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International Regimes, Domestic Veto-Players, and Capital Controls Policy Stability

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States' decisions about regulating international capital movements are shaped in part by institutions and partisanship at the domestic level, but the effects of domestic-level variables are themselves contingent on the constraints imposed by the international system. We amend the veto-players hypothesis to account for the effects of international regimes on the political influence of domestic players in state decision-making. The history of changes in international financial regulations over the past four decades provides an ideal case to study the interaction of international regimes and domestic decision-making systems. We create a data set of all capital controls policy changes that 19 OECD parliamentary democracies made during the years 1951–1998. Using these new data, we find that states with a higher number of veto-player parties in government enact fewer capital controls policy changes. Furthermore, ideologically right-of-center governments in these industrialized countries are more likely than others to enact capital controls liberalizations. We also find, however, that the independent effects of these domestic-level variables disappear after the mid-1980s, when the systemic constraints imposed on individual states increased substantially.

This article addresses state policies on international capital mobility. Why do some states maintain consistent capital account regulatory policies while other, similarly situated states do not? Much of the literature that examines capital controls focuses primarily on the direction of change; that is, it seeks to explain why states either liberalize their capital accounts or impose restrictions. However, the propensity to change capital controls policies is itself an important characteristic of a state since the ability to change these regulations gives states greater flexibility to manage crises. For example, states might use this tool during periods of temporary uncertainty to help avoid speculative attacks (McIntyre, 2001; Simmons, 1994; Shafer, 1995). The flip side of this, however, is that governments that can act quickly (and are widely known to be able to act quickly) to change their regulatory policies may lack policy credibility. Instability in the ways states regulate capital movements can raise the hazard that potential investors face, which may lead them to demand higher returns or avoid the market altogether (Henisz, 2000).

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We argue that states' decisions about the regulation of international movements of capital are shaped in part by institutions and partisanship at the domestic level, but that the effects of domestic-level variables are themselves contingent on the constraints imposed by the international system. In order to test our hypotheses, we create a unique data set of all capital controls policy changes made by 19 OECD parliamentary democracies during the years 1951-1998, as reported by the International Monetary Fund. Using this new data, we find that, all else equal, states with a higher number of veto-player parties in government enact fewer capital control policy changes. Furthermore, ideologically right-of-center governments are more likely than others to enact capital controls liberalizations.

We also find, however, that the independent effects of these domestic-level political variables disappear after the mid-1980s, when the systemic constraints on individual states increased substantially. These constraints took the form of an emerging international capital controls regime (as evidenced, for example, by the signing of the Single European Act in 1986) that appeared against a backdrop of rapid growth in international capital flows.

Our study contributes to the existing literature in two ways. First, our findings contribute to a growing body of scholarship that describes policy outcomes in terms of government institutions, as we apply the theory of veto-players (Tsebelis, 1999) to a policy issue area with strong links between domestic and international politics. Existing literature that considers the consequences of interactions between domestic politics and the international environment (Martin, 2000; Milner, 1997; Simmons, 1994) shows how a single domestic veto-player can shape the international behavior of states. We show the reverse: the nature of the international regime in which a state is embedded can change the incentives and constraints that domestic decision-makers face and can thus alter the salience of domestic political factors. That is, the influence of any number of veto-players (in the broader sense that Tsebelis uses) is contingent on the nature of international constraints.[1] Our findings with respect to capital controls may be applicable to any state policy choice that has an international dimension, such as labor, tax, and environmental policy.

Second, the forces that drove international capital market deregulation in the 1970s and 1980s are not completely understood. Neither societal interests (Henning, 1994) nor systemic factors (Sobel, 1994) alone can completely account for variation across states. Although existing partisan political models (Quinn and Inclán, 1997; Oatley, 1999) have advanced our understanding of the effects of parties on capital controls, the extent to which these models can be used to test specific political hypotheses is limited by the type of capital controls data that are currently available. The new data we introduce offer a way around some of these limitations, as they allow us to integrate the veto-players approach more fully into our understanding of the determinants of global financial regulation.

This article proceeds in five sections. In section 1, we discuss the current literature on international financial regulatory change. In section 2, we develop our theoretical argument and derive hypotheses from it. We here underscore the applicability of veto-player theory to capital controls policy, but stress that the effects of domestic political institutions and interests themselves hinge on the nature of international constraints. In section 3, we discuss the methodological issues that the study of financial regulation raises and evaluate existing measures of policy outputs. The several existing measures are each useful for different purposes, yet each has potential drawbacks when applied to the question of the determinants of policy

[1] In this article, we focus primarily on international regimes as structural constraints on domestic policy-making. Others (Rogowski, 1989; Frieden, 1991; Li and Smith, 2002b) have shown that the nature of the international environment also affects the coalitional behavior of domestic interest groups. We recognize the importance of this effect, but focus on the direct effects of international regimes.

change. We argue that currently available data make it difficult to test our hypotheses, and show that an approach of measuring specific policy changes directly has certain advantages. Here we introduce and describe our data. In section 4, we use the data to test our hypotheses, and we conclude in section 5.

1. Determinants of Capital Controls Policy Changes

What determines states' capital control policies, and what causes states to change them? These two questions are obviously related, but the second one, on the sources of policy change, is uniquely important. The current literature on the political determinants of capital control policies identifies three broad influences.

Societal approaches emphasize the distributive implications of financial regulations within states and typically treat capital controls as a class issue since they affect labor and capital differently. Rodrik (1997) shows that owners of capital benefit from mobility to the extent that it allows them to diversify country-specific risk, but workers may lose, since capital movements in response to shocks can put wages at risk. Capital mobility also shifts the burden of taxation, since controls prevent owners of capital from moving their assets offshore as a way of avoiding taxes (Alesina and Tabellini, 1989). To the extent that capital controls permit greater monetary policy flexibility—which itself creates the potential for redistribution—they should be a subject of contention between labor and capital (Epstein and Schor, 1992; Clark and Hallerberg, 2000; O'Mahony, 2003).[2]

Institutional approaches emphasize that, since preferences are aggregated through institutions, it is ultimately the interaction of interests and institutions that shape outcomes. Randall Henning (1994) links high levels of capital controls to subordinate central banks and strong private connections between banks and industry; Grilli and Milesi-Ferretti (1995) provide further support to his conclusions in their 61-country study. Daniel Verdier (1998), examining the period from 1870 to 1914, argues that international capital market liberalization was most likely in centralized states since, in decentralized states, local government units that had been captured by uncompetitive banks could veto regulatory liberalization to protect those banks from the international economy.

