ARE SOME DEMOCRACIES MORE DANGEROUS THAN OTHERS? DOMESTIC INSTITUTIONS AND THE INITIATION OF CONFLICT

by

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(Under the Direction of Markus M.L. Crepaz)

ABSTRACT

Two prominent literatures, the democratic peace and veto player theory, maintain that the democratic use of force is constrained by separated institutions of government and the placement of political actors whose agreement is required for policy decisions. I argue differently, positing that *institutional context* is the chief dimension along which democracies vary in their international conflict behavior. Reframing conventional veto player theory, I argue that the context in which veto players operate, whether collective or competitive, mediates their impact. In the former, political actors have motive to facilitate, not oppose, the policy process even when they have the opportunity to do so. In the latter, political actors have both the opportunity and motive to block policy change. An argument centered on institutional context refines how scholars think about domestic politics and international relations. In terms of the democratic peace research agenda, for instance, this dissertation elucidates the manner in which domestic institutional variation influences the foreign policy making of democracies. This dissertation also refines how scholars employ veto player theory as an explanation for the impact of domestic politics on aggregate policy outcomes. Veto players, whether institutional or partisan, do not always veto policy. While different institutional configurations produce a varying number of

veto players, these veto players do not always constrain the democratic use of force. Results of pooled time-series cross-sectional analyses of twenty-nine democracies in the postwar period indicate that a collective institutional context, represented by weak legislative opposition to the executive and large coalition cabinets, has an enabling effect of the democratic initiation of conflict. Conversely, a competitive institutional context, represented by strong legislative opposition to the executive and smaller ruling coalitions, has a constraining effect. This dissertation concludes that democratic policy outcomes are determined not by any static institutional organization (as postulated by the democratic peace literature), or on any rigid distribution of veto players (as advocated by conventional veto player theory), but on a more dynamic dimension of institution context.

INDEX WORDS: Democracy, Democratic Peace, Political Institutions, Veto Players, International Conflict, Foreign Policy, Institutional Context

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CHAPTER 1

INTRODUCTION

The scholarly study of international relations has been long characterized by the paradigmatic debate between realism and liberalism. The two most prominent schools of thought, neorealism and neoliberalism, vehemently disagree over a variety of issues including the prospect of cooperation among states in the international system, the nature and consequences of international anarchy, the impact of international institutions on the behavior of states, the priority of state goals, the pursuit of absolute versus relative gains, and the importance of power as a tool of statecraft. While neorealism and neoliberalism are thus seemingly at constant odds, they both look toward the systemic level to explain international phenomenon without reference to the internal character of states. For realists, international relations are best understood by reference to the distribution of power in an anarchic international system. For neoliberals, international relations are better explained by considering international institutions as a mitigating factor to anarchy and the distribution of power.

More recently, however, systemic theories (particularly neorealism) have come under forceful criticism for failing to capture the importance of the domestic characteristics of states and their consequent linkage to international phenomenon. Especially within the field of international security studies (long the singular domain of neorealism), numerous studies have asserted that political structure and processes *within* states affect the international behavior of

those states. In particular, scholars working within the liberal democratic peace tradition, the most prominent strain of literature in this genre, have noted that democracies do not go to war with other democracies, a proposition that at least one prominent scholar considers "the closest thing we have to an empirical law in the study of international relations" (Levy 1989: 88). What distinguishes this line of research is its reliance on the institutions of democracies themselves to explain this distinct international behavior: democracies, it seems, behave differently than non-democracies.

While copious empirical evidence supports the democratic peace assertion, it tells us little about the precise domestic mechanisms, and the accompanying variation among them, by which states formulate their decisions to engage in specific international behavior. Moreover, most of this research overlooks within regime variation, treating democracies as largely alike. Despite this conventional treatment of democratic homogeneity within the democratic peace literature, however, scholars of comparative politics have long know that democracies exhibit considerable variance both cross-nationally and temporally; institutional structures and their political control vary widely across states and over time. If democratic foreign policy making is indeed unique (which most scholars agree that it is), and if democracies vary in their institutional configuration across time and space, then might these institutional variations be associated with variations in democratic conflict behavior?

In the pages that follow, I set out to answer this question. In doing so, I develop a theory of domestic politics and the initiation of conflict by democracies wherein changes within domestic institutions affect state conflict behavior. I argue that *institutional context* is the principal dimension along which democracies vary in their international conflict behavior. As the institutional context between political institutions changes over time and space, so does a

democracy's conflict behavior. Distinguishing institutional context moves this research beyond the democratic peace literature which classifies states according to their unchanging institutional structure whereby states are either democratic, or they or not, with the democratic institutions of separated powers and electoral accountability constraining the use of force as a tool of statecraft.

The democratic initiation of conflict, I argue, depends not only on such democratic institutional structures (which are relatively static), but also on the control of those political institutions (which is highly dynamic). Specifically, when control of political institutions creates a "competitive" institutional context, democracies are more constrained in initiating conflict. Conversely, when control of political institutions creates a "collective" institutional context, democracies are less constrained in initiating conflict. Under a competitive institutional context, political actors (such as strong legislative oppositions) have the motive and opportunity to block the use of force by democratic executives. Alternatively, under a collective institutional context, political actors (such as multiple partners in coalition cabinets or weak legislative oppositions) have either no motive or no opportunity to constrain democratic belligerence. Given a certain institutional context, some democracies are thus more "dangerous" than others.

In this chapter, I introduce the rationale behind this dissertation, namely, uncovering the impact of domestic political institutions on a democracy's ability to initiate conflict abroad. To that end, I first present the two literatures that form the conventional wisdom regarding the domestic politics of international conflict: the democratic peace and veto player theory. While the chapters that follow discuss in greater detail these literatures, even a cursory examination reveals a number of puzzling relationships left unresolved by the conventional wisdom. Then, I present briefly the argument advanced by this dissertation that resolves these puzzles, follow with a description of the scope of this study, and end by outlining the plan of this dissertation.

A Liberal Perspective

In this dissertation, I present what scholars term a "liberal" perspective on international relations. Liberal, in this sense, distinguishes scholarship in this tradition from "realist" perspectives that frequently dominate the field of international relations, particularly studies of international conflict. While realism is a diverse research program, all realist perspectives agree that power—who has it and how it is distributed internationally—is the chief factor that determines state behavior regardless of the politics within states. Noting that other-than-power considerations are also relevant to international politics, however, a large research effort has grown in the liberal tradition that challenges the core assumptions and ideas of realism. In particular, this research criticizes realism for failing to capture the importance of the domestic characteristics of states and their consequent linkage to international phenomenon. Especially within the field of international security studies, numerous studies have asserted that a state's *domestic* political structure will affect its *international* behavior. This underlying liberal logic—that domestic politics serves as a fundamental determinant of state behavior in international relations—forms the basis of this study.

The most prominent strand of liberal research in the international relations literature, and in fact the main reference point for this dissertation, is the "democratic peace" research agenda. According to this line of scholarship, democracies seldom, if ever, go to war with one another. Following the insights of Immanuel Kant, the theoretical explanation for the democratic peace revolves around two models, one normative and the other structural. According to the former, shared democratic norms and liberal ideology that emphasize regulated competition through peaceful means inhibit conflict between two democracies. Democratic states, accustomed to solving domestic disputes in a nonviolent fashion, extend the norm of peaceful conflict

resolution to the international arena. Alternatively, structural or institutional theories of the democratic peace emphasize the complex and competitive institutions of democracies that constrain democratic leaders from initiating war with another democracy. Specifically, the institutional separation of powers and the democratic requisite of the consent of the governed make democracies unlikely to initiate conflict. The former generates institutional constraints against such behavior as checks and balances within a democracy constrain belligerent foreign policies. The latter produces additional costs for democratic leaders as those who typically bear the costs of war (citizens) also select their leaders. In this manner, political leaders can expect their political institutions and citizenry to penalize costly foreign policies such as war, so leaders avoid such policies.

The relationship between these two explanations and peace is robust; scholars now confidently predict that democracy has immutably pacific benefits for interstate relations. What is more, recent research has begun to articulate that the democratic peace is not only a dyadic phenomenon (occurring between two democratic states), but a monadic one as well (extending from democratic states to non-democratic ones). Particularly in relation to the structural mechanisms cited by democratic peace scholars, there is an emerging consensus that at least some of the institutional constraints that prevent democracies from going to war with one another operate irrespective of a potential adversary's domestic governing structure.

The democratic peace finding presents a persuasive challenge to realist formulations of international relations. While realists argue that regime type in no way mitigates the effect of the immutable "laws" of the international system, democratic peace scholars have shown that power considerations do not necessarily predetermine democratic state behavior. In short, democracies exhibit conspicuously different international behavior than realism would otherwise predict.

Domestic factors, it seems, play a fundamental role in determining interactions between states, particularly in regard to how democratic constraints shape the decision to go to war.

Unfortunately, our confidence that an international system populated by democracies leads to more peaceful relations among states is equaled only by our uncertainty as to what specific factors cause such a peace. In fact, the explanation for a liberal peace treats democracy as a fixed set of institutions and norms that place impediments upon belligerent foreign policies without reference to any specific processes or institutions within democracies. Accordingly, democracy, as form of domestic political organization different from autocracies, changes little if any over time and across different democratic states, yet democracies vary widely both in their institutional form *and* their foreign policies. The current state of the democratic peace thus leaves us with a puzzle: democracies are more constrained than non-democracies in the use of force internationally, yet democratic countries demonstrate considerable variance in their conflict propensity without explanation. Among the traditional "major powers" of Europe in the post World War II period, for instance, both France and the United Kingdom are five times more likely than Germany or Italy to initiate an international dispute. Even among "lesser powers," Portugal and Australia are at least twice as likely as Spain or Belgium, and ten times more likely than Austria or Denmark, to initiate conflict (Chapter 4 presents more detailed data on rates of conflict by democracies). This is especially puzzling given that all of the cited countries are considered "equally democratic" by most democracy scholars.

How might we begin to resolve this puzzle regarding the widely different rates of conflict among equally democratic countries? The solution, I will argue, can be found in developing a better understanding of how domestic political institutions constrain the use of force among democracies. The democratic peace cites two possible explanations for the pacific nature of

democratic foreign policy: norms and institutions. While norms of peaceful conflict resolution are common across all established democratic states, institutional configurations are not. This is to say that while all democracies employ some means of separating powers, and all democracies base their political legitimacy on the consent of their citizens, the methods by which they do so vary. It follows that institutional variation among different forms of democracies should produce variation in their international behavior.

In order to uncover why democracies vary so widely in their foreign policy, we must then differentiate democracies according to their varying political institutions. The most theoretically developed manner to so differentiate democracies, one that currently is receiving the most scholarly attention, is through the theory of veto players. It is toward this second strand of literature that forms the conventional wisdom regarding the effect of political institutions on policy making—including the policy decision to initiate conflict—that I now turn my attention.

Veto Players

According to the theory of veto players, as articulated by its most prominent proponent George Tsebelis, we can differentiate democratic political systems by their capacity to produce policy change. Changing the policy status quo in a democracy requires the consent of the political actors who have the power to "veto" or block the policy process, and emerge from variously configured political institutions. These political actors Tsebelis calls "veto players," and they may be institutional (such as presidents and legislative chambers) or partisan (parties in government) in nature. While institutional veto players emerge from the constitutional separation of powers (say between the President and Congress in the US presidential systems), and partisan veto players are common to parliamentary systems (such as multiple partisan partners in a

coalition cabinet), the effect of both is the same: more numerous veto players have the effect of decreasing the capacity for democracies to implement any policy change.

Veto player theory has been used to explain a variety of aggregate policy outcomes, from social welfare policies to, more recently, foreign policies regarding international conflict. In a general sense, all of the literature on veto players agrees that in those systems that diffuse power the most—whether between separated institutions or among more political parties within those institutions—policy production (i.e. passing laws) becomes more difficult because more veto players exist to block policy. As applied to the explanadum of this study, veto player theory predicts that *the potential to initiate conflict as a foreign policy choice decreases with the number of veto players*.

The effect of veto players on a state's ability to go to war is intuitive enough: the more institutional hurdles that exist within a democracy, the more difficult it becomes for that democracy to commit to such a costly foreign policy. In particular, research in this area has focused on the constraints that democratic executives face in committing to international conflicts, postulating that where institutional configurations establish the most veto players vis-à-vis the executive, conflict initiation is less likely. As the following chapters outline, two principal conclusions that one might draw by applying veto player theory to democratic war involvement are that, first, because of the institutional separation of legislative and executive powers, presidential systems might be particularly constrained in the use of force (i.e. there are more institutional veto players). And second, multiparty governing coalitions are more constrained than single party governments because of the inclusion of more partisan veto players.

Unfortunately, these expectations do not square with empirical findings in the literature. The most belligerent democracy, perhaps not unexpectedly, is the United States, which initiates

international disputes at rates many times greater than parliamentary systems with no institutional separation between the legislative and executive branches. That the U.S. political system separates power between at least two institutional veto players (Congress and the President) does not seem to inhibit the use of force compared to other democracies. In fact, a review of the literature on institutional veto players and international conflict reveals two conflicting expectations. First, presidential systems, even with an institutional separation of powers, are more prone to conflict than parliamentary systems because of executive policy dominance in foreign affairs, and second, parliamentary systems are more belligerent than presidential systems due to the inter-institutional fusion that gives parliamentary executives greater flexibility. On the question of partisan veto players, similarly inconsistent findings appear in the literature. While veto player theory holds that multiparty executive coalitions are more constrained in initiating conflict due to the inclusion of more veto players, there is just as much evidence that they are less constrained because they enjoy greater parliamentary support for their policy decisions.

Thus, while veto player theory can be useful as a means to describe the constraining effects of domestic institutions as well as to differentiate democracies, its use has produced conflicting theoretical expectations in some cases and inconclusive empirical findings in others. Veto players, it seems, may not always constrain belligerent foreign policies; or to state it differently, veto players do not always seem to veto. This literature therefore raises another puzzling question: why do veto players not always have a constraining effect on the use of force by democracies?

What is needed to resolve the puzzles emerging from the democratic peace and veto players literatures, as I argue in this dissertation, is a contextual theory, one that considers

conditions existing within otherwise static institutional structures that can explain these unresolved theoretical expectations and empirical patterns. Certainly, constitutionally determined democratic structures can constrain the use of force as a foreign policy tool by establishing more potential veto points. But these institutional structures change rarely, if ever, while seemingly having an irregular effect on foreign policy outcomes. Political actors within these institutions, however, change frequently as a result of the normal democratic political process (through elections, governing coalitions, and the like), and I will argue that this variation within political institutions (hitherto overlooked by the conventional wisdom) accounts for the unexplained and unexpected variance in rates of international conflict by democracies.

The Argument

The democratic peace and veto player literatures form what best can be called the conventional wisdom regarding how democratic political institutions might influence the initiation of conflict. Taken together, these perspectives maintain that the democratic use of force is constrained by separated institutions of government and the placement of political actors whose agreement is required for policy decisions. In the former, democracies are more constrained than non-democracies because of the institutional hurdles of separated powers and the consent of the governed (i.e. veto players). In the latter, some democracies are more constrained than others because of the inclusion of additional institutional and partisan veto players required to make the political decision to initiate conflict. In short, those political systems that require the consent of more political actors are simply more constrained: the greater the number of veto players, the less likely conflict will emerge as a foreign policy decision.

Yet this conventional wisdom leaves us with two inextricable puzzles. The mechanisms cited by democratic peace scholars have an indeterminate effect: democracies are more pacific than non-democracies, yet equally democratic countries vary widely in their use of force in international relations. Likewise, the veto player literature raises the puzzling prospect that similar institutional arrangements can cause widely divergent outcomes in terms of conflict propensity—or that diverging institutional configurations are associated with similar rates of conflict.

To resolve these puzzles, this dissertation argues that the theories underlying the conventional wisdom are mis-specified in that they describe an overly static conception of political institutions. The democratic peace literature, for instance, presents an exclusively bivariate understanding of domestic institutional organization: states are either democratic or they are not, and variation between democracies in their foreign policy thus can not be accounted for. By the same token, veto player theory paints an equally rigid picture of domestic institutional constraints on democratic war-making: those institutions that establish more individual or collective actors (i.e. veto players) are simply the most constrained. The process by which democracies initiate conflict, I argue in the following pages, is more dynamic. Veto players have a more variable effect than the literature currently presents.

The main argument of this dissertation is that the effect of veto players is contingent upon the setting in which they operate. This setting is what I term *institutional context*, and is defined by both the opportunity for political actors to veto policy change as well as the motivation underling veto player behavior. Institutional context can be either collective or competitive. In the former, political actors have motive to facilitate, not oppose, the policy process even when they have the opportunity to do so. In the latter, political actors have both the opportunity and

motive to block policy change. Under this framework, defining veto players by their opportunity to block policy change is not sufficient; we must also understand veto players' motive before we know their effect on the policy process.

I further argue that institutional context operates along two dimensions. The first is in *the nature of the ruling coalition*, which I term "executive institutional context." When executive cabinets are comprised of broad multi-party coalitions, there is considerable motive to facilitate the policy process among numerous political actors in order to maintain the governing coalition. Broad coalition cabinets thus represent a collective institutional context, one in which veto players have motive to support, not constrain, the policy process so as to maintain their positions in power. Institutional context becomes more competitive as the size of the ruling coalition decreases. Unlike coalition governments that hold a large share of parliamentary votes and whose members act collectively, single party majority governments command much less legislative support and typically face more hostile and stronger oppositions. The constraints facing minority cabinets are even more pronounced as they face even larger oppositions and encounter more veto players who have motive to oppose executive policy.

The second dimension on which to measure institutional context is in *the nature of the political opposition*, which I term "executive-legislative institution context." Whether in presidential or parliamentary systems, political oppositions have strong motive to oppose executive attempts to change policy for they seek to gain political office or redirect public policy in a different direction. In presidential systems, when the political opposition controls the legislative branch (i.e. in the case of divided government), the opposition can veto executive policy. In parliamentary systems, political oppositions can constrain executive policy when it is sufficiently strong to expose deficient government policies and offer viable alternatives. In these

instances, a competitive institutional context predominates. Institutional context on this dimension becomes more collective when political oppositions are weak. Weak political oppositions do not have the opportunity to redirect or veto public policy for they either do not control an institution with veto power (in the case of presidential systems) or they can not field a viable policy alternative (in the case of parliamentary systems).

In a nutshell, veto players do not always block the policy process. Their effect is more dynamic, and depends on institutional context. Under a collective institutional context, policy change, including a policy to initiate conflict, is easier. But when veto players operate within a competitive institutional context, policy change becomes more difficult. With broad coalition cabinets within the executive, and exceedingly weak political oppositions in the legislature, we can say that veto players exist within a collective institutional context—collective because, while there may be opportunity for oppositional behavior, there is no motive to do so. Rather, veto players have motive to facilitate the policy process because they share political responsibility and accountability. On the other hand, with smaller ruling coalitions in the executive, and strong political oppositions in the legislature, we can say that veto players exist in a competitive institutional context—competitive because there is opportunity and motive for oppositional behavior toward executive initiatives to change policy.

Scope

Since this dissertation is motivated in large part by the puzzle regarding why democracies vary so widely is their foreign policies, I limit my scope first to the universe of democracies. This is unlike the democratic peace literature which focuses on differences in international behavior according to regime type (democracies versus non-democracies). By limiting the scope to only

democracies, I can more meaningfully assess differences between democracies, taking seriously the notion that heterogeneity among democracies may be systematically related to differences in their conflict behavior.

I further limit the unit of analysis to only the "most democratic" regimes, thus excluding countries that may have some democratic attributes while missing others. There is, of course, some debate as to which countries qualify as a democratic or not (post-Soviet Russia is a good example), but I rely here on established scholarship regarding what constitutes a stable, fully democratic regime. Specifically, I select countries in which there is wide agreement in the literature that they are democratic and for which dependable data is available. For this, I rely on data derived from Reiter and Tillman (2002) who base their list of stable democracies on Lijphart (1999) and Mainwaring (1993), who in turn rely on the oft-cited Polity data set (Marshall and Jaggers 2000). From the Polity data set, only those countries that are considered "strongly democratic" are included: those countries that are characterized by competitive political participation, open and competitive executive recruitment, and institutionally constrained executive powers. This set of democratic countries is additionally limited by Mainwaring's criteria for a stable democracy, which, following Dahl (1971), includes only those countries in which there is participation in competitive elections and protection of individual rights. Lijphart further restricts this list of democracies to those that have a minimum of nineteen years of democratic experience, thus ensuring that each democracy is "reasonably stable and consolidated" (1999: 53).

From this combined Polity-Lijphart-Mainwairing list of democracies, I was able to collect government-level data (including parliamentary and executive cabinet metrics) for twenty-nine established democracies over the period 1945 to 1992. The countries included in this

dissertation are Australia, Austria, Bahamas, Belgium, Botswana, Canada, Denmark, Finland, France, Germany, Greece, Iceland, India, Ireland, Israel, Italy, Jamaica, Japan, Luxembourg, Malta, the Netherlands, New Zealand, Norway, Portugal, Spain, Sweden, Switzerland, the United Kingdom, and the United States.

Practically, limiting my scope to the most democratic regimes has the methodological advantage of including only those cases in which there is readily available and consistently reliable data. More importantly, however, so limiting the scope maintains a "most similar systems" research design in which we can isolate differences in democratic political institutions which I hypothesize are related to differences in their foreign policies. This is an important distinction in that, since I place theoretical importance on political institutions (one of the explanatory models of the democratic peace), I can essentially "hold constant" norms of peaceful conflict resolution (the second explanatory model of the democratic peace) that should be ingrained in all of the most democratic regimes. In this sense, the "most democratic" regimes are a good sample because they share, as close as possible, the norms of peaceful conflict resolution that democratic peace scholars theorize have a pacifying effect. This is to say that, while there is considerable variance in among the developed democracies in their political institutions, there is no *a priori* reason to expect similar variance in their norms of peaceful conflict resolution. Therefore, this dissertation proposes to hold democratic norms constant by looking only at the "most democratic" regimes, thus isolating the effect of democratic political institutions on the dependent variable.

In terms of the dependent variable, this dissertation is concerned in a broad sense with the conflict behavior of democracies—how prone they are to engaging in international conflict. While there are many ways to capture the participation of states in various international conflicts,

I limit my scope to those instances of conflict *initiation* by democracies. Measuring the dependent variable as "conflict initiation" rather than "conflict participation" or "conflict reciprocation" (two other common measures) is perhaps the best way to capture how democratic institutions affect state conflict behavior. The entire premise of the liberal democratic peace, as well as this dissertation, is that certain democratic institutions constrain the use of force as a foreign policy tool. It is in the *initiation* of conflict that we might expect to see variance for some institutional configurations may make it easier, or more difficult, for democratic leaders to overcome barriers to committing to such a belligerent foreign policy. In the case of retaliation or reciprocation of conflict, on the other hand, there is no reason to expect one democracy to defend itself differently than another. Moreover, as much of the international relations literature has demonstrated, states seldom attack other states for defensive purposes, and once force has been used against a democracy, those constraints that normally pacify democratic foreign policy are removed: both public opinion and oppositional political institutions, which might be otherwise opposed to force, would likely recognize the necessity of defending an attack. Conflict initiation, then, is a good measure to capture a democracy's decision to become involved in international conflicts, and best fits the question posed in the title of this dissertation as those democracies that can more readily start a war can be considered more "dangerous" than others—or, more precisely, any democracy can be more dangerous under certain institutional circumstances.

Plan of the Dissertation

In the chapters that follow, I lay out in greater detail the concepts introduced in the preceding pages, propose a research design to test the claims of my arguments against the conventional

wisdom, present the results of an empirical analysis, and consider the implications of using "institutional context" to understand how democracies develop their foreign policies.

In the next chapter, Chapter 2, I review the liberal theoretical approach that forms the foundation for this dissertation, one that stresses the liberal insight that democratic processes within states are inextricably linked to international relations between them. On the question of the conflict behavior of democracies, this review demonstrates three principal points. First, international relations between democracies are unique and unusually pacific. Second, those mechanisms that support peaceful relations between democracies, particularly the constraining effects of political institutions, have a systematic effect on a democracy's conflict behavior regardless of the domestic political organization of other states. And third, since democracies exhibit considerable variation in those political institutions that constrain democratic belligerence, these variations are likely associated with the commensurate differences in foreign policies across democracies. While this liberal scholarship demonstrates the fundamentally important notion that democratic executives are constrained by their domestic political institutions in making policy decisions to go to war, I suggest that current formulations of the democratic peace are flawed in that they identify a conception of domestic political institutions that is overly static. Political institutions, I will argue in Chapter 3, are simply more dynamic.

Chapter 3 presents the main theoretical argument of this dissertation concerning the impact of domestic institutions on the initiation of conflict by democracies. In order to lay the foundation for my argument revolving around institutional context, I first review the literature regarding veto player theory, currently the "state of the art" in understanding how political institutions shape aggregate policy outcome. In doing so, I pay close attention to the work of George Tsebelis, highlighting the theoretical and logical shortcomings of this approach before

proposing a reformulation of veto player theory's assumptions regarding the motive and opportunity for political actors to block the policy process. I then specify my domestic institutional theory of international conflict behavior revolving around institutional context among the universe of democracies. I define institutional context, which varies across countries and changes over time, as the political setting in which veto players operate. This political setting is determined by the partisan composition of the various institutions of government, which in turn infuses political actors with different opportunities and motive for veto behavior. I argue that a competitive institutional context has a constraining effect on the democratic use of force, whereas a collective institutional context has an enabling effect.

Chapter 4 details the research design employed to test the claims of my argument against those advocated by the conventional wisdom regarding democracies and conflict initiation. Here, I present the hypotheses and variables utilized in this study, summarizing the theories that underlie the hypotheses to be tested and the sources of the data used to measure these variables. The hypotheses include not only my own regarding institutional context, but also ones derived from the democratic peace, veto player theory, neorealist theories regarding the influence of power in international relations, and neoliberal perspectives concerning the role of international institutions and economic interdependence. I also describe the methodologies applied to test the theoretical formulations of this dissertation.

Chapter 5 explains the empirical analysis of domestic institutional context and conflict initiation by democracies. In doing so, I present evidence to support the claims of my argument while also highlighting the empirical weaknesses of the conventional wisdom regarding democracies and conflict initiation. The data and analyses provide strong support for my argument that institutional context has a measurable impact on the likelihood of democracies to

initiate international conflict. When institutional context is competitive, conflict initiation by democracies is less likely. When institutional context is collective, the opposite is true. I also find that realist factors—namely major power status, alliances, and borders with autocratic states—make conflict initiation more likely. Additionally, the results indicate the other "liberal" factors—especially economic openness and voter participation—reduce the likelihood of a democracy initiating conflict. I do not, however, find support for the liberal claim that "more democratic" states are any more pacific than "less democratic" ones, and my results directly oppose the expectations of conventional veto player theory.

Chapter 6 is the final chapter of this dissertation. In it, I conclude that institutional context mediates, and even alters, the impact of democracy (including democratic institutional structure) and veto players on the initiation of conflict, confirming the expectations of my theoretical formulations. I also point to some definable circumstances under which democracies can be considered the most "dangerous," that is, when democracies are most likely to be able to initiate conflict should their executives so desire such a course. Finally, I end by considering the implications of relying on institutional context as a concept for refining how scholars think about domestic politics and international relations.

CHAPTER 2

A LIBERAL PERSPECTIVE ON INTERNATIONAL RELATIONS

This dissertation takes what international relations scholars term a "liberal" perspective. Liberal, in this sense, is intended to distinguish scholarship in this genre from "realist" perspectives that frequently dominate the field of international relations, particularly studies of international conflict. While realism is a diverse and heterogeneous research program, all realist perspectives agree in some fashion that power—who has it and how it is distributed internationally—is the chief factor that determines state behavior. Due to the Hobbesian nature of international politics in which each state must provide for its own security, states are overly concerned with acquiring power for themselves and balancing the power of other states. In this realist view of the world, power ensures survival, states seek power for themselves while balancing the power of others, anarchy (the absence of a "world government") limits cooperation among states, the internal characteristics of states are irrelevant, and war will always be the final arbiter of inevitable conflict. In short, realists understand international relations by reference to the distribution of power in an anarchic international system.

Noting that other-than-power considerations are also relevant to international politics, a large research effort has grown in the liberal tradition that challenges the core assumptions and ideas of realism. In particular, this research criticizes realism for failing to capture the importance of the domestic characteristics of states and their consequent linkage to international

phenomenon. Especially within the field of international security studies (long the singular domain of realism), numerous studies have asserted that a state's *domestic* political structure will affect its *international* behavior. Most prominently, scholars working within the liberal democratic peace tradition have noted that democracies, due to shared moral values and distinctive political institutions, are unique in their making of foreign policy. Such domestic characteristics are overlooked by realists (as well as many other systems-level theories), and to liberals indicate an important mitigating factor to the nature of anarchy and distribution of power across the international system that for realists is the fundamental cause of war.

This chapter and the next describe the theoretical approach used in this dissertation, one that stresses the liberal insight that democratic processes within states are inextricably linked to international relations between them. In doing so, this study reflects Moravcsik's conception of a "liberal theory of international politics" that specifies in what manner domestic political factors affect world politics (1997). According to him, a variety of "institutions and practices exist, each of which privileges particular demands; hence the nature of state institutions, alongside societal interests themselves, is a key determinant of what states do internationally" (1997: 518). This underlying liberal logic—that domestic politics serves as a fundamental determinant of state behavior in international relations (to paraphrase Moravcsik 1997: 513)—forms the basis of this study.

In this chapter, I present the liberal theoretical foundation upon which this dissertation is built. I do not provide a comprehensive literature review of all liberal scholarship, but instead focus on the liberal democratic peace thesis and extensions thereof, an understanding of which is necessary before establishing my main argument. I proceed in three parts. First, since the need for an extensive treatment of the liberal democratic peace is not readily apparent, I introduce its

basic formulation only briefly: international relations between democracies are unique and unusually pacific. This is due in large part to domestic political institutions: the democratic peace literature focuses on the institutional constraints of executive behavior as a principal source for that pacific international behavior. According to this basic formulation, these institutional constraints, which operate in tandem with shared moral values, create a separate peace between democracies. Scholars refer to this as the "dyadic peace proposition," whereby peace requires two democracies (or a dyad) to support it.

Second, I present evidence that the democratic peace is not limited to democratic dyads. If democratic institutions are a primary source of this unique international behavior, then irrespective of other states' domestic political organization, democratic institutions should, and in fact do, have a systematic effect on a democracy's conflict behavior. Scholars refer to this is the "monadic peace proposition," one that has received a revived interest. What is important here according to the monadic proposition is not whether democracies are any more or less pacific than non-democracies, although evidence suggests that this is the case. Instead, what is significant is that patterns of conflict behavior by democracies are systematically related to their domestic political institutions, especially in regard to the constraints facing democratic executives in their use of force as a tool of statecraft.

Third, I address emerging literature that more rigorously identifies how democratic political institutions support the supposedly unique foreign policy making of democracies. The greatest flaw of the democratic peace literature, in its dyadic and monadic formulations, is that it fails to identify the precise domestic mechanisms, and the accompanying variation among them, by which states formulate their decisions to engage in specific international behavior. In short, most of this research overlooks within regime variation, treating democracies as largely alike

even though democracies exhibit considerable institutional variance both cross-nationally and temporally. If democratic institutions operate monadically, and those democratic institutions vary from one democracy to the next, then we should see variance in democratic behavior based on variance in democratic institutions. I refer to this strand of democratic peace scholarship as the "democratic differentiation" literature (to modify the terminology of Clark and Nordstrom 2002).

Each of these literatures represent a progression of liberal scholarship: first, by acknowledging that democracies enjoy peaceful relations among themselves, second, by recognizing that the mechanisms that support such a separate peace—especially democratic institutions—should and do operate outside of democratic dyads, and third, by arguing that those peace-supporting mechanisms vary across democracies, causing variation in democratic foreign policy. Nevertheless, I suggest that current formulations of the democratic peace demonstrate theoretical and logical shortcomings, identifying a conception of domestic political institutions that is overly static. Political institutions do constrain the use of force as a foreign policy tool, but those constraints change over time and space in a way that democratic peace theorists fail to recognize. Political institutions, I will argue in Chapter 3, are simply more dynamic.

The Democratic Peace

> Ultimately, the best strategy to ensure our security and to build a durable peace is to support the advance of democracy elsewhere. Democracies don't attack each other; they make better trading partners and partners in diplomacy.
>
> —President William Jefferson Clinton, State of the Union Address, January 25, 1994

Despite the hegemony of realism in the international relations literature, a considerable consensus has emerged that democracy is not only a desirable form of domestic political organization, but also that it has beneficial implications in terms of relations among states in the international system. International relations scholars and foreign policy practitioners within the liberal tradition have long advocated such a view. Echoing the scholarly liberal literature for instance, former President Clinton's deputy secretary of state Strobe Talbott argues that promoting and supporting democracy abroad is paramount to a more interdependent, cooperative, and peaceful international system. Democracies, Talbott remarks, are more "integrate[d] with the outside world," "more likely to maintain their international commitments" and "less likely to make war on each other" (1996: 48-49). Even some self-avowed foreign policy "realists" concede the importance of expanding the realm of democratic influence as a conduit for reducing international conflict. As the eminent historian John Lewis Gaddis notes of President George W. Bush's recently published national security strategy (a document that is otherwise realist in orientation), "Bush insists [that] the ultimate goal of U.S. strategy must be to spread democracy everywhere" as a means to, among other things, build more peaceful relations among states and remove the causes of tyranny and terrorism (2002: 53).

The notion that democracy contributes to more peaceful relations among states who share that political organization has generated a copious amount of scholarly attention. Particularly over the last two decades, scholars have amassed a considerable amount of empirical evidence supporting a separate peace among democracies (see especially Russett 1993, Owen 1994, Ray 1995, Doyle 1997, and Russett and Oneal 2000). The relationship between democracy and peace is now widely accepted among most scholars of international relations; most, if not all, research

in international security studies, in fact, now include the pacific effects of democracy within their models.

This liberal democratic peace research agenda argues that democracies seldom, if ever, go to war with one another (see, for instance, Kant 1983[1795], Doyle 1997, Levy 1989, and Russett 1993). Most scholars agree that the linkage between democracy and peace is dyadic—occurring between two democracies, but not between democracies and non-democracies. In an empirical sense, this democratic peace proposition was first observed inductively. Most contemporary scholars recognize Babst as generating recent scholarly interest when he concluded in a widely cited article that "no wars have been fought between independent nations with elective governments between 1789 and 1941" (1972: 55). Small and Singer (1976), authors of another early empirical study, also found that democracies rarely if ever went to war with one another, even though they were just as war prone as other types of states in general. While Small and Singer dismissed the dyadic finding as spurious (due mostly to the failure of international relations scholars to theorize about this effect before its observation), more recent multivariate analyses has presented persuasive findings that the dyadic peace is robust (especially Maoz and Russett 1993; Oneal and Russett 1999a; Russett, Oneal, and Davis 1998).

While scholarship on the democratic peace is primarily of a contemporary vintage, Immanuel Kant provides the philosophical foundation for the idea that democracy promotes peace. Following Kant, the theoretical explanations for the democratic peace hypothesis revolve around two specific, yet non-exclusive, models: a normative model and a structural model (Owen 1994). According to the normative model, shared democratic norms and liberal ideology that emphasize regulated competition through peaceful means inhibit conflict between two democracies. Democratic states, accustomed to solving domestic disputes in a nonviolent

fashion, extend the norm of peaceful conflict resolution to other democratic states that share such norms. As Bruce Russett explains, "to use or to threaten to use force is not usually normatively acceptable behavior in disputes between democracies . . . [and democracies] not only do not fight each other, they do not expect to fight each other, or significantly prepare to fight each other" (1993: 42). Democracies do not, however, extend the same courtesy to non-democracies for they lack such norms and will likely take advantage of the inherent peacefulness of democracies. Relations between democratic states, then, are based on a certain mutual respect based on an understanding that conflicts should be resolved peacefully (Doyle 1997). The shared "democratic ethos" of peaceful competition and compromise, according to this model, explains the absence of war in relations between democracies (Maoz and Russett 1993).

