

When Preferences and Commitments Collide:  
The Effect of Relative Partisan Shifts on International Treaty Compliance

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## Introduction

Many students of international relations believe that international laws and institutions can ameliorate the conflict-producing properties of international anarchy. They hope by consequence that such arrangements may provide a reasonably clear path along which states may proceed toward mutually beneficial forms of cooperation, and thereby attain and fortify peace among them. For these students, international law and institutions “matter” in world politics insofar as they can constrain otherwise independent states to select behaviors that are prescribed by the laws and institutions to which they adhere and to eschew forms of behavior that are proscribed by them.

Yet, for much of the past decade, students of international law and institutions have had to confront an argument put forward by Downs, Rocke, and Barsoom (1996). Their key proposition is that even a high level of compliance by states with international legal or institutional rules is not in fact evidence of the latter’s efficacy or independent effects on state behavior.<sup>1</sup> Downs, Rocke, and Barsoom suggest that states adhere to such laws and institutions not because they are constrained or compelled to do so; rather, states typically comply with their international legal and institutional obligations because they construct and sign only those accords that stipulate behaviors that the signatories prefer to pursue even in the absence of their external obligations.<sup>2</sup> International laws and institutions, then, have modest if any autonomous impact on states, if such impact is measured by their compliance with legal or institutional obligations, according to Downs, Rocke, and Barsoom, since both the content of those obligations and compliance are endogenous to the preferences of the states that construct the laws and institutions in question.<sup>3</sup>

Simmons (2000) has put forward the first and to date the most persuasive reply to Downs, Rocke, and Barsoom.<sup>4</sup> Simmons does so in an analysis of international law on the application by states of restrictions between the late-1960s and the late-1990s on foreign exchange transactions undertaken to accommodate current account transactions. She finds that states that were adherents during that period to Article VIII of the Articles of Agreement of the International Monetary Fund, which prohibits such restrictions, were less likely to impose them than were states that belonged to the IMF but had not made such a commitment regarding current account transactions.<sup>5</sup>

Most important for our purposes, Simmons tries to confront directly the Downs, Rocke, and Barsoom challenge by including a wide range of macroeconomic indicators that she believes reflect upon the decision calculus of state leaders on the desirability of the openness of foreign exchange markets and the possible need for some level of closure of those markets. The problem, we suggest below, is that such macroeconomic indicators are more likely to provide us with information on the *constraints* under which policy leaders are operating, and not very much about the *preferences* that inform their decisions.

Hence, to complement Simmons's analysis, and to confront more directly the Downs, Rocke, and Barsoom challenge, we recommend below that it is necessary to consider a type of political change in a state that is likely to represent a change in national preferences about international economic openness in general and foreign-exchange and capital-market openness in particular. That political change, we suggest, is a relative shift in the left-right orientation of the party in control of the executive branch of the national government. Incorporation of an indicator that captures relative movements along the left-right dimension at the apex of national politics, we suggest, allows us to shed substantial light on the impact of an international

commitment such as Article VIII. As we demonstrate below, in accord with Simmons's central work on this matter (2000), as well as her follow-on study with Hopkins (2005), Article VIII does have a constraining effect on the likelihood that signatory governments will impose controls on foreign exchange transactions for the purpose of clearing the current account. At the same time, and in accord with arguments put forward by Downs, Rocke, and Barsoom (1996) and von Stein (2005), the magnitude of the constraining impact of Article VIII on the likelihood of capital controls, while statistically and substantively significant at least in the short term, is less than that estimated by Simmons once we take into account changes in the preferences of some national governments, and in particular those that experience leftward shifts in the political orientation of the political parties in charge of those governments.

To develop this line of analysis, we proceed in four steps. In the next section we suggest that current ways of controlling for national preferences and shifts in such preferences may not meet frontally or entirely effectively the Downs, Rocke, and Barsoom challenge, and we suggest that a focus on shifts in the left-right orientation of the governing party may provide better leverage in controlling for national preferences and changes in them when estimating the effects of external obligations. Second, we present a discussion of the data and methods that we bring to bear in the discussion of the efficacy of Article VIII, including a focus on shifts in the left-right orientation of national governments. We present and discuss our empirical results in the third section of the paper. Our basic point in that section is that, after we take into account shifts in national preferences, Article VIII still has a significant constraining effect on the imposition by signatories of capital controls, as suggested by Simmons; however, as would be anticipated by Downs, Rocke, and Barsoom, the constraining effect when we control for shifts in national preferences is less than what is observed compared to when we do not take such shifts in

preferences into account. In the fourth and final section we offer some important qualifications on our conclusions, and we identify future paths for potentially fruitful research on the matter of national preferences and institutionalized international cooperation.

### **Compliance with International Law and the Problem of National Preferences**

To date, Simmons's work on the impact of Article VIII is the most sustained response to the Downs, Rocke, and Barsoom thesis that compliance with international law is largely if not solely endogenous to national preferences. Simmons cites the Downs, Rocke, and Barsoom problem at the outset of her discussion of her research design for the question of why do some states adhere to Article VIII while others do not (822). Then, on the particular matter of whether Article VIII has a subsequent impact, or as she puts it, "whether the law matters" (829), she includes in her analysis a range of economic variables that reflect, she suggests, on the decision-calculus of states to impose restrictions on financial transfers required to accommodate current account transactions. She specifies, for example, that she has designed her study so that "Article VIII commitment is forced to compete with a broad range of economic conditions that clearly are associated with restrictions: volatility in the terms of trade, reserves, and the balance of payments; poor economic growth rates; and low GNP per capita" (829). In reviewing her results, she also emphasizes that "Most important for my purposes, however, is that controlling for every likely macroeconomic influence on the decision to implement current account restrictions, a formal declaration of adherence to Article VIII obligations has a strong negative effect on the probability of imposing restrictions" (830).

For Simmons, then, the inclusion of macroeconomic variables allows her to control for the decision of states to impose restrictions on currency exchanges needed for the accommodation of current account transactions. Having done so, she can then proceed to estimate the effect on the imposing of restrictions of adhering or not to Article VIII.

An interesting debate has taken place in recent years between von Stein (2005) and Simmons and Hopkins (2005) on whether we should conclude that Article VIII had an independent effect on that behavior or whether, as anticipated by Downs, Rocke, and Barsoom, those countries that adhered to Article VIII preferred not to have restrictions *ex ante* and thus had a lower incidence of imposing them *ex post* than did non-signatories. We return to elements of this exchange below. For the moment, however, we wish to focus on another matter, *the question of how to conceptualize and measure the “decision” calculus of states.*

As noted above, Simmons suggests that the inclusion of macroeconomic variables provides her with leverage to control for the factors other than international law that are likely to influence the decisions of state officials concerning restrictions of financial transactions for the current account. In this way, Simmons believes that she has tapped into and controlled for changes in the orientation of those state decision-makers toward restrictions on financial transfers associated with the current account, leaving space for the analysis of the impact of Article VIII itself on the behavior of those state decision-makers. Having done that, one might believe that there is an amelioration of the Downs, Rocke, and Barsoom problem that compliance with an accord is simply endogenous to the orientation or preferences of state leaders.

Our view is that while the inclusion by Simmons of macroeconomic control variables helps us understand the calculus of choice of state decision-makers in regard to Article VIII and capital controls to an extent, doing so does not address frontally the Downs, Rocke, and Barsoom

problem about the endogeneity of state compliance to state preferences. We come to this view as a result of wishing to draw a distinction between the preferences of state leaders in regard to such compliance, and constraints that may, at a given point in time, be operating on their capacity to do so. Decision-makers looking at the issue of restrictions on the current account (as perhaps all individuals do when they face choices among different courses of action in regard to any important matter), are likely to be prompted by many motivations, and among them are their *preferences*, or the states-of-the-world they would like to bring about, and the *constraints* they face, or the limitations on their capacity to reach and maintain their preferred states-of-the world. The question is, do the macroeconomic controls employed by Simmons reflect more on the preferences of state leaders, or the constraints operating on them?