Institutions alone cannot account for the ultimate sources of, or demands for, these policy changes. Quinn and Inclán (1997) show that societal demands, expressed through government partisanship, interact with resource endowments to shape policy. Left governments, representing labor, tended to maintain capital controls unless they were in countries with an advantage in skilled labor. Institutional features were important as well, since states with subordinate central banks tend to maintain capital controls. Thomas Oatley (1999) argues that the differences between left and right governments are conditional on the type of exchange rate regime. He finds that under fixed exchange rates left governments have higher absolute levels of capital controls than right governments. These partisan models highlight the interactions between partisanship and the institutional constraints on policymakers.

Systemic approaches have highlighted the constraints that the international environment imposes on individual states with regard to capital control policies, and argue that changes in the structure of international financial markets lead to individual governments' policy changes (Sobel, 1994). Goodman and Pauly (1993) argue that increasing globalization during the 1970s and 1980s increased the costs of capital controls substantially. The rapid growth of international financial

[2] Jeffry Frieden (1991) approaches societal interests from a sectoral rather than a class perspective. Geoffrey Garrett (1998) and Layna Mosley (2000 and 2002) demonstrate that capital mobility does not constrain governments from engaging in social democratic redistribution. Nevertheless, there is a strong empirical regularity between government ideology and capital controls policy.

markets, combined with a general move by businesses toward a global configuration, made it much easier for individual firms to evade existing capital controls. States found controls could only be effective if they were made more stringent, but stricter controls encouraged firms to exercise a previously unavailable exit option by transferring operations abroad. States soon found, in short, that the economic costs of capital controls outweighed their benefits in the new world economy. Eric Helleiner (1994) also emphasizes that increased capital mobility, combined with U.S. and British liberalizations and support for the Euromarket, helped to spark a competitive deregulation movement in other states. David Andrews (1994) similarly points to the competitive pressures introduced by capital mobility, and observes that such mobility arises not only because states choose to liberalize controls, but through technological and market changes as well. Beth Simmons (2001) and Andrew Sobel (1998) argue that systemic liberalization was triggered by domestic politics that led to liberalization within a few big states (mostly the United States with the repeal of capital controls in the early 1970s); these isolated changes combined to create a systemic increase in global capital markets, which then created systemic pressures that swept up everyone, including those big states, in a process of liberalization.

Of particular interest are studies that weigh the relative importance of the domestic versus the international environment on the capital controls policies of individual states. In examining a large cross-section of countries from 1967 to 1992, David Leblang (1997) finds that states have considerable discretion in this regard: domestic factors play a larger role in determining capital controls policies than systemic factors. Indeed, a key point in Helleiner's (1994) study is that, despite the presence of clear international constraints, the process of financial liberalization is not beyond the control of individual states. Quan Li and Dale Smith (2002b) show that governments respond to both systemic pressures and the demands of domestic supporters of capital controls liberalization when they consider changing states' capital controls policies.

We advance this research program by continuing this integration of domestic political and systemic approaches to capital control policy changes. In particular, we seek to specify the conditions under which domestic factors should be most salient, and when systemic variables should be of primary importance.

2. A Two-Level Approach to Capital Controls Policy Changes

We argue that two aspects of the domestic polity are crucial to predicting the likelihood of a particular state enacting capital controls policy changes: the number of government actors that can thwart policy change and the partisanship of the governing coalition. We also argue, however, that the predictive power of these domestic attributes is itself contingent on the constraints states face in the international arena. Specifically, changes in the international environment since the mid-1980s have significantly increased the costs to individual state leaders of pursuing independent capital controls policies. As a result, we expect to find that the independent effects of domestic political institutions and interests on states' capital controls policy choices have declined significantly over the past 15 years.

Veto-Players, Partisanship, and Capital Controls Policy Changes

Veto-players models focus on relationships among component actors within the government in explaining policy change (Tsebelis, 1999:593). Consider a government in a parliamentary system. When a government seeks to change a policy, it must propose a change to which all necessary members of the government coalition will agree. In a government coalition composed of multiple parties, the ability of any one party to influence policy rests on its importance to the survival of the

coalition. This is not a function of the number of seats the party has in parliament or the number of portfolios it holds in the cabinet. Rather, it is a function of whether or not the government needs the party's support to secure the confidence of the parliament.[3]

Consider, for example, a government of two parties that makes up a minimum winning coalition. Even if one of these parties is a junior partner, it may still have a large influence as it can threaten to leave the coalition if its interests are not met.

Empirical findings in other policy areas have supported the veto-players approach. Tsebelis (1999) finds that, in the drafting of labor laws, veto-players hypotheses were supported by the record of actual policy outputs. Robert Franzese (1996) shows that countries with high numbers of institutional veto-players tend to have persistent government debt, whereas countries with fewer veto points in the policy process tend to keep government debt under control. Mark Hallerberg and Scott Basinger (1998) show similar findings for national tax policies, and Andrew MacIntyre finds that the dispersal of veto authority within governments was a key determinant of various policy responses to the Asian economic crisis (2001) and to policy reforms generally (2002).

Applying the theory to changes in international financial capital regulation leads to the following straightforward hypothesis: the greater the number of parties that act as veto-players in a particular government, all else equal, the fewer discrete policy changes we expect. Conversely, when there are fewer veto-players, policy changes are more likely and more numerous. When examining the number of veto-players in any government, we focus in particular on the number of *parties* that act as veto-players, which in turn depends on the type of government in power. We describe precisely how we measure the number of veto-player parties within particular governments in section 4 below.

Theories about the distributive effects of capital controls contend that, holding the number of veto-players constant, partisan effects should also be evident. In wealthy countries, in which capital is relatively abundant, owners of capital should prefer an open economy in order to take advantage of risk diversification. In countries in which capital is relatively scarce, by contrast, capital interests should oppose capital market liberalization since they would be at a competitive disadvantage vis-à-vis foreign capital (e.g., Quinn and Inclán, 1997; Li and Smith, 2002a). Since Right and Center-right governments are generally more representative of capital interests, we follow the existing literature in hypothesizing that, all else equal, liberalizing capital control policy changes should occur more frequently when such governments are in power in relatively capital-abundant countries.

Two hypotheses emerge from this discussion:

H1: *All else equal, governments with more veto-player parties in government enact fewer capital controls policy changes than governments with fewer veto-player parties in government*.

H2: *In capital-abundant countries, all else equal, Right and Center-right governments enact a greater number of liberalizing changes in capital controls policies than other governments*.

The International Dimension of Capital Controls Policy Changes

Capital controls policy has an international dimension, since the structure of the international economy can constrain the range of choices available to a state (Sobel,

[3] We treat each party in the coalition as a unitary actor. Laver and Schofield (1990:15-35) conclude that this assumption is appropriate for studies of parliamentary parties on issues related to cabinet-formation. It is even more appropriate in our case since we are interested in the behavior of politicians once their party leaders are in government.

