

A Case for Veto Player Theory

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Introduction

Lave and Marsh's (1975) undergraduate text explains what constitutes a model, and what makes a model good, in the social sciences. While its examples are now sorely outdated, the text remains a classic because of the clarity in which it explains how to build theory. It also provides a nice yardstick to measure the usefulness of a given model. The authors argue that a model is abstract. It simplifies an enormously complex world. It generates hypotheses that are testable and falsifiable. A model is good if it is "truthful" and if it is "beautiful."¹ A "truthful" model should have some accuracy, which in practice means that its implications should receive some empirical support. It should also be generalizable to different cases and different settings. A "beautiful" model is one that is simple and that generates counter-intuitive implications.

Based on Lave and Marsh's (1975) definition, I argue that veto player theory constitutes a good model. This model is both abstract and testable. There is enough empirical evidence to suggest that the model's implications have some "truth." One can apply the theory across different cultural and political settings. While one can make the discussion of veto players more complex through the use of geometry and even calculus, the basic point of the model is simple—increase the number of veto players as well as their ideological distance from one another and one decreases the degree of change from the status quo.² There is little doubt that this model is a beauty according to Lave and Marsh's definition.

However, while the veto player model may be "good," it has not yet achieved the greatness that it ultimately deserves. This essay begins with a discussion of the definition of "veto players." It focuses on the definition that Tsebelis provides, but it also presents alternative conceptualizations. The discussion places an emphasis on how the authors translate the theory into testable concepts. The section concludes with a review of the difficulties in operationalizing what constitutes a "veto player" in practice, with some of the problems conceptual and others practical. The second part of this paper evaluates the empirical evidence in support of the approach. To date the evidence is quite encouraging, and the findings are more supportive of the veto player approach than findings for most theories that are no more than a decade old. However, there are limitations in the cases scholars have studied as well as in the policy areas they have explored. Most empirical studies examine the effects of veto players in developed countries with parliamentary systems. They also generally restrict their dependent variables either to law production or to some type of economic policy. The relative "thinness" of the empirical literature does suggest that much more remains to be done given the promise of the approach.

¹ Astute readers will note that I am not including their third category in which to evaluate a model, namely "justice."

² I would argue, in fact, that the use of geometry and calculus potentially simplifies the model even further.

Veto Players: Definitional Issues

The veto player approach in comparative politics is young; indeed, it is difficult to find works more than a decade old that explicitly considers “veto players.” Immergut (1992) and Huber, Ragin, and Stephens (1993) discuss “veto points” and “constitutional structures” respectively, and, as indicated below, each of these concepts is at least in the veto player family. Tsebelis (1995) is the key article that introduces the term “veto player.” Since his 1995 article the term has come into vogue. Yet, as Tsebelis (1995; Forthcoming) himself points out, this approach is rooted in a rich tradition that considers the role of institutions. The potential policy effects of multiple “checks and balances” shows up at least as early as Montesquieu (date), and this argument motivates Madison (year) in Federalist Paper #10. Romer and Rosenthal (1978) use spatial models to develop an argument about the importance of agenda-setting control as well as the critical role a “reversion point” can play in determining the final outcome. Scholars of presidential systems may wonder what is new about any discussion of a “veto player.”

Nevertheless, there *is* something new, and the contemporary use of the concept is most associated with George Tsebelis’ work (1995, 1999, 2002). I begin my discussion with his definition: “A veto player is an individual or collective actor whose agreement is required for policy decisions (1995, 293).” In principle, the relevant veto players vary across countries and even within countries. In the American system, for example, the president is usually a relevant veto player because his assent is needed for a bill to become law unless 2/3 of the House and the Senate override his veto. In Germany, the President is expected to sign all legislation unless he regards it as unconstitutional. The president is therefore a relevant veto player in the United States but not in Germany.³ The relevant player(s) can also vary within countries. Tsebelis (1995) points out that on defense policy one Senator, Sam Nunn, constituted a “veto player” by himself, while in other dimensions he was simply a member of the Democratic Party.

To understand the relevance of different veto players one needs to know three things: their number, their congruence in relevant policy directions, and their cohesion (Tsebelis 1995, 301). If there is just one veto player, then that player gets its policy choice, and there remains nothing more to explain. If there are multiple players, then one must determine where they stand on relevant policy issues and whether it is realistic to treat them as one actor. If there are many actors but they all hold the same policy preferences, then according to the “absorption rule” the researcher should treat them all as one. Cohesion refers to the policy positions of the different individual actors that constitute the collective actor (311).