We believe that the controls reflect on the latter, but not the former. Let us imagine that government leaders of a developing country adhere to Article VIII at year 0, and that the country experiences an unexpected and serious deterioration in the balance of payments during year 1. A developing country that so experiences a sharp, unexpected payments deterioration may be vulnerable to a currency and banking crisis; to forestall that, while also maintaining a freely operating foreign exchange market, the country's economic leaders are likely to need to consider such policy options as increases in interest rates or cutbacks in government spending, both of which are likely to impose domestic economic costs on government supporters. Hence, in these circumstances, it is reasonable to believe that the country's leaders will estimate that adherence to Article VIII and freely operating currency markets have become markedly more difficult, difficult in the sense of pursuing both those objectives while also achieving other important economic policy goals. By consequence, we as observers might look at this case and say that

the *constraints* on the ability of the government's leaders to maintain openness with respect to the current account have become more severe.

At the same time, these government leaders may still believe that freely operating exchange markets are a desirable state of affairs. They may also wish by consequence to bring freely operating exchange markets into effect for their country. In this instance, then, they would retain as strong a *preference* for such open exchange markets, and the absence of restraints on them, as when they adhered to Article VIII.

Inclusion by Simmons of macroeconomic performance variables, then, provides us with information about the *constraints* on the capacity of state leaders to adhere to Article VIII by not imposing current-account exchange restraints. However, their inclusion does not shed light on changes in the *preferences* of those leaders to so maintain openness on the current account. By consequence, even with the inclusion of the macroeconomic variables that are in the Simmons analysis, we cannot judge whether Article VIII adherents have been less likely than non-adherents to impose restrictions because of the adherence of the former to Article VIII, or because the former have a stronger preference than do the latter to open exchange markets. Given that the central point made by Downs, Rocke, and Barsoom is that state behavior in regard to international obligations is endogenous to state preferences (not state constraints), at the end of the day their challenge seems to be unanswered notwithstanding the important work by Simmons.

Moving forward on the question of the effects of an international legal-institutional arrangement on the behavior of adherents to the arrangement requires that we compare the behavior of the former to that of non-adherents, and in doing so to take into account not just

changes in constraints on the capacity of adherents to comply, but *changes in national preferences in the issue area covered by the arrangement*.

Good measures of national preferences, and of changes in them, are not readily available in the field of international relations. A potentially very efficacious measure of leadership preferences might consist of the coding of responses to similar policy-related questions that are posed to state leaders across nations and through time. Some response data are available on samples of U.S. government officials (Chicago Council on Foreign Relations 2004). However, such data are not available for the leadership cohorts of other countries through time, and no data are available that tap directly into the preferences of the very top leaders in the United States or elsewhere. The development of this type of optimal measure is likely to be the victim of logistical and resource problems, and is unlikely to be accomplished in the near term.<sup>6</sup>

At least at present, it is not possible to tap directly into changes in the preferences of “states,” or of “governments.” It is also not possible to tap directly into the minds of national leaders and to measure their preferences. However, we can observe, and in this paper we investigate, a key kind of activity that may reflect changes in such preferences.

The proxy for changes in national preferences that we study below relates to changes in the left-right partisan placement of the executive branch of a government under review. In particular, we follow the lead of a substantial body of political-economy scholarship on how different parties reflect different social and economic groups in society, and suggest that, other things being equal, a leftward shift in a government’s partisan placement is likely to result in a set of official policy views that are less hospitable to the liberalization of markets, including the market for foreign exchange, notwithstanding international legal commitments made by previous governments to maintain or deepen such liberalization.

This general expectation is based on a substantial body of literature in the field of comparative political economy. A robust finding in that field is that party systems in most advanced industrial countries and in many developing countries are grounded in significant measure on class divisions, with left-leaning parties tending to be relatively more attentive to the interests of workers, and right-leaning parties having a propensity to promote the interests of the owners of capital.<sup>7</sup> In regard to trade policies, left and right-leaning parties appear to craft their policies to a significant degree along the lines that is anticipated by the Stolper-Samuelson theorem regarding the income effects of trade liberalization, namely, that such liberalization tends to benefit the owners of the factor of production in relative abundance in the country, and tends to penalize the owners of the factor that is relatively scarce. For example, Milner and Judkins found that, in advanced industrial countries during the period from the mid-1940s to the late-1990s, left-leaning parties were more likely than right-leaning parties to articulate support for trade protectionism during election campaigns. Similarly, Dutt and Mitra (2005) analyzed of trade policies of a sample of developed and developing countries during the 1980s, and found that, in relatively capital-abundant countries--that is, in those countries in which expansion of trade is likely to enhance the income of capital owners and constrains that of labor--left-leaning parties were more likely than right-leaning parties actually to impose protectionist trade policies (Dutt and Mitra 2005).

We may also consider that while differences in the organization of the labor market and the level of international economic and financial exposure mitigate and modify the impact of partisan orientation on macroeconomic policies, left-leaning parties in general promote the interests of their constituents through a combination of expanded government spending and taxation on higher-income earners, and have a tendency to prefer monetary expansion and lower

interest rates; right-leaning parties promote the interests of capital owners by seeking to lower taxes, to restrain the growth of government spending, and to be somewhat more hesitant to expand the money supply (see, for example, Alesina, Roubini, and Cohen 1995; Garrett 1998; Iversen 1999; Boix 2000; and Swank 2002). This policy orientation of left-leaning parties toward macroeconomic expansionism may create a higher risk of current account deficits, downward pressure on the value of the nation's currency, and an exacerbation of the tradeoff between monetary and fiscal expansion on the one hand and more pronounced price inflation on the other by way of depreciation of the currency. By consequence, then, of the fundamental macroeconomic preferences of left-leaning parties, such parties may be expected to have a lower commitment to international economic openness in financial matters, as Simmons found in respect to the Great Depression (1994).

Thus, in connection to the specific matter of compliance with Article VIII, we expect that if a government comes into office that is to the left of the government that originally adhered to the article, then, controlling for other conditions, that new relatively more left-leaning government is likely to have a relatively weaker preference for freely operating markets than was true of the government that signed Article VIII, and therefore for freedom of currency transactions for the current account. By consequence, we may expect that periods of time in which a more left-leaning government is in office, relative again to the party that headed the government that originally adhered to Article VIII, are likely to witness a greater risk that the country will derogate from the article and impose restrictions banned by it.

This enhanced risk of derogation may be the result of the independent and combined effect of two distinct political forces. First, and in accord with the argument put forward by Simmons (2000), it could be that right-leaning governments that sign Article VIII wish to signal

to international business communities that they are committed to liberalization and are tying their own hands in this matter by adhering to an international legal commitment relating to such liberalization; when a left-leaning government later comes into office, it may be less committed to that policy stance and thus is more likely to derogate from Article VIII. Alternatively, it could be the case that right-leaning governments are not only trying to tie their own hands, but those of their successor governments, especially if those successors are led by left-leaning parties. Such left-leaning successor governments may, or may not, accept those constraints, and thus may or may not accept continued compliance with Article VIII.<sup>8</sup>

### **Data and Methods**

Rigorous testing of the relationship between national preferences (as opposed to constraints) and state behavior is fraught with methodological difficulties. The most basic of these difficulties lies in the measurement of the preferences themselves. While there are ample theoretical reasons (reviewed above) for believing that “leftward” or “rightward” political orientations are *directional* categories which have relatively consistent meanings across countries, previous research in this area has been stymied by the difficulty of making cross-national comparisons of the *degree* of partisanship which characterizes the specific location of a party on the left-right continuum. Making such comparisons seems to require the development of a cardinal partisanship scale that would allow parties in one country to be positioned relative to parties in other countries. It seems to require, in other words, a partisanship scale which transcends the particularities of separate domestic regimes.