1994; Helleiner, 1994). During periods in which there is an established set of international rules and norms, states will be relatively more constrained than they will be during periods in which international rules and norms are weaker. In the absence of an international capital controls policy regime, state leaders need consider only the direct costs and benefits of a particular policy shift. Consider, for example, a rational state leader trying to maximize her domestic political position. Subject to the constraining effects of domestic institutions, she would weigh the costs and benefits that any proposed capital controls policy change would carry to her supporters against the costs and benefits such a change would impart on the overall domestic economy.

In the context of an international regime, however, her calculations would be more complicated. Under such circumstances, she would need to consider whether the proposed change is in violation of the regime, and if so, the potential costs associated with such a violation. These costs might take the form, among other things, of reputational damage when bargaining in other issue areas she deems to be important, reprisals by other members of the regime, or provoking dissent by domestic groups that had become mobilized due to the presence of the regime (Cortell and Davis, 1996).[4] All else equal, the higher the costs associated with violating the standards of an international regime, the more a leader would be willing to sacrifice other goals to pursue policies in accordance with that regime.

States will still introduce policy changes in the presence of an international regime. However, as the costs of noncompliance with the standards of the regime increase, those policy changes will be correlated increasingly with attributes of the regime itself rather than domestic-level characteristics. So, in the presence of an international regime regulating states' capital controls policies, the independent effect of domestic political variables should be reduced.

From the establishment of convertibility in December 1958 through the early 1980s there existed no clear international regime regarding capital control policies. Under Bretton Woods, states were given autonomy regarding the regulation of capital flows; as Helleiner (1994) notes, states were hardly discouraged from resorting to capital controls as a policy choice. Uncertainty concerning the nature of the international financial system reached its height, of course, with the demise of the Bretton Woods system in the early 1970s. During the 1980s, however, the international financial system became more regularized, and a clear expectation of liberalization arose regarding capital controls in the industrialized world. Indeed, by the early 1980s, the world's three financial centers—the United States, Great Britain, and Japan—had committed themselves to a program of capital account openness. Beginning in the early 1980s the United States demonstrated a greater willingness to pressure other countries to liberalize their capital accounts, in stark contrast to its tacit acceptance of capital controls in other states only a decade earlier (Henning, 1998). For example, the yen-dollar agreement of 1984, primarily a product of U.S. concerns and pressure, committed Japan to substantial financial liberalization (Helleiner, 1994; Frankel, 1984).[5] In 1986, members of the European Community committed themselves to lifting restrictions on capital flows with the signing of the Single European Act. After signing the act, moreover, key members of the EC were willing to pressure other members into adhering to its openness demands. This was evidenced by external demands (especially German) on Italy to liberalize its capital controls during the late 1980s (Goodman and Pauly, 1993). In

[4] Simmons (2000) shows that IMF legal requirements can influence state behavior even though the IMF lacks direct enforcement capabilities since willingness to comply with IMF requirements acts as a signal to market actors that a state has a responsible economic policy. The logic should apply to informal regimes as well.

[5] The agreement was motivated in part by a belief that liberalization of capital inflows (into Japan) would strengthen the yen, and thus decrease its bilateral trade surplus with the U.S. (see Frankel, 1984; also Helleiner, 1994:149, fn. 7).

short, by the mid-1980s an international regime had emerged that encouraged open capital accounts within the industrialized world (Sobel, 1998).

This regime change coincided with rapid growth in international financial markets, which served as an additional source of constraint on domestic capital controls policy choices. Goodman and Pauly (1993) show that deepening international financial markets increased the ability of firms to evade capital controls and also increased their exit options. Given such exit options, the decision by many important actors to liberalize from the late 1970s through the early 1980s meant that other states ignored this trend at potentially great cost to their competitiveness in international markets.

The presence of an international capital controls regime, combined with burgeoning international financial markets, served as a major international constraint on the ability of individual states to pursue independent capital controls policies. We therefore expect to find that the independent effects of domestic political variables on capital controls policy—the focus of our first two hypotheses—decline sharply after the mid-1980s.

H3: *The independent effects of domestic veto-players and partisanship should be significantly higher before the mid-1980s than after.*

Note that our argument is not about the initial sources of the global movement toward liberalizing regulations on capital flows. That is, we do not seek to explain or even describe the change in global financial regimes. Rather, our analysis takes regimes as a given and examines the process of decision-making within individual states. Note also that H3 is a modification of H1 and H2. A model that tests the interactive effects between veto-players and international constraints, or partisanship and international constraints, should find significant interactive effects. Conversely, the independent effects of veto-players or partisanship should become weak or insignificant in the presence of the interactive effects.

3. Capital Controls Data

The dependent variable in this study is change in capital controls. Measuring capital controls, however, is not straightforward. Scholars have tended to employ two strategies: indirect and direct observations. Indirect observations measure the extent of capital controls by calculating differences across states in the rates of return to capital. The intuition here is that in a world of perfectly mobile capital, investors would arbitrage out all differences in factor returns. Thus, one proxy for capital controls is interest rate differentials for short-term government debt. Large and persistent covered (insured against currency fluctuation) differentials in interest rates between government bonds are indirect evidence of capital immobility (Shepherd, 1994:265–271).[6]

Using interest-rate differentials as a measure of policy output is problematic, however, because of missing data[7] and because, to be reliable, the measures must aggregate data over several years making it impossible to attribute policy changes to any one particular government.

The second way to measure capital controls is to observe published national regulations directly. Quinn and Inclán (1997) code states' external capital controls based on the IMF's annual report on exchange restrictions (Quinn, 1997 describes the data). The measure they create is a 14-point scale of total financial openness.

[6] Interest rates may differ for debt issued by different states if investors expect that the national currencies are changing in their relative values. One way to measure expectations of currency value changes is the price of currency risk insurance. Factoring this in leads to the calculation of "covered" interest rate differentials.

[7] The problem is not one of data collection; interest rate coverage insurance was a financial service that simply did not exist in some periods.

These measures are on an absolute scale,[8] although they use yearly change in the scale as a dependent variable in their study.

Both the indirect and existing direct measures are extremely useful for showing trends over relatively large amounts of time and for cross-national comparisons of financial openness, since each one is a measurement of openness as close as possible to an absolute scale (Quinn, 1997; Garrett, 1998; Eichengreen, 2001 reviews measurement issues).