Alternatively, structural or institutional theories of the democratic peace emphasize the complex and competitive institutions of democracies that constrain democratic leaders from initiating war with another democracy. As Owen (1994: 140) summarizes the logic behind structural arguments, "chief executives in democracies must gain approval for war from cabinet members or legislatures, and ultimately, from the electorate." Two structural processes make democracies less belligerent: first, the institutional separation of powers and, second, the democratic requisite of the consent of the governed. The former generates institutional constraints against such behavior as checks and balances within a democracy constrain belligerent foreign policies. The latter produces additional costs for democratic leaders as those who typically bear the costs of war (citizens) also select their leaders. In this manner, political leaders can expect citizens to penalize costly foreign policies such as war, so leaders avoid such policies.[1]

[1] Fearon (1994), Schultz (1999), and Bueno de Mesquita et al. (1999) have refined this argument, explaining that democracies are able to generate significant audience costs. Unlike non-democratic

The first process of separated institutions makes executive commitment to belligerent foreign policies particularly difficult due to the oversight of competitive branches of government. In a democratic form of government, separate political institutions provides checks and balances against the concentration of power into any single branch, a point recognized by many democratic philosophers from Kant to James Madison who warned against tyrannical majoritarian rule. In *Federalist 51*, for instance, Madison argues that "great security against gradual concentration of the several powers in the same department consists in giving to those who administer each department the necessary constitutional means and personal motives to resist the encroachment of others" (*The Federalist No. 51* 1961 [1788]: 356). The prevention of concentrated power, particularly that of concentrated executive power in war-making, has the effect of constraining democratic belligerence. As Layne (1999: 180) summarizes this view: "States with executives answerable to a selection body, with institutionalized political competition, and with decisionmaking responsibility spread among multiple institutions or individuals, should be more highly constrained and hence less likely to go to war."

As to the second process behind the structural constraints facing democratic war-making, Doyle (1986) argues, building on Kant, that democratic governments are less inclined to go to war because they are held accountable to their citizenry. There is an electoral dis-incentive for pursuing a war for which the citizens must pay the price. Moreover, democratic foreign policy making is debated openly before the public, making both the electorate and policy makers more attentive to the costs associated with war (Layne 1999). Kant himself provides the rationale behind the electoral constraint that pacifies democracies (1983[1795]: 100):

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leaders, democratic leaders are able to signal resolve and make credible threats through public statements and transparent foreign policies, thus reducing the chances of costly international conflict.

> If, as is inevitably the case under [a democratic] constitution, the consent of the citizens is required to decide whether or not war should be declared, it is very natural that they will have a great hesitation in embarking on so dangerous an enterprise. For this would mean calling down on themselves all the miseries of war, such as doing the fighting themselves, supplying the costs of the war from their own resources, painfully making good the ensuing devastation, and . . . having to take upon themselves a burden of debts.

In non-democratic systems, electoral constraints are obviously absent. Again, quoting Kant (1983[1795]: 100):

> But on the other hand, in a constitution which is not republican and under which the subjects are not citizens, a declaration of war is the easiest thing in the world to decide upon, because war does not require of the ruler the least of sacrifice of the pleasures of his table, the chase, his country houses, his court functions, and the like.

There are simply fewer political costs for using force in relations with other states, no oversight by the public or other facets of government, and no opposition for an executive's actions. Leaders of non-democratic states can more easily commit the state to war, so it is a more common tool of statecraft.

As in the normative model, structural mechanisms that constrain conflict operate dyadically. Given these institutional and electoral constraints, democratic leaders can not act quickly to mobilize war as long as the potential adversary is also democratic. Russett (1993: 39) explains: "If another nation's leaders regard a state as democratic, they will anticipate a difficult and lengthy process before the democracy is likely to use significant military force against them. They will expect an opportunity to reach a negotiated settlement." If, however, a democracy's leaders do not regard an adversarial state as democratic, they will not expect a particularly

lengthy process before the non-democracy can resort to conflict for it faces so few constraints in mobilizing military force. Expecting no opportunity for peaceful resolution, and anticipating a high likelihood of mobilization by a non-democracy, democracies are much less constrained in the use of force against a non-democracy for fear of their very survival. Despite the canonical status of this proposition, it is worthy to note that recent research challenges this conventional view of the dyadic effect of structural mechanisms, particularly in regard to *initiating* conflict. Since structural constraints are always present in a democracy, they likely have monadic effects as well—a point I return to in the following section.

The relationship between structural and normative explanations and peace is robust; scholars now confidently predict that democracy has immutably pacific benefits for interstate relations. What is more, these two models, while sometimes presented separately and as competing arguments, are mutually reinforcing. As Owen (1994) points out, domestic institutions and norms work together to produce peace: norms of nonviolent conflict resolution restrain belligerent impulses, and political institutions ensure that democratic leader pursue pacific courses in foreign policy. Because of the results of numerous empirical findings (especially Maoz and Russett 1993; Oneal and Russett 1999a; 1999b; and Russett et al. 1998a) and theoretical work to support it (especially Russett 1993, Ray 1995, Doyle 1997, and Elman 1997), the democratic peace proposition is almost universally accepted. As Gleditsch (1995: 318) concludes in a recent empirical investigation for all states between 1816 and 1986, "double democracy is a near-perfect *sufficient condition* for peace."

The persuasiveness of the democratic peace finding presents a potent challenge to realist formulations of international relations. While realists argue that regime type in no way mitigates the effect of the immutable "laws" of the international system, democratic peace scholars have

shown that international anarchy and the distribution of power do not necessarily determine democratic state behavior. In short, "democratic states have foreign policies that are strikingly different from those of autocratic states" (Ray 1995: 37, summarizing Russett 1993 and Morgan and Schwebach 1992). It seems that, without question, democracies exhibit conspicuously different international behavior than realism would otherwise predict. This leads to a firm conclusion that domestic factors play a fundamental role in determining interactions between states, particularly in regard to how democratic constraints shape very costly decisions like the one to go to war.

Monadic Effects of the Democratic Peace

Do the strikingly different foreign policies of democracies extend outside of the case of democratic dyads, or are the pacific effects of democracy limited to those who share its political organization and norms? While earlier empirical research (particularly that of Small and Singer 1976, Chan 1984, Weede 1984, Maoz and Abdolali 1989, and Morgan and Campbell 1991) concluded that democracies are no more pacific in general than other regimes, recent theoretical and empirical work has questioned this widely held perspective. There is an emerging consensus that democracies are in fact more pacific in relations with all other states, regardless of regime type. As Elman (1997: 16) finds in a recent review of the democratic peace literature, "much of the aggregate data on the democratic peace phenomenon suggest that democracies are less war prone in general, and that it is not only in their relations with each other that the pacifying effects of democracy emerge."

Given the theoretical foundation of the democratic peace, for those who advocate the monadic perspective there is good reason to expect democratic peacefulness to extend to all

types of regimes. According to the normative argument, democratic leaders possess norms of peaceful conflict resolution, view foreign policies formulated around such norms as the most legitimate, and will attempt to extend those foreign policies wherever possible, including to those states that are not democratic. According to Rousseau et al. (1996), democratic leaders, politically socialized in a system that relies on compromise and negotiation, internalize normative values of peaceful conflict resolution. Once socialized, these leaders regularly pursue foreign policies bases on nonviolence and respect for compromise. Regardless of the opposing state, democratic leaders consider peaceful foreign policies as legitimate, coercive and violent policies as illegitimate.

Leng (1993) and Bueno de Mesquita and Lalman (1992) reach similar conclusions. In an examination of international crisis bargaining, Leng reports that "democracies [are] indeed more likely to choose reciprocating [bargaining] strategies than authoritarian regimes" (1993: 28). Reciprocation, according to Leng, is a norm ingrained in democratic political processes that is not reflected in leaders without such processes (i.e. authoritarian leaders). Findings of bargaining and reciprocation, then, support the normative argument that "democratic regimes are more likely to choose influence strategies that promote compromise and peaceful settlements" (1993: 14). That bargaining and negotiation are common practice by democracies that face decisions to use force is also supported by Bueno de Mesquita and Lalman (1992). In analyzing European militarized interstate disputes, they argue that democracies are more likely to negotiate than use force. They find that this negotiating behavior, one consistent with normative arguments, arises if either the initiator or the target is a democracy; it is not necessary, therefore, that both states be democratic for negotiation behavior to occur.

Notwithstanding this evidence suggesting that normative constraints encourage democratic peacefulness towards all regime types, the normative argument clearly has a dyadic formulation that calls into question its monadic application. As presented by dyadic theorists, democracies extend the norm of peaceful conflict resolution to democracies, but not to non-democracies, because the former will reciprocate but the latter will not. Given this, the regime type of the opposing state clearly enters the calculus of democratic leaders. Moreover, the normative argument is logically unsustainable by itself. Looking exclusively through a normative lens whereby a democracy is normatively opposed to using force as a tool of statecraft, it would be unlikely to remain an independent, sovereign state (Rousseau 1996). This is because if a democracy is pacifist in its relations to all states regardless of regime type, it would be unlikely to use force at any time, and it would unable to defend itself, or its interests, in an anarchic international system that includes states without peaceful norms. Therefore, it follows that democracies can hold to their normatively defined pacific course of foreign policy only in relations with other states that hold the same norms.

The dyadic effect of the structural argument is less definitive. Note, for instance, Kant's philosophical reasoning for the structural constraints facing democracies—they operate irrespective of other state's domestic governing structure. According to Kant, citizens constrain war for they naturally hesitate "embarking on so dangerous an enterprise" for which they pay the price. These costs, including "supplying the costs of the war from their own resources, painfully making good the ensuing devastation, and . . . having to take upon themselves a burden of debts," remain the same no matter the adversary. Likewise, opposing political elements in a democracy constrain executive war making not because of who the adversary may be, but because they wish to take advantage of any failed executive policy. Since war-making carries

such high risks, and since the likelihood of success is not easily predictable, democratic executives avoid risky international conflicts. Thus, "checks and balances" do not appear to operate dyadically. These institutional constraints are, as Layne (1999), Gartzke (2001), and Prins (2003) suggest, nation-specific attributes that are embedded in democratic regimes and operate independently.

In this sense, democracies can not "extend" the constraining effects of their political institutions to some states but not others as is the case with norms of peaceful conflict resolution. The presence of structural constraints is not logically dependent on the regime type of an adversary; they exist domestically only in a democracy and are in no way determined by another state. No matter the opposing regime type, opposing political institutions and a reluctant public still constrain the executive use of force and the cost of policy failure is the same. Therefore, particularly in relation to the structural mechanisms cited by democratic peace scholars, there is reason to expect that at least some of the institutional constraints that prevent democracies from going to war with one another would operate irrespective of a potential adversary's domestic governing structure. Ultimately, it is most likely that normative and structural mechanisms operate in tandem to produce the monadic effect as seems to be the consensus in the dyadic and monadic literature (Ray 1995). As Gartzke (2001) argues, both mechanisms are fundamentally monadic in that they are specifically state-level attributes: norms of peaceful conflict resolution and domestic political institutions are features of a single democracy, not a democratic dyad. Nevertheless, support for the monadic peace proposition tends to emphasize the structural mechanisms that constrain democratic war making.

According to a monadic structural argument, then, democratic leaders must always be sensitive to the political costs of belligerent foreign policies regardless of the regime type of

potential adversaries. Here, the supporting evidence is more copious. Drawing on structural arguments, Rummel (1983, 1997) presented one of the first rigorous analyses advocating democratic pacifism towards all regimes. Stressing the constraining effects of institutional checks and balances, and the unwillingness of the populace to bear the costs of any war, he finds that "democracies are in fact the most pacific regimes" (1997: 64). Since wars are costly and have a high risk of failure, and since democratic leaders associate these costs and risks with their ability to retain office, democracies are simply more likely to exhibit pacific tendencies.

Research by Geller (1985) also supports the monadic-structural argument, finding that democratic states are less prone to violence, conflict escalation, and war than non-democratic states. For him, "regime structure is a principal factor in the explanation of foreign policy behavior," whereby "regimes whose actions must satisfy a large and varied number of interests are presumably more constrained than regimes which must satisfy only a few." Moreover, states that have another restrictive institutional mechanism, elections, are also more constrained: "regimes which stand for election relatively frequently are more constrained than regimes whose authoritarian positions are fixed for relatively long intervals" (Geller 1985: 63; also quoted in Ray 1995: 18-19). In short, regime types with more constraints (whether they be elections or more numerous interests in society) are more pacific than regime types with fewer constraints.

Whereas Rummel and Geller focus only on simple regime type as a source of foreign policy behavior (i.e. democracy versus non-democracy), Morgan and Schwebach (1992) offer a more detailed argument for the monadic structural model. For them, it is not just regime type, but the level of "decisional constraints" that make some states more pacific than others. Operationally, "decisional constraints" increase as political participation becomes more open, executive recruitment becomes more competitive, and executive power encounters more

independent political institutions. These decisional constraints are the same ones defined within the popular Polity continuous measure of democracy, where the more decisional constraints facing an executive, the more democratic the state (Marshall and Jaggers 2000). While this may be overly insensitive to the wide variety of institutional mechanisms that democracies actually exhibit cross-nationally, it is a slightly more descriptive manner in which to measure domestic political structures. Focusing on disputes that end in war compared to those that do not from 1816-1976, they find that "domestic political structures can serve to create a constraining influence on the belligerent impulses of state leaders" and that the effects of these structures "is felt through a reduction in the probability that a state will opt for war and *does not appear to depend on the domestic structure of the opponent*" (Morgan and Schwebach 1992: 318). This is compelling evidence in favor of the theoretical processes that are hypothesized to prevent democratic war-making at the monadic level: the more constraints facing an executive, the more pacific the state.

While the theoretical and empirical evidence thus seems to favor a structural-monadic effect, most studies, recognizing that normative and structural process work together to produce peace, do not model the two arguments separately. Citing both models as a source of democratic pacifism, for instance, Bremer (1992) offers perhaps the most comprehensive statistical analysis. While he investigates the democratic peace dyadically (examining all pairs of states in the international system from 1816-1976), one of his most interesting findings is that "the presence of *a* democracy in a dyad significantly reduces its war propensity (1992: 329). Similarly, Maoz and Abdolali (1989), while usually cited for demonstrating only the dyadic peace, report in their findings over the same time period as Bremer's analysis that regardless of the regime type of the opposing state, "the proportion of disputes in which [democracies] participate that escalate to

war is significantly lower than that of non democratic polities (1989: 18). These studies demonstrate that as long as there is one democracy in any pairing of states, war is less likely.

In addition to the sheer volume of findings supporting a monadic effect, the robustness of the monadic peace proposition has increased as scholars have shifted focus from the simple *frequency* or *likelihood* of war involvement toward more precise considerations surrounding democratic war making. In fact, the strongest argument advanced within the monadic literature is not that democracies are more pacific in all circumstances, but that democracies are especially pacific when contemplating conflict *initiation*. This conclusion is supported by large-N statistical studies performed by Rousseau et al. (1996), Rioux (1998), and Schultz (2001), all of whom find that democracies are significantly less likely to initiate conflict with all types of states. As MacMillan (2003: 235) concludes in a review of the literature, "one point on which a consensus does appear to be consolidating is that liberal states have a lower propensity to *initiate* violence."

Given that the democratic peace emphasizes the constraints facing executives for committing to war, the finding regarding conflict initiation is not unexpected. All things being equal, democracies are simply more constrained in committing first to a belligerent foreign policy for their leaders face such high political costs for policy failure. Rousseau et al. (1996: 514) make this point nicely: "Regardless of the regime type of the adversary, the higher cost of failure should make democratic states less like to initiate and escalate [violence]." Responding to force (such as defending an invasion by another state), on the other hand, is much easier for democracies. Once force has been used against a democracy, those constraints that normally pacify democratic foreign policy are removed: both public opinion and oppositional political institutions, which might be otherwise opposed to force, would likely recognize the necessity of defending an attack (Rousseau et al. 1996). The use of force, then, is a behavior consistent with

democratic foreign policy making, but it is seen as legitimate only as a last resort and is most possible for defensive purposes.

In a thoroughly argued review of the state of the democratic peace research agenda, Russett and Starr (2000: 97), citing this cumulative evidence (particularly that of Rioux 1998, Rousseau 1996, and Rousseau et al. 1996), endorse this "revisionist" monadic perspective: "The emerging majority view now seems to be that the monadic effect of democracy is real, and discernible in most of the evidence when one looks carefully—especially in the relatively low frequency with which democracies, in general, *initiate* militarized disputes." Russett and Starr go on to cite several reasons why the monadic effect of democracy on international relations has been underemphasized in the literature. First, the monadic finding, while detectable, has been overshadowed by the very robust dyadic finding: "the monadic effect is not nearly as strong or robust as the dyadic effect that emerges in the interactions between two democracies" (Russett and Starr 2000: 97). That the monadic finding is less robust than the dyadic effect is not evidence of its absence.

Second, democratic peace researches have failed to uncover a robust monadic effect because of methodological shortcomings. Russett and Starr suggest that researchers must "think multivariately" in their research designs in order to capture the effect. That is, the monadic effect is relatively weak upon bivariate inspection, and researchers take this finding to indicate that democracy has no general effect. Bivariate analyses do not, however, consider other factors that may influence conflict behavior, so important monadic effects are infrequently considered in multivariate research designs. Once multiple factors are included in the analyses, especially geopolitical concerns, democracy indeed has a pacifying effect regardless of the regime type of other states.

Benoit (1996) reinforces this view that methodological deficiencies have contributed to our lack of attention to the general peacefulness of democracies. Reevaluating the empirical analyses of Small and Singer (1976), Chan (1984), Weede (1984), Maoz and Abdolali (1989) and Morgan and Campbell (1991), all of whom concluded that the democratic peace was limited to democratic dyads, Benoit finds that "democracies really are more pacific (in general)" (1996: 636). Benoit finds fault with previous studies for failing to properly model monadic effects (essentially concluding an absence of a monadic peace while modeling only dyadic effects), utilizing poor operational definitions of democracy, underspecifying controls in statistical equations, and basing conclusions on the absence of statistical significance rather than (extant) causal relationships. In other words, proper methodology, together with more sensitive measures of democracy, control variables, and proper causal modeling, should and does reveal positive empirical results for the monadic peace proposition. His sophisticated statistical analysis concludes that "democracies were significantly less likely, on average, to be involved in international wars during the 1960s and 1970s than less-free states" (1996: 654).

Finally, Russett and Starr note that inattention to the monadic effects of democracy arises from researchers' failure to think strategically, a perspective that gained currency only after scholars first observed the dyadic effect. Some have applied the strategic perspective with great success in recent studies, particularly in regard to the circumstances under which democracies may initiate conflict. According to strategic perspectives, policy makers in democracies must pay close attention to the expected utility of going to war. This utility calculation is based on the likelihood of winning a conflict, whereby defeat in war is particularly costly for political survival. As Bueno de Mesquita et al (1992: 644) argue, "Leaders can anticipate that they will be held accountable for failed foreign policy adventures. Consequently, the choice of war-related

behavior is likely to be dampened by the fear that the regime will be punished if things go awry." Given that democratic leaders face punishment for policy failures, they tend to choose courses of action with the lowest risk of defeat (Bueno de Mesquita and Siverson 1995). Central to the constraints democratic leaders face that produce strategic behavior, according to Russett and Starr (2000: 102), are "the nature of the constituencies to which decision makers are responsible and the institutional context within which the leader-constituency relationship is situated." That is, both the electorate and opposing political institutions can deliver political punishment for executive policy failure, and executives consider this when calculating the expected utility of war. Once researches accommodate these institutional mechanisms and the strategic behavior they produce, democracies, unlike autocracies that do not face such swift political punishment for failed policies, demonstrate a distinct tendency to avoid many conflicts. This requires much more nuanced and detailed hypotheses and variables than previously employed by most of the extant literature.

From this review of the monadic effects of democracy, several propositions are clear. First, the monadic effects of democracy on war-making are genuine. As Elman (1997: 15) summarizes the monadic peace literature, "the more democratic the state, the less violent its behavior toward other states," and copious empirical work supports this proposition. Second, the pacific effect of democracy is most robust in cases of conflict initiation. Given the nature of the constraints facing democratic executives, committing to violence first rather than for defensive purposes is exceedingly difficult. And third, political institutions are a critical component of the monadic peace proposition. While political institutions work together with pacific democratic norms, evidence supporting a monadic-structural argument is the most theoretically persuasive and empirically supported. This last point is especially important when considering only the

universe of established democracies, as this dissertation does. Holding democratic norms constant by looking only at the "most democratic" regimes, the political institutions of democracy evidently constrain democratic leaders in war making.

Unfortunately, our confidence that democracy has a pacific effect on international relations is equaled only by our uncertainty as to what specific factors cause such an effect. In fact, the explanation for a liberal peace treats democracy as a fixed set of institutions and norms that place impediments upon belligerent foreign policies without reference to any specific processes or institutions within democracies. Accordingly, democracy, as form of domestic political organization, changes little if any over time and across different democratic states. So even while norms of peaceful conflict resolution are common across all established democratic states, institutional configurations are not. Thus, while most international relations liberals consider democratic political institutions to be an important part of the explanation for the uniqueness of democratic foreign policy, scholarship in this area has largely overlooked the varieties of political institutions that exist among democracies—the very sort of weakness to which Russett and Starr and Benoit refer.

In fact, scholars working within the liberal democratic peace typically cite only two vaguely defined mechanisms (the separation of powers and the consent of the governed) that support this unique international behavior of democracies. Consequently, the international relations literature has largely failed to explain the varieties of institutions that operate within democracies and how they may influence the international behavior of democratic states. While all democracies employ some means of separating powers, and all democracies base their political legitimacy on the consent of their citizens, the methods by which they do so vary. It follows that institutional variation among different forms of democracies should produce

variation in their international behavior. If, for instance, the separation of powers is an important institutional component of democratic foreign policy making, are parliamentary systems (with a fusion between the executive and legislative branches) more or less likely to engage in conflictual behavior internationally than presidential systems with no such fusion? In the same manner, if the consent of the governed is also important, does the nature of how the people elect their leaders affect the likelihood of democratic involvement in international conflict?

Only recently have scholars begun to scrutinize more closely the varieties of democratic institutions and their consequent impact on the international behavior of states (a topic I return to shortly). Nevertheless, as Reiter and Tillman (2002) note, the bulk of the liberal democratic peace research agenda does not adequately account for differences in democratic institutions. Most researchers conceptualize democracy relatively simply, conceptualizing it either dichotomously (such as the Freedom House distinction between democracy and non-democracy) or uni-dimensionally (such as the Polity scale from strongly democratic to strongly autocratic) (Reiter and Tillman 2002: 812). Elman's (1997: 15) observation that "the more democratic the state, the less violent its behavior toward other states" is a typical conclusion of this literature, but notice that such a proclamation explains little in terms of how democracies go about formulating less violent behavior. Do "more democratic" states have more institutional constraints or greater norms of nonviolent conflict resolution than "less democratic" states? Given the state of the democratic peace literature, Elman's widely expressed sentiment would seem to suggest an affirmative. Unfortunately, such an assertion makes little sense if one limits the cases to only the most democratic states. In fact, among the "most democratic states" (those that score the highest possible score on the often used Polity democracy index), rates of conflict

initiation vary widely (as presented in Chapter 4), leaving wholly unaddressed how democracy is related to rates of international conflict, if it is at all.

The use of such simple conceptualizations of democracy has obvious limitations. In addition to ignoring the bulk of mainstream comparative political scholarship, such an approach assumes that democracies are largely the same, when in fact, institutions within different democracies varies widely. The Polity data, for instance, registers equal "democratic" scores for the United States and most Western European countries (Marshall and Jaggers 2000). Comparative political scientists, however, have built careers on noting the institutional differences between the two, and as Gleditsch and Ward (1997) have suggested, scholars of international relations can benefit from disaggregating the constituent elements of democracy in their studies. In the next section, I consider the nascent literature that has begun to differentiate democracies from one another as a means to explaining varying foreign policy behavior of democracies.

Differentiating Democracies

Since the liberal democratic peace research agenda is based largely on the dyadic effects of democracy, most scholars working within this tradition treat the subject dichotomously (that is, democracy versus non-democracy). This scholarship is founded on the argument that the democratic peace should exist between any two democracies despite any internal differences between them. Even in its monadic formulation, scholars typically only note whether democracies exhibit uniquely pacific international behavior as compared to non-democracies. But as the research agenda has progressed, scholars have begun to question the veracity of treating all democracies alike. While distinguishing democracies from non-democracies has been

informative in anticipating the divergent foreign policies of states, doing so is incomplete in that democracies vary widely in their institutional form. Particularly since the recent findings indicating that the pacifying effect of democracy is also a monadic phenomenon, some scholars have begun to investigate whether variations across the universe of democracies explain differences in their conflict behavior. As the following review demonstrates, not all democracies exhibit the same pattern of conflict behavior. Under certain circumstances, some democracies are simply more dangerous, or more peaceful, than others.

Despite the conventional wisdom that the fields of comparative politics and international relations often times overlap, scholars of international relations have largely overlooked many of the insights of comparative politics in understanding the democratic peace. In fact, only recently have scholars begun to take seriously the notion that heterogeneity among democracies may be systematically related to differences in their conflict behavior. One of the most common ways in which scholars have begun to differentiate democracies is by employing (either implicitly or explicitly) the logic of veto players (Reiter and Tillman 2002; Ireland and Gartner 2001; Morgan and Campbell 1991). Generally, the literature of veto players asserts that particular constitutional configurations shape aggregate policy outcomes. Thus, domestic constitutional structures themselves are centrally important explanatory variables independent of endogenous ideological factors or exogenous international (or systemic) concerns. The basic claim of this line or research asserts that *the greater the number of veto players, the more difficult it becomes for governments to enact certain policies, or in the case of this research, engage in conflict behavior.*

A veto player can be defined as "an individual or collective actor whose agreement is required for a policy decision" (Tsebelis 1995: 293). A typical example of a veto player is the number of partisan partners in a parliamentary cabinet. As the number of partners increases, the

number of veto players increases. Alternatively, as the number of partners decreases (as in a single party/bare majority cabinet), the number of veto players decreases. But veto player logic has a much longer and richer pedigree, and it has been used at least implicitly since *The Federalist* when Madison noted the restraining effects that the separation of powers has on a democracy's ability to govern (or at least, a democracy's ability to usurp the rights of its citizens). Beyond coalition partners, veto players (or veto points) can include anything from institutional or constitutional hurdles such as separate offices or branches within the government, to partisan or societal actors such as the number of parties in the legislature or the number of interest groups that lobby before the government. Hallerberg and Basinger capture the impact of veto players on aggregate policy outcomes: "the higher the number of veto players, the harder it is to pass laws" (1998: 339).

The effect of veto players on a state's ability to go to war is intuitive enough: the more institutional hurdles that exist within a democracy, the more difficult it becomes for that democracy to commit to such a costly foreign policy. In particular, research in this area has focused on the constraints that democratic executives face in committing to international conflicts. As democratic peace scholars have argued theoretically and demonstrated empirically, executive accountability to legislatures and citizens constrain their policy decisions (Kant 1983, Doyle 1997, Levy 1989, and Russett 1993). Such constraints restrict executives' policy options (Dixon 1994; Russett 1993; Morgan and Schwebach 1992; Maoz and Abdolali 1989), restrain the initiation of conflict (Bueno de Mesquita and Siverson 1995; Bueno de Mesquite et al. 1992), and limit the kinds of conflicts in which to become involved (Goemans 2000; Reiter and Stam 1998a, 1998b; Siverson 1995). The fewer such constraints exist, the fewer the number or veto players, and the more flexibility executives have in choosing conflict as a foreign policy option.

We can use veto player logic to differentiate democracies on several dimensions, each with a distinct effect on international conflict behavior. One such dimension is the macro-institutional configuration of democracies—namely, presidential versus parliamentary systems. In this case, the logic of veto players suggests that presidential systems are more constrained in policy making than parliamentary ones. The principal distinction to draw here is that between parliamentary systems with fusion between the executive and legislative branches, and those systems that have relatively (semi-presidential systems) or completely (presidential systems) independent executives. As Strøm (2000) argues, presidential systems exhibit competing checks, or veto players, between rival branches. By contrast, parliamentary systems create a chain of power that flows from the legislative branch to the executive, thus removing institutional checks or vetoes. In the classic Westminster parliamentary model of the UK for instance, power is concentrated into an executive that is fused to the legislature. In this manner, the executive is not constrained by an institutionally independent legislature, making the party in power relatively unencumbered in formulating foreign policy decisions including the use of force.

On the question of presidential versus parliamentary systems, however, another perspective holds that a president is much less constrained. As Maoz and Russett (1993: 626) argue, "Presidential systems should be less constrained than parliamentary systems, in which the government is far more dependent on the support it gets from the legislature." In the case of the United States, for instance, separation between the executive and legislative branches gives each department significant control over certain policy domains (Reiter and Tillman 2002). Particularly in the executive use of force abroad, presidents often times act unchecked. What is more, executive terms are fixed in a presidential system, meaning that whatever course of action a president takes, he can not be removed (barring impeachment) by an opposition political

branch (Auerswald 2000). This is very much unlike a parliamentary system in which the executive cabinet is subject to irregular elections and votes of no confidence to replace a recalcitrant executive.

Presidential systems, then, are either more constrained than parliamentary systems due to additional institutional vetoes in the former, or less constrained due to the largely unchecked policy dominance in that office's use of force. While either may be true, both can not, and research on this question has reached no firm conclusion over the veracity of one hypothesis over the other. Auerswald, for instance, provides case studies to support that presidential systems are less constrained in the international use of force, but Reiter and Tillman find no systematic support for either perspective in their large-N quantitative study. Given the conflicting nature of these two perspectives, it seems that scholars have yet to find how, or under which circumstances, veto players at the macro-institutional level have a systematic effect of international conflict behavior.

The nature of a state's political party system constitutes a second dimension along which scholars have differentiated democratic foreign policy making. Again using veto player logic, Reiter and Tillman (2002) argue that as the number of parties in a democracy's legislature increases, the less likely that democracy will initiate involvement in international conflicts. An increase in the number of political parties, Reiter and Tillman suggest, indicates the incorporation of additional veto players that may constrain costly foreign policies. This argument can be criticized on a number of grounds, the most compelling of which may be that partisan veto players, if excluded from the policy making process as they often are when government is controlled by an opposing majority, may have no constraining effect on policymaking whatsoever. In fact, Reiter and Tillman find no systematic evidence that more partisan veto

players are associated with fewer international conflicts. Partisan veto players, then, are clearly not always veto players. This has not yet been widely recognized in the literature, but nevertheless forms a conclusion consistent with Clark's (2000) findings that political parties constrain U.S. initiation of conflict only when there is divided partisan control of the executive and legislative branches of government.

The nature of a democracy's ruling coalition defines a third dimension of democratic differentiation, one that has received perhaps the most scholarly interest. The insight of Maoz and Russett (1993) constitute what can best be described as the conventional wisdom: "coalition governments or minority cabinets are far more constrained than are governments controlled by a single party" (626). The logic here is again consistent with the veto player literature. Single party majorities, particularly in parliamentary systems, are relatively "unencumbered" by the opposition party for the latter's consent is not necessary for the formulation and successful implementation of public policy. Majority governments simply face fewer veto players than other types of executive cabinets. Coalition and minority cabinets, on the other hand, require parliamentary approval across a multitude of parties. The more political parties that are required to pass public policies, the more veto players there exist to hinder the process.

While the case for veto players on this dimension seems robust, the scholarly findings are mixed. Ireland and Gartner (2001), Koch (2002), and Clark and Nordstrom (2002) all find that minority governments are in fact more constrained than their coalition or majority counterparts in participating in militarized disputes. It should be noted, however, that many of these studies find that both single party majorities *and* coalition majorities have a positive relation with international conflict compared to minority governments. In fact, most of these studies fail to distinguish adequately coalition majorities from single party majorities in their empirical

findings, leaving only the one firm conclusion that minority governments are the most heavily constrained of all governments. Standing the veto player logic on its head, however, Prins and Sprecher (1999) find that coalition governments are much less constrained. For them, coalitions should be less constrained than majority and minority cabinets for they typically hold a much larger share of parliamentary votes, and their empirical findings demonstrate that coalition governments are more likely to reciprocate militarized interstate disputes. Palmer, Regan, and London (2002) and Reiter and Tillman (2002), however, find no systematic relationship between the nature of the ruling coalition and international conflict behavior. Evidence of how ruling coalitions affect the conflict propensity of democracies is thus not definitive. What is needed to clarify this dimension, then, is a theoretical construct that can capture under which circumstances ruling coalitions may or may not be constrained.

Finally, scholars have also differentiated democratic foreign policy making by noting differences in the nature of electoral consent. The evidence here is more robust and consistent than the previously discussed dimensions. The democratic peace literature maintains that public consent is a check on democratic war making; since the public bears the costs of war, it would naturally disfavor war or punish democratic leaders who initiated international conflicts. At the dyadic level, Bueno de Mesquita et al. (1999) find that electoral incentives constrain democratic war making. An executive's winning coalition (the electoral group to which the executive is accountable) is generally large in a democracy meaning that a large proportion of the population severely constrains an executive's behavior, particularly a commitment to which the population should generally not support. It also follows from this proposition that the larger the winning coalition, the more heavily constrained an executive for he faces more demands or potential "vetoes." At the monadic level, Reiter and Tillman (2002) argue that because elections are the

most powerful constraint citizens can place on an executive who may initiate international conflict, electoral systems that allow for greater participation should be more constrained. They find that the more inclusive an electoral system, the less likely a democracy would use force as a foreign policy tool.

Thus, while veto player theory has been useful as a means to describe the constraining effects of domestic institutions as well as to differentiate democracies, its use has produced conflicting theoretical expectations in some cases and inconclusive empirical findings in others. Veto players, it seems, may not always constrain belligerent foreign policies, or to state it differently, veto players are apparently not always veto players. In the chapter that follows, I propose a different method by which to distinguish the varieties of democracy. I doing so, I focus not on static institutional organization (as in the democratic peace literature), or on a rigid distribution of veto players (as in the "democratic differentiation" literature), but on a more dynamic dimension of institution context. I argue that variation along a competitive-collective institutional axis is the principle dimension along which democracies vary in their likelihood to initiate international conflict.

Reconceptualizing how political institutions affect aggregate policy outcomes by distinguishing the institutional context within which political actors operate resolves the puzzling relationships outlined in this review of the liberal literature. Those mechanisms cited by democratic peace scholars, in its dyadic and monadic formulations, seem to have an irregular effect: democracies are more constrained than non-democracies (or highly democratic are more constrained than less democratic) in the use of force internationally, yet equally democratic countries demonstrate obvious variance in their conflict propensity. The United States, for instance, is forty times more likely to initiate an international dispute than Austria and Finland,

and twice as likely to do the same as compared to the United Kingdom (Jones, Bremer, and Singer 1996; Reiter and Tillman 2002). While some may argue that the United States is unique in that it has far more international interests at stake that may require force to protect, both France and the United Kingdom are three times more likely than Canada, four times more likely than Spain, and seven time more likely than Germany to initiate international disputes (see Table 4.2 for a detailed listing of conflict initiation rates). This is especially puzzling given that all of the cited countries are considered "equally democratic" by most democracy scholars (Marshall and Jaggers 2000, Mainwairing 1993, Lijphart 1999). What is more, the mechanisms that supposedly cause these relationships are left wholly underspecified by democratic peace scholars. How do we know precisely which mechanisms matter when we do not specify, much less theorize about, which of them have which effects?

The "democratic differentiation" literature also raises an obvious puzzle: the same democratic institutions seem to produce different outcomes. Evidence suggests that similar institutional configurations can either constrain or enable a decision to go to war. Presidential systems, for instance, are more prone to conflict than parliamentary systems because of executive policy dominance, or parliamentary systems are more belligerent than presidential due to inter-institutional fusion that gives parliamentary executives greater flexibility. Similarly, multiparty executive coalitions may be more constrained in initiating conflict due to the inclusion of more veto players, or they may be less constrained because of greater parliamentary support. This literature thus raises an compelling question: how can the same institutional structures cause differing outcomes—or, alternatively, how can widely divergent institutions cause the same outcome?