Attempts to construct such scales have been made on the basis of cross-national public opinion surveys such as the Eurobarometer and World Values Survey, on the basis of expert

opinion surveys (Castles and Mair 1984; Huber and Inglehart 1995), and on the basis of factor analysis of party platform elements (Laver and Budge 1992; Laver and Garry 2000; Huber and Gabel 2000). However, the richness of the data required to perform such analyses has forced these researchers to focus their data collection efforts on relatively small country samples which are strongly skewed towards the OECD. These data thus represent an inadequate means by which to test the efficacy of a causal variable – Article VIII acceptance – which varies primarily amongst non-OECD countries.

Partisanship data for a far broader sample of country-years is provided by the *Database of Political Institutions* (Beck et al. 2001) which records the left-right orientation of the party heading the executive branch for 182 countries since 1975. However, in contrast to the more detailed measures developed for OECD countries, the DPI data makes no attempt to develop a cardinal scale which would be applicable across such a wide range of countries. Instead, parties that differentiate themselves along economic lines are coded either as “left”, “right”, or “center” on the basis of party names (e.g. a party with the term ‘socialist’ in its name is assumed to be left-wing) and a variety of secondary sources. Parties that do not differentiate themselves along economic lines are placed in fourth, residual category. The simplicity of this coding scheme allows the DPI data to cover a far wider range of countries, but renders cross-national comparisons difficult. While it is relatively straightforward to determine that party A is to the left of party B on the political spectrum defined by the domestic regime in which they are competing, there is no good reason to believe that such categories have consistent meanings across countries. In fact, what counts as “left” in one country might be considered “center” or even “right” in another country. Because there is no Archimedean point from which to judge the

positions of all parties simultaneously, treating any one of the DPI categories as a simple predictor of financial openness or Article VIII compliance would be highly problematic.

The solution we propose to this problem is to abandon the attempt to measure absolute position on a single, global, cardinal scale. Instead we characterize our central causal variable as the presence or absence of *relative partisan shifts*. We treat the DPI categories of “left”, “right,” and “center” as representing ordinal values which are comparable within countries, but not across countries. We then use particular events of policy change to define “landmark” reference points on the political spectrum in each country and measure partisanship as relative shifts subsequent to each landmark.

More specifically, we code **ORIENTATION** for each party in our dataset as  $-1$  if DPI categorizes them as “left”,  $+1$  if DPI categorizes them as “right”, and  $0$  if DPI categorizes them as “center” or if they do not differentiate themselves along economic lines. We then code **GOVERNMENT<sub>it</sub>** as the *ORIENTATION* of the party heading the executive branch in country  $i$  and year  $t$ . Finally, we code **SHIFTLEFT<sub>it</sub>** equal to  $1$  if  $GOVERNMENT_{it} < GOVERNMENT_{iu}$  (where  $u$  is the year of our “landmark” event) and  $0$  otherwise, and similarly code **SHIFTRIGHT<sub>it</sub>** equal to  $1$  if  $GOVERNMENT_{it} > GOVERNMENT_{iu}$  and  $0$  otherwise. By using simple dichotomous indicators for the presence or absence of such shifts, we seek to remain agnostic about the scale upon which the shifts are occurring, while still capturing information about the direction of the shifts.

This of course still leaves open the question of what relevant “landmarks” on the political spectrum could be used to judge such relative shifts. We propose two separate specifications, which correspond to the two main observable implications of our theory. In the first specification we set  $u$  equal to  $t - 1$ , effectively treating each country-year as the relevant

landmark for the subsequent country year. We accomplish this through a difference-in-differences specification, of the form:

$$\Delta Y_{it} = \Delta X_{it} + SHIFTRIGHT_{it} + e_{it} ,$$

where  $Y_{it}$  is the level of financial openness adopted by country  $i$  in year  $t$ ,  $\Delta Y_{it} = Y_{it} - Y_{it-1}$ ,

$X_{it}$  is a vector of control variables, and  $\Delta X = X_{it} - X_{it-1}$ . We thus seek to predict year-to-year

changes in openness on the basis of year-to-year changes in  $X$  and the presence or absence of

year-to-year rightward shifts in the partisan orientation of the executive branch. In addition to

matching the functional form of our hypothesis, the difference-in-differences specification also

has the added benefit of automatically controlling for any confounding factors which are

constant within countries. Note that in this specification, each observation of openness  $Y_{it-1}$

serves as a landmark baseline against which to judge  $Y_{it}$ , just as each observation of

$GOVERNMENT_{it-1}$  serves as a landmark baseline against which to judge the presence or

absence of  $SHIFTRIGHT_{it}$ . Thus, even if we do not know that the category of “right” in one

country represents the same *absolute* position on the political spectrum as the category of “right”

in another country we can still be confident in judging whether a *relative* shift has occurred, and

even if we remain agnostic as to the cardinal *size* of the shift we can still be confident in judging

its *presence* or *absence* and make statistical predictions on that basis.

The second specification uses a similar logic in defining relative partisan shifts, but

transports this logic to a separate empirical domain: treaty compliance. Here, we follow

Simmons (2000) and Simmons and Hopkins (2005) in specifying a logistic regression of the

form:

$$RESTRICT_{it} = ART8_{it} + SHIFTLLEFT_{it} + X_{it} + e_{it} ,$$

where the dependent variable  $RESTRICT_{it}$  is a dichotomous indicator of whether current account restrictions were imposed in a given country-year,  $ART8_{it}$  is a dichotomous indicator of whether Article VIII obligations have been accepted for a given country-year, and  $X_{it}$  is a vector of control variables. For this specification, rather than setting  $u$  equal to  $t - 1$ , we set  $u$  equal to the year Article VIII was signed by a particular country.  $SHIFTLEFT_{it}$  thus treats the moment of Article VIII acceptance as the landmark baseline against which to judge the presence or absence of a relative partisan shift. This essentially renders  $SHIFTLEFT_{it}$  as an interaction term with  $ART8_{it}$ , which equals 0 for all country-years prior to the signing of Article VIII, 0 for all country-years subsequent to the signing of Article VIII for which the party heading the executive is *not* positioned to the left of the party in power when Article VIII was signed, and 1 for country years subsequent to the signing of Article VIII for which the party heading the executive *is* to the left of the party in power when Article VIII was signed. This term thus represents the ideal test of whether shifts away from the configuration of national preferences which produced the original decision to sign Article VIII serve to condition the probability of compliance with the treaty.

For both specifications we use the same set of control variables, the only exception being those variables which are constant within countries and therefore automatically drop out of the difference-in-differences specification. First, we include every macroeconomic variable from Simmons (2000) which is used as a predictor of Article VIII acceptance or restriction behavior. These are *Exchange Rate Flexibility*, *Trade Dependence*, *GNP Per Capita*, *GDP Growth*, *Reserves/GDP*, *Reserve Volatility*, *Balance of Payments/GDP*, *Terms of Trade Volatility*, *IMF Surveillance*, *Use of IMF Credits*, *Universality of Article VIII*, and *Regional Restrictions*.<sup>9</sup> Second, we include three dichotomous indicators of regime characteristics that may make

frequent partisan shifts in the executive branch more or less likely. *Military* equals 1 if a uniformed military officer heads the executive branch, *Term Limit* equals 1 if the executive's maximum term of office is constitutionally limited, and *Parliamentary* equals 1 if the country has a parliamentary system in which the legislature can recall the leader of the executive branch. To control for duration dependence, the logit specification also includes a term counting the number of years since the last restriction, along with squared and cubed transformations of this term, as recommended by Carter and Signorino (2007). Financial openness, the dependent variable in the first specification, is measured using the Chinn-Ito *KAOPEN* index for the period 1970-1997 (Chinn and Ito 2006). Data on restriction activities, the dependent variable in the second specification, is taken directly from Simmons (2000) for the period 1967-1997.

Within these temporal bounds we face a substantial degree of missing data. Especially problematic in this regard are the data for our partisan shift variables, which are only available beginning in 1975.<sup>10</sup> To avoid the biases that may result from simple listwise deletion, we fill in missing cells using multiple imputation (King et al. 2001; Honaker and King 2006). We generate five multiple-imputed datasets for each of our two specification forms, including all of the variables from each specification along with country fixed effects in our imputation models. By jointly analyzing each set of five, we can incorporate the uncertainty associated with the imputation into our estimates of causal effects.