Despite their advantages, these existing data on capital controls have three general problems when used for analyses of political decision-making. First, they are yearly (or larger) aggregates of policy. Each observation in the Quinn and Inclán data is a snapshot of official policy at the beginning of the year. This is problematic for some models of politics, since governments enter and leave office at different times during the year, creating the potential for misattributing policy choices to governments. Second, governments issue seemingly conflicting policies at times. In some states in some years we examined, states issued one set of new regulations and then reversed them within the same year. Third, states issue many capital controls regulatory changes that are not large enough to turn up in Quinn's data. These changes do, however, have consequences for economic actors; we should therefore expect them to trigger similar sorts of distributive conflicts as larger changes, which means they should be subject to variation according to the same sets of independent variables.

We coded a new measurement of capital controls policy changes using the International Monetary Fund's annual publication *Annual Report on Exchange Arrangements and Exchange Restrictions*, a yearly summary of each member state's financial regulations. In the country reports each year, the IMF describes and dates all significant changes in each state's regulations on trade, payments, and capital movement policies. We converted the IMF's written descriptions for each reported change in our sample of countries into a set of numeric variables.

We coded changes for the 19 parliamentary democracies that have been continuously democratic since 1951, the first year for which the IMF reported regulatory changes. Our sample therefore covers Australia, Austria, Belgium, Canada, Denmark, Finland, France, Germany, Iceland, Ireland, Israel, Italy, Japan, The Netherlands, New Zealand, Norway, Sweden, Switzerland, and the United Kingdom.[9] The sample years run from 1951 through 1998. As our interest was in investment capital, we did not include regulatory changes relating to tourist allocations or payments for services. In all, we coded 1,687 observations.

To each regulatory change we assigned the date on which it occurred as well as a dummy variable measuring whether the change was restricting or liberalizing, based on the IMF descriptions. Table 1 contains five representative examples, drawn from different years and countries, of policy changes and how we coded them. Figure 1A summarizes liberalizations, restrictions, and total changes over time for the entire data set. Figure 1B breaks down the number of liberalizing changes, restrictive changes, and total changes by country.

The advantage of this data set is that it addresses the potential drawbacks of the previously available data. Since it measures policy changes and each policy change occurred on a specific date, policy changes can be attributed to a particular government.

[8] The IMF also codes the level of capital controls in each country, but these codes are in the form of dummy variables and do not capture the large variety of controls. Quinn's data set is superior since it treats capital controls as a range of policy choices from which states can choose. Since his data set is an ordinal index, however, it introduces problems of scaling that both the IMF data and our data avoid.

[9] Luxembourg should qualify based on our criteria, but its international economic regulatory policies are subsumed by Belgium's. We exclude the United States since its nonparliamentary political institutions make comparison difficult. Switzerland and the French Fifth Republic are not purely parliamentary, but have enough of the major characteristics that the variables of interest to us are comparable across cases.

TABLE 1. Coding Samples

Denmark, February 25, 1954, liberalization. "The authority given to authorized exchange dealers to permit capital transfers was expanded. Income from foreign capital investments in Denmark could be transferred to the country of the owner of the capital or to any EPU country, or freely credited to Krone Accounts IV; previously, individual permission was required."

Austria, November 15, 1963, liberalization. "Residents were permitted to obtain from nonresidents loans with maturities of five or more years for investment purposes (e.g., for expansion of production equipment); to receive loans from the foreign associates of resident enterprises. . . . All these transactions and operations were permitted provided that they were at market rates of interest . . . and with residents of countries with which Austria made settlements in convertible currencies."

Australia, June 12, 1974, restriction. "The government announced an extension to the system of screening foreign investment proposals to include those which came under notice through the exchange control mechanism and did not involve the take-over of Australian businesses. Existing procedures under which the applications for exchange control approval for foreign investment proposals were processed through the Reserve Bank were not affected."

Switzerland, November 14, 1978, restriction. "The Swiss National Bank modified the changes introduced on October 2 by: (1) prohibiting nonresidents from adding the proceeds from sales of certain investment accounts which are exempt from the negative interest requirements . . ."

France, July 11, 1980, liberalization. "In a relaxation of certain foreign exchange controls, the following main measures were introduced … [including the] relaxation of restrictions on direct investment by nonresidents in France and by residents abroad, principally by raising the maximum amount for which no authorization was required to F 5 million (from the previous level of F 3 million in general and F 1 million for nonresidents establishing individual business enterprises in France)."

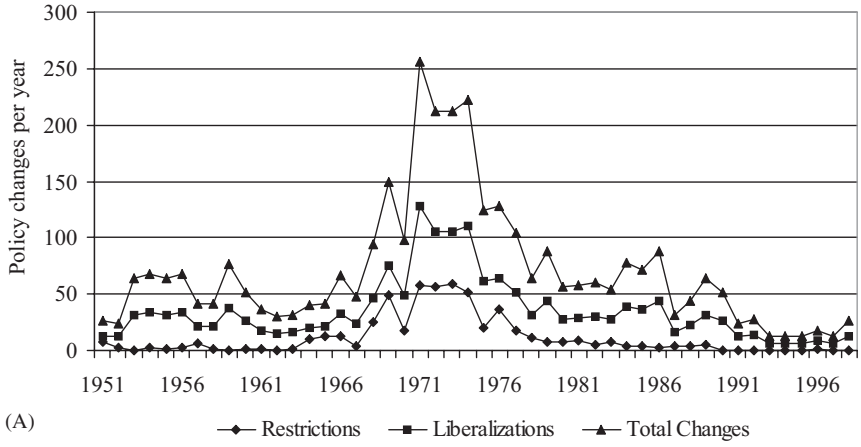
However, event-based data in general, and our data specifically, have a significant drawback in that the policy changes are each weighted equally. Clearly this is not ideal, since policy changes all have different magnitudes that are not captured in our measure. The use of this data should therefore be limited to instances in which the hypotheses being tested do not depend heavily on the magnitude of the change. The data are reasonably well-suited to our argument since we expect states' choices about both large and small changes to be influenced in similar ways by the interaction of domestic and international political pressures. That is, to test our hypotheses, we are more interested in the factors influencing states' decision-making processes than we are in the policy positions they eventually reached.

4. Models and Results

We test the hypotheses using our capital controls data and data on the economic and political conditions in the 19 countries that make up our sample. In this section we describe two models. The first takes the total number of capital controls policy changes per government as its dependent variable.[10] The second focuses only on liberalizing changes as its dependent variable. We believe that a natural start date for the analysis is 1960, since OECD currencies became fully convertible in 1959. We thus drop the first nine years of our policy changes data, making the sample years 1960–1998.