What is needed, as I argue in the next chapter, is a contextual theory, one that considers conditions existing within otherwise static institutional structures that can explain these unresolved theoretical expectations and empirical patterns. Certainly, constitutionally determined democratic structures can constrain the use of force as a foreign policy tool by establishing more potential veto points. But these institutional structures change rarely, if ever, while seemingly having an irregular effect on foreign policy outcomes. Political actors within these institutions, however, change frequently as a result of the normal democratic political process (through elections, governing coalitions, and the like), and I will argue that this variation within political institutions accounts for the unexplained and unexpected variance in rates of international conflict by democracies.

CHAPTER 3

VETO PLAYERS, INSTITUTIONAL CONTEXT AND CONFLICT

In the previous chapter, I presented the liberal theoretical tradition in international relations upon which this dissertation is based. On the question of the conflict behavior of democracies, the subject of the present study, the preceding discussion of liberal scholarship demonstrated three central, and intellectually progressive, points. First, international relations between democracies are unique and unusually pacific. Domestic political institutions, operating along with peaceful democratic norms, constrain democratic executives from engaging in conflict with one another. Second, those mechanisms that support peaceful relations between democracies, particularly the constraining effects of separated political institutions and electoral accountability, have a systematic effect on a democracy's conflict behavior regardless of the domestic political organization of other states. In this manner, the international relations of democracies, especially their ability to initiate conflict, is inextricably linked to their domestic political organization. And third, since democracies exhibit considerable variation in those political institutions that constrain democratic belligerence, these variations are likely associated with the commensurate differences in foreign policies across democracies. Fundamental to this liberal scholarship is the notion that democratic executives are constrained by their domestic political institutions in making policy decisions to go to war, particularly in regard to the initiation of international conflict.

I also introduced the concept of "veto players" as a valuable way to understand the constraints facing democratic executives that block foreign policy decisions to go to war. Veto players, that is to say "individual or collective actor[s] whose agreement is required for a policy decision" (Tsebelis 1995: 293), form decisive impediments that restrain democratic executives from committing to a costly foreign policy decision to go to war. The democratic peace literature, in its dyadic and monadic variants, implicitly recognizes veto players by its reference to separated political institutions and electoral accountability, both of which "veto" or severely punish those leaders who make belligerent foreign policies. The "democratic differentiation" literature invokes the concept of veto players even more explicitly: those democracies that diffuse power the most, whether through intra- or inter- institutional checks or greater public accountability, create more veto points that inhibit belligerent foreign policy choices by democratic executives.

The intellectual progression of liberal scholarship, combined with the theoretical rigor introduced by veto player theory, represents a distinct improvement in how we understand the foreign policy making process of democracies. That democracies exhibit unique foreign policies, particularly in regard to the use of force as a tool of statecraft, is no longer doubted by reasonable scholars of international relations or practitioners of foreign policies. Despite this scholarly progress, however, we are less certain as to why democracies vary among themselves in their foreign policy. In particular, the manner in which political institutions affect the conflict propensity of democracies, the central concern of this dissertation, remains a vexing issue. The current state of the field concerning the impact of domestic institutions on the initiation of conflict has raised puzzling relationships and unresolved questions. The mechanisms cited by democratic peace scholars, for instance, have an indeterminate effect: democracies are more

pacific than non-democracies, yet equally democratic countries vary widely in their use of force in international relations. Likewise, the "democratic differentiation" literature raises the puzzling prospect that similar institutional arrangements can cause widely divergent outcomes in terms of conflict propensity—or that diverging institutional configurations are associated with similar rates of conflict.

If our current understanding of democratic foreign policy making is inadequate, and veto player theory is the leading theoretical tool utilized by scholars to understand it, then by critically examining and recasting veto player theory, we may arrive at a novel understanding of how political institutions affect the decision to go to war. To that end, this chapter explicates the main theoretical argument of this dissertation about the effects of domestic institutions on conflict initiation by democracies. In it, I argue that veto player theory, the dominant way in which political scientists understand how political institutions work, is mis-specified in that it describes an overly static conception of political institutions. The democratic peace literature, for instance, presents an exclusively bivariate understanding of domestic institutional organization: states are either democratic or they are not, and variation between democracies in their foreign policy thus can not be accounted for. By the same token, the "democratic differentiation" literature paints an equally rigid picture of domestic institutional constraints on democratic war-making: those institutions that establish more individual or collective actors (i.e. veto players) are simply the most constrained. The process by which democracies initiate conflict, I argue in the following pages, is more dynamic than the "black box" picture portrayed by the current state of the literature. The institutional setting within which veto players operate determine their impact.

This chapter proceeds in three parts. In order to lay the foundation for my argument revolving around institutional context, I first review the literature regarding veto player theory,

currently the "state of the art" in understanding how political institutions shape aggregate policy outcome. In doing so, I pay close attention to the work of Tsebelis (1995, 1999, 2002), who provides the most rigorous treatment of veto players and is in fact the theoretical source of most, if not all, contemporary works that utilize veto players as an explanatory tool. The basic insight offered by veto player theory is that domestic institutional configurations specify the number of veto players within each system, whether they be institutional veto players (i.e. separated branches of government) or partisan veto players (i.e. coalition partners and parties in parliament). As they become more numerous, these veto players increasingly constrain the policy making process including the decision to use force as a tool of foreign policy.

Next, I present evidence that not all veto players constrain the policy making process as postulated by veto player theory. Reviewing the work of Crepaz (1998, 2000, 2002; also Birchfield and Crepaz 1998; Crepaz and Moser 2002), I demonstrate that the effect of veto players depends on the setting in which they operate. Central to the discussion here is the distinction between what Crepaz terms "competitive" and "collective" veto players. Competitive veto players hinder the policy making process as hypothesized by traditional veto player theory and emerge from political institutions that diffuse political power between "competing" agencies or branches of government. Collective veto players, on the other hand, have an "enabling" effect on the policy making process and emerge from political institutions within which multiple partisan actors operate. The key insight of this perspective is that when multiple veto players interact within the same political institution, incentives for collective agency emerge. In this manner, collective actors that we might normally expect to veto or constrain the policy-making process have the opposite effect.

Building on the insights of "collective" versus "competitive" veto players, I then specify a domestic institutional theory of international conflict behavior revolving around institutional context among the universe of democracies. I define institutional context, which varies across countries and changes over time, as the political setting in which veto players operate. This political setting is determined by the partisan composition of the various institutions of government, which in turn infuses political actors with different opportunities and motive for veto behavior. I argue that a competitive institutional context has a constraining effect on the democratic use of force, whereas a collective institutional context has an enabling effect. While this approach is similar to that of Crepaz in that I define circumstances under which veto players act "competitively" or "collectively," my approach differs in that I define competitive versus collective agency across (not just within) political institutions. More importantly, my approach differs from veto player theory (particularly the work of Tsebelis) in that multiple partisan political actors within the executive do not constrain the use of force, and that strong political opposition in all legislatures does. Specifically, smaller ruling coalitions and strong legislative opposition to the executive represent a competitive institutional context, thus constraining the initiation of conflict. Conversely, large coalition cabinets and weak legislative opposition to the executive represent a collective institutional context, thus making an executive decision to initiate conflict more likely. Given a certain institutional context, some democracies are more "dangerous" than others.

Veto Players

That the political institutions of democracy constrain government power is hardly a novel idea. In the context of limiting government power as the best means to secure liberty, Montesquieu

writes in *The Spirit of Laws*, "the legislative body being composed of two parts, one checks the other, by the mutual privilege of refusing . . . . Sufficient it is for my purpose to observe that [liberty] is established by their laws" (1977: 210-11, quoted in Tsebelis 2002: 9). For Madison, too, the separation of powers is the central component of a restrained government that best protects the freedom of the citizenry: "the legislative, executive, and judiciary departments ought to be separate and distinct . . . . [as an] essential precaution in favor of liberty" (*The Federalist* 1961 [1788]: 336). Central to the passages written by Montesquieu and Madison is the notion that deliberately separating power, whether within a single institution or across a multitude of institutions, will restrain the actions of government.[1] This is the essence of what contemporary scholars mean when they reference veto players.

While the pedigree of the argument presented by veto player theory is thus centuries old, only recently has it become one of the more often utilized concepts in the political science literature. Most of this literature can be found in the field of comparative politics within which scholars have frequently cited the variable effect of different institutional forms on policy production, especially social welfare policies. Ellen Immergut (1992), commonly cited as one of the earliest contemporary scholars to employ a veto player logic in systematic empirical studies, demonstrates the impact of political institutions on the national health insurance policies of Switzerland, France, and Sweden. She argues that the state is best understood as a set of institutions that channels the influence of groups and other interests into public policy. The nature of these institutions is fundamentally important in that different forms of political institutions channel interests differently, explaining, at least in Immergut's case, the varying health care policies of different countries. Of particular relevance is how different institutional

[1] Tsebelis (2002) traces the history of veto player logic as far back as Livy's *History of Rome* and even Thucydides' *Histories* (Tsebelis 2002: 10).

forms allow some groups to block or "veto" policy, or alternatively, allow bare parliamentary majorities to enact legislation due to the institutional exclusion of "veto players" who might otherwise hinder its passage.

Building on Immergut's study, Huber, Ragin, and Stephens (1993) offer a more sophisticated analysis of the effects of political institutions on welfare state expansion. Again alluding to a veto player logic, Huber et al. argue that "constitutional structure[s] that disperse political power and offer multiple points of influence on the making and implementation of policy are inimical to welfare state expansion and will be negatively associated with . . . welfare state effort" (1993: 722). Constitutional features such as the political institutions of federalism, presidentialism, bicameralism, single-member-district electoral systems, and referenda provisions all disperse political power in such a way as to block legislation to expand welfare state provisions. These finding are similar to those of Hicks and Swank (1992) who demonstrate that welfare spending is easiest in those systems that centralize, rather than disperse, political power the most: "centralized/unitary government reduces veto points for the obstruction of pro-welfare reforms and streamlines political instruments for the enactment of such reforms" (1992: 661; Crepaz [1998: 63] also summarizes these findings). States whose political institutions disperse political power across a multitude of veto players, on the other hand, have restrictive effects on the expansion of welfare policies (Swank 2001, 2002).

In these studies of welfare state expenditures, political institutions that create more veto players make changing the policy status quo toward an expansionary direction more difficult. But as Crepaz (1998, 2002) explains, some of these findings are quite puzzling for the greatest welfare state expansion has occurred in those states that typically have the most veto players (i.e. those systems with political features that diffuse power the most, especially multi-party

governing coalitions). Crepaz (1998, also Crepaz and Moser 2002) notes the counterintuitive finding that the most generous welfare spenders (such as Sweden, Norway, Belgium, Netherlands, Denmark) are also the systems with the greatest number of parliamentary and governing parties that, according to veto player theory, are supposed to restrain such spending. As in the case with the democratic differentiation literature, not all of the expectations yielded from veto player theory match the actual state of the world. I turn to the resolution of these puzzling relationships in the following sections.

Beyond social welfare policies, scholars have applied the insights of veto player theory to a wide variety of questions in comparative politics. Hallerberg and Basinger (1998), for instance, find that changes in tax policy are more difficult when agreement is required across institutions and among political parties, both of which constitute veto players. Kreppel (1997) reaches similar conclusions in his study of Italy, finding that legislative output declines with additional government parties, and likewise, Bawn (1999) finds that changes in government spending in Germany is more difficult with more parties in government. Kastner and Rector (2003) explain the impact of veto players on the capital control policies of the industrialized democracies. Arguing that both domestic and international level variables affect the movement of international capital, they find that while certain features of the international system can reduce the importance of domestic political institutions, "states with a higher number of veto player parties in government [nonetheless] enact fewer capital controls policy changes" than states with fewer veto players (2003: 1).

On the question of the potential politicization of independent central banks, Keefer and Stasavage (2003) find that the inclusion of multiple domestic veto players enhances the credibility of monetary policy. In the same vein, MacIntyre (2001, 2002) demonstrates that

additional veto players have a fundamental impact on the response of Asian governments to economic crisis and policy reform more generally. Franzese (1996) shows similar findings for levels of national government debt, explaining that countries with many veto players are unable to change established patterns of national deficits, and Treisman (1998) demonstrates that multiple veto players in both developed and developing countries lock their governments into the same patterns of inflation.

In the international relations literature, the integration of veto player theory is also becoming increasingly common. As presented in the preceding chapter, for instance, democratic peace theorists note that separated institutions of government, multiple partners in coalition governments, multiple parties in legislatures, and even greater electoral participation infuses democracies with additional veto players that inhibit a democratic executive's use of force in international relations. In addition to studies of the democratic peace, at least one article on the topic of international institutions specifically references veto players. In a comparative study of membership in international organizations among advanced industrialized democracies, Minnich (2004) finds that where constitutional configurations create more veto players, domestic commitment to international institutions (like all policy making) becomes more difficult. Such veto players emerge from multi-party coalition cabinets, rigid constitutions, and separated institutions of government. All of these studies agree in some fashion that across a variety of policy dimensions, changing the status quo is more difficult when more veto players are involved in the democratic policy process.

The recent work of George Tsebelis (1995, 1999, 2002; also Tsebelis and Chang 2001), however, provides the most theoretically rigorous treatment of veto players and is in fact the source of most of the previously cited works that reference them. A closer examination of the

logical underpinnings of Tsebelis's formulations is thus warranted. According to Tsebelis, the key to understanding how the various political institutions of democracy work is to understand how they afford different political actors the power to affect policy choices. While political scientists have long differentiated democracies along a variety of institutional dimensions (such as presidential versus parliamentary regimes, unicameral versus bicameral legislatures, strong versus weak parties, or multiparty versus two party systems), comparison between these dimensions has been difficult. For example, how does one best compare the composition and policy output of presidential regimes with a weak two-party system to parliamentary regimes with multiple strong parties? The answer, according to Tsebelis, is to develop a comparative framework that describes the political differences between democracies along a single, but inclusive, dimension: the number of veto players. According to his theory of veto players, "the fundamental political differences between countries are generated by the number of veto players (individual or collective actors whose agreement is necessary for a change of the status quo)" (Tsebelis 1999: 591).

What, precisely, constitutes a veto player? In his recently published book, *Veto Players: How Political Institutions Work*, Tsebelis (2002: 2) provides a succinct answer:

> In order to change policy [i.e. the legislative status quo] . . . a certain number of individual or collective actors have to agree to the proposed change. I call such actors *veto players*. Veto players are specified in a country by the constitution (the president, the House, and the Senate in the United States) or by the political system (the different parties that are members of a government coalition in Western Europe). I call these two different types of veto players *institutional* and *partisan* veto players, respectively.

There are thus two types of veto players, institutional and partisan; the former are established by the constitution or other formal "rules of the game," and the latter are generated by the political process and exist "inside institution veto players" (Tsebelis 2002: 79). Counting veto players is straightforward: any veto player, whether institutional or partisan, counts as a veto player if it can block the adoption of any proposed policy.

Institutional veto players are those consistent with the Madisonian conception of "checks and balances." The most important of these occur as a result of macro-institutional differences among democracies (i.e. presidential versus parliamentary regimes) and exist, at least in Tsebelis's formulation, almost exclusively in presidential systems (with the exception of parliamentary systems with bicameralism and federalism). The number of institutional veto players increases with the diffusion of government authority across constitutionally determined political institutions. In the United States, for instance, the constitution specifies three potential veto players: agreement among the president, the House of Representatives, and the Senate are all required to pass legislation. Each of these has the institutional power to block policy—hence, they are each (potentially) a veto player. Federalism (particularly federal states that diffuse power widely, such as the United States, Germany, and Canada), along with bicameralism (especially "symmetric" bicameral legislatures, such as those found in the United States and Germany), can generate additional veto players, but only insofar as an argument can be made that these institutions can veto policy change. As Tsebelis notes for instance, Britain and Austria are both bicameral, but their upper houses do not constitute a veto player because they have only the power to delay legislation.[2]

[2] In fact, Tsebelis classifies Britain, Austria, and France as unicameral legislatures, and Germany as a mixed case (where only in policies concerning federalism do both legislative chambers have formal veto power).

In contrast to presidential regimes, parliamentary systems have only one institutional veto player (at least at the macro-institutional level) due to the institutional fusion of the legislative parliament and executive cabinet. In counting veto players in parliamentary systems, the parliament itself does not qualify because it can not formally veto policy. Unlike presidential systems, parliamentary systems create a chain of power that flows from the legislative branch to the executive, thus removing institutional checks or vetoes. In the classic Westminster parliamentary model of the UK for instance, power is concentrated into an executive that is fused to the legislature. In this manner, the executive is not constrained by an institutionally independent legislature; the parliament thus can not "veto" the executive.

Partisan veto players—that is to say, political parties—are those that operate within political institutions, are generated by normal political processes, and are most prevalent in parliamentary regimes. Again, a political party counts as a veto player if it can block policy change; this occurs in multi-party government coalitions, but not in single party governments, the former having multiple veto players and the latter having only one.[3] Accordingly, a one party parliamentary government that is replaced by a two party coalition government changes the regime from a one-veto player system to a two-veto player system (assuming a unitary state with a unicameral legislature). In Italy from 1986 to 1990, for instance, a series of governing coalition that included five parties required the agreement of all five partisan veto players to pass policy proposals (Hallerberg and Basinger 1998).

The interaction of institutional and partisan actors can also affect the number of veto players. Under Tsebelis's "absorption rule," political institutions in some circumstances "absorb"

[3] For Tsebelis, single party majority and single party minority governments have only one veto player. Nevertheless, since minority governments have to rely on noncabinet parties in parliament to change policy, most of the literature on veto players considers the additional noncabinet parties to have "veto" power, at least politically, over the minority cabinet. Thus, there is not strong support in the literature for the case of single party minority governments as constituting a single veto player regime.

or eliminate certain partisan veto players. That is, the number of institutional veto players is reduced when two or more institutions are controlled by the same political party—they are, in other words, ideologically congruent. In the United States, for instance, the three constitutionally determined veto players can be reduced to two if a single party controls two different institutions, or to one if a single party controls all three. After the 2002 midterm elections, for example, the number of veto players was reduced from two (the Republicans controlled the presidency and the House, but not the Senate) to one when the Republican maintained their hold on the House and won control of the Senate.

Besides the principal institutional and partisan veto players highlighted by Tsebelis, other institutions and political processes generate less visible veto players in some cases. For example, courts, interest groups, legislative committees, specific executive cabinet posts, and even influential political and military leaders can block change in the policy status quo (Tsebelis 2002). The Supreme Court of the United States is a particularly good example of an influential veto player outside of those already discussed. It can be a much greater constraint in the policy process for a presidential system versus a parliamentary one because of its ability to strike down, or literally "veto," a public policy if it violates the formal constitution. But because these comparably minor institutional differences are not common across all democracies, Tsebelis instead focuses primarily on those broad macro-institutional differences that generate the major institutional veto players and partisan political actors who exist inside of them. He does note, however, that these "less frequent" veto players may play more significant roles in certain policy areas and so must be accounted for in individual policy-specific or case studies where their impact is likely.[4]

[4] In the case of international affairs, for instance, some legislatures may have additional power over certain foreign policy decisions that other legislatures do not. The U.S. Senate requirement to

What effect do veto players have on policy production? The principal effect of veto players, both partisan and institutional, is to constrain—literally veto—the policy making process. The case is straightforward for institutional veto players. In the United States, for instance, the president can veto legislation passed by the House and the Senate (subject to an override), and support that does not cross the majority threshold in either legislative chamber never reaches the president for his consideration. On the other had, parliamentary executives can enact legislation more easily for they face no institutional veto from the legislature.

In terms of partisan veto players, Tsebelis focuses on the importance of additional parties in parliamentary cabinets. The partisan composition of the legislative branch, on the other hand, is only important if that legislative branch can qualify as an institutional veto player, such as in the case of presidential systems with an institutional separation of power, but not parliamentary ones with an institutional fusion of power. In this manner, political opposition in the legislative branch is only a veto player in a presidential system (i.e. divided government). In parliamentary systems, legislative opposition, because it is not part of the governing coalition—and thus not a player in the policy process—does not qualify as a veto player.

With this in mind, more parties in government have a restraining effect on policy change. According to Tsebelis, "veto players' theory predicts that policy stability (defined as the impossibility of significant change of the status quo) will be the result of large coalition governments" (1999: 591). Coalition governments can be described as either minimal winning coalitions (that is, governments that are composed of only those parties that are necessary to form a parliamentary majority) or oversized coalitions (governments that are composed of more parties than required to form a majority). Whether minimal winning or oversized, all coalition

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approve presidential treaties is one example. This additional "veto" power is accommodated in this dissertation's research design.

governments with multiple partners requires agreement among a greater number of collective actors to change policy, making policy change more difficult: "the more partners there are in government, the more difficult it becomes to induce a significant change" (1999: 594). Quite simply, more parties in government results in more veto players, and the addition of veto players makes any policy change more difficult.

Majority governments (that is, governments where a single party controls the cabinet), on the other hand, face the fewest veto players in the policy process. According to veto player theory, majority governments face the fewest decisional constraints and should thus be the least constrained. In parliamentary cases, single party majority governments have only a single veto player because with this type of cabinet, parliamentary and executive rule are the same (Tsebelis 1995, Ireland and Gartner 2001). Therefore, single party majority governments, constituting a single veto player and requiring only those votes of the party in power, can change policy very easily.

I should note that Tsebelis equates single party governments with minimal winning coalition governments insofar as, in both types, "the government coincides with a majority in parliament" (2002: 94). What distinguishes these two, however, is that while both enjoy a majority in parliament, minimal winning coalitions have an additional veto player [or players] to overcome: the additional party or parties required to form the majority government coalition. Tsebelis explains: "the replacement of a single-party majority by a two-party majority inside any institutional veto player transforms the situation from a single partisan veto player to two partisan veto players" (2002: 79). That is, the chief difference between a single party majority government (which constitutes one partisan veto player) and a multiparty majority government (which constitutes the number of veto players equal to the number of parties in the coalition) is

the addition of more partisan veto players. The proper distinguishing characteristic between governments that enjoy at least majority support in parliament, then, is the number of partners in the coalition: as the number of parties in the governing coalition increases, so do the number of veto players, or constraints, that hinder policy change.

A final type of government, the minority cabinet, is the most constrained. While minority governments, like majority and coalition governments, have the institutional power of agenda control, passing a policy agenda is much more difficult. In these cases, policy change requires support from outside the cabinet because the government in power does not have majority support in the legislature. Therefore, the members of a minority government (which may be one in the case of single party minority governments, or more in the case of minority coalitions) plus the additional support of parliamentary parties not in the coalition all constitute veto players that can block changes in policy.

There is some complication in counting veto players in minority governments, however. A single party minority government, according to Tsebelis, would qualify as a one veto player system just as a single party majority government would. This means that a single party minority government would have the same number of partisan veto players as a single party majority government, and fewer partisan veto players than a multiparty coalition government. In changing the policy status quo, single party minority governments would thus appear equally constrained as single party majority governments, and less constrained than coalition majorities. But because minority governments, whether composed of a single party or multiple parties, face the additional institutional hurdle of not comprising a majority, most scholars employing the theory of veto players consider them the most constrained of all governments. Frognier (1993) and Lijphart (1999), for example, argue that minority governments have particularly constrained

freedom of action because of the need to acquire noncabinet support to implement policy. Even Tsebelis admits that minority governments require the support of additional noncabinet parties to change the legislative status quo. The constant negotiation with parliamentary parties required of minority governments to maintain office and implement policy thus affords parliament with political "veto" power.

In the literature considering the effect of government type on international relations, scholars agree that minority governments, whether single party or coalitions, have the least amount of decision-making freedom. Reiter and Tillman (2002) argue, for instance, that minority governments require a broad consensus across multiple political parties to change policy—particularly a change to an assertive or belligerent foreign policy. These parties, although not members of a governing coalition, nonetheless constitute veto players in that their approval is necessary for the minority government to change the policy status quo. As Ireland and Gartner note of the precarious position of minority governments, "Parliament is always a potential veto player" (2001: 552). Single party majority governments, on the other hand, need not seek out or maintain consensus from any additional veto players to change or implement its preferred policies (Reiter and Tillman 2002). Noting the "veto power" of parliament vis-à-vis minority governments, Prins and Sprecher agree: "Minority governments should demonstrate the least ability in developing and sustaining a coherent and stable foreign policy regardless of whether the minority cabinet consists of a single party of a coalition of parties . . . because of the necessity of acquiring outside support to implement a policy option" (1999: 274). Therefore, compared to coalition and majority governments, minority governments are especially unlikely to change the policy status quo for they face the most veto players. Given the logic of the veto

player literature, this effect should be most pronounced for minority government with multiple partisan coalition partners.

While the effect of minority governments on the policy process is clear from a veto players perspective, not all scholars agree that minority governments are so constrained. Lijphart (1999) and Crepaz (2002, Crepaz and Moser 2002), for instance, argue that, in many circumstances, minority governing is quite similar to coalition governing. As Lijphart explains, minority cabinets must "negotiate continually with one or more noncabinet parties both to stay in office and to solicit support for its legislative proposals; this bargaining relationship, typically with different noncabinet parties for different purposes, makes such minority cabinets resemble oversized coalitions" (1999: 104). For Lijphart and Crepaz, this distinction is important because, for them, oversized coalitions and minority coalitions represent cabinets with the most power diffusion. Single party majority cabinets, on the other hand, represent governments where power is most concentrated (minimal winning coalitions and one party minority cabinets fall in an intermediate position between theses two). Disagreeing with Tsebelis's perspective, Lijphart and Crepaz argue that where diffusion of executive power is most dispersed, the shared power and responsibility that comes with it actually infuses a considerable amount of cooperation between coalition members which, in turn, facilitates policy formation. This is a key element of Lijphart's theory of consensus democracy, and in fact informs the argument offered in this dissertation. From the theoretical perspective of veto players, however, coalition cabinets and especially minority cabinets face more veto players than do single party majority cabinets, and so are more constrained in changing policy.

A corollary to the constraining effect of additional partisan veto players on the policy process also applies. In addition to the *number* of veto players present in any system, their

ideological distance from one another should also restrain the ability of government to produce policy change. The reason, according to Tsebelis, is that as the ideological distance between veto players increases, the room for an agreement on policy change (i.e. the size of the "win-set") decreases. While Tsebelis himself finds that greater ideological distances between veto players does reduce policy change (at least for "significant legislation," including budgetary reform) in parliamentary systems (1999, 2002), his hypotheses remain untested for presidential systems, and Hallerger and Basinger (2001) find no evidence that ideological distance matters at all. Regardless, finding that ideological distance has any effect on the policy process is not a necessary condition for demonstrating that restraining effect of veto players. The distance between parties in governments matters less than that the parties themselves constitute veto players (which they do in Tsebelis's formulations). Therefore, irrespective of their ideological distance, multiple parties in government make policy change more difficult.

In summary, we can use the theory of veto players to distinguish democratic political systems by capacity to produce policy change (Tsebelis 1995: 289). Changing the policy status quo in a democracy requires the consent of the political actors who have veto power in the policy process. These veto players may be institutional (such as presidents and legislative chambers) or partisan (parties in government) in nature. While institutional veto players emerge from the constitutional separation of powers (say, between the President and Congress in presidential systems), and partisan veto player are common to parliamentary systems (for example, emerging from multiple partisan partners of a coalition executive cabinet), the effect of both is the same: more numerous veto players have the effect of decreasing the capacity for democracies to implement any policy proposal. As applied to the explanadum of this study, *the potential to initiate conflict as a foreign policy choice decreases with the number of veto players*.

The greatest advantage of the veto player perspective offered by Tsebelis is that it places our current understanding of how political institutions work into a comprehensive and parsimonious framework, distilling the effects of a wide variety of political institutions and actors into straightforward, yet theoretically rich, propositions. While the ideas underlying the theory of veto players is not particularly new, veto player theory is fast becoming one of the more frequently utilized concepts in the political science literature.

Unfortunately, the same framework that can be lauded for its parsimony and deductive appeal leads to some puzzling relationships and unresolved empirical patterns. As demonstrated in the previous chapter, for instance, the logic underlying veto player theory—that the institutional configurations that generate the most veto players constitute the most heavily constrained democracies in the use of force—does not seem to match the actual conflict behavior of democracies. "Equally democratic" regimes, facing the same institutional constraints of checks and balances, vary widely in their conflict behavior—or, alternatively, systems with more domestic veto players seem to be just as conflict prone as those with supposedly fewer. Crepaz uncovers a similar puzzle in applying veto player theory to social welfare polices. Why, despite veto player theory's expectations to the contrary, do expansions in social welfare policies occur in those systems with some of the most numerous veto players?

Besides these specific cases that point to inconsistencies in veto player theory, is it accurate in a general sense to assume, as Tsebelis does, that partisan veto players will always constrain efforts to change the policy status quo? What, for instance, is the incentive for one partisan coalition partner to "veto" proposed policies of another coalition partner when such a maneuver might endanger the coalition deal? In this manner, partisan veto players, while theoretically afforded a "veto" role, may have no incentive for playing it. Similarly, one might

also query as to whether institutional veto players that share party affiliation—as is the case in unified presidential governing—might actually block each other's policy proposals. While Tsebelis's "absorption rule" suggests that institutional veto players are reduced (or "absorbed") during periods of unified partisan control, Madison himself assumes that institutional actors, each having "a will of its own," have an incentive to check one another. After all, the purpose of separating power into multiple institutions is so that the government's "several constituents parts may, by their mutual relations, be the means of keeping each other in their proper places" (*The Federalist No. 51* 1961 [1788]: 355). As such, institutional veto players, even though they may share similar ideological positions, may still play their constitutionally determined veto role.

One may also add to this list of puzzling relationships the fact that oppositions in parliament frequent do have a restraining effect on policy production. While Tsebelis argues that such parliamentary oppositions are neither an institutional nor partisan veto player, parliamentary oppositions frequently attempt to expose deficient government policies as a means to prevent the passage of unpopular or unsupported proposals. After all, political oppositions, even if excluded from the policy process as a minority, often search for political ammunition in the way of exposing failed or costly policies. In this sense, parliamentary oppositions may indeed serve as a political check or veto on policy proposals, one overlooked by Tsebelis's theory of veto players.

In order to resolve these puzzling questions, I next turn to evidence that veto players have a much more variable, or dynamic, effect on the policy making process than Tsebelis's theory of players postulates. The effect of veto players, it seems, varies according to the political setting—or context—within which they exist.

From Veto Players to Institutional Context

The liberal scholarship presented in the previous chapter is clearly consistent with "veto point" or "veto player" logic. In the dyadic and monadic democratic peace literature, democracies are more constrained than non-democracies because of the formal establishment of more veto points—the institutional hurdles of the separation of powers and the consent of the governed. In the "democratic differentiation" literature, some democracies are more constrained than others because of the inclusion of additional institutional "veto players" required to make the political decision to initiate conflict. In short, those political systems that require the consent of more political actors are simply more constrained: the greater the number of veto points, the less likely conflict will emerge as a foreign policy decision.

The most striking feature of this liberal scholarship, and the veto player logic underlying it, is its implicit assumption that all veto players possess a "competitive" motivation. This is to say that all of the literature cited to this point assumes that the inclusion of additional veto players will always constrain the policy process—that because an opportunity to veto or block policy exists, a veto player will always exercise it. Thus, veto player theory holds that all collective actors will act in opposition towards one another, either rivaling the executive as an institution (in the case of institutional veto players) or opposing one another within a government coalition (in the case of partisan veto players). Accordingly, the political context within and across institutions is always the same: the inclusion of more veto players creates a competitive institutional setting in which actors with mutual veto power have motive to constrain government (that is, executive) decision-making, including the initiation of conflict abroad. I argue differently. While it is accurate that different democratic systems produce a varying number of veto points, the *character* of these veto points is not always alike. As Crepaz argues in his

research on partisan and institutional veto players, "not all veto points are created equal" (2002: 173). To state it another way, depending on the institutional context, veto players do not always constrain the policy process.

According to Crepaz (1998, 2000, 2002; also Birchfield and Crepaz 1998, Crepaz and Moser 2002), the key to understanding the impact of veto players on policy making is to understand the "setting" in which these veto players operate. For Crepaz, veto points can be either "competitive" or "collective" in nature. The former is consistent with the traditional notion of veto players; that is, individual or collective political actors who have veto power over public policy production. Competitive veto points exist when constitutional or institutional structures diffuse political power (federalism and bicameralism are the archetypal form). Such institutions, Crepaz argues, "operate through separate institutions with mutual veto power" and should thus "have a tendency to restrain government" (2002: 175). This line of reasoning shares with Tsebelis the Madisonian notion that separate agencies will, by their very nature, protect their own power by preventing the concentration of power into any single political institution. Each political institution is, in a sense, infused with the motivation to prevent rival branches from acquiring an inordinate share of political power as a means to protect their own power.

But there is another type of veto player, one that characterizes institutional or partisan settings in which political actors "share collective authority and interact with each other on a face-to-face basis" (Crepaz 2002: 175). These veto points, which Crepaz labels "collective veto points," are generated by partisan or societal actors, and can be defined by institutions with multiple political parties and corporatist interest mediation (Crepaz and Moser 2002). Multiple political parties in coalition governments are the best example of collective veto players. Parties in coalition governments, Crepaz argues, tend to logroll with coalition partners in order to retain

their cabinet positions. Such partisan partners operate within the same institution without the hindrance of "separate institutions with mutual veto power" (2002: 174). This institutional arrangement instills a considerable degree of "collective agency" and "shared responsibility" among these partisan actors, effectively eliminating competition among them (Crepaz 2002; Goodin 1996). With such collective agency, parties in the coalition understand that all members of that coalition will be held accountable for policy success or failures. In a counterintuitive interpretation of what Tsebelis terms "partisan veto players," collective actors that we might normally expect to veto or constrain the policy-making process have the opposite effect.

The chief difference between collective and competitive veto players are the incentives or motives that each face. Competitive veto players, as Crepaz labels Tsebelis's institutional veto players, operate according to an oppositional motivation assumption: such collective actors "represent separate agencies with mutual veto power which compete against each other" (Crepaz and Moser 2002: 13). Because competitive veto players exist in separate political institutions, they do not share political responsibility or electoral accountability; they are thus motivated to compete against one another. In this sense, politics between institutional actors is a zero-sum game; each institutional actor gains at the expense of the other. Collective veto players, on the other hand, have a different motivation. In Crepaz's formulations, these veto players operate in positive-sum game, one where all members of a coalition stand to gain by facilitating policy production. Coalition partners, Crepaz contends, prefer to support the policies of the coalition rather than constrain the policy process to the detriment of their own position in power. The motivation underlying coalition members is one of cooperation toward achieving policy goals. There is thus no motive for such veto players to restrain or veto one another.

Crepaz demonstrates the "enabling," rather than constraining, effects of collective veto points on expansionary fiscal policies. Coalition partners (as collective veto players), Crepaz argues, have significant incentives to pursue expansive government spending. Each partner, with distinct constituencies and interests, tends to "support each other's budget plans through logrolling mechanisms" in good economic times. Even in bad economic times, coalition partners find it difficult to curb spending because "each party wants to protect its own part of the budget, while favoring cuts in other parties' budgets" (2002: 174). Each partner, then, is faced with a collective action problem in which they all agree to increase (or maintain) spending at the cost of rising deficits for fear of sacrificing their own individual spending priorities. Crepaz and Moser summarize the effect of collective veto points on government spending, positing that "shared responsibility and the face to face interaction of coalition members, together with the option of engaging in logrolling, yields a lower capacity to exercise restraint in spending" (2002: 14). In this instance, multiple coalition partners do not constrain government in changing budgetary policies in an expansive direction at all as veto player theory would lead us to conclude. Passing fiscal policies that include a wide variety of provisions even in rough economic times is actually facilitated by collective veto players. Collective veto points define a collective institutional setting, one that is characterized not by a restrained executive cabinet, but rather a relatively "enabled" one in which coalition partners do not "veto" each other.