The final methodological hurdle concerns the possibility of selection bias. As Von Stein (2005) argues, a standard logistic regression which treats restrictions as the dependent variable and Article VIII acceptance as an independent variable may overstate the significance of the treaty's effect if states are self-selecting into signing. The problem is that countries which experience an event, such as the signing of Article VIII, may be systematically different from

countries that do not experience the event, making naive comparisons between these two groups inherently problematic. Even if the relevant control variables are included in the model, nonlinearities in their effects which correlate with the selection process may still bias our causal inferences (King and Zeng 2006). We agree with Simmons and Hopkins (2005) that the best solution to this difficulty is not to rely on the dubious distributional assumptions which underlie Heckman-style selection models, but rather to pursue nonparametric matching approaches (Ho et al. 2007). Matching procedures control for bias on observables by seeking balance on the propensity to have received a treatment. In other words, we can use our covariates to estimate each observation's probability of having received the treatment (e.g. Article VIII acceptance), and then limit our comparisons to pairs of observations that had similar probabilities of receiving the treatment, even though one in fact did and the other did not. This allows us to create treatment and control groups which more closely approximate the experimental ideal of random assignment even though our data is observational.

For each set of five multiply imputed datasets, we form treatment and control groups through nearest-neighbor propensity score matching. For our first specification (the difference-in-differences model of openness) the primary threat to our causal inferences stems from the process by which states select themselves into experiencing rightward partisan shifts. Using our full set of control variables, our matching model therefore seeks balance on the propensity for  $SHIFTRIGHT_{it}$  to equal 1. For our second specification (the logit model of compliance with Article VIII) the primary threat to our causal inferences stems from the process by which states select themselves into experiencing Article VIII acceptance. Our second matching model therefore seeks balance on the propensity for  $ART8_{it}$  to equal 1.

A certain amount of trial and error is involved in maximizing balance across our matched sets. This is perfectly acceptable from a methodological standpoint, as the matching procedure is ‘blind’ to the dependent variable. Much like pharmaceutical experimentalists who may redraw their randomly assigned samples if they find that their initial attempt produced unbalanced treatment and control groups (e.g. significantly more women in the group receiving the treatment than in the group receiving the placebo), matching practitioners can safely draw and redraw their samples until satisfactory levels of balance are achieved. For our first specification, this process led us to draw, with replacement, a ratio of five control observations for every treatment observation using a caliper of 0.1 (meaning that observations must have propensity scores within 0.1 standard deviations of each other to be matched), leaving us with samples of 536 to 574 control observations and 123 to 136 treatment observation across our five datasets, out of an initial pool of 3,941 observations. For our second specification, this process led us to draw, with replacement, treatment and control observations in a one-to-one ratio with a caliper of 1.0, leaving use with samples of 706 to 733 control observations and 1,767 treatment observations out of an initial pool of 4,362 observations.

No methodological consensus exists in the matching literature as to the levels of balance required for reliable causal inferences or the proper tests for judging whether such balance has been achieved. In fact, Sekhon (2007) argues that the very idea of “testing” for balance is incoherent, because balance should be maximized without limit and there is thus no null hypothesis against which the observed level of balance could be compared. Nevertheless, there are still some indicators which can be used as a rough assessment. First, one can examine differences in covariate means between treatment and control groups. We note that across our ten datasets, 133 of the 165 covariates see substantial (i.e. greater than 25%) reductions in mean

differences as a result of matching. To get a fuller picture, one can also examine empirical quantile-quantile (eQQ) plots to get a sense of the differences in the distributions of the covariates across treatment and control groups that may be obscured by simple differences in means. We find that the eQQ means are improved for 151 of our 165 covariates and eQQ maximums are improved for 117 of our covariates. Thus, while the balance we achieve is far from perfect, it represents a substantial improvement over naïve estimates of causal effects based on the full unmatched samples. At the very least, the matching procedure provides us with an important robustness check for the results we report below.

## **Results and Discussion**

Our first task in estimating the impact of Article VIII commitments on monetary restrictions is to test the effectiveness of our measure of preferences for monetary openness. As discussed above, a substantial literature on political parties and economic policies suggests that the basic left-right dimension along which political parties are frequently arrayed will correlate strongly with the political parties' preferences regarding monetary openness. Leftist parties generally tend to represent the interests of the working class while rightist parties tend to represent the interests of capital. Thus we would expect leftist governments to be more willing to intervene in capital markets in order to create economic conditions that are beneficial to their constituents. Rightist governments, on the other hand, should tend to allow capital markets to operate freely because their constituents expect to benefit from these conditions.

Thus to assess the impact of Article VII controlling for the preferences of state governments we measure the preferences of the government in power according to the left/right spectrum. Developing a cross-national measure of the left-right partisan dimension, however, is

no easy task. Some have sought to categorize political parties based on the perceptions of “left” and “right” as measured in cross-national surveys such as the World Values Survey (WVS) or the Eurobarometer. Such measures would seem to be most useful when explaining mass behavior – such as voting – since they are rooted in public opinion, but would seem somewhat less appropriate for measuring government preferences regarding monetary policy – an issue that is not salient to many voters. Second, some scholars have sought to array parties on the left-right spectrum through surveys of expert opinions (Castles and Mair 1984; Huber and Inglehart 1995). This approach has the benefit of allowing experts to weigh the relative importance of different policy issues in assigning positions. For example, the salience of different issues may vary over time, and the positions taken by parties may need to be contextualized by other circumstances. On the other hand, of course, since such measures are a function of scholarly perceptions and expectations about party behavior, we must always be concerned that their left-right placement of parties may implicitly be a function of the behavior we are seeking to describe with our statistical model. More recently, scholars have launched the ambitious Comparative Manifestos Project (CMP), which performs a content analysis on party platform documents in order to place them on the left-right spectrum. Huber and Gabel (2000), for example, generate left-right placement by performing a factor analysis on all CMP party positions and label the dominant dimension “left-right.” While others (Laver and Budge 1992; Laver and Gary 1999) identify specific issue-clusters and perform country-specific factor analyses to identify left and right parties. The reliance on party platforms has the advantage of linking party placement directly to the public statements that parties make about their stances on various issues. It can become difficult, however, to determine which statements reflect preferences that parties will seek to implement when in power and which statements are “cheap talk.”

While each of the above measures have differing strengths and weaknesses, all of them are difficult to construct and are therefore only available for a relatively small number of countries. However, as we noted above, researchers for the World Bank have also developed the Database of Political Institutions (DPI), which codes party characteristics based on some relatively basic characteristics that are widely available (Beck et. al. 2000). For example, the DPI begins by coding parties on the basis of their name. “Socialist,” or “Communist” parties are coded as leftist, while “Conservative” and “Christian democratic” parties are coded as rightist. In the event that left-right leanings are not signaled in the name, the DPI coders looked at the parties position regarding the extent of government redistribution of wealth through taxation and spending on social safety net programs. DPI’s measure is crude inasmuch as it results in simple dummy variables identifying “left,” “right,” and “center” parties, but it has the advantage of being much more widely available than the other measures, which are highly labor intensive and difficult to construct.

Because of our desire to include the widest possible range of countries in our analysis of Article VIII commitments, we rely on the DPI codings for left and right parties. In order to ensure that these measures adequately capture the variation described by the more nuanced left-right scales, we correlated the DPI dummy variables for “left” and “right” parties with the more complex indices described above. The results of these correlations are displayed in Table 1. These analyses clearly indicated that – while admittedly crude – the DPI dummy variables clearly measure the same left-right variation captured by the more nuanced but less widely available indices. The correlations between the DPI dummies and other indices generally range between 0.7 and 0.8. We see the weakest fit between the DPI variables and the Huber and Gabel (2000) omnibus factor analysis of the CMP data. Even in this case, however, the correlations are

a robust 0.61 and 0.58. These relationships are remarkably strong in light of the use of dummy variables by the DPI.