Our approach is to model the data as an event count; that is, how many capital controls policy changes of a particular kind is a government, under some specified conditions, likely to enact? We use a generalized negative binomial distribution,

[10] Taking government as the unit of observation (rather than, say, the year) allows us to avoid the possibility that policy changes will be attributed to the wrong government. When year is the unit of analysis, government transitions create a coding problem: do political variables for a transition year get coded based on the pre-transition government or the post-transition government?



(A)

Country	*Liberalizations*	*Restrictions*	*Total Changes*
Australia	44	23	67
Austria	39	32	71
Belgium	43	39	82
Canada	13	14	27
Denmark	58	13	71
Finland	51	2	53
France	122	55	177
Germany	88	58	146
Iceland	19	7	26
Ireland	29	14	43
Israel	54	21	75
Italy	72	42	114
Japan	193	29	222
Netherlands	83	21	104
New Zealand	22	16	38
Norway	38	18	56
Sweden	50	15	65
Switzerland	68	65	133
United Kingdom	74	43	117

(B)

FIG. 1A. Data Trends over Time; FIG. 1B. Capital Controls Policy Changes by Country

which has all of the properties of a Poisson distribution: both model situations in which the dependent variable is a natural number (0, 1, 2, …) and allow the modeler to make a prediction about the number of events that will occur given a set of independent variables.

We use the more flexible generalized negative binomial, rather than the Poisson, since our dependent variable is overdispersed, meaning that the variance is greater than the mean. In other words, many governments had few policy changes, while relatively few governments enacted a great many. The generalized negative binomial includes two parameters for unobserved variance in the number of events (policy changes) across observations (governments). This corrects for a problem, analogous to the problem of heteroscedasticity in standard least-squares regressions, that would otherwise lead to an underestimate of the standard errors (King,

1989). The model we use corrects for this source of distortion and provides correct standard errors.

Although correcting for the dispersion level means the model corrects for autocorrelation *within* governments, it does not control for serial autocorrelation *across* governments. We therefore include a lagged dependent variable in order to reduce the potential influence on our results of omitted variables that show persistent effects but that are otherwise not captured in our model. Since our unit of observation is a government, and different governments are in office for different lengths of time, our lag variable takes the form of the number of policy events enacted by the previous government divided by the total number of days the previous government was in office.

In addition to our new capital controls data, we use three other types of variables: economic and other control variables, measures of parliamentary veto-players, and measures of partisan preferences.

We include four economic variables in order to control for conditions that might lead governments to change policy regardless of partisanship or veto-players. These include the rate of inflation and a dummy variable for the exchange rate regime set to 1 in the case of a fixed exchange rate and 0 otherwise. We treat all varieties of pegs as a fixed rate. High inflation rates should be positively correlated with the number of capital controls policy changes: inflation could trigger capital flight and lead governments to use capital controls policy as a way of managing capital flows. The presence of a fixed exchange rate should also be positively correlated with capital controls policy changes in either direction, as the presence of a fixed rate may lead states to use capital controls as a policy tool to prevent speculation against that rate, and then to relax those controls when the currency is not under threat in order to return to reaping more of the potential benefits of mobility.

The next two variables are, for the first year of each government's term, the percent change in foreign currency reserves from the previous year and the percent change in the balance of payments from the previous year. In the first model, in which the dependent variable is total policy changes of any sort, we use the absolute value of each of these variables, since any large change in either direction might lead a government to enact a policy change. We expect both variables to be positively correlated with the number of capital controls policy changes in model 1. In the second model we use the raw change since our interest there is in the direction of policy changes. An increase in a country's reserves or an improvement in its balance of payments should reduce the need for capital controls; thus an increase in either variable should be correlated with a larger number of liberalizations. We also ran models, not reported here, with the annual government deficit (or surplus), current account deficit (or surplus), and interest rates; these were all highly collinear with existing controls and did not change our results.[11]

Using government as the unit of observation has a drawback since the economic variables do not vary over the course of the government. This is particularly problematic for governments that are in office over several years. We use economic data from the first year a government is in office, which reduces the potential bias resulting from capital controls policies affecting economic control variables. The consequences of this specification are probably limited, since most governments (70 percent) are in office for less than two years. Since we are primarily concerned with political variables, we have chosen an approach that ensures that no policy changes get attributed to the wrong government. In an alternative specification, we used the mean value of the economic variables over the life of the government and found

[11] This is consistent with other findings. In his model of partisan effects on capital controls, Oatley (1999) finds that the coefficients on budget and current account balances approach zero and are not significant.

similar results. Elsewhere (Kastner and Rector, 2002) we show on a different sample of countries from 1961 through 1986 that our basic results hold when the unit of observation is a period of time as small as two weeks.

We also include three noneconomic control variables. First, following earlier studies, we include a control variable for central bank independence (Cukierman, Webb, and Neyapti, 1994), a continuous variable that ranges from 0 to 1. The more independent its central bank is, the less benefit a government gets from insulating monetary policy from international economic influences since it cannot exercise that policy itself.

As an additional control, we use a continuous variable for the total number of changes by all countries in the data set during the first year of the government *(total systemic capital controls policy changes)*. This is a rough measure of the total amount of change taking place in global market regulation. We interpret this variable as changes in the potential payoffs to policy changes for constituents. As more changes take place system-wide, states have more interest in altering their policies.We also control for the number of days the government was in office.

Our measures of the substantively interesting independent variables are based on a data set of the types and partisan compositions of parliamentary governments developed by Jaap Woldendorp, Hans Keman, and Ian Budge (1993, 1998). In the 19 countries under observation, they describe 393 governments that began after January 1, 1960. We use the government data to create a continuous variable that serves as a proxy for the number of veto-player parties in any government. We call this variable *effective number of veto-player parties*. This variable measures the number of parties that can individually block a policy change. We calculated the effective number of veto-player parties for the four different types of parliamentary governments:

Single party majority governments: These are coded 1. In single party majority governments, only votes from the governing party are necessary to pass legislation. Thus, only the governing party acts as a veto-player party.

Surplus majority coalitions: Coded as the minimum number of government parties required to attain majority status. In surplus majority coalitions, the support of all governing parties is not necessary to pass legislation. At a minimum, a majority can be achieved without the support of the smallest party. For each surplus majority coalition, we determined the fewest number of government parties necessary to achieve a majority in the legislature. For example, the Craxi government following the 1983 elections in Italy was a surplus majority coalition consisting of five parties: the Christian Democrats (225 seats), the Socialists (73 seats), the Social Democrats (23 seats), the Liberals (16 seats), and the Republicans (29 seats). The total number of parliamentary seats was 630, meaning that 316 were needed to reach a majority. Since the Christian Democrats, the Socialists, and the Republicans (or the Social Democrats) together could generate more than 316 votes, the effective number of veto-player parties for the Craxi cabinet was 3.