In addition to citing logrolling and collective action problems as a motivation for collective agency, Crepaz also notes additional institutional arrangements that engender compromise, instead of competition, between coalition members. He notes that "informal (e.g. coalition discipline) and formal institutional arrangements (e.g. written coalition agreements) in many parliamentary countries that exert a material force leading to compromises between the

coalition parties" that facilitates policy change (Crepaz and Moser 2002: 12). The purpose of such informal disciple or formal agreement is to achieve consensus among members, enforce deals that are made between parties, and to protect the constituency interests of each government member. These additional institutional arrangements represent, quite literally, "face to face interactions" that instill a sense of shared political responsibility, or "morality of duty" among coalition partners. The effect, Crepaz argues, is one of cooperation, not competition, between partisan actors. This sort of shared responsibility, logrolling, and cooperation between partisan veto players solves the puzzling relationship to which Crepaz alluded: that the most generous welfare spenders are also they systems with the greatest number of partisan veto players. The collective agency and shared responsibility among coalition partners leads to changes in social welfare spending in an expansionary direction.

Crepaz distinguishes only partisan or societal actors as having collective agency. Institutional actors, he argues, may have a different effect: "As far as institutional veto players (separate agencies with mutual veto powers, as in bicameralism and federalism) are concerned, one would indeed expect them to inhibit change in the status quo [of current public policy]" (2002: 174). In the United States and other presidential systems, for instance, there is a distinct absence of coalition governing (a single party majority always controls the executive branch) and shared responsibility across separated political institutions. In such systems, we might expect the lack of any collective agency across institutions as one institution is not held responsible for the successes and failures of another (Goodin 1996). Absent are the collective agency, shared political responsibility, and mutual accountability common to veto players that share political control of a single institution.

While plausible, and entirely consistent with Tsebelis's formulations regarding institutional veto players, this may be an overstatement. There is already a sizable amount of scholarly evidence suggesting that collective agency, emerging from shared political responsibility and electoral accountability, occurs across institutional veto players. In the U.S. case for instance, the electorate regularly judges political parties across separate political institutions for the various successes and failures of policy production. This has been demonstrated frequently in the divided government literature in American politics. Tufte (1975), for instance, finds that congressional midterm elections often serve as a referendum on the executive's performance, awarding the President's party for successes, penalizing his party for failures. If different political institutions are held accountable for partisan successes and failures, then there is every reason to expect at least some collective agency and shared responsibility across political institutions. Similarly, Sundquist (1988) posits that unified partisan control of government across institutions facilitates more coherent, and more quickly adopted and implemented, policy proposals, suggesting that in fact collective agency occurs across institutions. Divided government, on the other hand, hinders the policy making process, even preventing the production of any legislation at all.

The reason for divided government constraining the policy process—and for unified government "enabling" it—is because partisan differences magnify the institutional separation of power, whereas partisan similarities reduce it (Cox and Kernell 1991). In the context of foreign policy decision making, Destler (1985) and Clark (2002) show that collective agency often times occurs across political institutions when partisan control of those institutions is not divided, making foreign policy changes (even controversial initiatives) much easier for the president. Similarly, Lohmann and O'Halloran (1994) find the president's ability to conclude

international trade policies are much easier under unified government. Thus, it seems that various institutional actors are judged at least in part by partisan successes and failures *across* institutions. When there is partisan congruence between these institutions, collective agency often results. Alternatively, when there is partisan incongruence across institutions, competitive agency emerges.

Tsebelis himself accommodates the findings in the divided government literature that unified presidential government actually facilitates or enables the policy process. His "absorption rule" recognizes collective agency across institutions when he argues that unified control of institutions in presidential systems reduces the number of veto players. More properly, there is not a reduction in the number of veto players because, institutionally, veto players still exist—there are still multiple institutions with the power to veto policy change. But the *motivation* of those who control those institutions is different during periods of divided versus unified government. Unlike periods of divided government, unified government removes the incentive or motive for an institutional veto player to hinder the policy process because there is shared partisan control (and shared political responsibility). While Tsebelis's theory does not recognize it, understanding this motivational difference helps distinguish Crepaz's collective veto players from competitive ones and provides the key to understanding why some veto players do not constrain the policy process.

With these insights, it is wholly consistent with Crepaz's argument to expect collective agency to occur institutionally as well in some contexts (that is to say, *across* institutional veto players). Collective agency emerges when political responsibility and accountability are shared by various veto players. When a case can be made that political responsibility and accountability are shared by institutional veto players, then collective agency has indeed emerged. If, for

instance, partisan control of the U.S. Congress is different than that of the executive, we might indeed expect veto points to be "competitive" between the two branches. But if partisan control of Congress and the Presidency is united, we might in fact expect collective veto points between the two branches (or at least more collective agency than in the case of divided government, keeping in mind that even actors of the same political party often have very different institutional interests). This is the very finding of Clark (2002) in his research on the effects of divided government on the U.S. President's use of force, one anticipated by Tsebelis and a large portion of the divided government literature which all cite the elimination of competition among veto players when they are "absorbed" by party affiliation.

It thus seems that both competitive *and* collective agency can occur between political institutions. Based on the preceding review, the case for this in presidential systems is straightforward: divided government represents competitive agency between institutions, whereas unified government reflects collective agency. A persuasive case can also be made for competitive versus collective agency in parliamentary systems. Collective agency between the legislature and executive cabinet would seem clear: with parliamentary fusion—in which legislative and executive control are the same—parliament does not, or can not, constrain the executive. With the exception of minority cabinets, parliamentary executives control at least a majority share in the legislature. In this instance, shared political responsibility and accountability is unmistakable.

Despite parliamentary fusion, however, there is good reason to expect competitive agency between the legislature and executive cabinet in the form of parliamentary opposition. While Tsebelis himself argues that parliamentary oppositions have no formal veto power (neither the parliament nor opposition within it constitute veto players), they can, and often times do,

hinder executive policy making. Assuming that all political parties seek political office (which they most certainly do), there is, as Schultz (1998: 831) indicates, "a conflict of interest between the government and the opposition" as both seek to gain power. Given this conflict of interest, political oppositions, even those in parliament, attempt to increase their own electoral success by challenging the party or parties in power. In this manner, oppositions in parliament are motivated to consistently compete with parties in the executive, even colluding with other (ideologically similar) parties to exploit policy failure and present alternative programs to the electorate. This effect should be especially pronounced in the democratic use of force given that a foreign policy decision to go to war is "particularly risky since the likelihood of success and the costs to be incurred are often difficult to predict" (Rousseau et al. 1996: 513). This ability of the opposition to compete with the executive by exposing potential policy failure and presenting alternative platforms thus serves as an *ex ante* check on the government's use of force.

So even though parliamentary oppositions do not constitute a "veto player" in Tsebelis's formulations, their presence can hinder the executive's use of force. This expectation is confirmed by accumulating evidence in the democratic peace literature. Rousseau et al. (1996) argue that "foreign policy leaders who face credible political opposition should be more concerned with protecting themselves from a political backlash by avoiding risky military confrontations" (513). Similarly, findings by Gleditsch and Ward (1997) suggest that an effective political opposition may inhibit the diversionary use of force. Schultz (1998) demonstrates that political oppositions force executives to be more selective in making threats to use force and that the probability of war decreases with the inclusion of any domestic opposition party. And in a study of dispute escalation by parliamentary democracies, Prins and Sprecher (1999) confirm the

"veto power" of opposition parties, finding that an increase in the size of parliamentary oppositions is inversely related to the escalation of militarized disputes.

While there is thus compelling reason to expect competitive agency between parliamentary executives and their oppositions that may hinder an executive's foreign policy making, there is a dearth of research in the veto player literature even considering the constraining effects of parliamentary opposition. Finding that oppositional forces in parliament do in fact inhibit executive policy making would therefore constitute an important contribution to veto player theory.

In summary, the effect of veto players on the policy process is more dynamic than postulated by conventional veto player theory. Specifically, not all veto players have a constraining effect; they can have an "enabling" influence as well. When veto players share political responsibility and accountability, changing the policy status quo actually becomes easier. In this manner, Crepaz's greatest contribution to veto player theory is to demonstrate that one can not assume a competitive motivation between all veto players. In many instances, veto players often are motivated by collective agency to propel, not hinder, policy production. While Crepaz makes a strong case for collective agency between partisan veto players and competitive agency between institutional veto players, I have presented evidence that both collective and competitive agency can occur between institutional veto players. Taken together, we may now conclude that veto players *can* constrain the policy process by blocking policy proposals, but they not always *will*. What is more, veto players that may constrain the policy process can arise where veto player theory does not predict: in parliamentary oppositions. In the next section, I define the circumstance under which veto players can (or can not) and will (or will not) constrain a democratic executive's ability to initiate conflict.

Institution Context and Conflict Initiation

The key insight of Crepaz's research on collective and competitive veto points is that when political responsibility is shared among multiple "veto players" within institutions, incentives for collective agency emerge. As such, collective actors that we might normally expect to veto or constrain the policy-making process have the opposite effect. Crepaz demonstrates that such collective agency occurs between partisan veto players, while I have submitted two additional propositions: that collective agency can occur between institutional veto players, and that competitive veto players arise where we might not normally expect. In what follows, I place these various insights into a more coherent conceptual framework that I label "institutional context," and apply this framework to the issue of conflict initiation by democracies.

Fundamental to understanding whether veto players have a constraining or enabling effect on policy change is to recognize their *opportunity* and *motive* for political behavior. As I presented in the preceding section, veto players, while having the opportunity to block legislative change, do not always have to motive to do so. In some instances, veto players (such as Crepaz's "collective" veto players in coalition cabinets, and Tsebelis's institutional veto players who are absorbed by shared partisanship) are driven to support, not oppose, policy change. Strøm (2000: 280), in his research on parliamentary governing, captures part of this aspect of veto behavior:

> A credible veto player must have both opportunity and motive to exercise his or her veto. Partisan veto players may have motive (although this is not always obvious), but they do not generally have opportunity. Institutional veto players by definition have opportunity, though not necessarily motive. Interestingly, Tsebelis (1995: 310) discounts institutional veto players that have no discernible motive, i.e. when their preferences are identical to those of other veto players, for example, in congruent bicameral legislatures [in which

> veto players are "absorbed" or eliminated by their partisan congruence]. The same treatment should also be accorded to partisan veto players . . . .

As this passage reveals, veto behavior requires two elements: both the opportunity to veto as well as the motive. Tsebelis is correct in assuming that, while having the opportunity, not all institutional veto players have the motive to restrain the policy process. In such instances, institutional veto players without motive (such as a US Congress and President with the same partisan affiliation) are therefore discounted, or absorbed.

Yet somewhat surprisingly, Tsebelis does not allow for partisan veto players to be "absorbed" when they have no apparent motive to block legislative change—this is the point Strøm is suggesting when he writes that the "same treatment" should be afforded to partisan veto players when a case can be made that they have no motive to veto. In terms of partisan actors, Tsebelis's theory of veto players postulates that partisan veto players, defined by their opportunity to block policy changes, will always have the motive to do so. This is why, according to Tsebelis, additional coalition partners in the executive cabinet have a restraining effect on policy production.

Crepaz, though, demonstrates that additional partisan actors do not necessarily block or "veto" policy production. Since coalition partners share political responsibility and accountability, there are powerful motives to facilitate policy production rather than oppose it.[5] In this sense, there are congruent interests among each of the partisan veto players within a coalition to maintain its position the government and keep the coalition intact. Put differently,

[5] It should be noted that this explicit point is also lacking in Strøm's formulation. Partisan veto players, while sometimes having the motive to block policy, do not always have such a motive. Here, Crepaz's insights are fundamental—we must identify under which circumstance partisan actors have the motive to block policy change.

one should also apply Tsebelis's "absorption" rule to partisan veto players, identifying coalition partners that have opportunity, but not *motive*, to exercise veto over policy change.

In addition to understanding that not all veto players have motive to block policy change, we must also recognize that those collective actors with motive to constrain policy have more opportunity than Tsebelis acknowledges. Here I am referring to the conspicuous oversight of parliamentary oppositions. Clearly, such oppositions have motive to block executive policy proposals for they seek to steer policy in a different direction. What is more, like presidential oppositions during periods of divided government, parliamentary oppositions actively attempt to block the policy process by exposing costly or failing policies and offering alternative, and potentially more appealing, policies. This has been shown to restrain the government *ex ante* insofar as executives consider the political costs of a policy initiative that may be exposed by oppositions. So, Tsebelis overlooks the fact that partisan veto players with opportunity to veto do not always have motive to block policy change; similarly, he neglects those situations in which institutional veto players who have motive to block policy change also have the opportunity to veto.

The effect of veto players is thus contingent upon the setting in which they operate. This setting is what I term *institutional context*, and is defined by both the opportunity for political actors to veto policy change as well as the motivation underling veto player behavior. Following the terminology of Crepaz, institutional context can be either collective or competitive. In the former, political actors have motive to facilitate, not oppose, the policy process even when they have the opportunity to do so. In the latter, political actors have both the opportunity and motive to block policy change. Under this framework, defining veto players by their opportunity to

block policy change is not sufficient; we must also understand veto players' motive before we know their effect on the policy process.

How might we measure institutional context? Following Crepaz, one dimension on which to measure the setting in which veto players operate is in *the nature of the ruling coalition*. As he demonstrates, when executive cabinets are comprised of broad multi-party coalitions, there is considerable motive to facilitate the policy process among numerous political actors in order to maintain the governing coalition. Broad coalition cabinets thus represent a collective institutional context, one in which veto players have motive to support, not constrain, the policy process. Institutional context becomes more competitive as the size of the ruling coalition decreases. Unlike coalition governments that hold a large share of parliamentary votes and whose members act collectively, single party majority governments command much less legislative support and typically face more hostile and stronger oppositions. The constraints facing minority cabinets are even more pronounced as they face even larger oppositions and encounter more veto players who have motive to oppose executive policy.

Yet the nature of the executive cabinet is not the only dimension on which to measure institutional context. We must also look toward legislative bodies that are, after all, important in the policy process for they can collaborate or compete with executives who attempt to change policy. *The nature of the political opposition* thus constitutes a second dimension on which to measure institutional context. Whether in presidential or parliamentary systems, political oppositions have strong motive to oppose executive attempts to change policy for they seek to gain political office or redirect public policy in a different direction. In presidential systems, when the political opposition controls the legislative branch (i.e. in the case of divided government), the opposition can veto executive policy. In parliamentary systems, political

oppositions can constrain executive policy when it is sufficiently strong to expose deficient government policies and offer viable alternatives. In these instances, a competitive institutional context predominates. Institutional context on this dimension becomes more collective when political oppositions are weak. Weak political oppositions do not have the opportunity to redirect or veto public policy for they either do not control an institution with veto power (in the case of presidential systems) or they can not field a viable policy alternative (in the case of parliamentary systems).

In a nutshell, veto players do not always block the policy process. Their effect is more dynamic, and depends on institutional context. Under a collective institutional context, policy change is easier. But when veto players operate within a competitive institutional context, policy change is more difficult. With broad coalition cabinets within the executive, and exceedingly weak political oppositions in the legislature, we can say that veto players exist within a collective institutional context—collective because, while there may be opportunity for oppositional behavior, there is no motive to do so. Rather, veto players have motive to facilitate the policy process in the manner described by Crepaz because they share political responsibility and accountability. On the other hand, with smaller ruling coalitions in the executive, and strong political oppositions in the legislature, we can say that veto players exist in a competitive institutional context—competitive because there is opportunity and motive for oppositional behavior toward executive initiatives to change policy.

Given these definitional criteria, how might institutional context influence, or even alter, the purported constraining effects of veto players on the democratic use of force? It is the contention of this dissertation that competitive institutional context has a constraining effect on the democratic use of force, whereas a collective institutional context has an enabling effect.

Specifically, institutional context affects the democratic use of force on the two aforementioned dimensions: the nature of *the ruling coalition* (which I term executive institutional context) and the nature of *political opposition* (which I term executive-legislative institutional context). Large coalition cabinets and weak legislative opposition represent a collective institutional context. Conversely, smaller ruling coalitions and strong legislative opposition to the executive represent a competitive institutional context. While these two dimensions of institutional context mirror to some degree Tsebelis's distinction between partisan and institutional veto players—and Crepaz's distinction between collective and competitive veto players—they are theoretically distinct in that they accommodate competitive and collective agency between partisan and institutional actors.

Executive Institution Context

The nature of the ruling government coalition constitutes the first dimension on which to measure institutional context. The reference point for this dimension is Tsebelis's partisan veto players—and Crepaz's collective veto players, both of whom cite the importance of partisan actors in governing cabinets. Governments can consist of single party majority, coalition majority, or minority cabinets. Of the three, there is no question that minority governments are the most heavily constrained of all governments in the use of force. All things being equal, minority governments face far more veto players with motivation to block policy change than all other types of government. In these cases, policy change requires support from outside the cabinet because the government in power does not have majority support in the legislature. Therefore, the additional support of parliamentary parties not in the coalition constitutes veto players that can block changes in policy. In addition to the increased opportunity to block

minority government policy proposals, political actors existing outside the minority government are comprised exclusively of oppositional forces who seek political office for themselves and endeavor to direct policy in their preferred direction. Therefore, minority governments operate within a competitive institutional setting, facing veto players with both opportunity and motive to block policy change

There is already considerable evidence indicating that minority governments are so heavily constrained. According to Strøm (1990), minority governments in parliamentary systems are the most frequently voted out of office through votes of no confidence. Similarly, Maoz and Russet (1993) and Ireland and Gartner (2001) find that minority governments are the most constrained in foreign policy flexibility for they face the most partisan hurdles. And Prins and Sprecher (1999) find that minority governments are less likely to reciprocate military force for they face the most decisional constraints and stiffest partisan opposition.

Note, however, that my distinction regarding minority governments differs from that of Lijphart and Crepaz. For them, minority governing is quite similar to coalition governing because the same sort of collective agency that occurs among coalition partners is also likely to emerge between a minority government and any noncabinet parties required to support policy change. Like coalition governments, minority governments represent situations in which power is most diffused. Power diffusion, for Lijphart and Crepaz, instills a considerable amount of shared power and responsibility that generates collective agency and an "unencumbered" executive.

While it is accurate that minority governments must collaborate with noncabinet members just as coalition governments must collaborate with cabinet members to change policy, my theory regarding institution context makes a crucial distinction between the motivation

underlying veto players in each situation. In the case of coalition governments, each coalition member is motivated to facilitate the policy process in order to retain its position in government. As such, each coalition partner supports executive policies to maintain the coalition. In minority governments, however, noncabinet members have no position in the governing coalition, and so are not motivated by incentives to maintain the coalition. In fact, noncabinet members are, by definition, opposition parties who may be motivated to oppose executive policy. This is not to say that minority governments find it impossible to form majority support for all legislative proposals, because in fact they frequently do fashion such majority support. Minority governments, though, must "negotiate continually with one or more noncabinet parties both to stay in office and to solicit support for its legislative proposals" (Lijphart 1999: 104). This continual negotiation for support, while sustainable for many policy dimensions (such as those that may distribute concentrated benefits to specific constituencies), should be much more difficult for issues concerning very high political costs for which opposition parties may be unwilling to commit, but able to expose as deficient.

Fortunately, the degree to which minority cabinets are constrained can be resolved empirically as the research design presented in the next chapter proposes. From the perspective of institutional context, though, minority cabinets are the most heavily constrained because they face the most competitive institutional setting, one defined by the lack of majority support to enact any policy proposals. This effect should be especially pronounced for a foreign policy proposal to initiate conflict in which there are very high political costs that may prevent the sort of "consensus" or cooperation between a minority party and its opposition to which Lijphart and Crepaz allude. Therefore, minority governments exist in the most competitive institutional context possible, and therefore should be the least likely to initiate international conflict.

While the argument advanced here is consistent with the current state of veto players literature in terms of minority governments, on the remaining two types of ruling coalitions, an argument revolving around institutional context arrives at a different expectation than the one advanced by the conventional wisdom. As Maoz and Russet (1993: 626) summarize the conventional view of coalition versus majority governments, "coalition governments . . . are far more constrained than are governments controlled by a single party." Again, the argument here relies on a "competitive" veto player logic: given more coalition partners within a cabinet, that cabinet should exhibit more partisan veto players that hinder the policy process.

Despite this conventional view, there is ample reason to expect coalition governments to be less constrained than their single party majority counterparts. Coalition governments create collective agency. Collective agency, in turn, allows a greater degree of flexibility in the executive's use of force. As Crepaz argues, "In order to stay in a coalition government, parties in general prefer to logroll and stay in the coalition, rather than to obstruct the policy process and jeopardize their own position in government (2002: 174). In addition to logrolling, coalition members share political responsibility and are judged collectively (Goodin 1996; Crepaz 2002; Crepaz and Moser 2002). There should thus be motive for members of a coalition to support, not constrain, executive actions *including the use of military force*. If coalition members support an executive's initiation of conflict, the coalition is a successful one. If, however, some coalition members oppose an executive's use of forces, internal cabinet dissent will naturally cause the governing coalition to fail. In this sense, there is a classic collective action problem for members of a coalition: support a belligerent executive foreign policy and remain in the governing coalition even if a partisan member prefers a more pacific course, or oppose that foreign policy to the determinant of the entire coalition and their place in it.

Majority governments, on the other hand, suggest a much more competitive institutional context, one that should be associated with a much more constrained executive. Unlike coalition governments that hold a large share of parliamentary votes and whose members act collectively, majority governments command much less legislative support and typically face more hostile and stronger oppositions. As Prins and Sprecher note, "single-party governments often face opposition parties that intend to redirect public policy in a very different direction [and thus] may act extremely cautiously to avoid providing political ammunition to the parties out of power" (1999: 275). In this situation, there is thus considerable motive for parties out of power to oppose a single party majority cabinet. Therefore, with coalition governments, we should thus expect a collective institutional context; with majority governments, we should expect a more competitive—and more constrained—institutional context.

In addition to the parliamentary reasons to act collectively in government coalitions, there are electoral reasons to do so as well. With large governing coalitions, there is a certain "safety in numbers" for potentially costly foreign policies. Strøm (1990) and Prins and Sprecher (1999) confirm this expectation. They demonstrate that coalition partners are often less accountable for failed policies, and so have greater flexibility in their policy proposals. As Prins and Sprecher (1999) find, coalition governments are less constrained than majority governments in reciprocating military force because the electorate is unable to place blame on any one party. Following Strøm (1990), they argue that coalition governments are less accountable than other types of governments: "With coalition governments, the voting public may be less able to attach responsibility to any one party for policy failures. Presumably, then, coalition leaders would have greater flexibility in their handling of foreign affairs" (1999: 275). There thus seems to be considerable motive for collective agency within coalition governments *and* the absence of

electoral accountability for belligerent foreign policies. So while veto player theory paints a stark picture of a heavily constrained coalition cabinet due the inclusions of multiple veto players into the ruling coalition, the argument presented here predicts the opposite effect. Based on its collective institutional setting, coalition cabinets should be the least constrained of all cabinets in the use of force as a tool of foreign policy.

Apart from describing institutional context according to executive cabinet type (minority, majority, or coalition), we can also describe institutional context by the number of parties in the governing coalition, arriving at expectations contradictory to those derived from Tsebelis's theory of veto players. According to Tsebelis, cabinets with multiple parties have more veto players to constrain on executive policy-making. As the number of parties increases in the cabinet, the executive is more constrained as there are more veto players in the policy process. According to the argument presented here, however, the number of government parties should have the opposite effect.

Multiple parties in government produces incentives for collective agency where logrolling and shared political responsibility are the norm. Moreover, more government parties in the executive reduce the political accountability of each party. For any given policy, the electorate is unable to assign blame to any single party for any specific policy. Therefore, as the number of government parties increases, motive for collective agency within coalition governments increases and electoral accountability for belligerent foreign policies decreases. This, in turn, reduces the constraints that executives face in initiating any conflict. Therefore, as the number of parties in government increases, the setting in which those parties operate becomes more collective, and the executive is less constrained in initiating conflict.

Executive-Legislative Institutional Context

In addition to the nature of the ruling coalition, the nature of the political opposition comprises another dimension on which to measure institutional context. Following research by Tsebelis and Crepaz, the reference point for this dimension are institutional, or "competitive," veto players who reside in separate political institutions and may hinder or block executive policy proposals. In terms of this dimension of institutional context, consider first the case of the United States. According to the conventional view, macro-institutional organization (formal, constitutionally determined decision making bodies) constrains democratic leaders (particularly executives) by establishing veto players. Accordingly, the U.S. president is institutionally constrained in the policy-making process by Congress; Congress, then, is a veto player that may constrain a president's use of force as a tool of foreign policy as its consent is required for a variety of such decisions. Such institutions may be permanent, but institutional *context* changes over time. In other words, the separation of powers between the executive and legislature always exists, but the actors occupying those institutions change with measurable consequences. In this manner, a U.S. President might be more constrained when Congress is controlled by an opposition party (a competitive institution context), less constrained when Congress is controlled by his own party (a collective institution context).

Clark (2000), who has demonstrated that the President is more likely to initiate international disputes under united (rather than divided) government, confirms this expectation. Congress, in this example, is a veto player in a competitive institutional context (divided government), but not a collective institutional context (unified government). In this case, reference to only institutional organization (that is, the separation of powers in the U.S. political system) can not account for variation in conflict behavior under divided versus unified

government. Only by considering the institutional *context* can we distinguish such variations. This is an interesting and persuasive solution to the puzzle discussed in chapter 2 regarding macro-institutional structure and conflict. Presidential systems are not necessarily more or less constrained than parliamentary systems. Rather, the constraining effect of the institutional structures in a presidential system is mediated by institutional *context* (divided versus unified government in this case) in which institutionally determined veto players do not always constrain the use of force. Divided government is clearly an example of institutional competitiveness. Unified government, by contrast, illustrates a more collective institutional context.

Tsebelis recognizes the possibility that not all institutional veto players will constrain the policy process by citing his "absorption rule" that eliminates the veto effect of a legislature under unified partisan control. In the terminology presented in this dissertation, unified government removes the motive for oppositional behavior by the legislature even though there is still opportunity, creating a collective institutional context. Divided government, on the other hand, represents a competitive institutional setting in which there is significant motive—and opportunity—for adversarial behavior by the opposing party in Congress.

If divided versus unified government can distinguish presidential systems along a competitive-collective institutional axis, how might we similarly distinguish parliamentary systems? Divided government is a measure of political opposition (that is to say *partisan* opposition) in the legislature, one that exists even in parliamentary systems in which the executive is institutional fused to the legislature. Political oppositions, as Rousseau et al. (1996) note, are eager to exploit policy failure so as to justify a change in its preferred direction. We can say that this eagerness to exploit policy failure represents a competitive institutional context, one in which oppositions compete with the government over the direction of public policy.

Whether or not parliamentary oppositions can "veto" policy change by executives turns on two points. First, parliamentary oppositions must have motive. Here, the case is clear. Unlike veto players who are part of a governing coalition—and have motive to facilitate the policy process to maintain their position in the coalition—parties that are out of power have significant incentive to oppose executive policy. Given that all parties seek political office, they are locked into a zero-sum game where electoral success by one party (or coalition of parties) is inversely related to the success of another. Should the political opposition successfully demonstrate that a belligerent course of foreign policy is either too costly or overly risky, the electorate is more likely to replace the party or parties in power with the opposition. Thus competition over political office infuses political oppositions with the motive to compete with the executive, constraining the government's ability to propose or implement policy change. This effect should be particularly pronounced for executive decisions involving war where costs are typically high and spread widely across the electorate.

While political oppositions in parliamentary systems have sufficient motive to oppose a particular course of policy, demonstrating that they have opportunity to block the executive use of force is also critical. Tsebelis argues that they do not: parliamentary oppositions, because they are excluded from a policy process that includes only the majority in government, do not constitute a veto player. But the degree to which parliament is excluded in the policy process depends on its partisan composition. Even with parliamentary fusion, a strong political opposition in the legislature serves as a constraint on executive decision making in that it has the opportunity to expose deficient or costly policies and offer alternatives to the electorate. Schultz (1998) has shown that the introduction of political opposition lowers the *ex ante* probability of war insofar as executives consider the political costs of a policy initiative that may be exposed by

oppositions. As the opposition to the executive grows (i.e. as the margin of support between the government and opposition narrows), the constraining effect should be even more pronounced as the likelihood that an opposition can replace the government increases.

Findings by Prins and Sprecher support this view. Arguing that strong parliamentary oppositions "inhibit executive decisionmaking by threatening to expose deficiencies in government policies," they find that as opposition strength increases, parliamentary executives are considerably more restrained in their foreign policy choices (1999: 275). As with divided government in a presidential system, a sizable partisan opposition in a parliamentary system offers viable policy alternatives where even minor shifts in parliamentary or electoral support can give the opposition control of the government. Given this, there seems to be a considerable opportunity for strong political oppositions to act competitively for there are electoral or parliamentary advantages in doing so: successful government opposition can lead to changes in government control.

When there is only weak parliamentary opposition—and large majority support in the parliament for the executive cabinet—we may indeed expect collective agency between parliament and the executive along with the absence of constraint for implementing executive policies. In this situation, there may be little opportunity or motive for small, weak opposition parties to oppose government policies. With weak support in the electorate and small representation in parliament, small political oppositions have little opportunity to present opposing platforms or offer a credible electoral alternative should government policy prove deficient (i.e. an unsuccessful war). Moreover, small political opposition may in fact be motivated by strong incentives to support government policies as a way to curry favor with the coalition in power. Thus, parliamentary opposition, if sizeable and strong enough to act

competitively, can be in theory a veto player that may constrain executive action because it has the motive and opportunity to do so. While this is, of course, at odds with Tsebelis's formulations, it is at this point only an empirical issue as to whether parliamentary oppositions do in fact have a constraining effect on the executive's use of force.

To summarize the executive-legislative dimension of institutional context, strong partisan opposition in any legislature should constrain executive decision-making, particularly in the realm of the use of force abroad in that such decisions typically have high political costs. In the case of presidential systems, strong political opposition in the form of divided government constrains policy making and executive decision making for it serves as a partisan and institutional hurdle in the political process ("competitive veto points" in the terminology of Crepaz). Unified partisan control of government has the opposite effect as the institutional motive to veto executive action is removed by partisan congruence. In the parliamentary case, a strong political opposition also constrains policymaking and executive decision making—such an opposition has strong motive as well as opportunity for acting competitively, challenging deficient government policies and offering viable policy alternatives. Weak partisan oppositions, on the other hand, establish a much more collective institutional context where the executive is less constrained. This discussions leads to a firm expectation that greater oppositional strength in the legislature (which reflects a competitive institutional context) should inhibit executive decision making, thus decreasing the likelihood of conflict initiation. Weak opposition, signifying a more collective institutional context, should have the opposite effect.

In conclusion, institutional context can be defined as collective or competitive. In the former, institutional and partisan actors operate within a setting defined by shared responsibility and collective agency, and should have an enabling effect on a democratic executive's use of

force. In the latter, institutional and partisan actors operate within a competitive institutional context in which there is motive and opportunity for oppositional political behavior. Competitive institutional context should thus constrain the executive's use of force as a tool of statecraft. This collective-competitive axis of institutional context operates along two dimensions: the nature of the ruling coalition and the nature of political opposition. Ruling coalitions that are comprised of broad, multi-party cabinets defines a collective institutional context in which there are few decisional or electoral constraints on the initiation of conflict. By contrast, majority, and especially minority, governments face a much more competitive institutional context and should thus be associated with a lower propensity to initiate conflict. On the second dimension, strong political opposition in the legislative chamber signifies a competitive institutional context, one that should inhibit executive decision making and consequently decrease the likelihood of democratic conflict initiation. Weak political opposition, on the other hand, generate a collective institutional setting in which the democratic initiation of conflict is less constrained.

The insights derived from considering the impact of institutional context on the democratic use of force help resolve the puzzling relationships outlined in chapter 2. The democratic peace literature raises the confounding prospect that equally democratic countries vary widely in their conflict behavior over time and space. Similarly, the "democratic differentiation" literature raises the puzzling expectation that similar institutional structures cause a variety of outcomes (or that widely divergent institutions cause the same outcome). What these two literatures have in common is their reliance on a conventional veto player logic, one that I have argued paints a static picture of how domestic institutions shape policy outcomes. The effect of political institutions is more dynamic than these literatures portray. Institutions do not have a uniformly constraining effect on a democratic executive's use of force as a foreign policy

tool. Rather, the process by which democracies initiate conflict is more dynamic, dependent upon both the opportunity and motive for veto players operating within political institutions to block or facilitate the policy process.

The following chapter presents the research design utilized in this dissertation to test the claims of my argument concerning institutional context against those advocated by the conventional wisdom regarding democracies, political institutions, and conflict initiation. In addition to testing my argument against the democratic peace thesis and Tsebelis's veto player theory, I also incorporate more traditional perspectives from the international relations literature that have long considered the causes of international conflict initiation.

CHAPTER 4

RESEARCH DESIGN

This chapter presents the research design utilized to test the claims of my argument against those advocated by the conventional wisdom regarding democracies and conflict initiation. Recall that in the preceding chapters, I outlined two literatures that form the conventional wisdom over how democratic political institutions might influence the initiation of conflict: the democratic peace thesis and veto player theory. Taken together, these perspectives maintain that the democratic use of force is constrained by separated institutions of government and the placement of political actors whose agreement is required for policy decisions. In the former, democracies are more constrained than non-democracies because of the institutional hurdles of separated powers and the consent of the governed (i.e. veto players). In the latter, some democracies are more constrained than others because of the inclusion of additional institutional and partisan veto players required to make the political decision to initiate conflict. In short, those political systems that require the consent of more political actors are simply more constrained: the greater the number of veto players, the less likely conflict will emerge as a foreign policy decision.

On theoretical grounds, however, I have argued that these perspectives are deficient in that they define a static institutional context, one that can be regarding as exclusively "competitive": the inclusion of more veto players creates a competitive institutional setting in which actors with mutual veto power constrain government (that is, executive) decision-making,

including the initiation of conflict abroad. I have argued differently. While it is accurate that different democratic systems produce a varying number of veto players, the *character* of these veto players is not always alike. Different political configurations within and across institutions also can be *collective* in character. That is, veto players can in fact create a competitive institutional setting that constrains government; but veto players can also create a collective institutional setting that enables government. To state it another way, depending on the institutional context—that is, *the setting in which veto players operate*—veto players do not always hinder policy-making, including the foreign policy decision to go to war.

To summarize my argument, institutional context influences, or even alters, the purported constraining effects of veto players on the democratic use of force. Institutional context, in turn, can be defined as collective or competitive. In the former, institutional and partisan actors operate within a setting defined by shared responsibility and collective agency, and should have an enabling effect on a democratic executive's use of force. In the latter, institutional and partisan actors operate within a competitive institutional context in which there is motive for oppositional political behavior. Competitive institutional context should thus constrain the executive's use of force as a tool of statecraft. This collective-competitive axis of institutional context operates along two dimensions: the nature of the ruling coalition (which I term executive institution context) and the nature of political opposition (which I term executive-legislative institution context).

On the first dimension, executive institutional context, ruling coalitions that are comprised of broad, multi-party cabinets defines a collective institutional context in which there are few decisional or electoral constraints on the initiation of conflict. By contrast, majority, and especially minority, governments face a much more competitive institutional context and should

thus be associated with a lower propensity to initiate conflict. On the second dimension, executive-legislative institution context, strong political opposition in the legislative chamber signifies a competitive institutional context, one that should inhibit executive decision making and consequently decreasing the likelihood of democratic conflict initiation. Weak political opposition, on the other hand, generates a collective institutional setting in which the democratic initiation of conflict is less constrained.

The remainder of this chapter proceeds in two parts. First, I present the hypotheses and variables utilized in this study, summarizing the theories that underlie the hypotheses to be tested and the sources of the data used to measure these variables. Then, I describe the methodologies applied to test the theoretical formulations of this dissertation. The next chapter, Chapter 5, presents the results of the empirical analysis.

Hypotheses and Variables

With my theoretical argument thus proposed, I am now prepared to investigate empirically the impact of domestic institutional context on the democratic initiation of conflict. My core theoretical hypotheses are that a competitive institutional context has a constraining effect on the democratic use of force, whereas a collective institutional context has an enabling effect. To test these hypotheses, I examine twenty-nine established democracies over the period 1945 to 1992. I use data derived primarily from Reiter and Tillman (2002) who base their list of stable democracies on the Polity data set (Marshall and Jaggers 2000), Lijphart (1999), Mainwaring (1993), and other democratic scholars. Table 4.1 summarizes the unit of analysis for this research design (which is country-year in the statistical analysis), listing each country by its geographic location and Polity score (the higher the score, the more “democratic” each country).