Table 1 about here

Thus we are confident that the left and right dummy variables coded by the DPI will provide us with a sound basis for measuring the preferences of party governments across the widest possible range of countries. Next, however, we need to demonstrate that the left-right placement of governments by the DPI actually measures the strength of preference for monetary openness. DPI coders did not evaluate monetary policy as part of their coding. Moreover, as noted above, it seems likely that monetary policy will not be salient to many voters. Thus we must determine whether rightist parties that represent the interests of capital actually do have a stronger preference for open capital markets than leftist parties that represent the interests of labor. Perhaps the more straightforward test of this argument is to determine whether rightist parties tend to open up their capital markets.

While this implication is rather straightforward, testing it is somewhat less so. For the measures of “left” and “right” parties produced by DPI – and all of the other indices discussed above – are country specific. That is, the variables score parties within each country as “left” or “right” relative to one another. But a party platform that qualifies as “leftist” in one country may be coded as “centrist” or even “right” in another. The Democratic party in the United States, for example, is a “left” party within the context of American politics, but a party with this same name and policy platform would not be coded as “left” in continental Europe. Given the country specific nature of these codings, we cannot in an absolute sense say that “rightist” governments

will engage in greater monetary openness. We can, however, make relative statements about the “rightness” of governments and their preference for capital openness that should hold on a cross-national basis if our measures truly are capturing a left-right dimension. That is, we can say that regardless of the “left” or “right” center of gravity within a particular political system, as the government shifts toward the right we should expect the state to engage in greater openness to capital. Conversely, as the government shifts toward the left we should see relatively less monetary openness.

The best test for this kind of expectation is a “difference in differences” model. The dependent variable in this model is the relative change in capital openness for each country during a particular year. The key independent variable is a shift in the government in power toward the right. This variable takes on a value of 1 if the government in power shifts from “centrist” to “rightist” in a particular year, or if the government shifts from “leftist” to either “centrist” or “rightist.” In addition, we control for a variety of domestic and environmental constraints that may influence a government’s decision to open its capital markets. These control variables are drawn from Simmons’ (2000) analysis of capital openness and Article VIII restrictions. In addition, we also control for other basic aspects of regime type coded in the DPI dataset in order to ensure that our SHIFTRIGHT variable does not capture effects that are spuriously correlated with regime type. In particular, we include a dummy variable for military governments and states with term limits on the chief executives since such leaders may be more insulated from public pressure to intervene in capital markets. We also include a dummy variable for parliamentary systems since such governments may be less insulated from such pressures. The results of our analysis are presented in Table 2.

## Table 2 about here

The first column in Table 2 analyzes the effect of changes in the Simmons (2000) variables on changes in capital openness. The second column adds our DPI control variables for regime type. Finally, the third column presents the effect of a governmental shift to the right on changes in capital openness after controlling for these other factors. These results are strongly consistent with our contention that shifts between left and right governments cause a change in leadership preferences regarding capital openness. The coefficient for our SHIFTRIGHT variable is 0.08 ( $p < .02$ ), indicating that a governmental shift to the right is associated with increases in capital openness. None of our other DPI regime related control variables have any impact on openness, but several of the variables from Simmons (2000) are significant. In particular, changes in state reserves as a proportion of GDP, the changes in the use of IMF credits, and changes in the incidence of capital restrictions within a state's region all influence capital openness. Like Simmons, we find that reserves are positively related to openness, while use of IMF credits and a regional norm of restrictions are associated with closure.

It is, of course, possible, that shifts toward right-leaning governments may be endogenous to other factors in our model. Thus as a robustness check on the causal effects estimated in Table 2, we reanalyzed our data using the matching technique advocated by Simmons and Hopkins (2005; see also Ho et. al. 2007). As noted above, the matching technique yielded substantially improved balance on the control variables between the treated (i.e. country-years shifting to the right) and untreated groups, although some statistically significant differences remain between the two groups. Nonetheless, the technique provides an improved method for accounting for possible selection effects. We estimate causal effects for the nearest

neighbor matching analyses using a fully parameterized regression model. This technique should allow us to control for any residual imbalance that may produce confounding effects.

The nearest neighbor matching analysis continues to indicate that shifts to the right are associated with increases in capital openness even after controlling for all of the constraints modeled in Simmons (2000) and the DPI regime type control variables. Specifically, the mean estimated casual impact of a governmental shift to the right is a 0.09 increase in capital openness. Judging the substantive size of this effect is somewhat complex, since the dependent variable is a factor score aggregating various policy choices. Moreover, while the KAOPEN index ranges from about -1.75 to 2.60, the vast majority of this variation is cross-sectional. Our difference in differences model, however, only focuses on cross-temporal changes in openness within each country. Judged on that basis, a 0.09 shift on the KAOPEN scale is notably but not overwhelmingly large.

Having confirmed our hypothesis regarding the link between left and right parties and capital openness, our next task is to assess the impact of Article VIII commitments on capital restrictions while controlling both for the constraints on capital openness modeled by Simmons (2000) and changes in government preferences for openness as captured in leftward and rightward shifts in the ruling party. Simmons and Hopkins (2005) demonstrate that even after engaging the case matching technique advocated by Ho et. al. (2007) to account for selection bias, Article VIII commitments have a statistically significant and substantial impact on the incidence of current account restrictions. As we argued above, however, the Downs, Rocke and Barsoom (1996) critique regarding self-selection into Article VIII commitments suggests that such selection will be determined not only by the external constraints highlighted by Simmons (2000) but also by internal governmental preference for financial openness as measured in the

left-right shifts of state governments. Following Downs, Rocke and Barsoom (1996) we begin with the presumption that states that make Article VIII commitments are doing so because they have a preference – contingent on the constraints they face - for financial openness. Thus we expect high levels of compliance from governments that make such a commitment even if the commitment does not constrain state behavior. The key question for our analysis is whether the impact of Article VIII commitments continued to be significant and substantial even after a state's government has undergone a shift to the left. Our previous analyses indicate that shifts to the right incline governments toward financial openness, and so we infer that governments that are to the left of the government that made an Article VIII commitment will have a reduced preference for financial openness. If such governments are still less likely to place restrictions on their current account in violation of Article VIII than governments that never made a commitment to Article VIII, then we can say that an institutional commitment to Article VIII actually constrains governments from restricting capital markets even if they themselves do not have a preference for financial openness.

We test this argument through the creation of a SHIFTLLEFT variable. Rather than recording shifts from the previous year – as was the case in our analysis of financial openness – in this instance we record shifts to the left relative to the government that initially made a commitment to Article VIII. Shifts relative to the signatory government are the relevant benchmark here because we begin with the presumption that whatever government signed on to the agreement had a preference for doing so. We would only expect governments to the left of the signing government to have a reduced preference for financial openness. Since SHIFTLLEFT is coded relative to the signatory government, it is – in effect – an interaction term. The variable SHIFTLLEFT takes on a value of 1 if the state has made an Article VIII commitment *and* the

government has subsequently shifted to the left. Given our positing of this interaction effect, then the coefficients for Article VIII signature and SHIFTLLEFT can only be interpreted in combination with one another. The coefficient for Article VIII signature captures the impact of that commitment for governments that are equal to or right of the government that originally signed the commitment. The coefficient for SHIFTLLEFT captured the reduction in the coefficient for Article VIII that occurs when a state's government shifts to the left of the signatory. And finally, the impact of Article VIII after a shift to the left is determined by summing the coefficients for Article VIII and "shift left." The results of these analyses are presented in Table 3

Table 3 about here

A number of the control variables are significant, including flexible exchanges rates, a government's use of IMF credits, IMF surveillance of currency activities, terms of trade volatility, trade dependence, and the time elapsed since the last time the state restricted capital markets. Nearly all of these variables operate as expected by the literature. Terms of trade volatility, for example, increases the incidence of capital restrictions. Trade dependence, on the other hand, leads to less frequent restrictions. Reliance on flexible exchange rates also reduces the incidence of current account restrictions. Finally, the time elapsed since a restriction is negatively related to the incidence of new restrictions. Thus the policy of open capital markets has a strong inertia to it that is self-reinforcing.

