(0.0319)
Log of GDP	–0.0778**	–0.0759**	–0.0797**	–0.0822**
	(0.0346)	(0.0344)	(0.0353)	(0.0347)
Log of GDP per capita	0.0408	0.0257	0.0416	0.0435
	(0.0743)	(0.0735)	(0.0751)	(0.0815)
Trade exposure	–0.0005	–0.0006	–0.0004	–0.0003
	(0.0016)	(0.0016)	(0.0016)	(0.0016)
Log of inflation	–0.0747	–0.0805	–0.0703	–0.0783
	(0.0775)	(0.0803)	(0.0772)	(0.0794)
International reserves	–7.00e–07	–4.56e–06	2.73e–06	–1.00e–06
	(7.82e–06)	(6.83e–06)	(7.81e–06)	(6.91e–06)
Capital controls	–0.0369	–0.0401	–0.0335	–0.0358
	(0.0267)	(0.0266)	(0.0259)	(0.0261)
Manufacturing sector	–0.0076	–0.0079	–0.0074	–0.0076
	(0.0067)	(0.0069)	(0.0065)	(0.0067)
Public-sector debt	–0.0022**	–0.0022**	–0.0023**	–0.0022**
	(0.0011)	(0.0011)	(0.0011)	(0.0011)
Private-sector debt	–0.0029	–0.0026	–0.0027	–0.0026
	(0.0040)	(0.0039)	(0.0040)	(0.0040)
Foreign liabilities	0.0011***	0.0011***	0.0011***	0.0011***
	(0.0004)	(0.0004)	(0.0004)	(0.0003)
Elections	0.0209	0.0099	0.0297	0.0211
	(0.1205)	(0.1224)	(0.1199)	(0.1213)
Obs (n)	4121	4120	4121	4121
Wald Chi^2	739.47	749.01	733.50	722.90
Prob > Chi^2	0.0000	0.0000	0.0000	0.0000

Notes: Standard errors in parentheses.
* = Significant at the 0.10 level.
** = Significant at the 0.05 level.
*** = Significant at the 0.01 level.
e = exponent.

TABLE 6. *Partial Correlation Matrix*

	POLITY2	XROPEN	XRCOMP	XCONST	PARCOMP	Civil liberties	CHECKS1	POLCONV
POLITY2	1.0000							
XROPEN	0.4955	1.0000						
XRCOMP	0.8819	0.7363	1.0000					
XCONST	0.9454	0.5956	0.8861	1.0000				
PARCOMP	0.8940	0.4898	0.7748	0.8223	1.0000			
Civil liberties	–0.8022	–0.4222	–0.6912	–0.7460	–0.8079	1.0000		
CHECKS1	0.5155	0.4225	0.5078	0.5100	0.4963	–0.4381	1.0000	
POLCONV	0.7652	0.4822	0.6955	0.7572	0.7209	–0.6394	0.5044	1.0000

Note: This matrix uses the data from the second set of tests, based on a dataset used by the author.

further attention. What needs discussion are certain discrepancies and what further testing the theories require.

The mixed results for the openness and competitiveness hypotheses could be the result of one of two differences between the tests. Broz's dataset contains both developed and developing countries, while the second dataset does not, which could imply that the open and competitive nature of elections has its strongest effect on exchange rate regimes in long-standing, mature democracies. When tests are run on a revised version of Broz's data with the OECD countries removed, however, the results for the open and competitive nature of elections remain significant at the 0.01 level. This implies that the data from long-standing, mature democracies do not make the difference. It is also possible that the results are due to the differences in the specification of control variables in the equations. The question of control variables raises more questions as to how robust the results actually are. Furthermore, if open, competitive elections are the root of the explanation, why are the results for these indicators not as robust as the results for the POLITY2 indicator? One possibility for settling this question would be to substitute the XROPEN and XRCOMP indicators for the POLITY2 indicators in other published research on exchange rate regimes and see if the correlation still holds. Another possibility would be to expand research on the extent to which electoral outcomes and cabinet shuffles depend on exchange rate changes in developed and developing countries.

The number of veto players also seems to influence the choice of exchange rate regime, but, unfortunately, the results depend heavily on how veto players are measured. The divergent results are probably the consequence of methodological differences in the indicators. As Table 6 demonstrates, the correlation between the CHECKS1 and POLCONV indicators is close to 0.5. Since POLCONV arguably captures the true degree of resistance to executive initiatives better than CHECKS1 does, it might be preferable to emphasize the diffusion hypothesis. It is also possible that some data that can fit a curvilinear pattern can arguably fit a linear pattern as well and it may be difficult for statistical modeling to settle the question of which is inherently superior, which raises problems concerning falsifiability.

Why, however, might these tests imply that political systems with a greater number of veto players are more likely to float, when a lot of research on exchange rate regimes (Bernhard and Leblang, 1999; Hallerberg, 2002; Leblang and Satyanath, 2006) implies that concentrated power should be more likely to float? Note that Hallerberg (2002) and Bernhard and Leblang (1999) focus their attention on parliamentary democracies and the number of parties in the governing coalition. Tests using a dataset including developing countries, however, confirm that when a wider collection of political systems is included their theoretical results do not necessarily apply. Leblang and Satyanath (2006), however, do employ datasets with developing-country data, but their focus is on the likelihood of exchange rate crises. The divergent results given here could simply indicate that while the presence of multiple veto players may deprive a state of credibility in discussing its economic fundamentals, this only affects the likelihood of crisis and not the average degree of exchange rate volatility in the long run. Crises are often more about short-run expectations, while long-term volatility also involves the government's management of capital controls, interest rates, and foreign exchange reserves.