Table 4.1. Unit of Analysis (Country and Polity Score)

Country	*Polity Score*	*Country*	*Polity Score*
Europe		**North America**	
United Kingdom	10	United States	10
Ireland	10	Canada	10
Netherlands	10	Jamaica	10
Belgium	10	Bahamas	--
Luxembourg	10		
France	9		
Switzerland	10	**Africa and the Middle East**	
Spain	9	Botswana	10
Portugal	10	Israel	9
Germany	10		
Austria	10		
Italy	10	**Asia Pacific**	
Greece	10	Japan	10
Finland	10	India	8
Sweden	10	Australia	10
Norway	10	New Zealand	10
Denmark	10		
Iceland	10		
Malta	--		

Sources: Polity IV Database (Marshall and Jaggers 2000), Reiter and Tillman (2002). *Notes*: The Polity score refers to the Polity democracy-autocracy combination score for 1992. This variable ranges from 10 (strongly democratic) to –10 (strongly autocratic). The Polity database does not include data for the Bahamas and Malta.

Dependent Variable

As the dependent variable, I use the initiation of a Militarized Interstate Dispute (MID). This data is derived from Reiter and Tillman (2002), made available by those authors, and based on the Militarized Interstate Dispute Data set (Jones, Bremer, and Singer 1996). The MID data codes all international disputes ranging in hostility from 1 (no militarized action) to 5 (war). The MID data also codes the originator of the dispute (that is, the initiator of the conflict). For this research design, I employ two measures of the democratic initiation of conflict. Both are dichotomous, and the first, which I refer to as "initiate any dispute," is coded 1 if the democracy

participates in any MID (ranging from 1 to 5 on the MID level of hostility) that it initiated during the year in question; the variable is coded 0 otherwise.

Table 4.2. Conflict Initiation by Twenty-nine Democracies

Country	*Initiate any Dispute*	*Country*	*Initiate Forceful Dispute*
United States	41	Israel	30
India	36	United States	25
Israel	34	India	24
France	22	France	15
United Kingdom	22	United Kingdom	12
Portugal	15	Australia	10
Australia	10	Portugal	10
Iceland	9	Iceland	9
Greece	8	New Zealand	8
New Zealand	8	Greece	5
Norway	7	Canada	4
Canada	6	Norway	4
Spain	5	Spain	3
Belgium	4	Germany	2
Italy	4	Ireland	2
Germany	3	Italy	2
Ireland	2	Belgium	1
Netherlands	2	Botswana	1
Austria	1	Denmark	1
Botswana	1	Finland	1
Denmark	1	Jamaica	1
Finland	1	Austria	0
Jamaica	1	Bahamas	0
Japan	1	Japan	0
Switzerland	1	Luxembourg	0
Bahamas	0	Malta	0
Luxembourg	0	Netherlands	0
Malta	0	Sweden	0
Sweden	0	Switzerland	0

Sources: Jones, Bremer, and Singer (1996), Reiter and Tillman (2002).

For robustness, I also include a second dependent variable, one that captures potentially high-salience disputes that escalate to the use of military force. This variable, which I refer to as "initiate forceful dispute," is coded 1 for any MID that reaches the threshold of 4 or 5 in the MID

data (the "use of force" or "war"), 0 otherwise. This second variable thus excludes instances of hostility "with no militarized action," the "threat to use force," and the "display of force" (coded 1, 2, and 3, respectively, in the MID data set). Table 4.2 describes the dependent variables used in this dissertation, listing each country in order of the most to least belligerent on each variable. The United States initiated the most disputes of any type over the period 1945-1992 (with 41 instances of disputes initiated); Israel initiated the most forceful disputes (30 instances).

Including a higher threshold for the level of dispute hostility in the second dependent variable has the advantage of testing highly salient, or politically costly, foreign policy decisions. Similar to the democratic peace literature, the argument presented here holds that democracies are constrained from going to war in part because the citizens who bear its costs will oppose its prosecution. It stands to reason, then, the more serious the level of hostility, the more democratic citizens should oppose it, and the more constrained a democracy will be. As Ireland and Gartner (2001) argue, democratic institutions are less likely to exert a constraining force of low-salience disputes, more likely in high salience ones. In this manner, high salience international disputes are more likely to be affected by democratic institutional context.

Measuring the dependent variable as "conflict initiation" rather than "conflict participation" or "conflict reciprocation" is the most appropriate way to capture how democratic institutional context affects state conflict behavior. The entire premise of the liberal democratic peace, as well as this dissertation, is that certain democratic institutions, and the political context of those institutions, constrains the use of force as a foreign policy tool. It is in the *initiation* of conflict that we might expect to see variance based on institutional constraints (rather than, say, retaliation or reciprocation of conflict, in which case there is no reason to expect one democracy to defend itself differently than another). Moreover, as Reiter (1995) has demonstrated, states

seldom attack other states for defensive purposes. Conflict initiation, then, is a good measure to capture a democracy's decision to become involved in international conflicts.

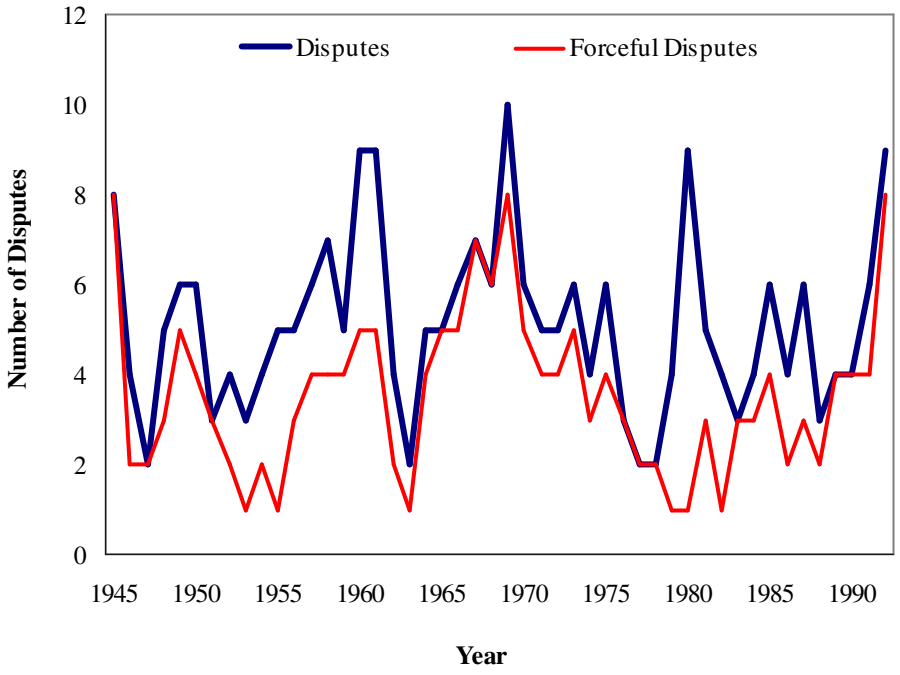

Figure 4.1. Conflict Initiation by Democracies, 1945-1992

Before moving on to the independent variables, Figure 4.1 illustrates the variance in the dependent variables over the period of study (1945-1992). As the graph suggests, there appears to be no discernable trend in the number of conflicts initiated by democracies over time. In fact, there is no statistically significant relationship between year and number of disputes, indicating that the dependent variables are not time dependent (i.e. conflict was no more or less likely during or after the Cold War).[1] There are, however, distinct high and low points of the number of disputes. The years 1969 (with 10 overall disputes and 8 forceful disputes initiated by

[1] The correlation coefficient between year and "any dispute initiated" is negative, but not significant (Pearson's r = -.06); the correlation coefficient between year and "forceful dispute initiated" is also negative, but not significant (Pearson's r = -.04).

democracies) and 1992 (with 9 overall disputes, 8 of them forceful) appear to be particularly belligerent. By contrast, 1963 (with 2 disputes total, 1 forceful), 1977 (2 disputes, both forceful), and 1978 (also 2 forceful disputes) were comparatively pacific.

Executive Institutional Context

The nature of the ruling government coalition constitutes the first dimension on which to measure institutional context. Governments can consist of single party majority, multi-party coalition majority, or minority cabinets. Of the three types of cabinets, there is no question that minority governments are the most heavily constrained of all governments in the use of force. According to veto player theory, minority governments face far more veto players than all other types of government, resulting in a heavily constrained executive. This is the same expectation as my argument centering on institutional context: minority governments exist in the most competitive institutional context possible (an institutional setting defined by executive control without majority support), and therefore should be the least likely to initiate international conflict.

Of the remaining two types of cabinets, an argument revolving around institutional context arrives at a different hypothesis than the one advanced by the convention wisdom. According to veto player theory, majority governments face the fewest decisional constraints and should thus be the least constrained: with single party majority governments, parliamentary and executive rule are the same (Tsebelis 1995, Ireland and Gartner 2001). Coalition governments, on the other hand, are more constrained: more coalition partners within a cabinet create more veto players that hinder the policy process.

I have argued, however, that majority cabinets should be associated with a more competitive institution context, and should thus be more constrained than coalition cabinets.

Unlike coalition governments that hold a large share of parliamentary votes and whose members act collectively, majority governments command much less legislative support and typically face more hostile and stronger oppositions. On the other hand, ruling coalitions that are comprised of broad, multi-party cabinets exhibit fewer decisional or electoral constraints on the initiation of conflict. With coalition governments, we should thus expect a collective institutional context; with majority governments, we should expect a more competitive—and more constrained—institutional context. Therefore, the likelihood of conflict initiation is highest for coalition cabinets, lowest for minority cabinets, and somewhere in between for majority cabinets:

> *Hypothesis 1:* Minority governments are the least likely of all types of government to initiate a conflict.

> *Hypothesis 2:* Coalition governments are more likely to initiate conflict than majority governments.

I use two measures to test these hypotheses. First, I code each of the three types of cabinets as a dummy variable. Data for cabinet type is derived from Reiter and Tillman (2002), Müller and Strøm (2000), and Woldendorp et al. (2000). Second, since the hypotheses predict an increasingly constrained executive as we move from coalition cabinets to minority cabinets, I also construct an index of *Cabinet Context*. This variable simply recodes the three separate dummy variables into one continuous measure that ranges from 0 to 2 and captures cabinet context on a single dimension. It is coded 0 if the cabinet is a minority one, 1 if a majority one,

and 2 if a coalition one. As this variable increases, cabinet context is more collective, and the likelihood of conflict initiation increases.

For veto player theory, my hypotheses regarding cabinet structure and conflict initiation are counterintuitive. According to Tsebelis, cabinets with multiple parties have more veto players (what he terms partisan veto players). As such, multiple parties in the executive cabinet have a constraining effect on executive decision-making. As the number of parties increases in the cabinet, the executive is more constrained as there are more veto players in the policy process. According to this dissertation, however, the number of government parties should have the opposite effect.

Multiple parties in government produces incentives for collective agency where logrolling and shared political responsibility are the norm. Moreover, more government parties in the executive reduce the political accountability of each party. For any given policy, the electorate is unable to assign blame (or credit) to any single party for any specific policy. Therefore, as the number of government parties increases, incentives for collective agency within coalition governments increase and electoral accountability for belligerent foreign policies decreases. This, in turn, reduces the constraints that executives face in initiating any conflict. We can thus specify another hypothesis, and propose a third variable, to capture the effect of executive institutional context and conflict initiation:

> *Hypothesis 3:* As the number of government parties increases, the likelihood of conflict initiation also increases.

Measuring the number of government parties is perhaps the most direct method to test the competing claims regarding the constraints facing the executive in its use of force. For veto player theory, as government parties become more numerous, more veto players constrain the initiation of conflict. According to this dissertation, however, as the number of parties in government increases, the setting in which those parties operate becomes more collective, and the executive is less constrained in initiating conflict. To measure *Government Parties*, I include the number of parties in the executive cabinet for each country-year. Data for this variable is derived from Müller and Strøm (2000) and Woldendorp et al. (2000).

Executive-Legislative Institutional Context

The nature of the partisan opposition in the legislature defines the second dimension of institutional context discussed in this dissertation. When political opposition in the legislature is strong, it serves as a constraint on executive decision making in that it contributes to a competitive institution context. A sizable partisan opposition offers viable policy alternatives where even minor shifts in parliamentary or electoral support can give the opposition control of the government. Given this, there are considerable incentives for strong political oppositions to act competitively for there are electoral or parliamentary advantages in doing so. By contrast, weak partisan opposition indicates a collective institutional context. In this situation, there may be little incentive or opportunity for small, weak opposition parties to oppose government policies. Offering no electorally viable policy alternatives, small parties have stronger incentives to support government policies as a way to curry favor with the coalition in power.

Hypothesis 4: Strong legislative opposition to the executive represents a competitive institutional context and should constrain democracies from initiating conflict. Conversely, weak legislative opposition to the executive represents a collective institutional context and should have the opposite effect.

To measure legislative opposition, I use two variables. The first, *Opposition Size*, is simply the total size (measured as a percentage) of all opposition parties (parties not currently holding any seats in the executive). The second variable, *Opposition Bloc*, measures the percentage of legislative seats held by the major opposition bloc. An opposition bloc is defined as the largest party or bloc of parties not holding seats in the executive cabinet.[2] This measure is perhaps more theoretically accurate in that it captures only oppositions that are coherent, and ideologically similar, enough to offer platforms or positions that oppose the executive cabinet. This is an important distinction in that a legislature with one large opposition party (or bloc of parties) is a much larger constraint than a legislature that is highly fragmented and unable to present a clear opposition message. Data for these variables are derived from Müller and Strøm (2000), Woldendorp et al. (2000), and Prins and Sprecher (1999).

My hypothesis regarding the constraining effect of the major legislative opposition bloc differs markedly from that offered by Tsebelis's veto player theory. In terms of partisan veto players, he argues that only in *presidential* systems does a strong legislative opposition bloc constrain an executive (i.e. in the case of divided government). In parliamentary systems, with

[2] Multiple opposition parties constitute a bloc if they occupy similar ideological locations on the left-right political spectrum as coded by Müller and Strøm (2000). So long as their ideological locations are proximate enough to form a coalition, or if multiple parties had previously formed a coalition, they were coded together as an opposition bloc. For instance, the German SDP (Social Democratic Party) and FDP (Free Democratic Party), which are placed adjacent to one another according to Müller and Strøm's ideological codings and formed multiple coalitions between 1969 and 1976, constitute an opposition bloc when both parties are excluded from the cabinet.

parliamentary fusion between a legislative majority and the party (or coalition) in executive power, Tsebelis denies that a partisan opposition bloc has a constraining effect (in his terms, it is neither a partisan nor institutional veto player). Legislative opposition in parliamentary systems, in this formulation, is *excluded* from the policy making process and so does not constrain the executive initiation of conflict. This, I have argued, is puzzling because legislative opposition blocs can constrain governments in that they can offer alternative policies should the party (or coalition) in power have failing or otherwise deficient policies. In addition to the constraining effect of strong legislative oppositions in presidential systems, then, legislative opposition blocs in parliamentary systems also constrain the initiation of conflict.

Given these divergent expectations of traditional veto player theory and institutional context, there is sufficient theoretical reasoning to inspect separately the effect of a strong legislative opposition bloc in presidential versus parliamentary systems. That is, since I expect *parliamentary* opposition to have a different effect than that hypothesized by Tsebelis, I should also test this relationship separately:

> *Hypothesis 5:* A strong legislative opposition bloc in parliamentary systems should constrain democracies from initiating conflict.

In addition to the theoretical reasons to treat parliamentary opposition separately, there are empirical reasons for doing so as well. Descriptive statistics indicate a considerable difference between the composition of parliamentary and presidential oppositions. In parliamentary systems, the mean size of the opposition bloc is 37% of all parliamentary seats (with a median opposition bloc size of 40% of legislative seats). This is significantly lower than

in presidential systems where the mean size of the opposition bloc is 51% of all legislative seats (with a median opposition bloc size of 56% of legislative seats).

The statistics regarding opposition in parliamentary systems reflect the characteristics that we might expect in multi-party systems. Multiple parties are common in parliamentary systems, most of which employ a proportional representation (PR) electoral system.[3] When there are more parties in the legislature, the proportion of seats held by each party decreases. On the other hand, the statistics regarding presidential systems (of which the U.S. is the only case) reflect characteristics of a two-party system (as a result of plurality voting in single member districts). When only two parties dominate the legislature, the proportion of seats held by the opposition (which is the party not currently occupying the executive) is relatively large. By contrast, when there are multiple parties in the legislature (as is the case in parliamentary systems), the opposition bloc tends to be smaller. By testing the interactive effect of legislative opposition blocs in parliamentary systems, I should thus be able to clearly demonstrate the divergent theoretical expectations of my hypotheses with Tsebelis's theory while also accommodating the empirical differences between parliamentary and presidential systems.

Institutional Organization

Recall that there are two competing perspectives concerning macro-institutional organization and international conflict. One perspective holds that presidential systems are more constrained than parliamentary systems due to additional institutional vetoes in the former. As Strøm (2000) argues, presidential systems exhibit competing checks, or veto players, between rival branches

[3] The number of parties is directly related to its electoral system. As Duverger's law (1964) states, the plurality (or majority) method of voting favors a two party system, where PR systems encourage the formation of multiple parties.

that should constrain an executive's use of force. By contrast, parliamentary systems create a chain of power that flows from the legislative branch to the executive, thus removing institutional checks or vetoes. Yet another perspective holds that presidential systems are less constrained due to the largely unchecked executive policy dominance in foreign affairs, including that office's use of force. Executives in parliamentary systems, under this perspective, are more dependent on the legislature, and thus more constrained, than presidential systems (Maoz and Russett 1993). This leads to the following competing hypotheses:

Hypothesis 6A: Presidential systems will be more likely to initiate conflict due to executive policy dominance in areas of foreign affairs.

Hypothesis 6B: Parliamentary systems will be more likely to initiate conflict because the executive is not constrained by an institutionally independent legislature.

While no consensus has been reached over which perspective is more accurate, I have argued that relying on such a static conceptualization of democratic institutions is misleading. Reference to only institutional organization can not account for variation in conflict behavior among democracies; only by considering the institutional *context* can we distinguish such variations. Presidential systems are not necessarily more or less constrained than parliamentary systems. Rather, the constraining effect of the institutional structures in a presidential system is mediated by institutional *context* in which institutionally determined veto players do not always constrain the use of force. Nevertheless, I control for institutional organization by including a series of dummy variables for the macro-institutional structure of each democracy (Reiter and

Tillman 2002). While I expect that institutional context will have the same effect in each type of democratic system (whether parliamentary, presidential, or semi-presidential), the effect of institutional context may be masked by the type of macro-institutional structure such as the aforementioned distinction between partisan opposition in parliamentary systems versus other types of systems.

The dummy variables in this research design include entries for Presidential systems (coded 1 for presidential systems, 0 otherwise), and Parliamentary systems (coded 1 for parliamentary systems, 0 otherwise). The excluded category is semi-presidential systems which includes premier-presidential systems (systems with an elected president and a parliamentary cabinet headed by a prime minister) and assembly-independent systems of which Switzerland is the only case (this system exhibits a strong legislative chamber, but without the power to remove the executive).

Treaty Ratification Power

The preceding hypotheses regarding macro-institutional structure and conflict are formed around the logic that some types of systems are more constrained than others in implementing a public policy. While these constraints may apply to a variety of policy domains, they may fail to capture more specific foreign policy constraints. Reiter and Tillman (2002) argue that democratic executives, irrespective of macro-institutional structure, are further constrained by specific foreign policy powers allocated to legislatures. Many constitutions, for instance, make special provisions regarding the conduct of foreign affairs that do not exist for the conduct of domestic policy making. The most obvious example is the U.S. constitutional requirement that the Senate must approve formal treaties made by the President. According to Reiter and Tillman (2002:

816), "such legislative involvement in treaty ratification is likely to extend to other areas of foreign policy." This is certainly relevant to the balance of executive-legislative relations as it pertains to conflict initiation, the central subject of this dissertation.

Given that the premise of this study is that the executive is the principal foreign policy actor, and that it is constrained or enabled by differing institutional contexts, the allocation of more foreign policy authority to the legislative branch may produce a more competitive institutional context. That is, *ceteris paribus*, a more independent legislative chamber may produce a more competitive institutional setting. While the independence of a legislative chamber in regard to foreign policy should be mediated by the context between branches of government, inclusion of the foreign policy power of the legislature is a relevant control variable that warrants inclusion.

Hypothesis 7: As legislative power regarding foreign policy increases, the likelihood of conflict initiation decreases.

To measure legislative power over foreign affairs, I follow Reiter and Tillman (2002) by including a variable describing the legislature's control over treaty ratification, labeled *Treaty Ratification* in the following analysis. This variable is coded 2 if legislative approval is always required, 1 if it is conditionally required, and 0 if no legislative approval is required. While this variable does not measure directly the legislature's control over the use of force, it does capture the general balance of power between the executive and legislature over foreign policy (Reiter and Tillman 2002). When the legislature's approval is always required for international treaties, legislative-executive power is more balanced, creating a competitive institution context. By

contrast, when the legislature's approval is not required for international treaties, the balance of power shifts toward the executive; this creates a less competitive institutional context.

Electoral Participation

The democratic peace literature maintains that public consent is a powerful check on democratic war making; since the public bears the costs of war, it would naturally disfavor war or punish democratic leaders who initiated international conflicts. One of the central tenets of the democratic peace argument is that citizens of a democracy will seldom choose to engage in, or otherwise support, warfare because it is they who incur the costs of war. In *Perpetual Peace*, Kant (1983[1795]: 100) himself explains his reasoning:

> The reason is this: if the consent of the citizens is required in order to decide that war should be declared . . . nothing is more natural than that they would be very cautious in commencing such a poor game, decreeing for themselves all the calamities of war [such as] having to fight, having to pay the costs of war, having painfully to repair the devastation war leaves behind, and . . . load[ing] themselves with a heavy national debt.

Given that citizens hold democratic governments accountable through elections, and given that citizens should disfavor war, there is a significant electoral dis-incentive for democratic executives to pursue a war for which the citizens do not support. It follows that democracies supported with high levels of political participation should increase the number of citizens who do not support war and who may wish to remove an executive from office following a decision to go to war. The effect of this dynamic is that greater participation increases the accountability of democratic executives, and thus constrains democratic belligerence.

Several recent empirical findings support this claim. Bueno de Mesquita et al. (1999) find that an executive's electoral coalition is generally large in a democracy meaning that a large proportion of the population severely constrains an executive's use of force. It follows that the larger the winning coalition, the more heavily constrained an executive for he faces more demands or potential "vetoes." Reiter and Tillman (2002) similarly argue that the broader political participation, the less likely a democracy would use force as a foreign policy tool. They find that because elections are the most powerful constraint citizens can place on an executive who may initiate international conflict, electoral systems that allow for greater participation should be more constrained. I thus expect that electoral participation will be negatively related to conflict initiation:

> *Hypothesis 8:* Higher electoral participation will reduce the likelihood of conflict initiation by democracies.

To control for this argument, I include electoral turnout in the most recent national election. Reiter and Tillman (2002) provide data for this variable which is measured as the percentage of a country's total population that voted. It ranges from 0 (no participation) to 100 (full participation).

Polity

Democratic peace theorists maintain that democracy has a pacifying effect on international relations. As discussed previously, this phenomenon is usually expressed in dyadic terms: democracies seldom, if ever, go to war with one another. Following Kant, scholars cite two

mechanisms that support such a peace: the institutional constraints of democracy that prevent leaders from choosing war as a foreign policy tool, and shared democratic norms of peaceful conflict resolution.

As I outlined earlier in this dissertation, however, more recent theoretical and empirical research has updated liberal thought on the effect of democracy and conflict: the democratic peace effect is found not only between democratic dyads, but extends monadically as well. Particularly in relation to the structural mechanisms cited by democratic peace scholars, there is reason to expect that the institutional constraints that prevent democracies from going to war with one another operate irrespective of a potential adversary's domestic governing structure. That is, the structural constraints cited by democratic peace theorists (the institutional separation of powers and the democratic requisite of the consent of the governed) should constrain democratic leaders from initiating any conflict.

The most common method scholars of international relations use to measure democracy is the Polity democracy-autonomy score, an index that captures how "democratic" a state is. On one end of the continuum, "strongly democratic" states are characterized by competitive political participation, open and competitive executive recruitment, and institutionally constrained executive powers. On the other end of the continuum, "strongly autocratic" states are characterized by regulated political participation, exclusive executive recruitment (usually by political elites), and institutionally unconstrained executive power (Marshall and Jaggers 2000). Following the literature on the democratic peace, democratic states should be less likely than autocratic states to initiating conflict for they face far greater institutional constraints to commit to such a policy.

Hypothesis 9: "More democratic" states are less likely to initiate conflict than "less democratic" states.

It is the tenet of this dissertation that the democratic peace is in fact monadic, but not as conceptualized by the conventional wisdom. Specifically, I have argued previous studies of the effect of democracy on international behavior is limited by utilizing simple dichotomous (democracy or non-democracy) or uni-dimensional (ranging from more to less democratic) measures of democracy. For comparative purposes, though, I include the Polity democracy-autocracy combination score (Marshall and Jaggers 2000). This variable, labeled *Polity*, ranges from 10 (strongly democratic) to –10 (strongly autocratic). Table 4.1 presents the Polity score for each country for which data is available in the last year of observation, 1992. Within this data set, this variable demonstrates only minimal variance. In 1992, for instance, twenty-three of the twenty-nine cases for which Polity codes data have a score of 10 (strongly democratic). Only one democracy, India, scores below 9 (with a score of 8). This variance is limited by the nature of the data set which measures democracy only along a single dimension (degree of democracy), ignoring the rich variety of democratic institutions.

The Polity index has obvious limitations, particularly in regard to differentiating democracies. Such a uni-dimensional approach assumes that democracies are largely the same when, in fact, institutions within different democracies varies widely. This is an especially important point for this dissertation in which the universe of cases are all democracies. The Polity data, for instance, registers equal "democratic" scores for the United States and most Western European countries (Marshall and Jaggers 2000). Disaggregating the constituent

elements of democracy based on institutional context, however, should improve our understanding of how domestic political institutions affect the war proneness of democracies.

Major Power

While this dissertation has taken a distinctly liberal perspective (arguing that domestic politics shapes international relations), I also take into account realist factors that may influence the initiation of conflict by democracies. Most, if not all, liberal scholarship recognizes that both liberal *and* realist perspectives matter. As Oneal and Russett (1999b: 3), two prominent liberal scholars, note: "both [realist and liberal perspectives] consider conflict and the threat of violence to be inherent in an anarchic world of sovereign states. The Hobbesian element of this understanding is central to realist theory, but it is also deeply embedded in the liberal tradition." That is, liberal scholarship, of which this dissertation is an example, takes seriously the notion that anarchy, or the lack of a sovereign above the level of the nation-state, accurately describes the social milieu of international relations. Moreover, agreeing with realists, liberals acknowledge that power matters in defining relations between states.

In this research design, I therefore assume that the international system is anarchic and power is important, and I cite those variables that realists (and most liberal scholars) include in their analysis of international conflict. The first such control that I include in this analysis is whether or not each country is a major power. A chief component of realist thought is that the relative power of a state determines its likelihood of involvement in international conflict. Because of extensive international interests, major powers may be expected to be more involved in international disputes (Reiter and Tillman 2002). The reasoning for this, as Geller (2000: 409) notes, is because "major powers tend to define their interests more broadly than do minor

powers, and the pursuit of those interests may bring them more frequently into violent conflict with other states."

Typically, major power states are defined by their relative power capabilities, especially in military terms (although most international relations scholarship also treats wealth, industrial capacity, and population as elements of power). Realists have long considered national military capabilities to be positively correlated with rates of conflict. Accordingly, when states consider international conflicts as a tool of statecraft, they often take into account power and capability of themselves and potential adversaries. When power capabilities are high, militarized conflict is more likely. This relationship has been demonstrated extensively in the quantitative literature. Beginning with Wright (1964), and continuing with the more comprehensive and recent findings of Small and Singer (1970, 1982), Bremer (1980), and Geller (1988), international relations scholars have found consistent and robust relationships between power status (measured in military, industrial, and demographic terms) and increased war proneness (including the frequency and severity of conflicts).

Major powers, which have extensive international commitments and the greatest power capabilities, should thus be expected to be involved in more international conflicts. Moreover, in terms of the dependent variable of this research design, democracies with major power status should be more likely to initiate conflict for they not only have the power resources to define the international system, but have the interest to do so. This relationship is reflected in the following hypothesis for this dissertation:

> *Hypothesis 10:* Democracies with major power status should be more likely to initiate conflict than other democracies.

Using the Correlates of War data, this variable, labeled *Major Power,* is coded 1 if the state is a major power at the time of the observation, 0 otherwise (Ghosn and Palmer 2003, Jones et al. 1996).[4] Within this data set, the United States, United Kingdom, France, Germany (1990-1992), and Japan (1990-1992) are coded as major powers.

Autocratic Borders

The next variable including in this research design captures the "realist" argument that proximity breeds hostility. Huth (1996), for instance, points out that international conflicts commonly arise from territorial disputes between proximate states. Following Huth, a state may be more likely to initiate a conflict when bordered by a greater number of states where opportunities for disputes over issues of territory are more likely to arise. There are two arguments within the international relations literature supporting this claim. First, as Geller (2000: 413) notes, proximity simply gives states more opportunity for conflict: "wars occur between bordering or proximate states because short distances provide the opportunity for violent conflict (i.e. proximity decreases the requirement of military reach)." Research by Richardson (1960) supports this "geographic opportunity" thesis. He reports that states engage in conflict in proportion to their number of borders. Likewise, Weede (1970) notes a positive relationship between a state's number of borders and its involvement in international conflict.

[4] To capture power status, I also constructed a summary index of military capabilities. This index is based on the Correlates of War data for military personnel and expenditures (Singer et al. 1972, Singer 1987). Because power status and military capabilities measure to a large extent the same phenomena (the power of a state), this index of military capabilities is highly correlated with major power status (Pearson's $r = .62$, $p<.01$). Inclusion of this index of military capabilities instead of (or in conjunction with) major power status in various (and unreported) model specifications did not significantly alter the results of the statistical analysis. Since the variable *Major Power* is more theoretically accurate than "military capabilities" in that it captures economic and demographic sources of power, for reasons of parsimony I have chosen to include only major power status in this research design.

The second argument concerning shared borders and war is more complex: proximity of states structures the security environment, or "context of interaction," between them (Geller 2000: 413), so that bordering states have more motivation to go to war. Starr and Most (1976, 1978, 1983), for instance, argue that proximate states view each other as more threatening than distant states. The context of interaction between them, then, is one that is more likely to lead to conflict. They present evidence in their studies that states with more numerous borders are involved in more conflicts. Similarly, Midlarsky (1975) finds that leaders of states with more borders have a greater degree of uncertainty of their neighbors which, in turn, increases the likelihood of war. This is consistent with the more general realist hypothesis that uncertainty, which increases with more great powers in the international system, leads to a higher incidence of war (Waltz 1979, Mearshimer 2001). In fact, Midlarsky (1975) goes on to find that the vast majority of wars have occurred between contiguous states (that is, states sharing land borders). More recently, Siverson and Starr (1990) suggest that the neighboring security environment alters a state's context of interaction. They conclude that wars tend to diffuse across borders: the likelihood of a state entering an ongoing war increases when that war is occurring along its borders.

But since democracies are particularly unlikely to fight (as Russett 1993 and other democratic peace scholars have persuasively argued), and as no two democracies under consideration here have come into conflict with each other, I follow Reiter and Tillman (2002) in including only the number of autocratic states bordering democracies. Any democracy, then, should have a higher likelihood of initiating a conflict when the number of bordering autocracies is high. This relationship is reflected in the following hypothesis for this research design:

Hypothesis 11: The greater the number of autocratic states bordering each democracy, the higher the likelihood of its initiating any conflict.

Data for this variable, labeled *Autocratic Borders* in the following analysis, is derived from Reiter and Tillman (2002), and it is coded as a count of the number of autocratic states bordering each democracy. Within this research design, India (6), Greece (4), and Israel (4) have the greatest number of autocratic borders; New Zealand, Australia, Japan, and the North American and most European cases have none.

Alliances

Scholars of international relations agree that states enter alliances in order to increase their own capabilities through external military ties (Midlarsky 2003). In fact, it is a mainstay of international security studies (particularly within realism) that states, which are primarily concerned with maintaining security as a means toward survival in an anarchic international systems, are best secured through building up their power (military, economic, or otherwise). To that end, states can either build power internally (by increasing the size of their military, economic production, and the like), or externally (through building alliances) (Waltz 1979, Mearshimer 2001). Moreover, realists such as Waltz and Mearshimer have long considered national military capabilities to be positively correlated with rates of conflict. Accordingly, when states consider international conflicts as a tool of statecraft, they often take into account power and capability of themselves and potential adversaries. Given that states enter alliances to increase their power capabilities, and that conflict is more likely when capabilities are high, militarized conflict is more likely when a state has more alliance ties.

Following the logic already presented regarding power status and military capabilities, alliances may precipitate conflict by causing, or exacerbating, arms races, conflict spirals, or military balancing (Midlarsky 2003, Geller 2003). Alliances may also increase the likelihood of war by dragging alliance members into conflicts as a third party. In the empirical literature, Singer and Small (1966) find that more numerous alliance commitments lead to greater and more intense conflict involvement. Similarly, Siverson and Sullivan (1984) have demonstrated that alliance ties have a positive and significant relationship with war involvement. Levy (1981) also reports that in the twentieth century, a vast majority of major powers committed to war following alliance formation.

All things being equal (including controlling for a state's power status), states with more alliance ties should be strengthened by the multilateral balancing provided by alliances with other states, and thus should be more likely to initiate conflict. Conversely, states with fewer alliance ties are relatively weaker militarily than states with greater alliance ties, and thus should be less likely to initiate conflict. This discussion leads to the following hypothesis for this research design:

> *Hypothesis 12:* The greater the number of defensive alliances to which a democracy is a member, the higher the likelihood of its initiation of conflict.

To measure the number of a state's alliance ties, I use the total number of formal defensive alliances per country-year, labeled *Alliances* in the statistical analysis. Data for this variable is derived from the Correlates of War Formal Alliance data set (version 3.03), available through the Correlates of War data project (Singer and Small 1966, Small and Singer 1969,

Gibler and Sarkees 2002). This variable measures only the number of defensive alliances (termed "defensive pacts" by the COW data set) during the year of observation. Such formal defensive alliances constitute the highest level of military commitment, requiring that signatories come to each other's aid if attacked by a third party (such as specified in the NATO charter). I exclude neutrality and non-aggression pacts (a lower level of commitment stipulating that signatories remain neutral, or otherwise not support the use of force, during times of crisis) and ententes (the lowest form of commitment requiring only consultation in times of crisis). While many cases within this data set have no alliance ties (37% of the cases have a value of "0" for this variable), the United States (with as many as 7 alliance ties from 1958-1979), the United Kingdom, and France have the most numerous alliance ties.

International Organizations

In addition to the aforementioned "realist" control variables, liberal scholarship in international relations cites yet another constellation of factors that affects the war proneness of states. The first such factor is membership in international organizations. According to Bruce Russett and his colleagues (Russett 1993; Russett, Oneal, and 1998; Russett and Starr 2000; Oneal and Russett. 1999b) international organizations have a pacifying effect on interstate relations. Specifically, this strand of liberal scholarship posits that, together with democracy and economic interdependence, international organizations are part of a "Kantian tripod for peace" (Russett et al. 1998b: 441). Founding this thesis in the writing of the republican liberal Immanuel Kant, this scholarship notes that peace between states results most likely from overlapping and multiple sources, and international organizations are an important component in reducing international conflict.