One somewhat surprising result, however, is that proportion of all states in the world that have signed on to Article VIII is positively related to the incidence of capital restrictions. It

should be noted, however, that this result occurs while controlling for each individual state's participation in Article VIII. Thus we would not draw the inference from this coefficient that the spread of Article VIII commitments led to the development of a norm supporting capital restrictions. Rather it seems likely that this coefficient indicates that as Article VII commitments became more prevalent, the incidence of restrictions among states that elected not to commit to Article VIII increased. That is, the spread of Article VIII may have allowed for a "separating equilibrium" in which states that seek open capital markets can distinguish themselves from ones that do not. Whether this equilibrium is due to a constraining or screening mechanism, however, remains the focus of our tests below.

Overall, these results are similar to those of Simmons (2000) in some ways, but differ in others. For example, Simmons (2000) also found that the volatility of terms of trade, the use of IMF credits and time elapsed since the last restriction were all powerful predictors of the incidence of new restrictions. Similarly, Simmons found that trade dependence inhibited capital restrictions – though this result was not quite statistically significant in her analysis. On the other hand, Simmons (2000) also found that GDP growth, the volatility of reserves, a states volume of reserves as a proportion of GDP and its balance of payments as a proportion of GDP were all significant predictors of capital closure. Our analysis does not find these variables to be significant.

The differences between our two sets of results seem likely to stem from one of two sources. First, while Simmons (2000) relies upon robust standard errors, she does not explicitly allow for a correlation among the error terms from each country. Our analysis, on the other hand, "clusters" the cases by country and allows for correlated errors – reducing the effective  $n$  and increasing the estimated standard errors of the coefficients. A more likely source for the

differences between our analyses and Simmons' is the fact that her analysis loses nearly 25% of the possible cases to listwise deletion as a result of missing data. Our recovery of these data points through multiple imputation should guard against bias due to non-random selection.

On the most critical issue of the impact of Article VIII commitments, however, our findings are generally quite consistent with Simmons (2000). The coefficient for an Article VIII commitment is -1.64 ( $p < .01$ ), indicating that states that sign on to Article VIII commitments are significantly less likely to restrict capital markets so long as the government in power does not shift to the left. This result is widely expected – even by skeptics such as Downs, Rocke and Barsoom (1996), however, since these governments have an equal or greater preference for open capital markets to the government that initially made the Article VIII commitment. Thus this result alone cannot distinguish between arguments that Article VIII acts as a constraint on capital restrictions or simply acts as a screen that separates states that prefer capital openness from states that do not.

Consistent with our findings regarding capital openness, the coefficient for a governmental shift to the left is 0.54 ( $p < .03$ ), indicating that a shift to the left increases the incidence of current account restrictions even after states have committed to Article VIII. This finding validates our concern that Simmons's analysis (2000) did not adequately account for states' preferences regarding capital openness. Moreover, it validates the concerns of Downs, Rocke and Barsoom (1996) that compliance with international rules may not necessarily indicate that the rules operate as a constraint.

The critical test for Article VIII as a constraint comes by testing the impact of Article VIII after the government in power has shifted to the left relative to the initial signing party. We test this hypothesis by evaluating the joint significance of the Article VIII and SHIFTLEFT

variables. The results in Table 3 indicate that coefficient for the impact of an Article VIII commitment after the government has shifted to the left is -1.10 ( $p < .01$ ). Thus even after the government's preferences have shifted away from monetary openness, an Article VIII commitment significantly reduces the probability that a government will restrict capital markets. While the impact of Article VIII is reduced by SHIFTLEFT, the treaty retains about two-thirds of the effect that occurs upon signing. This result provides powerful evidence that the act of committing to Article VIII actually constrains governments from restricting capital markets – even governments that did not initially sign the treaty, and governments that do not have as strong a preference for monetary openness as the signatory government. Committing to Article VIII does perform as a screen that may signal to investors and to other states that a government is committed to open capital markets. But this signal also ties the hands of subsequent governments. While we do see some slippage in the knots, the commitment appears to bind even governments that are unlikely to have made the initial commitment.

As a robustness check on our initial analysis, we once again reanalyze these data with the matching technique advocated by Ho et. al. (2007). As discussed above, we matched the “treated” cases of states that have signed an Article VIII commitment with states that had a similar estimated propensity to sign the treaty but had not done so. Although the distribution of the data made ideal matching difficult, our use of the matching technique produced dramatically increased balance between the treated and untreated groups. As insurance against any lingering selection effects, we estimated the causal impact of Article VIII and SHIFTLEFT based on a regression model that included all of the matching variables as controls (Ho et. al. 2007). Specifically, using the matched data and the logit model displayed in Table 3, we estimated the probability of monetary restrictions under three conditions: 1) a state that had not signed Article

VIII, 2) a state that had signed but had not undergone a governmental shift to the left, and 3) a state that had signed and then experienced a shift to the left. The predicted probabilities and 95% confidence intervals around the predictions are displayed in Figure 1. The predicted probabilities of a current account restriction depending on the status of states' Article VIII commitments are depicted by large diamonds. The vertical lines extending above and below each prediction depict the 95% confidence intervals surrounding that prediction.

Figure 1 about here

Clearly, the probability of monetary restrictions during any given year by states that have not signed Article VIII is relatively high at 39%. Not surprisingly, states that commit to Article VIII are much less likely to restrict their capital markets. The predicted probability of such a restriction is 11%. Thus Article VII successfully operates as a screen that signals a government's desire to maintain open capital markets. Consistent with the concerns and expectations of Downs, Rocke, and Barsoom (1996) we see that compliance with Article VIII commitments is reduced once we have a shift in government away from a preference to comply with the treaty. States that have undergone a shift to the left after signing have an estimated annual 19% probability of restrictions. Nonetheless, consistent with Simmons (2000) and Simmons and Hopkins (2005), we find that the probability of monetary restrictions remains substantially lower for states that have committed to Article VIII, even if the government in power is politically to the left of the government that initially signed the treaty. Specifically, the risk of monetary restrictions under these circumstances is cut in half relative to a state that has never signed Article VIII.

## Conclusion

We believe that the analysis above provides a rigorous test of the claim that international institutions constrain states to behave in ways that they otherwise would not, taking in account what may reasonably be seen to be changes in the preferences of signatory-states. The evidence we have gathered is consistent with the view that committing to Article VIII provides governments with a hands-tying commitment mechanism that restricts to an observable, significant degree the freedom of choice of subsequent governments. Hence, it appears that an Article VIII commitment may, as Simmons suggests, signal the desire of the current regime to attract capital, and we would add that it effectuates to some degree a longer-term constraint on the imposition of restrictions in the future. However, as anticipated by Downes, Barsoom, and Locke, changes in the apparent preferences for capital openness of signatories that shifted to the left reduce to some degree the constraining effects of Article VIII.

While we believe that our analysis provides a rigorous test of the impact of international institutions, it does, of course, have its limitations. Perhaps most importantly, while our data are consistent with the argument that governmental leaders make international commitments with the intention to signal international investors and constrain future governments to maintain their desired policies of monetary openness, we lack data on the perceptions and intentions of government leaders that would be needed to test such an argument definitively. We are not aware, for example, of research that examines whom leaders seek to target by signing international commitments, nor are we aware of research on whether the targets – such as international investors, institutions, or other state leaders – receive and interpret the signals accurately. Future research should collect and explore these kinds of data in order to map out the causal chain linking institutions to state behavior.