Minimal winning coalitions: Coded as the number of government parties. By definition, a minimal winning coalition loses its majority if any party drops out. Thus, each government party acts as a veto-player. For example, the Kohl government following the 1983 elections in Germany was a minimal winning coalition consisting of two parties: the Christian Democrats (244 seats) and the Free Democrats (34 seats). Since the total number of parliamentary seats was 498, the support of both parties was needed to maintain majority status, and the effective number of veto-player parties for the Kohl cabinet was 2.

Minority governments: Coded as the number of government parties plus the minimum number of outside parties necessary to achieve a majority. In minority governments, all legislation must gain support from outside the governing party or parties if it is to pass. Thus, the number of veto-player parties exceeds the number

of government parties (usually by only 1). For example, the Krag government following the 1971 elections in Denmark was a minority government consisting of only one party: the Social Democrats (70 seats). Since the total number of seats in parliament was 175, the support of at least one party outside of the government was necessary to achieve majority status (for example, adding the support of the Liberals, who held 30 seats, would have been sufficient). Thus, the effective number of veto-player parties for the Krag cabinet was 2.[12]

To measure the effects of partisan preferences, we use the Left-Right ideological scale developed by Woldendorp and his co-authors. For each government, this measure ranges from 1, furthest right, to 5, furthest left. To avoid problems of scaling, we used this measure to create a dummy variable *(Right/Right-center government)* that takes a value of 1 if the government ideology score is 1 or 2. In our initial test using this variable (model 2 below), we expect all Right governments in our sample to prefer liberalization, since OECD countries are capital abundant relative to the rest of the world. However, as an additional robustness check, we also report results from a model that uses a revised version of this variable, meant to control for different factor endowments across OECD countries. In this revised version, only Right/Right-center governments in countries that are abundant in capital relative to other OECD nations are coded as 1.[13]

Finally, we use a dummy variable to test the effects of the two different time periods. The variable *weak international constraints* is coded as 1 for the period 1959 to 1985 and 0 for 1986 to 1998. (Running the models using each of the years 1984–1987 as the break point does not change our results.)[14]

Before proceeding to the multivariate regression analyses, it may be useful to present some basic descriptive patterns in the data. Tables 2A, 2B, and 2C suggest that, before controlling for other influences on capital controls policy changes, the patterns hypothesized in section 2 are largely born out by the data. Table 2A shows that governments with only one veto-player party enact fewer capital controls policy changes than governments with more than one veto-player party before 1986. Table 2B shows a similar result for liberalizing capital controls policy changes, and Table 2C shows that Right and Right-center governments enacted liberalizations with considerably greater frequency than other governments before 1986. Meanwhile, in all three cases, the effects of partisanship and veto-players become substantially smaller after the start of 1986, while the overall rate of policy changes clearly declines after 1986 as well. In short, broad patterns in the data seem to corroborate the hypotheses derived in section 2; we now turn to more nuanced analyses using event-count models.

The first model, an analysis of all capital controls policy changes including both those that liberalize and those that restrict capital account transactions, tests H1 and H3. The second model is an analysis of only those changes that liberalized capital account transactions, and therefore tests all three hypotheses.[15] We subjected both

[12] Tsebelis (1999) argues that the number of veto-player parties in minority governments is best coded as the number of parties in government. His argument is based on the ability of a government to make a vote on any particular piece of legislation a vote of confidence as well—thereby putting pressure on nongoverning parties to support legislation they might otherwise oppose. While the power to turn a particular vote into a vote of confidence undoubtedly raises the costs of voting against the government, the government nonetheless remains an agent of the parliament and must produce policies that are palatable to a majority of legislators or else it risks losing confidence anyway.

[13] According to the Penn World Tables, Israel, Iceland, Ireland, the United Kingdom, and Japan before 1980 had lower capital stock per worker than other OECD countries.

[14] In separate models we also tested the independent effects of the Bretton Woods system and membership in the European Union; neither variable turned up significant.

[15] In a separate model, not shown, we tested the effects of partisanship, veto-players, and the international system on the number of policy restrictions that each government made. We found that in neither period were Left and Left-center governments significantly more likely than others to move to restrict capital flows, although all of the coefficients were in the expected direction.

TABLE 2. Descriptive Patterns in the Data

2A: The number of veto-player parties in government and capital controls policy changes (Average changes per year)		
	Pre-1986	1986 and after
1 Veto-player party	3.55 (N = 62)	.89 (N = 20)
>1 Veto-player parties	2.13 (N = 236)	1.16 (N = 76)
2B: The number of veto-player parties in government and liberalizing changes (Average changes per year)		
	Pre-1986	1986 and after
1 Veto-player party	2.31 (N = 62)	0.76 (N = 20)
>1 Veto-player parties	1.21 (N = 236)	1.06 (N = 76)
2C: Partisanship and liberalizing changes (Average changes per year)		
	Pre-1986	1986 and after
Right/Right-center governments	1.78 (N = 188)	0.90 (N = 54)
All other governments	1.14 (N = 110)	1.10 (N = 42)

of the models we report to a series of robustness checks, dropping explanatory variables and countries one by one; the results we report are robust to a variety of different specifications.

Table 3 shows the results of the first model, testing the total number of policy changes of all types adopted by each government.[16] As the table shows, the interactive term between the effective number of veto-players and an unconstrained (that is, pre-1986) international system is negative and significant. So, prior to the strengthening of international constraints in the mid-1980s, domestic political institutions can at least partly account for variation in the adoption of new laws governing capital movements. After the mid-1980s, however, this relationship disappears, and domestic politics no longer seem to matter as much.[17]

For a substantive interpretation of the results of model 1, consider Table 4. Here we show simulations on the model (King, Tomz, and Wittenberg, 2000) to translate the results from the model into predictions of the number of annual policy changes by a hypothetical government with the mean sample values of all continuous control variables, as well as a fixed exchange rate.

Table 4 shows the expected number of annual policy changes that the model predicts as the values in the two columns. These show the possible combinations of a government with either one or three veto-player parties operating in a system with either low (pre-1986) or high (post-1986) international constraints. In a constraining international setting, the column on the right, the two types of

[16] When the mean values of the economic control variables over the life of the government are used instead, the reserves variable is significant and positive. The coefficients and significance levels of the variables of interest, however, are nearly identical.