The triad of factors that promote peace, Russett and Starr (2000: 116-117, quoting Kant) note, is based on Kant's prescriptions for a "perpetual peace":

> (1) "Republican constitutions" (representative democracy) would constrain autocratic caprice in waging war, (2) "cosmopolitan law" (economic interdependence) would reinforce structural constraints and liberal norms by creating transnational ties that encourage accommodation rather than conflict, and (3) international law and "federations" of independent states (international organizations) . . . would provide the moral and legal edifice for peaceful conflict resolution.

Even while most scholars cite only the pacific benefits of democracy, all three factors, according to Kant, contribute to peace. Woodrow Wilson understood this Kantian insight when he advocated in his Fourteen Points at the close of World War I that peace between nations is best maintained by an international system comprised of democratic states who operate through international organizations. Such international bodies, so the argument goes, provide the security that states require and prevent them from seeking security through otherwise dangerous mechanisms such as balance of power politics.

In large measure, international organizations operate much as democratic institutions do domestically. Within states, laws, rules, norms, and other procedures (such as legislatures, courts, and the like) facilitate peaceful conflict resolution through law and established legal institutions among the citizenry. Democratic leaders, accustomed to such norms of conflict resolution, tend to use international organizations similarly. International organizations, then, fill the same function among states as they do within states, particularly among democracies (Russett and Starr 2000). Obviously, the effects of international organizations on interstate relations are intertwined with the mechanisms of democracy. That is, since democracies are accustomed to

utilizing domestic institutions for conflict resolution and political management, it is they who also seek out international organizations to conduct international relations. Moreover, democracies enter many such organizations specifically for the economic benefits derived from interdependence (consider GATT, NAFTA, or the WTO), thus further conflating the individual effects of democracy, economic interdependence, and membership in international organizations. As such, Russett et al. (1998b) emphasize that in order to untangle, or at least trace, these potentially interactive effects of mutually supportive phenomena, researches must accommodate all three elements of the "Kantian Tripod for Peace."

While the effect of international organizations on interstate relations follows logically the process of similar organizations in domestic relations, Russett et al. (1998b) also cite other processes that emanate from membership in international organizations that may inhibit conflict among states. Intergovernmental organizations, they argue, reduce the likelihood of conflict for they coerce norm-breakers, mediate among conflicting parties, reduce uncertainty by conveying information, problem solve, socialize and shape international norms, and generate mutual identification among states. Moreover, according to Russett et al., the pacific benefits of international organizations (the most significant of which are intergovernmental organizations, or IGOs) increase for states as the "network" of IGOs becomes denser. That is, the more IGOs of which a state is a member, the less likely that state will rely on conflict as a means of statecraft. While Russett et al. base their analysis on dyadic state relations (that is, joint dyadic membership in IGOs), the relationship should also hold in the monadic setting: as a state's membership in IGOs becomes more numerous, nonviolent conflict resolution should become more frequent. This discussion leads to the following hypothesis for this research design:

Hypothesis 13: The denser the network of Intergovernmental Organization (IGO) membership, the less likely a democracy will be to initiate conflict.

To measure the density of IGO membership, I use the number of IGO memberships per country-year, labeled *IGOs* in the following analysis. This data is derived from the Correlates of War (COW) IGO data set (version 2.0) compiled by Pevehouse et al. (2003), made available by those authors and accessible through the Correlates of War data project. This data set catalogs the world's formal international organizations and includes those organization that are established by governments and "consist of at least three members of the COW-defined state system", "hold regular plenary session at least one every ten years," and "possess a permanent secretariat and corresponding headquarters" (Pevehouse et al. 2003:1). This data set includes only organizations formed by sovereign states, thus excluding nongovernmental organizations, or NGOs, as well as "ad hoc" conferences or other organizations with no permanent bureaucratic structure. It also includes only traditional IGOs—those IGOs formed by treaty or executive agreement—thus excluding "emanations," which, while far more numerous than traditional IGOs, are theoretically distinct in that they are not formed by sovereign states. Since the COW IGO data set also includes formal defensive alliances, and since defensive alliances are hypothesized to have a different effect on conflict initiation, I have subtracted the number of defensive alliances from each entry for this variable. Within this data set, France (with 117 IGO memberships in 1992), the Netherlands (105 memberships in 1992), Germany (103 memberships in 1992), and Belgium (103 memberships in 1992) have the densest networks of IGOs.

Economic Interdependence

Another system attribute that may contribute to war or peace is the level of economic interdependence of states, one of the three "legs" of the "Kantian Tripod" (Russett et al. 1998b). In addition to the pacifying effects of democracy, one of the mainstays of liberal scholarship is that interdependence reduces conflict. While interdependence may take several forms, the most common and theoretically distinct is the argument concerning economic interdependence (typically measured as international trade as a percent of GDP). In this economic conceptualization, liberals (such as Kant 1983, Deutsch et al. 1957, Keohane 1984, and Keohane and Nye 2001) tend to stress that the costs associated with conflict are too high in an interdependent system, so states are economically constrained from conflict. As states rely on each other economically, and profit handsomely from interdependence, wars become too costly and incentives to maintain peaceful interstate relations too strong. As Rosecrance (1986) eloquently argues, trading is better than invading. Moreover, for sociological liberals such as Karl Deutsch, interdependence creates transnational social ties that create a security community where war is "subrationally unthinkable." Some of the most recent liberal scholarship advocating the pacifying effects of interdependence include Domke (1988), Kim (1998), Mansfield (1994), Oneal and Ray (1997), Oneal and Russett (1997a, 1997b, 1999), Oneal et al. (1996), Reuveny and Kang (1996), and Russett et al. (1998). These studies present persuasive empirical findings that trade promotes peace.

That economic interdependence causes peace is, however, still a contentious debate between liberals and realists. Realist arguments, by contrast, posit either no relation or a positive relation between interdependence and conflict. Accordingly, increased interdependence causes asymmetrical dependence and increased opportunities for contact among competing interests,

thus increasing the likelihood of conflict. As Waltz (1979) and Mearshimer (2001) remind us, economic interdependence makes states more vulnerable to other states (especially if states rely on other states for war making goods such as oil and steel). Vulnerability, in turn, generates security competition, which causes security dilemmas that may spiral into war. Alternatively, interdependence may have no effect on conflict because interdependence is an economic, not military or security, issue. Realists who have advocated that trade has a negligible to positive impact on interstate conflict include Barbieri (1996a), Betts (1993/94), Buzan (1984), Gilpin (1977), Hoffman (1963), Holsti (1986), Levy (1989), and Waltz (1979). Barbieri (1996a) reports, for instance, that in 14,341 dyad years for the period between 1870 and 1938, trade interdependence actually increases the probability of dyadic war.

In a recent, and comprehensive, review of the literature on economic interdependence and international conflict, however, McMillan (1997) finds more compelling support for liberal claims that economic interdependence reduces conflict. Assessing twenty empirical studies, she finds that on balance the majority of the evidence supports liberal claims that interdependence (measured only in terms of trade) inhibits conflict. Not surprisingly, this conclusion is corroborated by a recent review by Russett and Starr (2000), longtime advocates of liberal arguments concerning international conflict. Unlike many of the studies already cited, Russett and Starr note that more recent work, particularly that of Oneal and Russett (1999a, 1999b), argues and convincingly demonstrates that once researchers properly account for temporal dependence, contiguity, the presence of a major power, democracy, alliances, and a nation's military capability (as this dissertation also does), economic interdependence and peace proves to be a robust relationship.

On balance, then, theoretical and empirical evidence suggests a strong relationship between economic interdependence and peace. Since most studies employ dyadic empirical models, the most common indicator used in the quantitative literature to capture this effect is bilateral trade (that is, the sum of a country's exports and imports with a dyadic partner, divided by its GDP). Since this study is monadic, rather than dyadic, in design, I employ here a different measure of economic interdependence, one that while systemic in nature nevertheless captures the hypothesis that increased reliance on international trade as a percent of GDP decreases the likelihood of conflict. Specifically, I measure economic interdependence as *economic openness* (that is, imports plus exports as a percent of GDP). I anticipate that because states with extensive trade dependence have an interest in maintaining peaceful interstate relations, more economically open states are less likely to initiate conflict.

> *Hypothesis 14:* Economic openness decreases the likelihood that a democracy will initiate conflict.

Data for this variable are derived from Katherine Barbieri's National and Dyadic Trade Datasets (1996b, 1998), made available by that author. She measures each state's dyadic and national trade flows and GDP for the period 1870-1992. Values for each entry in this database are standardized and reported in millions of US dollars (current dollars). For each country and year, I divide the value of a state's total national trade (total imports plus total exports) by its GDP.

Methodology

In the empirical analysis that follows, I employ a variety of statistical techniques including descriptive statistics, bivariate correlations, and bivariate and multivariate logistical regression analysis. The data employed is pooled cross-sectional time series with 29 countries over a time period of 1945-1992. All of the multivariate regression models utilize General Estimating Equation (GEE) models. While most of these techniques require little explication, the latter warrants a more detailed discussion.

GEE is quasi-likelihood statistical methodology developed specifically for pooled cross-sectional time series data analyses in which data is correlated (Zorn 1998, 2001). According to Zorn (1998, 2001), data may be correlated for a number of reasons, including temporal correlation and data that is clustered. Temporal correlation arises in time-series data, pooled cross-sectional time series data, and panel designs. Such data occurs in a variety of political science studies, including those that model multiple temporal observations of states (nation-states) on a variety of political, economic, or social indicators (of which the present study is an example). Correlated data can also appear in data that is clustered in a non-temporal sense (Zorn 1998). Data clustering occurs in a variety of applications, including studies in which the observations share a spatial context (of which this study, where the data is clustered into groups of nation-states, is an example). Within the data set employed here, I thus expect temporal correlation (that is, correlation in the observation of the dependent variable from one year to the next), and correlation within clusters (that is, correlation between the dependent variable and each cluster of units). A GEE model is thus the most appropriate statistical methodology to employ. GEE models are becoming more common in the political science literature (Zorn 2000), and recent applications of the methodology to studies of international conflict include Reiter and

Tillman (2002). Bennett and Stam (2000), Bliss and Russett (1998), Leeds and Davis (1997), and Oneal and Russett (1999a, 1999b).

When using GEE models, researchers may specify different functional forms to link the independent variables to the dependent variable as well as choose the correlation structures of the error terms within groups. Since the dependent variables are dichotomous, I specify a logit functional form. Following Reiter and Tillman (2002) and Oneal and Russett (1999a), I also assume that the panel data exhibit a first order autoregressive process (where the value of the dependent variable at time *t* may be correlated to the value of the dependent variable at time *t-1*), and specify the correlation structure in the statistical analysis accordingly. This is an intuitive assumption given that the likelihood of a state's participation in an international conflict in any year is related to its participation the previous year. This methodology thus allows for temporal dependence within the time-series of the data without the inclusion of a lagged dependent variable. Explanatory primacy is thus given to the theoretically specified variables. To take into account possible clustering of my data, I estimate statistical significance using robust standard errors, again following the recommendations of Zorn (1998, 2001).[5]

[5] Another common methodology used to accommodate statistical problems generated by panel data where data may be correlated is the Beck and Katz method of using panel corrected standard errors and a lagged dependent variable (see, for example, Crepaz and Moser 2002). The methodology used here is similar in that I specify the structure of the "working" correlation matrix standard errors that are robust (thus accommodating cross-sectional data correlation). Beck and Katz also recommend lagging the dependent variable to accommodate temporal dependence. The method of specifying a first order autoregressive correlation structure within a GEE model, as illustrated by Zorn (2000), Reiter and Tillman (2002), and others, however, has the advantage of retaining all observations while giving the substantive terms—rather than the methodologically specified lagged term—theoretical primacy. On both types of data correlation (temporal and spatial clustering) then, GEE models are preferable because they offer greater flexibility in specifying, a priori, the correlation structure.

All analyses, the results of which are presented in the next chapter, were conducted utilizing the "xt" battery of commands in Stata 7.0. The command to run a GEE model is "xt gee." To specify the correlation structure as one that is autoregressive, the command is "corr(ar1)." To obtain robust standard errors, thereby statistically accounting for data clustering, the command is "force robust."

I also lag all independent variables by one year in order to ensure that their values are not in any way determined by the instance of international conflict to be explained. That is to say, conflict initiation in any year is determined by the values of the independent variables in the previous year. Given that military disputes that are not defensive in nature are likely to require a substantial amount of time for mobilization, lagging the independent variables is more meaningful in that the decision to initiate conflict in one year was likely made by the governing structure of the previous year. This also ensures that, in a causal sense, the dependent variable is not determined by any observation of the independent variables that occurs *afterwards.*

The following chapter explains the findings of this research design, presenting compelling evidence that institutional context—the setting in which veto players operate—has a significant impact on the initiation of conflict by democracies.

CHAPTER 5

FINDINGS

This chapter explains the empirical analysis of domestic institutional context and conflict initiation by democracies. In doing so, I present evidence to support the claims of my argument while also highlighting the empirical weaknesses of the conventional wisdom regarding democracies and conflict initiation. The data and analyses provide strong support for my argument that institutional context has a measurable impact on the likelihood of democracies to initiate international conflict. When institutional context is competitive, conflict initiation by democracies is less likely. When institutional context is collective, the opposite is true. Moreover, I find that there is considerable "value added" by including institutional context as a theoretical construct in terms of explaining instances of conflict initiation by democracies. I also find that realist factors—namely major power status, alliances, and borders with autocratic states—make conflict initiation more likely. Additionally, the results indicate the other "liberal" factors—especially economic openness and voter participation—reduce the likelihood of a democracy initiating conflict. I do not, however, find support for the liberal claim that "more democratic" states are any more pacific than "less democratic" ones, and my results directly oppose the expectations of conventional veto player theory. It seems that institutional context mediates, and sometimes alters, the impact of democracy (including democratic institutional

structure) and veto players on the initiation of conflict, confirming the expectations of my theoretical formulations.

The following three sections explain the results of this dissertation's research design. In the section that immediately follows, I detail the empirical evidence that supports my hypothesis that executive institutional context has a significant effect on the initiation of conflict by democracies. I next examine the second dimension of institution context: legislative opposition and conflict initiation. Then, I present results of a full model of domestic institutional and the initiation of conflict by democracies. I divide the analysis into three sections due the complexity of the proposed research design, which includes of variety of methodologies and variables. The first two of these sections offer comprehensive evaluations of the proposed hypotheses, while the third offers a more parsimonious examination that facilitates comparison with competing explanations and perspectives. I end the chapter with a brief summary of the results.

Executive Institutional Context and Conflict Initiation

In this section, I describe the empirical results of the first dimension of institutional context on conflict initiation. I begin by presenting descriptive statistics and bivariate correlations, continue with bivariate logistical regression analysis, and end with more complex multivariate regression models utilizing Generalized Estimating Equations. Taken together, these results provide strong evidence that when executive institutional context is collective, conflict initiation in more likely than when it is competitive.

Figure 5.1 illustrates graphically the relationship between cabinet type and conflict initiation. The vertical axis represents the number of disputes; the horizontal axis corresponds to each type of cabinet. The shaded bar depicts all disputes initiated (those disputes coded 1-5 in the

MID data set), and the dark bar represents only forceful disputes (those coded 4 and 5). The comparison matches the expectations of my hypotheses that, contrary to the conventional wisdom, conflicts are initiated most frequently by coalition cabinets (112 disputes, 85 forceful), followed by majority cabinets (107 disputes, 65 forceful), and finally minority cabinets (17 disputes, 11 forceful).

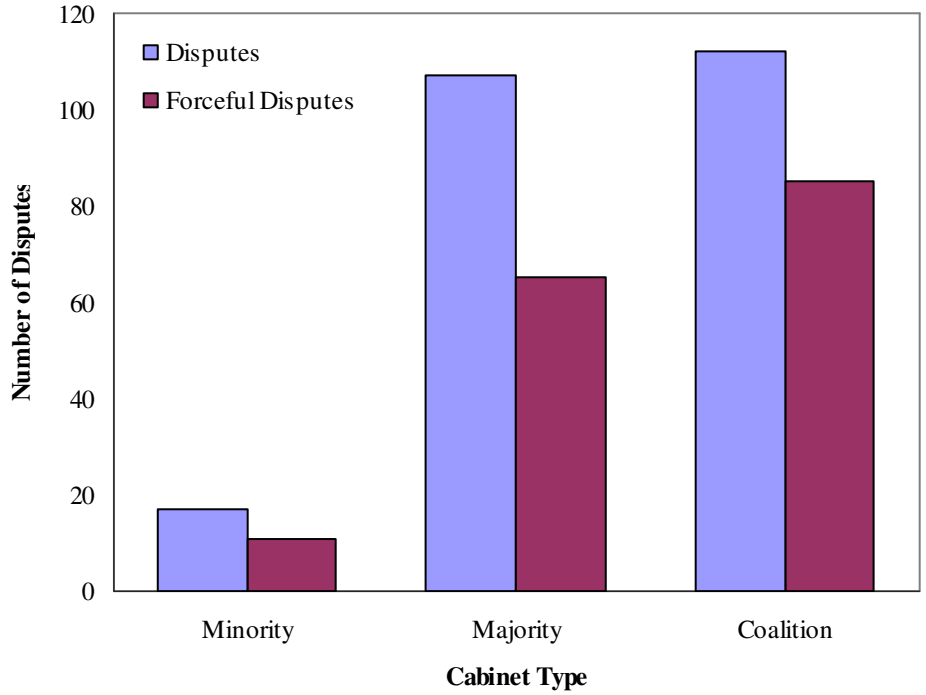

Figure 5.1. Conflict Initiation by Cabinet Type, Number of Disputes

While Figure 5.1 is persuasive, there is the issue of comparability—are coalition cabinets simply more common than other types of cabinets? The answer is yes. Within this data set, there are more cases of coalition cabinets (N=581) than majority (N=483) and minority (N=211) cabinets. To adjust for this problem of comparability, we can also illustrate the dispute frequency of each type of cabinet (that is, the rate at which each type of cabinet is associated with conflict

initiation). Figure 5.2 duplicates the previous graph, but with "dispute frequency" depicted on the vertical axis. Here, the evidence is more mixed with the exception of minority cabinets (which initiated a dispute at a rate of 8% for any dispute, 5% for forceful disputes). For all disputes, 22% of all majority cabinets in the data set initiated a conflict, slightly more than the dispute frequency of 19% for coalition cabinets. This trend is reversed for forceful disputes: 15% of coalition cabinets initiated a conflict, while only 13% of majority cabinets did so. Statistically, both coalition and majority cabinets are positively associated with conflict initiation (for each type of cabinet and dispute, the correlation coefficient is positive and statistically significant). By contrast, minority cabinets have a negative and significant correlation with both "any dispute" and "forceful disputes."

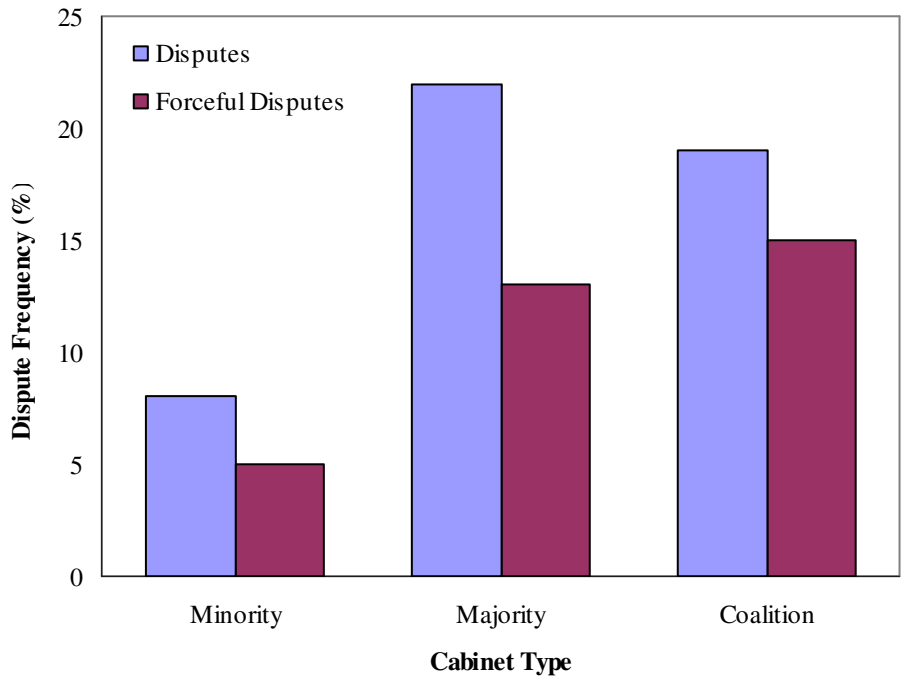

Figure 5.2. Conflict Initiation by Cabinet Type, Dispute Frequency

In addition to specifying cabinet type to capture institutional context, I have also argued that as the number of parties in government increases, the setting in which those parties operate becomes more collective. Conflict initiation should thus be more frequent with more government parties. Figure 5.3 displays a cross-tabulation of government parties and dispute frequency. The graph displays a parabolic or U-shaped relationship, but governing coalitions with 3 or more parties clearly are associated with the highest dispute frequency (23% for all disputes, 17% for forceful disputes).

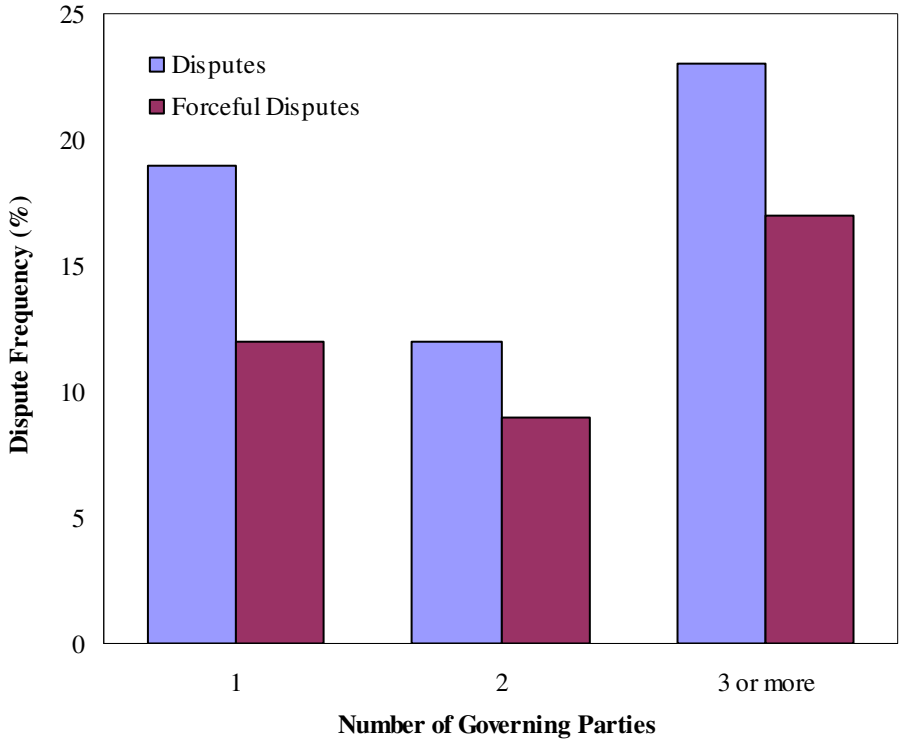

Figure 5.3. Number of Governing Parties and Conflict Initiation

The most compelling comparison is that between coalition cabinets, represented by the latter two sets of columns (the first set of columns above represent only single party cabinets). While veto player theory unambiguously predicts that more parties in government should

constrain its conflict initiation, the opposite relationship is most evident. There seems to be a strong trend that as the number of coalition parties increases, conflict initiation also increases. Governing coalitions with three or more parties have a higher dispute frequency than governing coalition with only two parties. Statistically, the correlation between government parties and conflict initiation is positive and significant for both dependent variables (for all disputes, r = .06, p<.05; for forceful disputes, r = .11, p<.01). Broad, multi-party coalitions do not appear to be as constrained as the conventional wisdom suggests.

Already, the summary statistics provide some initial support for my hypotheses. Tables 5.1 and 5.2 present results of bivariate logistical regression models that better highlight the weakness of the conventional wisdom regarding governing coalitions and conflict initiation. Table 5.1 displays the results on the dependent variable "initiate any conflict"; Table 5.2 duplicates the analysis with "forceful disputes." In these tables, both coalition cabinets and majority cabinets have a positive relationship with conflict initiation. Interestingly, the results for *Coalition Cabinets* are more robust for forceful disputes (Table 5.2) than for any dispute (Table 5.1). The opposite is true for *Majority Cabinets*. Minority cabinets, on the other hand, have a negative, statistically significant relationship, indicated that these types of cabinets are in fact more severely constrained in initiated conflicts. On balance, these relationships demonstrate that my theoretical variables regarding cabinet type exhibit the expected relationship. This is a particularly important task before combining them into the summary index of *Cabinet Context*, an index that would be meaningless if the component variables showed an unexpected relationship. When the variables are aggregated into the variable *Cabinet Context*, the relationship is positive and robust with both dependent variables. As hypothesized, when this variable increases, the likelihood of conflict initiation increases.

Table 5.1. Bivariate Analysis of Cabinets, Parties, and Conflict Initiation (Any Dispute)

Variable	*Coefficient*	*Constant*	*N*	χ^2
Coalition Cabinet	.050 (.144)	-1.499 (.099)***	1275	0.03
Majority Cabinet	.449 (.146)***	-1.665 (.096)***	1275	9.35***
Minority Cabinet	-1.145 (.271)***	-1.350 (.076)***	1275	23.22***
Cabinet Context	.271 (.102)***	-2.120 (.254)***	1275	7.27***
Government Parties	.130 (.052)***	-1.805 (.135)***	1245	5.96***

Notes: The top numbers refer to regression coefficients obtained from logistical regression analysis. Standard errors are reported for each coefficient in parentheses. All significance tests are one-tailed (*p<.10; **p<.05; ***p<.01). χ^2 expresses whether the bivariate model is significantly different from the null model. *N* is lower for the variable "government parties" due to missing values for Portugal prior to 1976.

Table 5.2. Bivariate Analysis of Cabinets, Parties, and Conflict Initiation (Forceful Dispute)

Variable	*Coefficient*	*Constant*	*N*	χ^2
Coalition Cabinet	.288 (.168)**	-2.079 (.121)***	1275	2.91**
Majority Cabinet	.164 (.172)	-2.000 (.172)***	1275	0.90
Minority Cabinet	-1.083 (.322)***	-1.812 (.088)***	1275	14.81***
Cabinet Context	.308 (.121)***	-2.655 (.302)***	1275	6.65***
Government Parties	.202 (.056)***	-2.362 (.152)***	1245	12.01***

Notes: The top numbers refer to regression coefficients obtained from logistical regression analysis. Standard errors are reported for each coefficient in parentheses. All significance tests are one-tailed (*p<.10; **p<.05; ***p<.01). χ^2 expresses whether the bivariate model is significantly different from the null model. *N* is lower for the variable "government parties" due to missing values for Portugal prior to 1976.

What is more, the bivariate model is significantly different from the null model as indicated by the chi square results. The variable *Government Parties* also demonstrates a statistically significant, positive relationship with both dependent variables, indicated that governments with more parties are also associated with more instances of conflict initiation. So far, there seems to be a good fit between the data and my hypotheses, and demonstrating these relationships has been an important first step toward deconstructing the conventional wisdom regarding the ability of democratic executives to initiate conflict. When considered alone, my primary theoretical variables regarding executive institutional context have the hypothesized effect on the initiation of conflict.

The descriptive statistics and bivariate regressions are clearly informative and initially compelling, but they do not represent clear causal relationships between executive institutional context and conflict initiation by democracies. For this task, I turn now to the multivariate research design utilizing GEE models. Tables 5.3 and 5.4 present the results of the multivariate design of the impact of executive institutional context on conflict initiation. Table 5.3 displays the results for the dependent variable "initiate any dispute," utilizing four different models to assess combinations of multivariate equations. In addition to the main theoretical variables regarding institutional context, I also control for a variety of domestic institutional variables, several "realist" variables that focus of power and other geopolitical concerns, as well as the other "liberal" variables that account for interdependence and economic openness.

Model 1 uses minority cabinets as a baseline, and model 2 uses single party majority cabinets as a baseline. This allows me to test for significant differences between the three executive types since I hypothesize (contrary to the conventional wisdom) that coalition cabinets are the least constrained of all cabinets, followed by majority then minority cabinets. In model 1,

the minority government baseline equation indicates that coalition cabinets are more likely to initiate conflict. The relationship is positive and statistically significant, indicating that this type of executive has a higher propensity to initiate conflict compared to minority governments. Somewhat surprisingly, majority governments demonstrate a negative relationship with conflict initiation. The relationship is not a statistically significant one, however, indicating no measurable difference between majority and minority governments in initiating conflict in this equation. This is an unexpected finding from the perspective of traditional veto player theory.

Table 5.3. Executive Institutional Context and Conflict Initiation (Any Dispute)

Variable	*Model 1*	*Model 2*	*Model 3*	*Model 4*
Coalition Cabinet	.854 (.495)**	.894 (.480)**		
Majority Cabinet	-.134 (.319)			
Minority Cabinet		-.108 (.336)		
Cabinet Context			.571 (.282)**	
Government Parties				.211 (.058)***
Parliamentary	1.069 (.769)*	1.025 (.771)*	.826 (.692)	.876 (.685)*
Presidential	2.623 (.780)***	2.570 (.790)***	2.277 (.746)***	2.156 (.807)***
Treaty Ratification	-.949 (.241)***	-.952 (.239)***	-.934 (.249)***	-.833 (.303)***
Participation	-.018 (.012)*	-.018 (.012)	-.018 (.012)*	-.018 (.012)*
Polity	.185 (.144)	.187 (.145)	.201 (.150)	.185 (.169)
Major Power	1.295 (.386)***	1.208 (.365)***	1.079 (.338)***	1.292 (.398)***

Table 5.3. (continued)

Autocratic Borders	.509 (.140)***	.505 (.142)***	.515 (.151)***	.501 (.147)***
Alliances	.273 (.173)*	.276 (.172)*	.280 (.169)**	.272 (.172)*
IGOs	.001 (.013)	.002 (.013)	.004 (.013)	.001 (.014)
Openness	-.022 (.005)***	-.022 (.005)***	-.021 (.005)***	-.020 (.005)***
Constant	-3.322 (1.780)**	-3.423 (1.573)**	-4.472 (1.830)***	-3.399 (1.810)**
N	986	986	986	986
χ^2	1047.33***	954.31***	767.99***	909.64***
% Correctly Pred./Null Model	87.6/80.1	87.3/80.1	87.4/80.1	87.4/80.1
PRE	.38	.36	.37	.37

Notes: The top numbers refer to regression coefficients obtained from GEE-logit analysis with AR-1 autocorrelation. Robust standard errors are reported for each coefficient in parentheses. All significance tests are one-tailed except for *Majority Cabinet*, *Polity*, *IGOs*, and the *Constant*, which are two-tailed. *p<.10; **p<.05; ***p<.01.

The next two variables, *Parliamentary* and *Presidential*, test whether macro-institutional structure is associated with variations in conflict initiation (semi-presidential is the excluded category). Both the parliamentary and presidential variables demonstrate a significant and positive relationship with conflict initiation (although *Parliamentary* reaches only the least restrictive level of statistical significance). Presidential systems are not necessarily more or less constrained than parliamentary systems. Rather, it seems that both types of systems are comparatively more belligerent than semi-presidential systems.

When I instead include "semi-presidential systems" as a dummy variable, it demonstrates a generally negative (though not always statistically significant relationship) with conflict

initiation (results not shown). This is true for both a parliamentary baseline model (in which parliamentary systems are the excluded category) and a presidential baseline model (in which presidential systems are the excluded category). In the parliamentary baseline model, presidential systems are more likely to initiate conflict than parliamentary systems (the coefficient is positive and statistically significant). Semi-presidential systems, on the other hand, have a negative (albeit not statistically significant) relationship with conflict initiation. In the presidential baseline model, parliamentary and semi-presidential systems tend to be less likely to initiate conflict than presidential systems (although the coefficient for parliamentary systems does not reach statistical significance). These results are clearly mixed, and not particularly robust. The strongest conclusion might be that parliamentary systems and semi-presidential systems tend to be less belligerent than presidential systems in some model specifications with the converse conclusion not being supported at all. The most important inference to be drawn from these findings regarding macro-institutional structure is that, as hypothesized, institutional context has the same effect in each type of democratic system (whether parliamentary, presidential, or semi-presidential). That is, regardless of macro-institutional structure, when executive institution context is collective, conflict initiation is more likely that when it is competitive.

The final three measures of democratic institutions (*Treaty Ratification*, *Participation*, and *Polity*) show some interesting results. As expected, *Treaty Ratification* has a negative, statistically significant relationship with conflict initiation. As legislative power regarding foreign policy increases, the likelihood of conflict initiation decreases. *Participation* is also negative and significant (although only at the 90% confidence interval). This indicates that a greater level of participation among the electorate reduces a democracy's belligerency. Confirming the findings of Reiter and Tillman (2002), countries with stronger legislative power

of treaty ratification and greater electoral participation are less likely to initiate conflict. As indicated by the *Polity* variable, however, this model does not confirm liberal claims that the most democratic countries are the most pacific, a point I return to shortly.

The three "realist" control variables are statistically significant and positive in the expected direction, indicating that major powers, those states that border more autocracies, and those states with more numerous alliance ties are also more likely to initiate conflict. These results thus confirm decades of empirical research on the effect of state power and geopolitical concerns on interstate conflict.

The last two variables are intended to capture liberal arguments regarding those factors typically overlooked, or underappreciated, by realist scholars regarding the war proneness of states. While the results are not statistically significant, *IGOs* demonstrates a relationship in the opposite direction from that expected. This finding thus does not support the hypothesis that the denser the network of Intergovernmental Organization (IGO) membership, the less likely a democracy will be to initiate conflict. It appears that membership in international organizations does not necessarily instill norms of nonviolent conflict resolution, inoculating states from initiating conflict. *Openness*, however, demonstrates a statistically significant relationship in the expected negative direction. This supports the hypothesis that because states with extensive trade dependence have an interest in maintaining peaceful interstate relations, more economically open states are less likely to initiate conflict.

In order to get a sense of goodness-of-fit, Table 5.3 also reports several diagnostic statistics. The chi square result indicates that model 1 fits better than the null model (with the null, or intercept-only model, equaling the prediction based on the modal category of the dependent variable), and that the model is statistically significant. The next row compares more

directly model 1 to the null. Model 1 predicts 87.6% of the cases correctly. This prediction rate may be more modest than it first appears, however, due to the skewed distribution of the dependent variable (where more than 8 out 10 country-years did not initiate conflict). More significantly though, the inclusion of the variables in this model correctly predicted more cases than the null model (87.7% compared to only 80.1% for the null model).

I also report the PRE (proportional reduction in error) statistic in the final row. The PRE statistic is perhaps the best model fit statistic for logit or probit models. It is a measure of how much prediction error is eliminated by using the regression model. It captures the difference in prediction error between the null model and the regression model—that is, how much more the regression model explains in comparison to the sample mean. The formula for predicting the PRE statistic is: (% correctly predicted-null model)/100. This statistic can vary from zero, indicating that the regression model leaves an error as large as the one occurring in the null, to 100, meaning that the regression model eliminated all of the error. Hence, the PRE statistic standardizes the proportion of variance "explained" by the regression model (Bohrnstedt and Knoke 1994).[1] For model 1, the PRE statistic is .38, indicated that 38% of the error was eliminated by the model (or, alternatively, 38% more cases are predicted correctly with this model compared to the null model). Not only does the model indicate strong support for my hypotheses regarding executive institutional context and conflict initiation, but it also represents a significant improvement in predictive power over the null.