Second, we have not yet explored the possibility that the impact of treaty commitments may vary over time. For example, does a commitment to Article VIII continue to constrain future governments a decade or more after the treaty was signed or does its impact fade? Simmons (2000:831) finds that the impact of Article VIII commitments erodes significantly after a state has not restricted its markets for 5 years. But this finding is due to a floor effect rather than an erosion of causal impact. Simmons' model does not include an interaction between Article VIII signature and time since last restriction. Consistent with our analyses, Simmons finds a strong temporal inertia in monetary restrictions. Once markets have been open for a length of time, it becomes increasingly unlikely that they will close in the future. Thus once states have maintained open markets for 5 years in a row, the probability of any restriction is very low and Article VIII commitments do not alter this fact. This erosion in impact is relative to time since the last restriction, however, not time since signature of the treaty. Thus if a restriction were to occur despite 5 or more years of compliance, Article VIII commitments will have a significant impact on reducing the probability of further restrictions. Thus Article VIII works to reestablish the cooperative equilibrium of open markets even if states temporarily move away from this policy.

Future research, however, should investigate whether the causal impact of treaty commitments erodes as time passes since their signature – not just since their last violation. Our research demonstrates that governments that did not themselves commit to Article VIII are nonetheless constrained to uphold this commitment, but we do not distinguish between shifts to the left that occur a few years after Article VIII signature and those that occur a few decades later. If the causal impact of institutional commitments erodes a few years after they are made, then the effects of institutional constraint estimated in our analyses may exaggerate their

substantive impact. On the other hand, if the effects that we describe in these data remain robust years and perhaps decades after the original commitment was made, then we can say with greater confidence that international institutional commitments do tie the hands of states to cooperation – albeit with slipknots.

Table 1: Correlations Among Measures of Government Partisanship

	DPI Left	DPI Right	WVS	Eurobar	C & M	H & I	CMP (H/G)	CMP (L/B)	CMP (L/G)
<b>DPI Left</b>	1 0.0000 76								
<b>DPI Right</b>	-0.7673 0.0000 76	1 0.0000 76							
<b>WVS</b>	-0.7722 0.0000 49	0.7403 0.0000 49	1 0.0000 67						
<b>Eurobarometer</b>	-0.8492 0.0000 36	0.7446 0.0000 36	0.9746 0.0000 44	1 0.0000 57					
<b>Castles and Mair</b>	-0.8028 0.0000 53	0.7734 0.0000 53	0.9248 0.0000 59	0.9293 0.0000 46	1 0.0000 75				
<b>Huber and Inglehart</b>	-0.7798 0.0000 52	0.7862 0.0000 52	0.8869 0.0000 54	0.9000 0.0000 35	0.9427 0.0000 56	1 0.0000 65			
<b>CMP (Huber/Gabel)</b>	-0.6141 0.0000 65	0.5780 0.0000 65	0.8242 0.0000 67	0.8236 0.0000 57	0.8364 0.0000 74	0.7805 0.0000 65	1 0.0000 125		
<b>CMP (Laver/Budge)</b>	-0.6944 0.0000 65	0.6975 0.0000 65	0.7381 0.0000 67	0.7841 0.0000 57	0.8019 0.0000 74	0.7937 0.0000 65	0.7918 0.0000 125	1 0.0000 125	
<b>CMP (Laver/Garry)</b>	-0.6978 0.0000 65	0.6936 0.0000 65	0.7315 0.0000 67	0.7747 0.0000 57	0.8168 0.0000 74	0.7918 0.0000 65	0.7488 0.0000 125	0.9732 0.0000 125	1 0.0000 125

Line 1:	Correlation
Line 2:	p-value
Line 3:	# Obs.

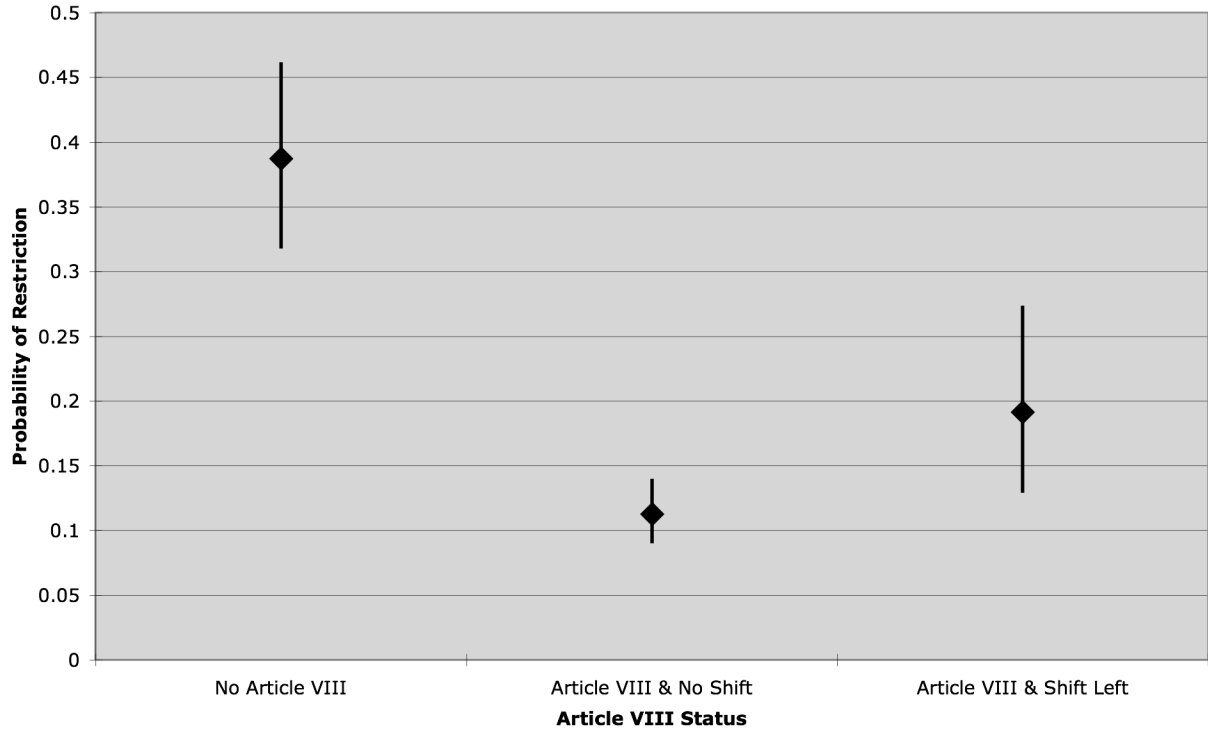
	Simmons Variables	Simmons & DPI Controls	Partisan Preferences
<b>Shift Right</b>			<b>0.079</b> <b>(0.033)</b>
Balance of Payments	0.00082 (0.00067)	0.00081 (0.00068)	0.00083 (0.00068)
<b>Reserves</b>	<b>0.54</b> <b>(0.20)</b>	<b>0.54</b> <b>(0.20)</b>	<b>0.54</b> <b>(0.20)</b>
GDP Growth	-0.00015 (0.00035)	-0.00014 (0.00035)	-0.00015 (0.00035)
<b>Use IMF Credits</b>	<b>-0.076</b> <b>(0.026)</b>	<b>-0.076</b> <b>(0.026)</b>	<b>-0.076</b> <b>(0.026)</b>
GNP Per Capita	0.000012 (0.0000080)	0.000012 (0.0000080)	0.000012 (0.0000081)
IMF Surveillance	-0.027 (0.040)	-0.026 (0.045)	-0.027 (0.044)
<b>Regional Norm of Restrictions</b>	<b>-0.0040</b> <b>(0.0018)</b>	<b>-0.0040</b> <b>(0.0018)</b>	<b>-0.0040</b> <b>(0.0018)</b>
Exchange Rate Flexibility	0.031 (0.026)	0.030 (0.025)	0.027 (0.025)
Trade Dependence	0.00094 (0.00058)	0.00093 (0.00060)	0.00097 (0.00060)
Proportion of States Signing Article VIII	-0.0056 (0.0030)	-0.0054 (0.0030)	-0.0053 (0.0030)
Military Government		-0.0020 (0.038)	-0.0039 (0.038)
Term Limitations		-0.016 (0.037)	-0.018 (0.036)
Parliamentary Government		0.052 (0.095)	0.049 (0.095)
<b>Constant</b>	<b>0.029</b> <b>(0.0061)</b>	<b>0.029</b> <b>(0.0060)</b>	<b>0.027</b> <b>(0.0061)</b>
N	3941	3941	3941

Note: Standard Errors in Parentheses. Coefficients statistically significant at 0.05 Level marked with **bold** type.