[17] We are also able to confirm our results via an alternative way of testing our hypothesis, one in which veto-players should matter before 1986 but not after, with two separate regressions, one on governments starting before January 1986 and one on governments entering office after that date. Both regressions contain the same set of explanatory variables as in model 1, but exclude the time-period dummy and the interactive variable. Our null hypothesis for the period before 1986 is that the number of veto-players does not exert a negative effect on the number of policy changes. In a regression limited to the years 1960–1985, we can reject this null hypothesis with 99 percent certainty: the coefficient on the variable *effective number of veto-player parties* is –0.184 with a standard error of only 0.075 (which is significant at 99 percent in a one-tailed test). For the post-1985 period, we do not expect the number of veto-players to have any effect on the number of policy changes. In a regression limited to post-1985 data, we reject the null hypothesis that veto-players do affect policy change, as the coefficient on the variable *effective number of veto-player parties* is not distinguishable from 0, even at the permissive 90 percent level of confidence (coefficient: 0.159; standard error: 0.116). This alternative test thus confirms our hypotheses that more veto-players should be associated with less policy change before 1986, but that this effect should disappear after the beginning of 1986.

TABLE 3. Generalized Negative Binomial Analysis, Total Capital Controls Policy Changes

Independent Variable	*Coefficient*	*Stand. Err.*	*Prob.*
Control variables[a]			
Lagged dependent variable	43.040**	7.150	0.000
Inflation rate	0.002	0.002	0.438
Exchange rate regime	– 0.025	0.130	0.847
Reserves change	0.170	0.151	0.259
Balance of payments change	– 0.692	1.968	0.725
Central bank independence	– 0.259	0.423	0.540
Total systemic changes	0.010**	0.002	0.000
Government duration	0.001**	0.000	0.000
Weak international constraints	0.892**	0.369	0.016
Effective number of veto-player parties	0.132	0.118	0.263
Weak international constraints **effective number of veto-player parties*	– 0.290*	0.139	0.037
Constant	– 1.089**	0.391	0.005
Log of Alpha	– 0.107	0.118	0.365
Pseudo R^2 0.100			
Sample size 393			

*Significantly different from 0 with at least 95% confidence in a one-tailed test.
**Significantly different from 0 with at least 99% confidence in a one-tailed test.
[a]See text for a description of the forms of the control variables.

TABLE 4. Expected Values Based on Model 1 (from Table 3)

Values in the columns are expected number of annual policy changes, with standard errors, for different types of government in different international environments. The difference in expected value, with a standard error, from moving from one government type to another is italicized.

	Low International Constraints	*High International Constraints*
Three veto-player parties	1.61	1.59
	(.20)	(.28)
Difference	*.60**	*– .35*
	(.30)	(.33)
One veto-player party	2.21	1.24
	(.29)	(.33)

*Significantly different from 0 with at least 95% confidence in a one-tailed test.

governments produce very similar outputs: 1.24 changes per year for a government with one veto-player party and 1.59 for one with three. The statistically insignificant value of the difference between them (0.35 with a standard error of 0.33) suggests that these estimates are not distinguishable.

In a less constrained international setting, however, the two types of governments do in fact behave differently. As the veto-players theory predicts, the government with one veto-player party enacts more changes than the government with three. In this unconstrained setting, moving from one to three veto-players reduces the expected annual changes by 0.60, a difference that is statistically significant with 95 percent confidence. The results of model 1, in short, confirm the predictions of hypotheses H1 and H3. This reveals something about the influence of international regimes on the structure of domestic decision-making—that the international system shapes the relative influence of veto-player parties at home—providing an important clarification of the intersection of international and domestic politics. Thus, even if these results were not large in an everyday sense, they would still be theoretically interesting.

As it happens, though, in addition to being statistically significant, these results are also substantively meaningful. The difference in expected annual changes (even assuming, conservatively, that the number of changes other states make is constant) means that under low international constraints, a state with only one veto-player party in the government makes on average over two changes per year, or a policy change about once every five months. By contrast, a state with three veto-player parties makes a change only about every seven months and a half. In this sense, then, governments with three veto-player parties produce one and a half times as much policy stability (good for maintaining credibility for investors) or policy inflexibility (bad for responding to crises) as governments with just one.

We now turn to model 2, which examines just those policy changes that liberalize capital account regulations. In this model, the eight control variables are the same controls as in model 1, with some different specifications. The lagged dependent variable is now the number of liberalizations by the previous government, not the total number of changes, and the reserves and balance of payments variables measure the raw percent change rather than the absolute value of the change. The variable for total policy changes in the system is now the total number of liberalizations, rather than all changes as before.

The dependent variable in model 2 measures the *number*, not the magnitude, of policy changes—as was the case in model 1. The difference is that in model 2 we only include changes that were in the liberalizing direction. We thus expect that in

the liberalization model, the veto-players variable should have an effect similar to its effect in model 1. Hypothesis 1 predicts that, holding all else equal (including partisan preferences), a greater number of veto-player parties makes capital controls policy change more difficult and hence less frequent: this should be equally true for changes only in the liberalizing direction. As such, we should find that a greater number of veto-player parties are associated with a smaller number of liberalizing capital controls policy changes. At the same time, holding all else constant (including the number of veto-players), hypothesis 2 leads to the expectation that Right or Right-center governments will enact a greater number of liberalizing changes than other governments. Finally, hypothesis 3 leads us to expect to find that the independent effects of veto-players and partisanship are greater before 1986 than after, as was the case in model 1.

The results of model 2, as reported in Table 5, generally conform to our expectations. As before, the variable for effective number of veto-player parties is not by itself significantly different from zero. Similarly, the variable for partisanship has little effect by itself. However, both are significant when interacted with the type of international system. During periods of low international constraints, governments to the right of center as well as governments with fewer veto-players distinguished themselves from other governments by enacting significantly more liberalizing policy changes. Each of these two separate domestic political conditions *independently* has a significant influence on policy during the earlier period but not during the later period.

The independent influence of low international constraints is now insignificant. Does this invalidate our claims concerning the presence of an international capital controls regime after 1985? We believe that it does not. The regime that emerged in the mid-1980s was one of openness: countries that had not yet liberalized their capital accounts were encouraged to do so, and those that already had would have found it difficult to re-impose restrictions. So, states would have found it more costly to implement *any* policy changes independent of the international environment. This is the regime change to which we referred earlier.