Model 2 details the results of the majority cabinet baseline model. As expected, minority governments demonstrate a negative relationship with conflict initiation suggesting that they are less like to initiate conflict due to a competitive institution context. The results, however, do not

[1] Similarly, the PRE statistic for OLS regression models is the coefficient of determination, or R-square.

reach established thresholds of statistical significance. The coalition cabinet variable, though, demonstrates a significant relationship in the hypothesized direction. This contradicts the conventional wisdom based on veto players which unequivocally expects coalition cabinets to be more constrained than majority cabinets. Coalition cabinets clearly have the opposite effect, an expectation consistent with institutional context. Ruling coalitions that are comprised of broad, multi-party cabinets exhibit fewer decisional or electoral constraints on the initiation of conflict. The remaining variables exhibit relationships consistent with the previous model with the exception that *Participation* does not reach statistical significance. Otherwise, this model again supports realist assertions that major power status, borders with autocracies, and alliances increase the likelihood that states will initiate conflict. It also supports the liberal hypothesis that economic openness reduces the war proneness of states. The diagnostic statistics vary little from the previous equation. Again, the model is statistically different from the null model, correctly predicting 36% more cases. This model provides robust support for the proposition that coalition cabinets are more likely to initiate conflict than majority cabinets.

Model 3 duplicates the previous models, but this time with the index of cabinet context as the main theoretical variable. Given that the findings thus far have supported my hypotheses regarding each type of cabinet, *Cabinet Context* proves to be a theoretically and empirically justifiable variable. As this variable increases (moving from minority to majority to coalition cabinets), cabinet context becomes more collective, and conflict initiation becomes more likely. The statistical evidence clearly supports this assertion as indicated by its robust, positive relationship with the dependent variable. Indeed, as executive institutional context becomes more collective, there is a measurable increase in the likelihood of conflict initiation. Again, the remainder of the variables demonstrate similar relationships as in the previous models, with

Participation this time reaching minimal thresholds of statistical significance ($p<.10$). The goodness-of-fit statistics are also consistent with previous specifications: model 3 correctly predicts 87.4% of the cases, 37% more cases than the null model.

Model 4 employs one final specification with the variable *Government Parties* included as a measure of executive institutional context. This final model specification indicates robust and consistent results with the previous three models. As indicated by the positive and statistically significant regression coefficient, as the number of government parties increases, conflict initiation becomes more likely. This provides clear support for my hypothesis regarding institutional context and conflict initiation: as the number of parties in government increases, the setting in which those parties operate becomes more collective, and the executive is less constrained in initiating conflict. This finding directly refutes veto player theory which hypothesizes that as government parties become more numerous, more veto players constrain the initiation of conflict. I thus find no support for Tsebelis's claim that partisan veto players constrain policy decisions. Rather, it seems that additional partisan partners in the executive instills a collective institution context, a political setting that actually facilitates the initiation of conflict should the executive so choose such a course. The goodness-of-fit statistics indicate that model 4 is significantly different from the null model, correctly predicting 87.4% of the cases, 37% more than the null model.

The results of the multivariate design of the impact of executive institutional context on conflict initiation continue on Table 5.4. It displays the results for the dependent variable "initiate forceful disputes," utilizing four additional models specifications. Models 5 through 8 duplicate the previous four models that tested executive institutional context, controlling for the

same domestic institutional, realist, and liberal variables. The findings are virtually identical for this dependent variable, demonstrating robust and consistent support for my hypotheses.

Table 5.4. Executive Institutional Context and Conflict Initiation (Forceful Dispute)

Variable	*Model 5*	*Model 6*	*Model 7*	*Model 8*
Coalition Cabinet	.791 (.397)**	.920 (.432)**		
Majority Cabinet	-.129 (.240)			
Minority Cabinet		.129 (.240)		
Cabinet Context			.579 (.240)***	
Government Parties				.252 (.032)***
Parliamentary	.992 (.940)	.992 (.940)	.736 (.879)	.845 (.843)*
Presidential	2.066 (.930)***	2.067 (.929)***	1.694 (.854)**	1.741 (.875)**
Treaty Ratification	-.936 (.375)***	-.936 (.375)***	-.989 (.362)***	-.835 (.429)**
Participation	-.002 (.016)	-.002 (.016)	-.001 (.017)	-.002 (.017)
Polity	.106 (.153)	.106 (.153)	.127 (.153)	.105 (.170)
Major Power	1.034 (.440)***	1.033 (.440)***	.867 (.421)**	1.033 (.478)**
Autocratic Borders	.454 (.126)***	.454 (.126)***	.453 (.133)***	.420 (.123)***
Alliances	.205 (.140)*	.205 (.140)*	.202 (.140)*	.182 (.136)*
IGOs	-.008 (.013)	-.008 (.013)	-.005 (.016)	-.007 (.016)
Openness	-.024 (.006)***	-.024 (.006)***	-.023 (.006)***	-.025 (.007)***

Table 5.4. (continued)

Constant	-3.013 (1.702)*	-3.142 (1.596)**	-4.294 (1.737)***	-3.118 (1.172)*
N	986	986	986	986
χ^2	227.60***	228.06***	162.70***	295.42***
% Correctly Pred./Null Model	87.8/86.6	87.8/86.6	88.0/86.6	87.9/86.6
PRE	.09	.09	.10	.10

Notes: The top numbers refer to regression coefficients obtained from GEE-logit analysis with AR-1 autocorrelation. Robust standard errors are reported for each coefficient in parentheses. All significance tests are one-tailed except for *Majority Cabinet*, *Minority Cabinet*, *Polity*, and the *Constant*, which are two-tailed. *p<.10; **p<.05; ***p<.01

Model 5 indicates that coalition cabinets again are more likely to initiate a forceful dispute than minority cabinets—the coefficient is positive and statistically significant. Majority cabinets, on the other hand, show no statistically significant propensity to initiate conflict compared to minority cabinets. Two of the domestic institutional variable, *Presidential* and *Treaty Ratification*, are both statistically significant in the expected direction. Here, presidential policy dominance in areas of foreign policy make conflict initiation in those systems more likely, but executive policy dominance to initiate a forceful dispute is limited when constitutions stipulate a greater degree of foreign policy power to legislatures. All three of the realist control variables are also statistically significant. Again, power status, borders with autocratic states, and alliances make the initiation of forceful disputes more likely. Only one other liberal variable, *Openness*, is statistically significant. Accordingly, economic openness constrains democracies from initiating conflict abroad. With the exception of *Parliamentary* (which reaches minimal levels of statistical significance in model 8), the control variables demonstrate consistent results across the remainder of the model specifications.

According to the results displayed in model 6, coalition cabinets are also more likely than majority cabinets to initiate a forceful dispute, confirming my hypothesis regarding institutional context while refuting those anchored in veto player theory. *Cabinet Context* again has the expected relationship, the results of which are display in the column for model 7. As executive context changes from competitive to collective, the initiation of forceful disputes becomes more likely. The alternate measure of executive institutional context, *Government Parties*, also demonstrates consistency in its direction and statistical significance. Institutional context, which becomes more collective in the executive as the number of partisan partners increases, indeed has the expected impact on the ability of a democracy to initiate conflict.

I should also note that all model specifications for "initiate forceful disputes" fit better than the null models as indicated by the chi square statistic. The results are thus robust and consistent when considering only those disputes initiated by democracies that escalate to the actual use of force. Even so, the goodness-of-fit statistics are not as impressive for this dependent variable as it is for all disputes. While each of the models explains between 87.8% and 88.0% of all the cases correctly, the PRE statistic never exceeds 10%. This last statistic requires some perspective, however, as the null model actually predicts a comparatively high 86.6% of the cases correctly (due to the fact that there are so few cases in which democracies initiated a forceful dispute).

The findings so far indicate no support for Tsebelis's claim that multiple coalition partners "veto" the policy making process. Rather, the findings presented here demonstrate that the collective agency and shared political responsibility common to coalition cabinets with multiple parties actually facilitate executive policy decisions, including a policy to initiate conflict. There indeed appears to be a classic collective action problem within coalition cabinets,

one that worsens with the addition of more partisan partners. Members of a coalition seem to prefer to support, rather than oppose, an executive initiative for international conflict. Moreover, as the number of partisan members increases in the coalition, the decision to initiate conflict becomes easier as there is a certain "safety in numbers" for the members of the cabinet. Any electoral disadvantage for a potentially costly foreign policy decision is removed for there is more widespread support built into the coalition.

In addition to refuting Tsebelis's veto player theory regarding executive constraints and conflict initiation, the results also begin to highlight the empirical weakness of the conventional wisdom regarding democracy and conflict initiation. In all of the model specifications thus far, the variable *Polity* demonstrates not only the absence of a statistically significant relationship, but a relationship in the opposite direction from that hypothesized by democratic peace theorists. In order to more fully understand this finding, Figure 5.4 illustrates a cross-tabulation between the number of disputes initiated and Polity score. The graph is at first glance striking. There appears to be a clear, an unexpected, upward trend: the more democratic states are also the most belligerent. Most of the disputes initiated (141 disputes, 97 of them forceful) were by democracies with the highest Polity score of 10.

These descriptive statistics are, however, slightly misleading. A vast majority of the cases in this study have a value of 10 on the Polity democracy-autonomy scale (84% of all country-years have a Polity score of 10), and this variable shows altogether very little variance (it varies between only 4 and 10). Given the highly skewed distribution of this variable, Figure 5.5 graphs a more suitable comparison between Polity score and conflict initiation. It illustrates the dispute

frequency of democracies, indicating a slight but inconsistent downward trend in democratic-ness and conflict initiation.[2]

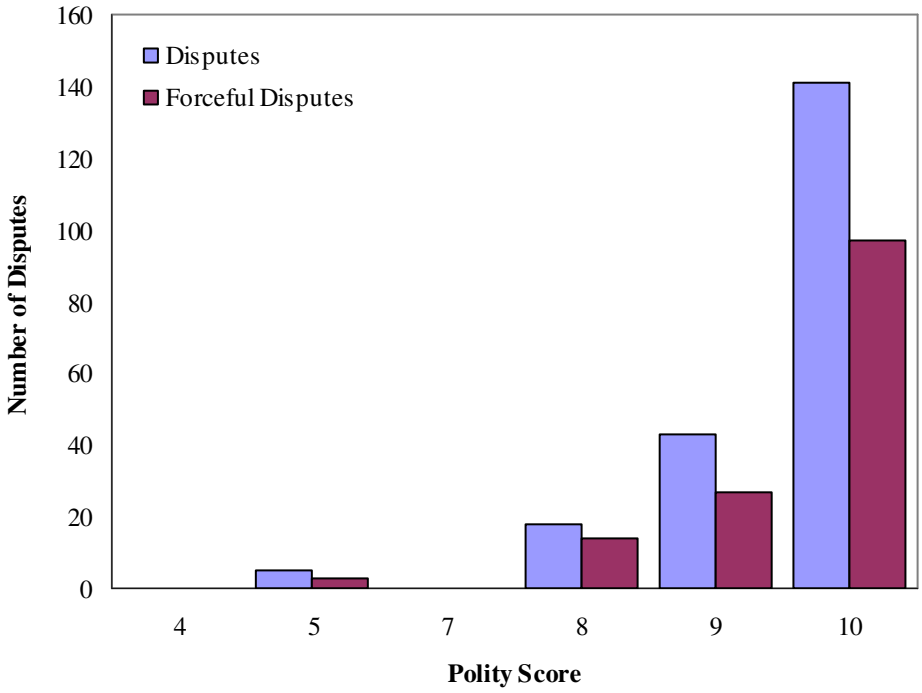

Figure 5.4. Conflict Initiation by Polity Score, Number of Disputes

Compared to those countries that score either 5, 8, or 9 on the Polity scale, for instance, countries that score a 10 are slightly more pacific. In these cases, democracies initiated any dispute 14% of the time (that is, 14% of the country-year units with a Polity score of 10 initiated a conflict). This is less that the 45% dispute frequency for Polity 5 countries, 32% for Polity 8 countries, and 41% for Polity 9 countries (there are 17 cases that score 4 on the Polity scale, 2 cases that score 7; none of these cases initiated any conflicts). Nevertheless, when the *Polity*

[2] The correlation coefficient (Pearson's *r*) between *Polity* and *Initiate any Dispute* is -.09 ($p<.01$). For *Polity* and *Initiate Forceful Dispute*, $r = -.07$ ($p<.05$).

variable is included in the multivariate regression models, it fails to demonstrate any statistically significant relationship, much less the expected one. These findings are largely duplicated in the remaining statistical results. Clearly, the *Polity* variable fails to distinguish democracies in their likelihood of initiating conflict for it not only shows very little variance, but also does not succeed in demonstrating any causal relationship.

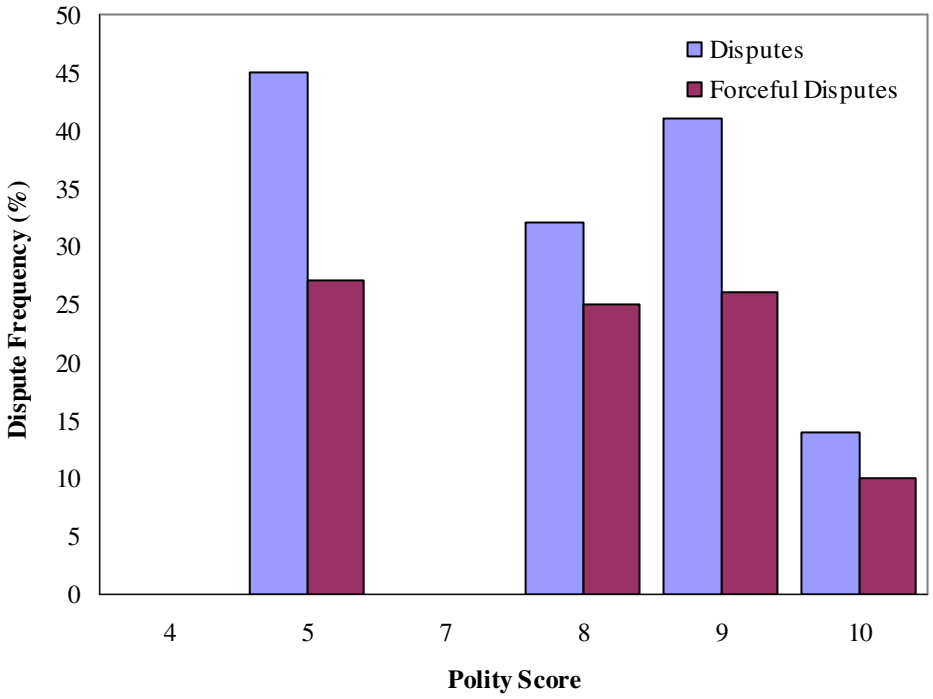

Figure 5.5. Conflict Initiation by Polity Score, Dispute Frequency

While the absence of a relationship between "democratic-ness" and the propensity to initiate conflict might ordinarily constitute a controversial finding, it does not refute the proposition that democratic institutions matter in terms of the conflict propensity of states. Rather, as I have argued throughout this dissertation, it seems that our theories regarding democratic foreign policy making are in need of revision in order to capture unexplained or

unresolved empirical patterns. It is in the tradition of building a progressive liberal research program (to use Lakatos's terminology) that my hypotheses regarding institutional context have been offered. Institutional context, it appears, mediates, and sometimes alters, the impact of democracy on the initiation of conflict, confirming the expectations of the hypotheses put forth in this dissertation.

On balance, the findings so far indicate strong empirical support for my argument that institutional context is a useful way of thinking about how domestic institutions affect the democratic initiation of conflict. When institutional context is collective (indicated by coalition governing), executives are less constrained in the use of force as a foreign policy tool. On the other hand, a competitive institutional context is a much more forceful constraint on democratic conflict initiation. These relationships hold even while controlling for realist and other liberal variables demonstrating the utility of the liberal perspective advocated in this dissertation.

Executive-Legislative Institutional Context and Conflict Initiation

In this section, I describe the empirical results of the second dimension of institutional context on conflict initiation: legislative opposition to the executive. As in the previous section, I begin by presenting descriptive statistics and bivariate correlations, continue with bivariate logistical regression analysis, and end with more complex multivariate regression models utilizing Generalized Estimating Equations. The findings present clear evidence that strong political opposition in the legislative chamber, signifying a competitive institutional context, constrains executive decision making, decreasing the likelihood of democratic conflict initiation. Weak political opposition, on the other hand, generates a collective institutional setting in which the democratic initiation of conflict is less constrained.

Figure 5.6 illustrates the relationship between opposition size and conflict initiation. The vertical axis represents dispute frequency; the horizontal axis corresponds to the total size of legislative opposition. The shaded bar depicts all disputes initiated, and the dark bar represents only forceful disputes. The comparison matches the expectations of my hypotheses that, in all types of democratic systems, conflicts are initiated most seldom when the opposition is largest. Dispute frequency (that is, the rate at which each category is associated with conflict initiation) is lowest when the opposition size exceeds 66% of all seats in the legislative chamber (12% of all cases with more than 66% opposition strength initiated any dispute, 10% of all cases initiated forceful disputes).

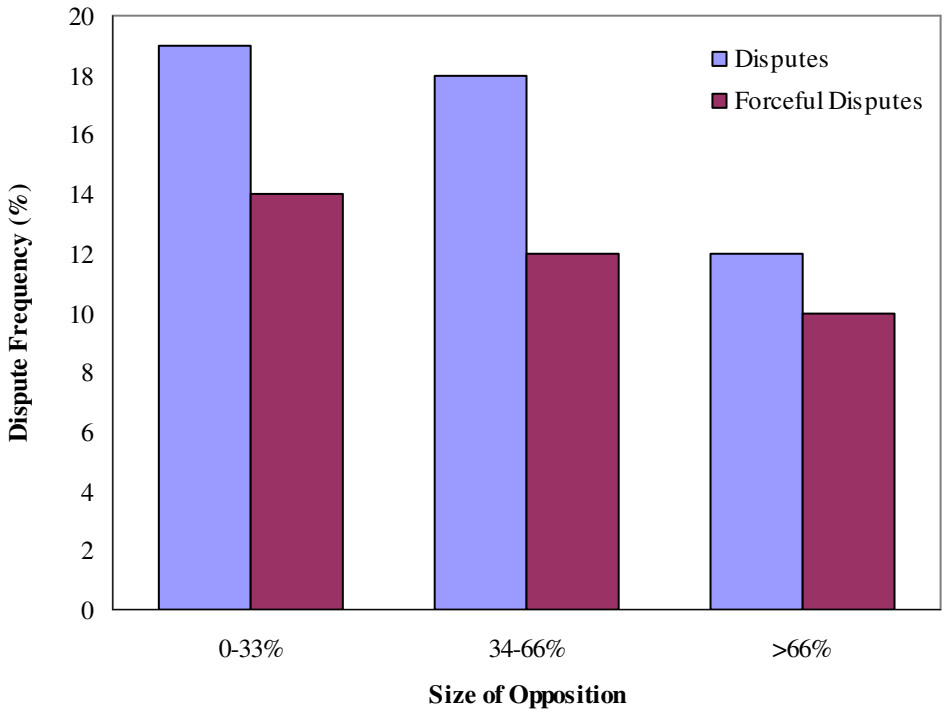

Figure 5.6. Legislative Opposition Size and Conflict Initiation

As legislative opposition decreases, dispute initiation becomes markedly more likely. When the legislative opposition size is between 34% and 66%, the frequency of dispute initiation increases to 18% for all disputes and 12% for forceful disputes. Similarly, when legislative opposition shrinks to 33% or less, disputes initiation is the most common. For this category, democracies initiate any dispute 19% of the time, forceful disputes 14% of the time. Clearly, the descriptive statistics indicate that conflict initiation is more frequent with opposition is lowest.

In addition to specifying total opposition size in the legislature to capture institutional context, I have also argued that the size of a coherent opposition bloc may be a more meaningful measure of institutional context. That is, a legislature with one large opposition party (or bloc of parties) is a much larger constraint than a legislature that is highly fragmented and unable to present a clear opposition message. Conflict initiation, I have hypothesized, should be more common when the size of the major opposition bloc is small, less common when it is large. Figure 5.7 presents a cross-tabulation of opposition bloc size and dispute frequency.

Figure 5.7 displays some counterintuitive metrics that do not appear to match the expectations of my hypotheses. In this chart, dispute frequency actually increases as we move from small opposition blocs (those that occupy 20% or less of all legislative seats) to moderately sized opposition blocs (between 21% and 40%), and again to large opposition blocs (those that occupy more than 40% of legislative seats). Democracies initiate any dispute 18% of the time (and forceful disputes 12% of the time) when the opposition is large compared to a dispute frequency of only 10% (9% for forceful disputes) when the major opposition bloc is small. Statistically, *Opposition Bloc* has a statistically significant, positive correlation with "initiate any dispute" ($r=.09$, $p<.01$). The correlation coefficient between *Opposition Bloc* and "initiate forceful disputes" is not, however, statistically significant (although it, too, is positive).

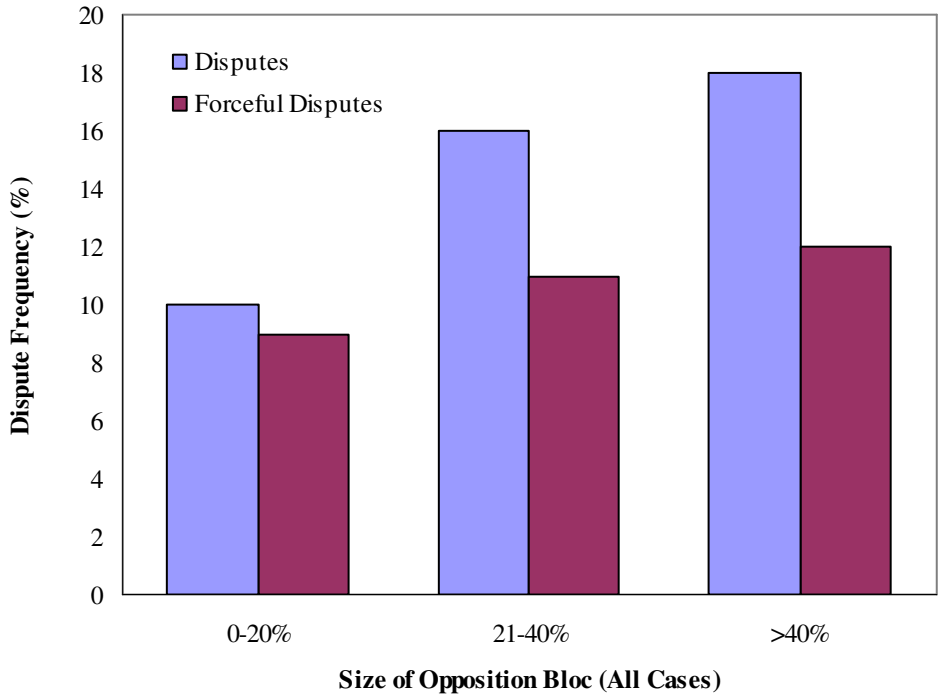

Figure 5.7. Legislative Opposition Bloc and Conflict Initiation, All Cases

Recall, however, that the type of macro-institutional structure may obfuscate the effect of institutional context. That is, the size of opposition blocs in presidential systems are systematically larger than those in parliamentary systems due to difference in electoral rules (and, consequently, party structure). Parliamentary systems, most of which use proportion representation to allocate legislative seats, typically have multi-party systems which reduce the relative strength of each party. Presidential systems, on the other hand, utilize single member districts with plurality voting, creating a two party system in which the opposition bloc (the party out of power) is comparatively much larger. Therefore, the high frequency of disputes for large opposition blocs illustrated in Figure 5.7 may be driven by systematic differences in presidential oppositions which are, on average, much larger than parliamentary oppositions. If we separate

parliamentary systems from the data set, as Figure 5.8 does, we see a much clearer picture of the impact of opposition bloc size on the initiation of conflict.

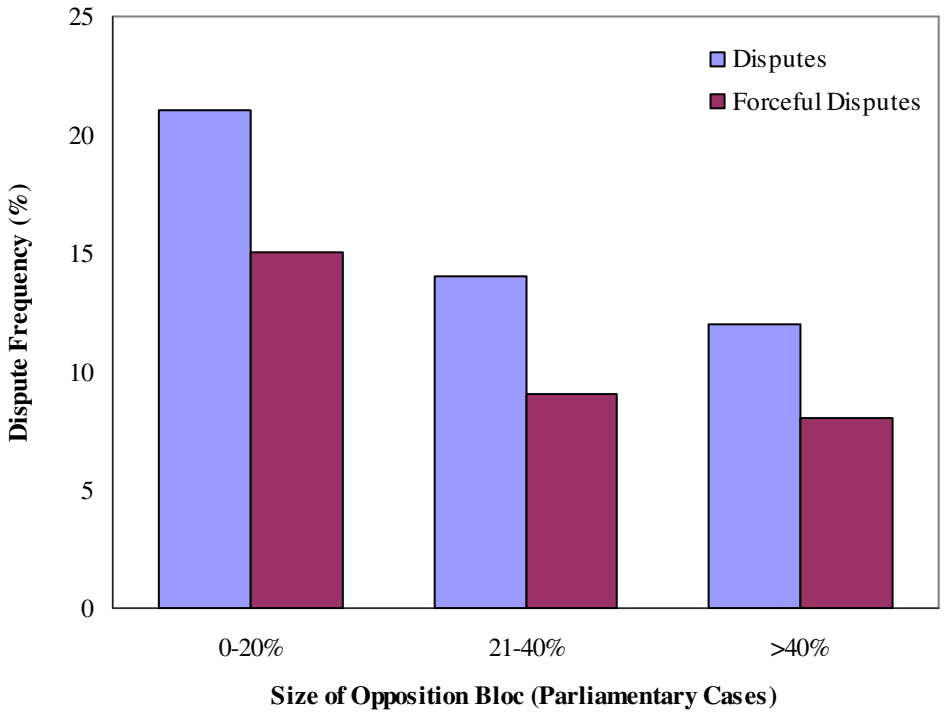

Figure 5.8. Legislative Opposition Bloc and Conflict Initiation, Parliamentary Cases

In this figure, a clear negative relationship emerges between the size of the major opposition bloc and dispute frequency. When the opposition bloc is small (0-20% of legislative seats held), dispute frequency is high (21% for all disputes, 15% for forceful disputes). As the opposition bloc increases, disputes are less common. When the opposition bloc size increases to 21-40% of legislative seats, dispute frequency decreases to 14% for all disputes, 9% for forceful disputes. This trend continues for large opposition blocs. When an opposition bloc is 40% or more of parliamentary seats, the dispute frequency falls to 12% for all disputes, 8% for forceful disputes. Statistically, opposition bloc size in parliamentary system is negatively, and

significantly, correlated with each type of dispute. It seems that, in parliamentary systems, disputes are most common when the opposition is small, indeed conforming to the expectations of institutional context.

The case for opposition bloc strength in presidential systems serving as a constraint on conflict initiation is much less persuasive. In presidential systems (of which the U.S. is the only example in this data set), the dispute frequency during periods of divided government is 93% for all disputes, 57% for forceful disputes. Under unified government (the defining criteria for executive-legislative collective institutional context for presidential systems), the dispute frequency equals 68% for all disputes, 42% for forceful disputes. These relationships do not match expectations. A larger sample size, unfortunately not available for this research design, may arbitrate these confounding relationships. But thus far, the data do not seem to support my hypotheses (nor those based in veto player theory) for presidential systems. For the excluded category, semi-presidential systems, the data are more encouraging. Within these hybrid systems, opposition bloc size demonstrates the expected relationship. When the opposition bloc size in semi-presidential systems is 50% or more, dispute frequency for all types of disputes is 0%. On the other hand, when the opposition bloc size equals 50% or less, dispute frequency for all types of disputes, including forceful ones, increases to 9%.

Progressing beyond descriptive statistics, Tables 5.5 and 5.6 present results of bivariate logistical regression models that more accurately capture the statistical relationship between my variables of interest. Table 5.5 displays the results on the dependent variable "initiate any conflict"; Table 5.6 duplicates the analysis with "forceful disputes." In these tables, opposition size demonstrates a relationship in the hypothesized direction, but the coefficient for this variable only reaches the lowest level of statistical significance for "forceful disputes."

Table 5.5. Bivariate Analysis of Oppositions and Conflict Initiation (Any Dispute)

Variable	*Coefficient*	*Constant*	*N*	χ^2
Opposition Size	-.325 (.510)	-1.381 (.222)***	1198	0.41
Opposition Bloc	1.972 (.662)***	-2.416 (.268)***	1100	9.28***
Parliamentary	-.480 (.167)***	-1.199 (.140)***	1198	7.93***
Opposition Bloc *Parliamentary	-1.646 (.425)***	-1.259 (.128)***	1100	15.03***

Notes: The top numbers refer to regression coefficients obtained from logistical regression analysis. Standard errors are reported for each coefficient in parentheses. All significance tests are two-tailed except for "opposition size" which is one-tailed (*p<.10; **p<.05; ***p<.01). χ^2 expresses whether the bivariate model is significantly different from the null model. N is lower for "opposition bloc" due to missing values for Bahamas, India, pre-1976 Portugal, and pre-1977 Spain for which reliable parliamentary data is unavailable.

Table 5.6. Bivariate Analysis of Oppositions and Conflict Initiation (Forceful Dispute)

Variable	*Coefficient*	*Constant*	*N*	χ^2
Opposition Size	-.760 (.594)*	-1.650 (.254)***	1198	1.63*
Opposition Bloc	1.059 (.752)***	-2.491 (.299)***	1100	2.03
Parliamentary	-.322 (.197)*	-1.755 (.167)***	1198	2.56*
Opposition Bloc *Parliamentary	-1.551 (.496)***	-1.259 (.148)***	1100	9.79***

Notes: The top numbers refer to regression coefficients obtained from logistical regression analysis. Standard errors are reported for each coefficient in parentheses. All significance tests are two-tailed except for "opposition size" which is one-tailed (*p<.10; **p<.05; ***p<.01). χ^2 expresses whether the bivariate model is significantly different from the null model. N is lower for "opposition bloc" due to missing values for Bahamas, India, pre-1976 Portugal, and pre-1977 Spain for which reliable parliamentary data is unavailable.

Similar to the descriptive statistics, *Opposition Bloc* again demonstrates the counterintuitive positive (and statistically significant) relationship. The results are more encouraging, however, when the dependent variables are regressed on the multiplicative term. Opposition blocs in parliamentary systems indeed have the expected effect. For all types of disputes (including forceful disputes), when the size of the major opposition bloc in parliament increases, the likelihood of conflict initiation decreases. The chi square results also indicate that this bivariate model is significantly different from the null. This provides additional support for my hypothesis that parliamentary opposition, contrary to the expectation of veto player theory, does in fact constrain the initiation of conflict. Interestingly, *Parliamentary*, when considered alone, has a negative and statistically significant relationship with both dependent variables.

To this point, the data do not fit the expectations of Tsebelis's veto player theory, but do provide some initial support for my hypotheses regarding institutional context. Large legislative oppositions are indeed associated with a lower likelihood of conflict initiation. This effect is most pronounced in parliamentary systems, directly opposing the expectations of veto player theory. The data seem less persuasive, however, for opposition blocs outside of parliamentary systems. It seems that large opposition blocs in non-parliamentary systems—especially in the single case of presidentialism—have a positive relationship with conflict initiation.

While the descriptive statistics and bivariate regressions provide some initial evidence to support my hypotheses, they do not represent clear causal relationships between executive-legislative institutional context and conflict initiation by democracies. For this, I again turn to the multivariate research design utilizing GEE models. Table 5.7 present the results of the multivariate design of the impact of executive-legislative institutional context on conflict initiation. Models 9 and 10 display the results for the dependent variable "initiate any dispute,"

and models 11 and 12 display the results for "initiate forceful dispute." I control for the same variables as in previous multivariate model specifications.

Table 5.7. Executive-Legislative Institutional Context and Conflict Initiation

Variable	*Model 9*	*Model 10*	*Model 11*	*Model 12*
Dependent Variable	Initiate Any Dispute	Initiate Any Dispute	Initiate Forceful Dispute	Initiate Forceful Dispute
Opposition Size	-5.178 (3.493)*	-6.170 (3.493)**	-3.429 (3.116)	-4.732 (3.130)*
Opposition Bloc	5.770 (4.111)	10.739 (4.170)***	3.516 (4.111)	9.678 (3.343)***
Opposition Bloc* Parliamentary		-5.359 (2.770)**		-7.060 (1.687)***
Parliamentary	1.665 (.780)**	3.515 (.967)***	1.712 (1.163)*	4.018 (1.137)***
Presidential	.922 (1.081)	.264 (1.194)	.819 (1.166)	-.193 (.963)
Treaty Ratification	-.391 (.309)	-.402 (.311)*	-.484 (.512)	-.516 (.518)
Participation	-.039 (.019)**	-.043 (.020)**	-.041 (.025)**	-.041 (.022)**
Polity	-.045 (.151)	-.063 (.131)	-.057 (.168)	-.069 (.149)
Major Power	1.187 (.272)***	1.189 (.286)***	.984 (.456)**	1.011 (.490)**
Autocratic Borders	.903 (.127)***	.855 (.131)***	.821 (.159)***	.759 (.139)***
Alliances	.379 (.197)**	.367 (.204)**	.220 (.136)**	.216 (.146)*
IGOs	.006 (.014)	.007 (.014)	.001 (.019)	.001 (.019)
Openness	-.016 (.004)***	-.016 (.004)***	-.025 (.009)***	-.026 (.009)***
Constant	-1.582 (1.756)	-2.309 (1.701)	-.661 (2.034)	-1.836 (1.634)
N	907	907	907	907

Table 5.7. (continued)

χ^2	1132.68***	1896.99***	300.14***	781.25***
% Correctly Pred./Null Model	87.8/82.2	88.3/82.2	89.7/87.8	89.5/87.8
PRE	.31	.34	.16	.14

Notes: The top numbers refer to regression coefficients obtained from GEE-logit analysis with AR-1 autocorrelation. Robust standard errors are reported for each coefficient in parentheses. All significance tests are one-tailed except for *Opposition Bloc, Presidential, Polity, IGOs,* and the *Constant*, which are two-tailed. *p<.10; **p<.05; ***p<.01

In model 9, *Opposition Size* has a statistically significant, negative relationship with "initiate any dispute," supporting my hypothesis that larger legislative opposition constrains the executive use of force. This is what we would expect given a competitive institutional context between the executive and legislative branches. *Opposition Bloc*, on the other hand, does not show a significant relationship, although the direction of the relationship is counter-intuitively positive. It seems that this variable does not have any significant independent effect on the likelihood of dispute initiation. Two domestic institutional variables, *Parliamentary* and *Participation*, demonstrate a statistically significant relationship with the dependent variable. Again, there are mixed results between macro-institutional structure and dispute initiation. Parliamentary systems have a positive effect of dispute initiation, indicating that these systems are more likely than others to initiate conflict. Presidential systems, on the other hand, do not demonstrate any causal relationship with the dependent variable. Electoral participation has a negative and statistically significant effect, indicating that the greater the electoral participation by the citizenry, the less likely a democracy is to initiate any dispute. This supports previous model specifications, as well as the empirical findings of Reiter and Tillman (2002) who found

that participation in elections is indeed evidence of the importance of public constraints on democratic foreign policy.

Model 9 also indicates that all three realist factors (major power status, more numerous borders with autocracies, and more alliance ties) increase the likelihood of a democracy initiating any dispute. The remaining two variables, *IGOs* and *Openness*, provide mixed results for liberal arguments concerning conflict initiation. International organizations do not appear to influence the initiation of conflict by democracies, but economic openness does. Similar to previous specifications, more economically open democracies are less likely to initiate conflict.

Model 9 is statistically different from the null model as indicated by the chi-square results. It correctly "explains" 31% more cases than the null model, predicting 87.8% of all cases correctly. This model thus provides support for the proposition that an increase in legislative opposition size, signifying a competitive institutional context, increases the likelihood of conflict initiation by democracies. The results do not yet support, however, the hypothesis that sizable opposition blocs constrain the use of force. This latter finding is not unexpected given the skewed nature of the data where opposition blocs in presidential systems are markedly larger than those in parliamentary systems.

Given the empirical difference between parliamentary and presidential opposition blocs, as well as the divergent theoretical expectations of my hypotheses compared to Tsebelis's theory, Model 10 includes the interactive term, opposition bloc size in parliamentary systems. In this model, *Opposition Size* again has a negative and statistically significant relationship with dispute initiation, confirming my theoretical expectations. *Opposition Bloc* has a positive and statistically significant effect, however, conforming to earlier patterns. It seems that an increase in the size of legislative opposition blocs does not constrain the use of force. It must be pointed

out, however, that the effect of this variable may be more modest than it first appears. Given that the interactive terms separates the effect of parliamentary opposition blocs, this variable is likely driven by oppositions in presidential systems. Nevertheless, this variable does not match the expectations of my hypotheses.