Table 3: Partisan Shifts, Article VIII Commitments and Current Account Restrictions			
	Simmons Variables	Simmons & DPI Controls	Article 8 & Preferences
<b>Article VIII Commitment</b>	<b>-1.53</b> <b>(0.24)</b>	<b>-1.56</b> <b>(0.25)</b>	<b>-1.64</b> <b>(0.25)</b>
<b>Shift Left Since Article VIII</b>			<b>0.54</b> <b>(0.24)</b>
<b>Exchange Rate Flexibility</b>	<b>-0.56</b> <b>(0.19)</b>	<b>-0.57</b> <b>(0.19)</b>	<b>-0.56</b> <b>(0.19)</b>
GNP Per Capita	-0.000026 (0.000020)	-0.000031 (0.000022)	-0.000032 (0.000021)
Regional Norm of Restrictions	0.0054 (0.0037)	0.0055 (0.0037)	0.0052 (0.0037)
GDP Growth	-0.0076 (0.0078)	-0.0069 (0.0079)	-0.0064 (0.0079)
Reserves	-0.079 (0.76)	-0.086 (0.78)	-0.079 (0.78)
Balance of Payments	-0.0057 (0.0065)	-0.0061 (0.0066)	-0.0060 (0.0065)
<b>Use IMF Credits</b>	<b>0.92</b> <b>(0.12)</b>	<b>0.93</b> <b>(0.18)</b>	<b>0.93</b> <b>(0.18)</b>
<b>IMF Surveillance</b>	<b>0.46</b> <b>(0.22)</b>	<b>0.50</b> <b>(0.22)</b>	<b>0.50</b> <b>(0.22)</b>
<b>Proportion of States Signing Article VIII</b>	<b>0.036</b> <b>(0.0096)</b>	<b>0.036</b> <b>(0.0097)</b>	<b>0.037</b> <b>(0.0097)</b>
Reserves Volatility	0.069 (0.16)	0.043 (0.16)	0.052 (0.17)
<b>Terms of Trade Volatility</b>	<b>0.34</b> <b>(0.12)</b>	<b>0.37</b> <b>(0.13)</b>	<b>0.36</b> <b>(0.13)</b>
<b>Trade Dependence</b>	<b>-0.0065</b> <b>(0.0021)</b>	<b>-0.0068</b> <b>(0.0022)</b>	<b>-0.0068</b> <b>(0.0020)</b>
Military Government		-0.32 (0.19)	-0.33 (0.19)
Term Limitations		-0.17 (0.22)	-0.17 (0.22)
Parliamentary Government		0.064 (0.20)	0.035 (0.20)
<b>Time Since Last Restriction</b>	<b>-1.67</b> <b>(0.10)</b>	<b>-1.67</b> <b>(0.10)</b>	<b>-1.67</b> <b>(0.11)</b>
<b>Time Since Last Restriction Squared</b>	<b>0.13</b> <b>(0.016)</b>	<b>0.13</b> <b>(0.016)</b>	<b>0.13</b> <b>(0.017)</b>
<b>Time Since Last Restriction Cubed</b>	<b>-0.0030</b> <b>(0.00058)</b>	<b>-0.0030</b> <b>(0.00058)</b>	<b>-0.0030</b> <b>(0.00058)</b>
Constant	-0.058 (0.84)	0.0094 (0.88)	0.042 (0.89)
N	4362	4362	4362

Note: Standard Errors in Parentheses. Coefficients statistically significant at 0.05 level marked with **bold** type.

**Figure 1: Preferences, Constraints, and Capital Restrictions**



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## Endnotes

<sup>1</sup> For examples of the impact by the analysis by Downs, Rocke, and Barsoom on discussions of international laws and institutions and cooperation, see Fearon (1998, 279, 285, 290); Koremenos 2001 (317-318); Reinhardt (2001, 179); Simmons and Martin (2005); Kahler (2003, 673); Mitchell (2002, 64-65; and 2006, 75, 80-81, 84); Gilligan (2004, 460, 462-63); Carrubba (2005, 669-670, 677-678, 686-687) Von Stein (2005); McGillivray and Smith (2006, 2510; Goodliffe and Hawkins (2006, 363); and Kelly (2007, 4).

<sup>2</sup> The premise of the Downs, Rocke, and Barsoom critique, and those who have sought to reply to them, is that a key problem that may impede the impact of international law and institutions is the problem of non-compliance, or of cheating. This is in accord with some of the main writing on international institutions, and more generally on the problem of international cooperation under anarchy, that was put forward by such authors as Keohane (1984) and Axelrod. Another pathway by which international institutions may be sapped of their efficacy concerns the distribution of gains that cooperation might produce: for an overview of the discussion of cheating and relative gains as constraints on international cooperation, see the essays in Baldwin (1993).

<sup>3</sup> The Downs, Rocke and Barsoom critique of the importance of international laws and institutions joins at least three other skeptical lines of analysis in regard to such laws and institutions. The first, realist-oriented critique of the efficacy of international laws and institutions suggests that they emerge from and influence the behavior of signatories as a product not of the preferences of their signatories taken as a whole, but rather of the material and even coercive power of their main proponents (Waltz 1979; Krasner 1991; Mearsheimer 1995). A second critique, again stemming from realist skepticism about international institutions and law, suggests that trade openness is a function of political hegemony (Gilpin 1975; Krasner 1976).

<sup>4</sup> See also the important effort by Kelly (2007) to learn if, taking into account the preference of states for the International Criminal Court, as represented by their decisions to become members, some members elected not to sign agreements with the United States that would shield U.S. citizens from the court because they had a stronger commitment to the rule of law.

<sup>5</sup> Simmons focuses on the question of whether international laws and institutions influence the *policy behavior* of states in economic matters, not on whether such laws and institutions actually contribute to the intensification or loosening of economic ties between nations through trade and various forms of capital transactions. The institutionalist premise seems to be that greater inter-state policy cooperation, or at least the absence of inter-state policy conflict, is conducive to enhanced international exchange. There is actually a lack of scholarly consensus on whether membership in international economic institutions has such an impact. For example, mutual membership in the General Agreement on Tariffs and Trade (GATT), now the World Trade Organization (WTO), has been found to be associated with a positive but statistically non-discernible effect on bilateral levels of trade (Mansfield and Bronson, 1997, 100); a negative effect on changes in trade (Mansfield and Bronson 1997, 103), but again to a statistically non-discernable degree; a largely non-significant impact on trade (Rose 2004, especially 104); a significant and positive impact only for a small group of elite “principal-supplier” industrial states (Gowa and Kim 2005); and a massively positive general impact on trade and at a statistically highly discernible level (Goldstein, Rivers and Tomz 2007).

<sup>6</sup> Another way forward might be to employ content analysis of speeches and other official documents of top decision makers; the potential utility of this approach has been demonstrated by Legro (2000).

<sup>7</sup> In this essay we examine how alterations in power of parties that reflect particular social cleavages influence a government’s orientation to international economic integration and compliance with international laws relating to such integration. It must be recognized, of course, that social cleavages themselves emerge in part as a relatively long-term result of a country’s integration with the world economy: on this process see, for example, Rogowski (1990) and Hiscox (2002).

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<sup>8</sup> An observable implication of the view that right-leaning governments are even more concerned about tying the hands of left-leaning successor governments than they are about tying their own immediate freedom of action or that of right-leaning successor governments is that right-leaning governments in countries that have had a more extensive history of left-right alternations in government should have a higher propensity to sign Article VIII than rights-leaning governments in countries with a lower incidence of such political alterations of the political orientation of governments.

<sup>9</sup> For details on the construction of these variables, please see (Simmons 2000).

<sup>10</sup> For countries which signed Article VIII before 1975, we recorded the party in power in year of Article VIII acceptance and then used DPI's coding criteria to place them in "left", "right", "center", or "NA" categories.