The Goldilocks hypothesis holds out the promise that the approaches behind the concentration and diffusion hypotheses might also be reconcilable. It is possible

that political systems with relatively few and relatively many veto players both have reason to fear exchange rate volatility and floating. The hypothesis would need further testing, especially with the POLCONV indicator, to demonstrate robustness, however. Alternative testing methods might yield results that could help estimate the size of the effects of veto players on exchange rate regime choice and whether the political systems with the most chance of floating are those with more than one veto player but less than five or more. More tests are also clearly needed on whether the number of veto players affects the volatility of exchange rates as the causal mechanism suggests. While the tests are not definitive, they point to interesting possibilities for research.

Such research also presents the interesting possibility that significant differences among different democratic institutions in developing countries, besides the majoritarian–PR distinction already researched for developed countries, have been conditioning exchange rate regime choices. Such questions are particularly relevant to regions such as Central and Eastern Europe or Latin America, where the key puzzle is that transitions to democracy took place at roughly the same time among economies with roughly similar conditions, yet exchange rate regime choices varied considerably after democratization. Much of the variation in exchange rate regimes in Central and Eastern Europe, as is already known, is due to variations in specific economic conditions, such as inflation rates (Klyuev, 2002), or variations in interest group pressure, such as the differences resulting from variations in banking-sector organizations (Grittersova, 2006). Thus it is necessary to construct statistical models that combine and control for the various effects of macroeconomic, interest group, and institutional influences, as the tests above do. Since democracies differ in the number of veto players involved and in the different possible combinations of electoral competitiveness and veto player structures, it may also be that such variations in democratic institutionalization represent an underappreciated influence on the variation in exchange rate policy in such countries. If so, we can begin to go beyond the democratic–dictatorship distinction into a richer understanding of how political institutions shape economic policy in developing and transitional countries.

Notes

1. A de jure exchange rate regime is the government's official, declared policy for the determination of exchange rates, either through government intervention (fixing) or currency markets (floating). A de facto exchange rate regime is the actual degree of government intervention in exchange rate markets in practice.
2. For examples, see Freeman et al. (2000), Bernhard and Leblang (1999), and Bernhard et al. (2003).
3. Results are sensitive to how democracy is measured, however. Using data from Broz (2002) and Hall (2006), I substituted the regime-type indicator developed by Alvarez et al. (1996) for the Polity indicators to see if the same results would hold. The Alvarez et al. indicator codes democracies as zero and non-democracies as one. The results (not included here) were insignificant.
4. POLCONV includes measures of the effects of judiciaries and subnational governments, whereas Henisz's earlier POLCONIII does not. I selected POLCONV as the indicator instead of POLCONIII on the grounds that POLCONV was presumably more up to date and a more nuanced measure of veto players. Moreover, Hallerberg (2002) emphasizes the role that subnational units play in determining exchange rate regimes, and thus tests with POLCONV are more effective tests of his theory.

5. All estimations of predicted probabilities were calculated using Monte Carlo simulations from the Clarify program developed by Tomz et al. (2001). See also King et al. (2000) for an explanation of this method.
6. Ideally, one needs to perform a likelihood ratio test on the combined effects of the squared and unsquared terms to test the significance of veto players in the whole equation (Braumoeller, 2004). Unfortunately, such likelihood ratio tests are not available when using robust standard errors. When using more conventional standard error estimation, a likelihood ratio test estimated the probability that the two indicators combined had no significance at 0.052, just shy of the 0.050 level. It is not clear, however, if this result is applicable to the equation reported here.
7. A likelihood ratio test using conventional standard errors, however, estimated the probability of insignificance at 0.055, again just shy of the 0.050 level.
8. This dataset was originally created for tests of the hollowing-out thesis (see Hall, 2006).
9. The same tests are performed without clustering for the sake of consistency with Broz's methods. The presence or absence of clustering does not create any significant difference in the results.
10. The index is adapted from Garrett (1995), and gives one point each for the presence of restrictions on the current account, the capital account, and bilateral payments, plus dual exchange rates.
11. These same tests are performed with fewer control variables (trade openness, inflation, capital controls, the size of the manufacturing sector, publicly guaranteed and private, non-guaranteed debt, foreign liabilities, and elections) in order to guard against multicollinearity. For most of the equations, the results are not significantly different when controlling for this more limited set of variables.
12. When using conventional standard error estimation, the likelihood ratio test on the squared and unsquared CHECKS1 terms is not significant at the 0.05 level. The test is significant on the squared and unsquared POLCONV terms, strangely enough. This implies that tests with POLCONV confirm the Goldilocks hypothesis, but the tests with CHECKS1 do not, which seems to be the reverse of the findings shown here. Since the likelihood ratio test is not available with robust standard error estimation, it is not clear that these tests apply to the equations reported here.

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Acknowledgments: Some parts of this work adapt material from a previous book (Hall, 2005). The author would like to thank David Bearce, Shale Horowitz, Jonathan Katz, Angela O'Mahony, and three anonymous reviewers for their helpful comments and J. Lawrence Broz for the use of his data and for his helpful advice. The author warmly thanks Tao Feng, Jeanette Mukayisire, and Mateusz Rozanski for their considerable help in collecting the data. All mistakes are the responsibility of the author.