The results for *Opposition Bloc*Parliamentary* definitely do match the expectations of my hypotheses, confirming that, in parliamentary systems, executive-legislative institutional context matters in determining the likelihood of conflict initiation. For veto player theory, legislative opposition in parliamentary systems might be among the most unlikely sources of constraint on the executive in that it constitutes neither a partisan nor an institutional veto player. The variable, however, shows both a negative and statistically significant relationship with dispute initiation. In support of my hypotheses, oppositions in parliamentary systems do indeed constrain the executive use of force.

The remaining variables demonstrate consistent results with the previous model specification with the exception that *Treaty Ratification* actually reaches the 90% level of statistical significance. In this model, additional legislative powers regarding foreign policy reduce the likelihood of conflict initiation. The goodness-of-fit statistics show a slight improvement with the addition of another theoretically relevant variable. Model 10 is statistically different from the null model as indicated by the chi-square results. It correctly "explains" 34% more cases than the null model, predicting 88.3% of all cases correctly. This model thus provides support for the proposition that an increase in total legislative opposition size in all democracies, as well as opposition bloc size in parliamentary systems, decreases the likelihood of conflict initiation by democracies.

The next two columns display the results for the dependent variable "initiate forceful dispute," duplicating the previous two model specifications. The explanatory variables show consistency in the direction of effect, but the results are, on average, less robust. In model 11, the exclusion of the interactive terms results in none of my theoretical variables of interest reaching established thresholds of statistical significance. The control variables, however, demonstrate similar results to the previous models. Parliamentary systems are more likely to initiate conflict; the coefficient is positive and statistically significant ($p<.10$). Electoral participation and economic openness both reduce the likely of conflict initiation (both coefficients are negative and significant). All of the realist variables also demonstrate robust and expected effects. Diagnostic statistics indicate that model 11 fits the data better than the null model, correctly predicting 89.7% of the cases. The PRE statistic shows a comparatively small amount of additional variation explained by the model (16%), but this is again likely due to the fact that there are so few cases in which democracies initiated a forceful dispute.

The final GEE specification is presented in the last column. The inclusion of the interactive term improves the performance of my theoretical variables significantly, suggesting that the previous model was underspecified. In model 12, *Opposition Size* demonstrates the hypothesized negative relationship. The variable is statistically significant indicating that there is a robust relationship between larger legislative oppositions and constrained executives. *Opposition Bloc* again demonstrates a positive and significant relationship, confirming earlier findings. The interactive term shows a robust, negative relationship with the dependent variable, again verifying that, contrary to the expectations of conventional veto player theory, opposition blocs in parliamentary systems indeed constrain the initiation of conflict. The control variables all demonstrate comparable results to previous models. Model 12 is statistically different from

the null model as indicated by the chi-square results. It correctly "explains" 14% more cases than the null model, predicting 89.5% of all cases correctly.

The results presented for executive-legislative institutional context demonstrate that partisan opposition in the legislature is a significant constraint on democratic war-making. This relationship is particularly strong for total opposition size, indicating that as the executive loses legislative support, it is less likely to initiate conflict. The results were somewhat more mixed when legislative opposition was measured as only opposition blocs (the major opposition party or bloc of parties that oppose the executive). While larger opposition blocs surprisingly had either no effect or a positive effect on the likelihood of conflict initiation for all cases, the effect of this variable was robust and negative for parliamentary cases. That is, opposition blocs in parliamentary systems did in fact decrease the likelihood of conflict initiation. The addition of the interactive term certainly increases the precision of our understanding of executive-legislative constraints as they apply to foreign policy while also directly refuting the claims of Tsebelis. Parliamentary opposition blocs do in fact constitute a "veto player," an insight that veto player theory can not accommodate. Indeed, when the major opposition party or bloc of parties in parliament increases, the context (or setting in which veto players operate) between the executive and legislative branches becomes more competitive, constraining the use of force.

On balance, then, the findings on this dimension of institutional context improve our understanding of the relationship between executive-legislative relations and the propensity to initiation conflict. When institutional context is competitive between the executive and legislative branches (indicated by large partisan oppositions in the legislatures), executives are more constrained in the use of force as a foreign policy tool. On the other hand, a collective institutional context, represented by smaller oppositions to the executive, has an enabling effect

on conflict initiation. These relationships hold even while controlling for realist and other liberal variables, indicting that regardless of macro-institutional structure, power and geopolitical concerns, and interdependence among states, institutional context between the executive and legislative branches has a measurable impact on dispute initiation.

Institutional Context and Conflict Initiation

In this section, the final empirical portion of this dissertation, I offer a more parsimonious examination of institutional context and conflict initiation by democracies, one that facilitates comparison with competing explanations and perspectives. All of the remaining model specifications use generalized estimating equations. I first establish a baseline of the systemic variables: power status, borders with autocratic states, alliances, IGO memberships, and economic openness. By beginning the analysis with a baseline, I can first establish the impact of systemic factors on the initiation of conflict by democracies. Then, by introducing domestic, or unit-level, characteristics, this study can assess the relative utility (or value added) of the liberal approach advocated in this dissertation. Tables 5.8 and 5.9 present the results of the full multivariate design, assessing the impact of democratic institutional context on conflict initiation. Table 5.8 displays the results for the dependent variable "initiate any dispute," utilizing four different models to assess combinations of multivariate equations.

In Table 5.8, model 13 establishes the baseline for this analysis. Four of the five variables demonstrate a statistically significant relationship in the direction hypothesized. Confirming realist expectations, major power status and borders with autocratic states contribute to a greater likelihood of dispute initiation. The variable *Alliances*, while demonstrating the correct sign, does not reach established thresholds of statistical significance. Both liberal variables, *IGOs* and

Openness, demonstrate negative and statistically significant relationships with conflict initiation. It appears that, when considered without domestic institutional variables, dense networks of international organizations and economic openness both have the hypothesized pacific effect on democracies. Model 13 is significantly different from the null model, predicted 84.8% of the cases correctly, an 18% improvement compared to the null. These goodness-of-fit statistics suggest the availability of considerable predictive room for the addition of the domestic institutional explanatory variables.

Table 5.8. Institutional Context and Conflict Initiation (Full Model, Any Dispute)

Variable	*Model 13*	*Model 14*	*Model 15*	*Model 16*
Cabinet Context			.456 (.274)**	
Government Parties				.143 (.061)***
Opposition Size			-5.426 (3.115)**	-6.056 (3.282)**
Opposition Bloc			10.854 (3.983)***	11.244 (4.083)***
Opposition Bloc* Parliamentary			-5.190 (2.788)**	-5.417 (2.686)**
Parliamentary			3.378 (.958)***	3.486 (.977)***
Presidential			.576 (1.090)	.416 (1.147)
Treaty Ratification			-.541 (.239)**	-.433 (.295)*
Participation			-.040 (.020)**	-.039 (.020)**
Polity		.270 (.152)*	-.030 (.126)	-.062 (.125)
Major Power	2.135 (.622)***	2.002 (.522)***	.917 (.264)***	1.169 (.286)***

Table 5.8. (continued)

Autocratic Borders	.495 (.150)***	.617 (.156)***	.781 (.122)***	.752 (.133)***
Alliances	.125 (.185)	.247 (.137)**	.378 (.203)**	.370 (.206)**
IGOs	-.019 (.009)**	-.018 (.010)	.010 (.013)	.005 (.014)
Openness	-.018 (.007)***	-.018 (.008)***	-.020 (.004)***	-.018 (.004)***
Constant	-.865 (.749)	-3.959 (1.718)***	-3.964 (1.297)***	-2.770 (1.547)*
N	1275	1174	907	907
χ^2	92.02***	153.40***	3881.11***	3472.64***
% Correctly Pred./Null	84.8/81.5	86.3/82.1	88.1/82.2	88.2/82.2
PRE	.18	.23	.33	.34

Notes: The top numbers refer to regression coefficients obtained from GEE-logit analysis with AR-1 autocorrelation. Robust standard errors are reported for each coefficient in parentheses. All significance tests are one-tailed except for *Opposition Bloc, Polity, IGOs,* and the *Constant*, which are two-tailed. *p<.10; **p<.05; ***p<.01

Model 14 expands the baseline analysis to include the *Polity* variable to facilitate comparison with this research design's measures of democratic institutions. With the addition of the *Polity* variable, all three realist control variable exhibit positive and statistically significant relationships with the dependent variable. This time, however, *IGOs* does not exhibit a significant effect on dispute initiation, although the sign is in the expected negative direction. *Openness* again demonstrates a statistically significant, negative relationship with dispute initiation.

The *Polity* variable demonstrates a statically significant (p<.10), positive relationship, indicating that more "strongly democratic" systems are more likely to initiate conflict. Similar to

previous model specifications, the variable shows an unexpected relationship when controlling only for systemic level variables. While the variable is only minimally significant in a statistical sense, this particular finding conforms to earlier empirical patterns of mixed results. In the more limited universe of democracies (specifically, the 29 included in this analysis), "democratic-ness" does not have a regular, pacifying effect on the likelihood of dispute initiation. Inclusion of the *Polity* variable does, however, increase the explanatory power of the preceding model. Model 14 is significantly different from the null model, predicted 86.3% of the cases correctly, a 23% proportional reduction in error compared to the null.

To increase the precision of our understanding of the effect of democratic institutions on conflict initiation, models 15 and 16 present the full model for this dissertation. They integrate the remaining domestic institutional variables, including those that capture institutional context. In model 15, the first measure of executive institutional context, *Cabinet Context* has a positive and statistically significant ($p<.05$) effect on conflict initiation. As we move from minority to majority then to coalition cabinets, the likelihood of conflict initiation increases. In model 16, the alternate measure of executive context, *Government Parties*, also has the hypothesized effect. As the number of parties in the governing coalition increases, institutional context becomes more collective and conflict initiation becomes more likely. Unquestionably, the institutional context in which executives operate has a significant impact on the democratic initiation of conflict.

The next three variables test the effect of the second dimension of institutional context. In both models 15 and 16, the nature of legislative opposition has a significant effect in the full empirical specification. *Opposition Size* indeed constrains dispute initiation. This variable is negative and statistically significant ($p<.05$), indicated that as the size of the opposition increases, democracies are less likely to initiate international disputes. *Opposition Blocs* in all

democratic systems seem to increase the likelihood of conflict initiation, but this is due most probably to the relatively large size of all opposition blocs in presidential systems. *Opposition Bloc* size in parliamentary systems, though, serves as a powerful constraint on democratic war-making. The interactive term—included because of the empirical difference between parliamentary and presidential opposition blocs, as well as the divergent theoretical expectations of my hypotheses concerning oppositions in parliament compared to Tsebelis's theory—has a negative and statistically significant ($p<.05$ in both models 15 and 16) relationship with the dependent variable, confirming the expectation that large cohesive oppositions are able to reduce the likelihood of the executive use of force abroad.

The macro-institutional variables again show mixed results. Parliamentary systems are systematically related to higher levels of dispute initiation, but presidential systems are not. On balance, then, the empirical evidence of the full model support the claim that parliamentary systems are somewhat more likely to initiate conflict because the executive is not constrained by an institutionally independent legislature. It should also be noted that parliamentary systems are, on average, more likely to exhibit a collective institution context that enables the executive use of force. These systems are more prone to produce collective agency than presidential systems through multi-party coalition cabinets and weak legislative opposition, both of which are less likely or constitutionally prohibited in presidential systems. Regardless, institutional context has the hypothesized effect even while controlling for presidentialism versus parliamentarism.

Models 15 and 16 also include the *Polity* variable. This time, *Polity* demonstrates the hypothesized directional relationship, but the results are not statistically significant. Without question, *Polity* is neither a distinguishing measure of institutional variation among democracies (only 23% of the cases in this analysis vary from the modal score of 10), nor is it an explanatory

one. All the baseline control variables included in this equation demonstrate comparable results to the first two equations, with the exception that *IGOs* does not reach statistical significance. All of the realist variables have a positive effect on conflict initiation, whereas as economic openness has a negative effect. In terms of goodness-of-fit, the fully specified models perform exceptionally well. Model 15 predicts 88.1% of the cases correctly, whereas model 16 predicts 88.2% correctly. Compared to the null, both of the models significantly eliminate more prediction error (PRE = .33 for model 15, .34 for model 16).

I test the robustness of these results by employing the second independent variable, "initiate forceful disputes" (Table 5.9). The results for each variable are virtually identical. In models 17 and 18, the systemic variables show consistent results with those presented in the previous table. *Polity* continues to have no systematic effect on dispute initiation. All of the institutional context variables retain their direction and statistical significance as in previous specifications, but *Cabinet Context* and *Opposition Size* reach only the 90% confidence interval. The robustness of *Treaty Ratification* is suspect as it fails to reach statistical significance in model 20. Taken together, though, the effect of the main theoretical variables is consistent, indicating that these are efficient measures of how domestic institutions affect conflict propensity among democracies. The goodness-of-fit statistics are less impressive for forceful disputes, however, indicated that although 87.4% (model 17) to 89.4% (models 19 and 20) of the cases are predicted correctly, the proportional reduction in error is rather low. The baseline and *Polity* models "explain" no more than an additional 4% points of the cases correctly. The goodness-of-fit improves markedly, however, for those models that include variables that capture institution context.

Compared to the original systemic-level baseline models and the *Polity* models, the inclusion of domestic political institutions and democratic institutional context has increased the explanatory power of this research design. For all disputes, the explanatory power nearly doubles for the full models compared to the baseline model. For forceful disputes, the relative goodness-of-fit of the full models as compared to the baseline models is even greater. In addition to this statistical benefit, the full models are also more theoretically rich in terms of the institutions and processes identified in the democratic policy making process, a richness overlooked by relying on a uni-dimensional indicator of democracy. The results demonstrate a distinct precision in distinguishing the varieties of democratic institutions that the Polity measure lacks.

Table 5.9. Institutional Context and Conflict Initiation (Full Model, Forceful Dispute)

Variable	*Model 17*	*Model 18*	*Model 19*	*Model 20*
Cabinet Context			.353 (.280)*	
Government Parties				.157 (.042)***
Opposition Size			-4.279 (2.803)*	-4.451 (2.897)*
Opposition Bloc			9.500 (3.190)***	9.748 (3.171)***
Opposition Bloc* Parliamentary			-6.725 (1.789)***	-6.734 (1.575)***
Parliamentary			3.792 (1.082)***	3.873 (1.087)***
Presidential			.097 (.898)	.065 (.931)
Treaty Ratification			-.629 (.456)*	-.562 (.509)
Participation			-.035 (.022)*	-.037 (.022)**

Table 5.9. (continued)

Polity		.188 (.153)	-.043 (.146)	-.072 (.145)
Major Power	1.187 (.635)***	1.599 (.524)***	.791 (.419)**	1.022 (.500)**
Autocratic Borders	.461 (.139)***	.531 (.153)***	.690 (.147)***	.634 (.144)***
Alliances	.012 (.141)	.093 (.012)	.232 (.144)**	.218 (.145)*
IGOs	-.022 (.011)**	-.019 (.012)*	-.002 (.018)	-.002 (.019)
Openness	-.018 (.006)***	-.019 (.005)***	-.027 (.008)***	-.027 (.008)***
Constant	-.929 (.770)	-3.096 (1.703)***	-3.145 (1.213)***	-2.225 (1.593)
N	1275	1174	907	907
χ^2	105.92***	132.29***	666.21***	716.31***
% Correctly Pred./Null	87.4/87.4	88.1/87.6	89.4/87.8	89.4/87.8
PRE	.00	.04	.13	.13

Notes: The top numbers refer to regression coefficients obtained from GEE-logit analysis with AR-1 autocorrelation. Robust standard errors are reported for each coefficient in parentheses. All significance tests are one-tailed except for *Opposition Bloc, Polity,* and the *Constant*, which are two-tailed. *p<.10; **p<.05; ***p<.01

Finally, I report the marginal effects of each statistically significant variable on the initiation of conflict from two full model specifications. Marginal effects are calculated by measuring the change in the predicted probability of conflict initiation after fluctuating each independent variable one standard deviation below the mean to one standard deviation above the mean (except dichotomous variables which are fluctuated from 0 to 1) while holding all other independent variables at their mean values. The results displayed in Table 5.10 show that not only does institution context have the hypothesized effect on conflict initiation, but the

magnitude of that effect is considerable, comparable in some instances to the magnitude of effect for realist factors.

Table 5.10. Marginal Effects of Variables on Conflict Initiation (Any Dispute)

Variable	*Model 15*	*Model 16*
Cabinet Context	+7%	
Government Parties		+4%
Opposition Size	-17%	-19%
Opposition Bloc	+32%	+34%
Opposition Bloc*Parliamentary	-22%	-23%
Parliamentary	+21%	+22%
Treaty Ratification	-10%	-8%
Participation	-14%	-14%
Major Power	+12%	+17%
Autocratic Borders	+23%	+22%
Alliances	+11%	+11%
Openness	-35%	-31%

Notes: Marginal effects are calculated by measuring the change in the probability of Y after fluctuating each independent variable one standard deviation below the mean to one standard deviation above the mean (except dichotomous variables which are fluctuated from 0 to 1) while holding all other independent variables at their mean values. Only variables that demonstrate statistically significant coefficients are included.

In model 15, the marginal effect of *Cabinet Context* is 7%, meaning that a change of one standard deviation around the mean increases the probability of conflict initiation by 7%. The marginal effect of *Government Parties* (listed in the third column under Model 16) is similar, but

not as great. The effect of *Opposition Size* is larger. In all democracies, moving from a legislative opposition size of 26% to one of 56% (a one standard deviation change around the mean) decreases the probability of conflict initiation by as much as 20%. The effect is greater still for opposition blocs in parliamentary systems.

A change in the size of parliamentary opposition blocs from 8% to 46% decreases the probability of dispute initiation by 22%-23%. The interpretation of the marginal effect for *Parliamentary* is more straight-forward. Holding all other variables at their mean values, parliamentary systems initiate conflict 22% more than non-parliamentary systems in model 16. This is persuasive evidence in support of the hypothesis that parliamentary systems face fewer constraints in decisions to initiate conflict.

The interpretation of the marginal effects of *Participation* is interesting. Going from a participation rate of 29% (a low, but plausible, turnout for many national elections) to 61% decreases the probability that a democracy will initiate conflict by 14%. Clearly, voter participation is a tangible constraint on the initiation of conflict by democracies. The marginal effects of the realist variables are similar to the other variables in terms of the magnitude of effect, but the marginal effect of economic *Openness* is striking. While controlling for all other variables, a change of one standard deviation around the mean decreases the probability dispute initiation by over 30%.

All told, there seems to be considerable utility, or value added, in the approach advocated in this research design. The evidence presented in these models unambiguously indicates the importance of institutional context on democratic foreign policy making even while controlling for a host of other factors previously cited by scholars of international relations.

Summary of Results

This chapter has analyzed the effects of domestic institutions on the initiation of conflict by twenty-nine established democracies from 1945 to 1992. In doing so, I have challenged two prominent literatures, the democratic peace thesis and veto player theory. Both of these perspectives maintain that the democratic use of force is constrained by separated institutions of government and the placement of political actors whose agreement is required for policy decisions. I argued differently, positing that *institutional context*—the setting in which veto players operate—is the chief dimension along which democracies vary in their international conflict behavior. The findings of this dissertation indicate that the initiation of conflict is determined not by any static institutional organization (as postulated by the democratic peace literature), or on any rigid distribution of veto players (as advocated by conventional veto player theory), but on a more dynamic dimension of institution context.

On the first dimension of institution context, the nature of the ruling coalition, my findings indicate no support for Tsebelis's claims that more veto players in the executive hinder the policy making process by increasing the number of political actors whose agreement is required for a policy decision. Rather, it seems that precisely because agreement is required among more numerous partisan veto players, collective agency emerges among partners in coalition cabinets. This collective agency, in turn, allows a greater degree of flexibility in the executive's use of force. In support of the findings of Crepaz (2002), it seems that parties in government coalitions do in fact prefer logrolling to obstruction in the policy process. Moreover, given that coalition members share political responsibility and are judged collectively by the electorate, coalition members in fact have a great incentive to support, not constrain, executive actions *including the use of military force*. Thus, a collective executive institutional setting,

defined by shared responsibility and collective agency among coalition cabinets with multiple parties, facilitates the initiation of conflict by democracies. A competitive institutional executive context has the opposite effect. Majority and minority cabinets, which operate with the absence of collective agency among multiple partisan actors and shared political responsibility, are much less likely to initiate conflict.

On the second dimension of institutional context, executive-legislative relations, the findings indicate that partisan opposition in the legislature is a significant constraint on democratic war-making. This relationship is particularly strong for total legislative opposition size, indicating that as the executive loses partisan support, it is less likely to initiate conflict. The results were somewhat more mixed when legislative opposition was measured as the size of the major opposition bloc. While large opposition blocs did not constrain the use of force in all systems, the constraining effect of coherent opposition blocs was particularly pronounced for parliamentary systems. Parliamentary opposition blocs do in fact constitute a "veto player," an insight that veto player theory has not yet recognized. The results thus demonstrate that an increase in total legislative opposition size in all democracies, as well as opposition bloc size in parliamentary systems, decreases the likelihood of conflict initiation by democracies.

Thus, the findings of this dissertation in no way support the claims of Tsebelis' veto player theory. Multiple partisan partners in the executive do not constrain the use of force, and parliamentary opposition does. It is also evident that these indicators of institutional context have the same effect in each type of democratic system (whether parliamentary, presidential, or semi-presidential). That is, regardless of macro-institutional structure, when institutional context is collective, conflict initiation is more likely that when it is competitive. I also found no evidence to support liberal claims that "more democratic" countries are any more pacific than "less

democratic" ones. While the results of this dissertation indicate that the effect of democracy on international conflict is in fact monadic, the effect is more complex than as postulated by the democratic peace thesis. Other domestic variables that prove to decrease democratic belligerence include voter participation and, to some extent, legislative powers over foreign policy. Finally, the findings of this dissertation confirm that institutional context operates even while controlling for the positive effects of power status, alliances, and borders with autocratic states, as well as the depressing effects of economic interdependence. What is more, the magnitude of the effect of institutional context is comparable to that for realist and other liberal factors.

CHAPTER 6

CONCLUSIONS

I began this dissertation with a question: are some democracies more likely than others to initiate conflict? Are some democracies, in other words, simply more "dangerous"? While one can not conclude that any single democracy is always, or inherently, more dangerous than another, my theory and evidence regarding institutional context point to some definable circumstances under which democracies are the most likely to initiate conflict should their executives so choose to follow such a course. In brief, because of the lack of the motivation or opportunity for political actors to oppose an executive who endeavors to initiate conflict, those democracies that exhibit a collective institutional context are the most dangerous.

A collective institutional context emerges from broad multiparty cabinets and the absence of a large legislative opposition to the executive. In the former, coalition partners share political responsibility and accountability, thereby infusing powerful motives to facilitate policy production rather than oppose it. In this sense, there are congruent interests among each of the partisan veto players within a coalition to maintain its position the government and keep the coalition intact. This is not to say, of course, that coalition cabinets have more belligerent preferences than other types of cabinets. Rather, when contemplating the use of force, there is much greater freedom of choice for an executive under a collective institutional setting for there are fewer institutional barriers—and greater political support. In the latter, weak political

oppositions do not have the opportunity to redirect or veto public policy for they either do not control an institution with veto power (in the case of presidential systems) or they can not field a viable policy alternative (in the case of parliamentary systems). Weak oppositions, then, further enable democratic belligerence.

Democratic pacifism, on the other hand, is most prominent under a competitive institutional context where motive and opportunity to constrain democratic belligerence is most apparent. Institutional competitiveness emerges from smaller ruling coalitions and strong legislative opposition. Unlike coalition governments that hold a large share of parliamentary votes and whose members act collectively, single party majority governments command much less legislative support and typically face more hostile and stronger oppositions. The constraints facing minority cabinets are even more pronounced as they face even larger oppositions and encounter more veto players who have motive to oppose executive policy.

Independent of these attributes regarding ruling coalitions, strong political oppositions in legislative chambers further limit an executive's use of force. Whether in presidential or parliamentary systems, political oppositions have strong motive to oppose executive attempts to initiate conflict for they seek to gain political office or redirect public policy in a different direction. In presidential systems, when the political opposition controls the legislative branch (i.e. in the case of divided government), the opposition has sufficient opportunity to constrain executive belligerence. In parliamentary systems, political oppositions have opportunity to hinder executive policy as long as it is sufficiently strong to expose deficient government policies and offer viable alternatives. In these instances, a competitive institutional limits the initiation of conflict by democracies.

Clearly, then, institutional context mediates, and even alters, the impact of democracy (including democratic institutional structure) and veto players on the initiation of conflict. In the remaining pages of this conclusion, I consider the implications of relying on institutional context as a concept for refining how scholars think about domestic politics and international relations, both in terms of contemporary scholarship and future research.

Veto Players

One of the most robust conclusions of this dissertation is that veto players have a much more dynamic effect on policy production than postulated by the conventional wisdom. The inclusion of multiple veto players in the policy process does not always constrain or block the policy production. Rather, the effect of additional veto players depends on the context within which they operate. This context, in turn, is defined by more than just the opportunity to block the policy process; political actors must also have the motive to veto. Defining the motivation underlying veto player behavior is thus a critical step in unraveling their behavior and resolves the puzzling relationship alluded to throughout this dissertation: veto players do not always block policy production because they do not always have the motive to do so.

An argument centered on institutional context therefore refines how scholars employ veto player theory as an explanation for the impact of domestic politics on aggregate policy outcomes. While the concept is not particularly new, veto player theory is fast becoming one of the more often utilized concepts in the political science literature. The increased popularity of veto player theory has not, however, been accompanied by very rigorous critical examination of its basic ideas and assumptions. In this study, I have scrutinized some of the basic formulations underlying veto player theory, finding that while it is accurate to say that different democratic

systems produce a varying number of veto players who have the opportunity to block policy, these veto players do not always have the motivation to do so. In this manner, the argument and findings presented in this dissertation are congruent with those of Crepaz. Veto players, we may now conclude, are clearly not always motivated to veto, a conclusion that is not yet widely recognized in the literature. Similar to Crepaz's findings, this dissertation supports the conclusion that democratic policy outcomes (in this case, a foreign policy to initiate conflict) are determined not by a rigid distribution of veto players, but on a more dynamic dimension of institution context.

In addition to finding that the effect of veto players depends on institutional context, this dissertation also found that veto behavior arises where we previously did not expect. The findings presented in the previous chapter point to the conclusion that all political oppositions, not just those in presidential systems, have a constraining effect on the democratic use of force. In fact, the strongest finding of this dissertation in regard to legislative oppositions is that large parliamentary opposition blocs have a particularly constraining effect on the executive's use of force. For veto player theory, legislative opposition in parliamentary systems might be among the most unlikely sources of constraint on the executive in that it constitutes neither a partisan nor an institutional veto player. The findings of this study, however, clearly establish that oppositions in parliamentary systems do indeed constrain the executive use of force—in short, parliamentary oppositions can be veto players. Precisely because this is at odds with Tsebelis's formulations, finding that oppositional forces in parliament do in fact inhibit executive policy making constitutes an important contribution to veto player theory. Given the hitherto dearth of research in the veto player literature even considering the constraining effects of parliamentary

opposition, future research should therefore accommodate the potential "veto power" of these oppositions.

One caveat, though, should be noted. While my findings indicate that institutional context has a robust and predictable effect on the initiation of conflict, I did not test my theoretical formulations on other substantive issues such as those traditionally studied by veto player scholars. Does institutional context also have a similar effect on welfare provisions (as Crepaz concentrates his research), law production more generally (as Tsebelis focuses his studies), or the wide variety of issues that scholars have approached using veto player theory? In many ways, international conflict initiation may be an "easy case" in which to test the effect of institutional context because the constraints facing the executive are so clear. In cases of war, the policy of committing to conflict initiation has a definable temporal and spatial context: an executive is faced with a highly salient decision that has immediate consequences and effects, even if their extent is not completely known. In this manner, should an executive wish to commit to a belligerent course of action, theorizing about the constraints he faces is straightforward.

On other issues, however, the constraints facing the executive are less clear and more difficult to define. How much opposition or constraint might an executive expect to face on such issues as welfare expansion or budget cuts? On such issues—when the question is perhaps less salient to the political actors involved and when there are fewer temporal constraints that may allow for greater amounts of compromise and negotiation—we might expect more complexity in the effect of domestic institutions on the policy process. I would submit, however, then even though substantive differences from issue to issue may affect the degree to which institutional context matters, the additional utility gained from this theoretical perspective would likely bear additional insights. Since the perspective offered here relies on the theoretical research of those

in other substantive areas (particularly the work of Crepaz on social welfare policies), my hypotheses derived from institution context are likely to produce similar, if even less robust, results.

For example, I argue in this dissertation that the executive is afforded greater flexibility in the use of force when his cabinet is composed of multiple partisan members. With such a coalition, there are motives for each partisan partner to support, not constrain, an executive action to use military force with the additional benefit of less opposition in the parliament (for there is greater support for the executive). If coalition members support an executive's initiation of conflict, the coalition is a successful one. If, however, some coalition members oppose an executive's use of force, internal cabinet dissent will cause the governing coalition to fail. Therefore, a multi-party executive, existing in a collective institutional context, is relatively "unencumbered" to initiate conflict: all the members of a coalition support a belligerent executive foreign policy so as to remain in the governing coalition even if one partisan member prefers a more pacific course. We would expect similar effects, as confirmed by the research of Crepaz, on an issue as different as welfare policy. As he demonstrates, the very same causal logic operates for multi-party cabinets: each member of the coalition supports other members' welfare policy efforts (i.e. they logroll) so as to maintain the coalition and their place in it.

Nevertheless, only further testing of my hypotheses regarding institutional context on other substantive issues will indicate its usefulness in areas outside of studies of international conflict among democracies. The insights garnered here regarding the mitigating forces of institutional context on the constraints facing executives in the policy-making process more generally, however, are an excellent starting point for such future studies.

The Democratic Peace and Beyond

In addition to the aforementioned contributions to veto player theory, the argument and findings presented here have several important implications in terms of how scholars think generally about domestic politics and international relations. First, this dissertation adds to our understanding of how different domestic political institutions among democracies shape variations in their foreign policy behavior. Specifically, this research indicates considerable utility in closely scrutinizing the varieties of democratic institutions and their impact on the international relations among states. Moving beyond conventionally employed measures of democracy that are either dichotomous or uni-dimensional, I have employed more descriptively rich and theoretically accurate indicators of democratic institutions. The results indicate considerable utility in such an approach, providing a more complete understanding of which institutions of democracy affect foreign policy.

Traditionally, international relations scholarship largely neglects the internal character of states as a significant explanatory factor in international relations, especially within "neorealist" and "neoliberal institutional" scholarship. Even when scholars do argue that the domestic attributes of the state matter, they do so only is sweeping generalizations. Especially within liberal scholarship for instance, research typically focuses on the difference in behavior of democratic states versus non-democratic states without differentiating the circumstances under which democracies themselves pursue belligerent foreign policies. In a recent review article of the democratic peace research program, for instance, John MacMillan writes that those working within this genre "will have to devise an alternative to the separate [democratic] peace theory's reliance upon regime type to differentiate between those circumstances in which liberal state do

and do not fight" (2003: 241). It is insufficient, in other words, to rely on static conceptualizations of domestic regime type to explain the conflict behavior of democracies.

The research presented here informs these lines of research. To the realist, the results of this analysis indicate that the internal characteristics of the state are important explanatory variables of international behavior. Indeed, democracy is a powerful mediator to what realists consider the deterministic effects of international anarchy and the distribution of power. Democratic institutions, and specifically institutional context, have a regular and predictable effect on rates of conflict initiation.

To the liberal, the results presented here indicate that the type of democracy matters: there is more than a single, regular relationship between regime type and international behavior as many theories of international relations, especially the democratic peace, suggest. It seems that the choice of democratic institutions, and their political control, has a significant and predictable impact on the international behavior of states. So differentiating democracies therefore resolves the puzzle to which this dissertation first alluded: equally democratic states demonstrate widely divergent foreign policies because their political institutions, and control of them, vary so considerably.

This dissertation further clarifies the democratic peace thesis by demonstrating that its purportedly dyadic effects are also monadic. The very premise of the liberal democratic peace is that democracies do not go to war with other democracies, but still go to war with non-democracies. The results presented here, however, support some of the more contemporary findings indicating that democratic institutions do have an impact of the conflict behavior of states *even outside the cases of democratic dyads*. Particularly in relation to the structural mechanisms cited by democratic peace scholars, it appears that at least some of the institutional

constraints that prevent democracies from going to war with one another would operate irrespective of a potential adversary's domestic governing structure.

This monadic finding does not, however, conform to the pattern postulated by democratic peace theorists. It does not seem that the more democratic a state is (as measured by the Polity score), the more pacific a democracy. This finding leaves an important implication in terms of the democratic peace research agenda. As I have argued throughout this dissertation, the effect of democracy on foreign policy behavior is more complex than democratic peace theorists contend. Clearly, relying on a simple uni-dimensional conception of regime type fails to distinguish democracies in their likelihood of initiating conflict for such a classification not only demonstrates little variance among the most democratic states, but also does not succeed in demonstrating any causal relationship. While the absence of monotonic relationship between democracy and conflict initiation may seem at first a controversial finding, it does not refute the proposition that democratic institutions matter in terms of the conflict propensity of states. Rather, it seems that the democratic peace is in need of revision in order to capture this unexpected empirical pattern. It is along these lines that we may conclude that institutional context mediates, and sometimes alters, the impact of democracy on the initiation of conflict.

Finally, more comprehensive examinations into the domestic institutional sources of international behavior along the lines presented in this study should help unravel questions that have vexed international relations scholars. For example, differentiated democracies based on institutional context is significant for diversionary theories of war. Like the democratic peace literature, diversionary theories of war claim that domestic political circumstances affect the likelihood of democracies engaging in war (Levy 1989; Gelpi 1997). There exists an underlying tension between liberal and diversionary claims, however. The former argues that democracies

are less likely to engage in conflict due to institutional and electoral checks on war making, whereas the latter claims that democracies are more prone to conflict in response to domestic discontent. While the two literatures are based on distinct theoretical and methodological premises, they arrive simultaneously at conflicting conclusions. By distinguishing more clearly how domestic institutions affect the foreign policy making of democracies, an argument centered on institution context mediates the tension between these perspectives.

Institutionally, for instance, we might expect minority governments (followed by majority governments) to be more likely to use diversionary force because they have the greatest incentive to do so. Minority governments in particular may face the greatest opposition and most competitive institution context that may require a diversion to avoid. On the other hand, broad multiparty coalitions have the fewest incentives to use diversionary tactics because they face the fewest domestic political problems that might require diversions. In the case of political opposition, diversionary theories predict that strong political opposition should encourage diversionary tactics such as conflict initiation. President Bill Clinton, for instance, was often accused of diversionary tactics because of domestic problems stemming largely from acute political opposition. Diversionary theories thus suggest that minority governments with strong political opposition should be the most likely to initiate conflicts. According to my argument, however, minority governments with strong political opposition are the least likely of all institutional contexts to encourage conflict initiation. Diversionary claims, then, are not supported by this dissertation, as coalition governments with weak political opposition are the most likely of all governments to use force.

Lastly, more thorough theorizing of democratic political institutions may also help arbitrate how democratization affects the war proneness of states, a topic of policy relevance

today as those who are building democratic nations seek to consolidate stable regimes. While international relations scholars have theorized that democratizing regimes are more war prone than stable and mature democracies (see Mansfield and Snyder 1995), empirical results to that effect have not been robust, particularly in regard to rates of conflict initiation (Russett and Starr 2000). Closer scrutiny of democratic institutions, however, will likely clarify the conditions under which democratizing regimes are more or less war prone. While new democracies may indeed lack the stabilizing institutions of mature democracies, some institutional configurations should contribute to pacifism in newly democratized states. Specifically, democratizing states that establish sufficient institutional competitiveness domestically should be less likely to initiate conflict than those that allow for considerable institutional collectivity.

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