Whether such a regime change implies an overall increase in the number of liberalizing *policy changes* after 1985 is unclear. While states that maintained capital controls in the mid-1980s would be expected to have implemented liberalizing changes, states that had already liberalized by that time would not. We would expect that states would have been less willing, after the mid-1980s, to use capital controls policy changes to pursue other state-specific (such as staving off speculative attacks, etc.) or partisan-specific goals: the decline in the use of capital controls as a policy tool would be expected to lead to both fewer restrictive changes *and* fewer liberalizing changes that are independent of systemic changes. In short, we would expect to find that after 1985, states were less willing to pursue independent capital controls policy shifts; whether the number of liberalizing changes should have increased is unclear. We would certainly expect to find, however, that fewer *restricting* policy changes occurred after 1985. Since the regime was one of openness, states that had already opened their capital accounts by the mid-1980s as well as states that were in the process of doing so would have found it costly to implement new controls on capital flows that bucked the international trend. A separate test confirmed that, in fact, governments after 1985 were significantly less likely to make restricting changes in their capital controls policies, holding all else constant.[18]

[18] Taking the number of restricting changes as the dependent variable, using the same sample size and set of control variables as used in Table 5, and controlling for the number of veto-players and the partisan composition of government, a generalized negative binomial regression finds that a dummy variable coded 1 if the government began after 1985 and 0 otherwise has a coefficient –1.76 against a standard error of 0.36. This suggests that governments implemented fewer capital controls policy restrictions after 1985 than before, holding all else constant.

TABLE 5. Generalized Negative Binomial, Liberalizing Capital Controls Policy Changes

Independent Variable	*Coefficient*	*Stand. Err.*	*Prob.*
Control variables[a]			
Lagged dependent variable	62.177**	11.738	0.000
Inflation rate	– 0.001	0.003	0.687
Exchange rate regime	– 0.118	0.140	0.399
Reserves change	0.005	0.128	0.969
Balance of payments change	1.453	1.438	0.312
Central bank independence	– 0.528	0.470	0.261
Total systemic liberalizations	0.017**	0.005	0.000
Government duration	0.001**	0.000	0.000
Weak international constraints	0.607	0.406	0.135
Effective number of veto-player parties	0.178	0.123	0.147
Weak international constraints *effective number of veto-player parties*	– 0.368**	0.147	0.012
Right/center-right government	– 0.355	0.281	0.207
*Weak international constraints *Right/center-right government*	0.674*	0.321	0.036
Constant	– 1.156	0.431	0.007
Log of Alpha	– .0727	.126	
Pseudo R^2 0.095			
Sample size 393			

*Significantly different from 0 with at least 95% confidence in a one-tailed test.
**Significantly different from 0 with at least 99% confidence in a one-tailed test.
[a]See text for a description of the forms of the control variables.

TABLE 6. Expected Values Based on Model 2 (from Table 5)

Values in the columns are expected number of annual liberalizing policy changes, with standard errors, for different types of government in different international environments. The difference in expected value, with a standard error, from moving from one government type to another is italicized.

	Low International Constraints	*High International Constraints*
Three veto-player parties;	1.13	.95
Right and center-right governments	(.16)	(.23)
Difference	*.52**	*– .26*
	(.26)	(.19)
One veto-player party;	1.65	.69
Right and center-right governments	(.28)	(.23)
Difference	*– .45**	*.29*
	(.23)	(.24)
One veto-player party;	1.20	.98
Left, center-left, and center governments	(.21)	(.28)

*Significantly different from 0 with at least 95% confidence in a one-tailed test.

In Table 6 we report the expected number of policy liberalizations for each type of government along the same pattern as Table 3. Assuming a government with the mean value on all of the control variables and a fixed exchange rate, the values in the cells report the expected number of yearly liberalizing changes for six combinations of one and three veto-player parties, right-leaning and other governments, and low and high international constraints. The pattern of liberalizing policy outputs is similar to that of all policy outputs—all else equal, domestic politics plays a significant role in determining changes to capital controls policy before 1986 but not afterwards.

Specifically, in a world of low international constraints the three types of governments (a Right or Right-center government with three veto-player parties, a Right or Right-center government with one veto-player party, and a Left, Left-center, or Center government with one veto-player party) behave noticeably differently according to their types. That is, the differences between the levels of policy output are significant when changing from more to fewer veto-player parties and also when changing from a conservative to a nonconservative ideology. In the more highly constrained world, however, these differences are smaller and are also statistically insignificant. The results support the predictions of all three hypotheses.

What happens if we modify the partisan effects variable to account for factor endowments differences? In H2, we followed Quinn and Inclán (1997) by predicting that conservative governments should only prefer liberalization in relatively capital abundant economies. In model 2 above, we assumed that, since our sample is limited to OECD countries, all countries are capital abundant in *world* markets. In a separate robustness test, we changed the coding of the Right/Right-center government variable to equal zero for all governments—regardless of partisan orientation—in countries that had low capital stock per worker relative to other OECD countries. The new interactive variable (between loose international constraints and Right/Right-center governments in non–capital scarce countries only) performed similar to the one in model 2, having a coefficient of 0.732 and a

standard error of 0.322.[19] These results offer further confirmation of hypotheses H2 and H3 above.

5. Conclusion

Our hypotheses are that the structure of veto-player parties and the partisan character of governments in parliamentary democracies influence the choices that those states make with respect to regulations on their international capital accounts, but that domestic politics have become less important as the international system has become more constraining. Our findings support these hypotheses.

These and other similar findings may have some broader implications for models of policymaking within the study of international relations. During periods of uncertainty—that is, during periods when the constraints imposed by international norms and expectations are weak—the structure of governmental decision-making appears to play a significant role in what choices states make. However, when the international system provides less slack, there is not a discernible difference across types of governments.

Our results are consistent with models of two-level games (Putnam, 1988; Milner, 1997). These posit that domestic veto-players will exert less of an independent influence on policy outcomes when two domestic veto-players (usually thought of as the executive and the legislature) are in relative agreement with respect to the options presented by foreign players. We extend the argument by explicitly showing the effects on policy outcomes of having more veto-players and by varying the international environment, and by showing that each of these are consequential. Our study demonstrates a way of thinking about the rest of the world more broadly than as a "foreign player" with a fixed set of preferences with which the home state bargains.

Our findings are also consistent with the emerging literature on policy contagion in capital controls policy. Beth Simmons and Zach Elkins (2003) argue that states tend to liberalize after others in their peer group do. We capture this process (in a less sophisticated way) in our "systemic changes" independent variable and in our discussion of regimes. In general, the international system creates opportunities and constraints that are felt through their sometimes subtle effects on domestic politics; our findings suggest that exploring this interaction will be a fruitful area of future research.

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[19] Surprisingly, the coefficient on the variable measuring the independent effects of partisan orientation actually became significant and negative in this separate test (with a coefficient of –0.476 and standard error of 0.287), meaning that in countries abundant in capital relative to other OECD countries, conservative governments were actually slightly less likely to liberalize after 1986. We do not have a ready explanation for this. It could, of course, simply be spurious correlation, but it is worthy of future research.

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