How should one apply this argument in empirical work? Tsebelis (1995) focuses especially upon how to define and to count relevant veto players. What one does

³ Note that I wrote this sentence *before* the recent Bundesrat decision to approve the government’s immigration bill. For this bill, it appears that the German President, Johannes Rau, will serve as a true veto player. Yet such occasions are quite rare.

ultimately depends upon the type of study one is undertaking. If one is doing a case study, then it is important to examine all relevant players. Less frequent veto players, such as the courts, referenda, or even a single legislator like Sam Nunn, can constitute a veto player and must enter the analysis.⁴ If one is doing a large-n study, the less frequent players essentially cancel out and become background “noise.” The scholar should focus on the relevant institutional and the relevant partisan veto players. The American Constitution requires that the House, the Senate, and the President approve most types of bills unless the Congress can overrule the President’s veto. There are therefore three institutional players in the United States. One then determines the number of partisan veto players whose assent is needed in the institutional veto players while keeping the “absorption rule” in mind. The United States has one veto player when a single party controls all three institutional veto players (President Clinton’s Administration in 1993-94) or two veto players at all other times. In countries with only one institutional veto player, the number of veto players is simply the number of parties in government. One should note that, according to Tsebelis (1995), whether the government has majority status in parliament does not matter. Minority governments as a rule hold a central position in the ideological spectrum, and, combined with their agenda-setting powers as the party in government, the outcome is essentially the same one that would have existed under a majority government with the same parties.

While Tsebelis (1995) discusses the three factors of number, ideological distance, and cohesion, the focus on how to use the approach in practice is on number. He does not provide a guide how to apply the concept of ideological distance in empirical work. The discussion of cohesion, which notes that knowledge of the spatial arrangement of individuals who compose a collective veto player is necessary to know for sure the collective player’s cohesiveness, makes it a seemingly Herculean task to determine cohesion empirically in large n studies.⁵ Yet in practice it is often not unreasonable to consider parties as cohesive and to count each distinct party as one veto player.

Tsebelis (1999), Tsebelis and Chang (2001), and Tsebelis (Forthcoming) consequently refine the application of the model further. The emphasis in the first two works is much more on the ideological distance among veto players. In Tsebelis (1999) he adopts the model to a single, left-right dimension. He computes two variables in practice—a “range” variable, which computes the absolute value of the ideological distance between the two players that are most distant, and an “alternation” variable, which computes the mid-range position of each coalition and takes the absolute difference between two successive coalitions. His prediction is that the greater the ideological distance between the veto players the lower the probability that there will be significant change from the status quo. Moreover, the

⁴ It should be noted that Tsebelis makes this argument in his original formulation of the theory. Many authors focus only on his coding rules for large n studies, which are necessarily less detailed.

⁵ Tsebelis 1995 does provide what are essentially rules of thumb—cohesion should increase the greater the number of people who compose a collective actor. It also increases the smaller the ideological distance among individuals that compose the collective player.

larger the alternation the greater the probability of change, as governments that are much different from previous governments will want to move away from the status quo. In Tsebelis (1999), the empirical focus is on significant law production in 15 European parliamentary democracies during the time period 1981-91. To compute his two independent variables he averages the standardized values of three measures of partisanship: Warwick (1994), Castles and Mair (1984), and Laver and Hunt (1992).⁶ He finds that both “distance” and “partisanship” have significant effects upon the number of laws produced in these democracies.

Tsebelis and Chang (2001) take the analysis to the budgetary realm. They find that increases in “distance” lead to smaller changes in budget composition across 19 advanced industrial countries during the time period 1973-95.⁷ Similarly, a larger “alternation” in the average position of the veto players leads to greater change in the composition of the budget. In their empirical tests they use the same measure for ideological distance with one change—they take the average of the ideological distance of the three measures in Tsebelis and average this figure with ideological distance in the foreign policy dimension provided in Laver and Hunt (1992), which is the given party’s position vis-à-vis the Soviet Union. In order to determine the location of the previous government’s status quo in order to calculate the measure of “alternation,” Tsebelis and Tsebelis and Chang assume in practice that it is the average position of the government; as Tsebelis and Chang write, “we will consider the average position of the veto players of the previous government as a proxy for the position of the status quo (17).”

Finally, Tsebelis (Forthcoming) addresses most completely the issue of cohesion of collective veto players. In Chapter 2, he derives the finding that the type of decision rule within a collective veto player is critical. Under simple majority voting, more cohesion leads to higher policy stability, while under qualified majority voting cohesion leads to *less* policy stability. Yet he also notes that in practice “collective veto players approximate the behavior of individual ones. We can approximate their preferences by a wincircle (which includes the actual winset) whether they decide by simple or qualified majorities...(91)”

A theoretically powerful claim of this approach is that it can integrate other types of institutional analyses into its framework. The key difference between autocracies and democracies is the way these systems select their veto players. One moves to a democracy when there is competitive selection of veto players. Within the democratic institutions literature, Lijphart’s (1999) majoritarian and consensual forms of democracy can be reformulated as systems with few (or one) veto players and systems with many veto players. This approach also clarifies normative discussions about the choice of institutions. If one desires a government that changes policies quickly, then one should choose a system with one veto player. If one wants to make

⁶ Note that in this article Tsebelis uses the question “increase services vs. cut taxes” to code the left-right scale in Laver and Hunt 1992.

⁷ They compute change in budget composition as the square root of the square of the change in nine budget categories as provided by the IMF’s *Government Finance Statistics Yearbook*.

it difficult for the government to change the status quo unless there is general consensus in society, one should design a system with multiple veto players that have different policy preferences.

Tsebelis (Forthcoming) also considers differences between parliamentary and presidential systems. In Tsebelis (1995), he treats presidential and parliamentary systems equally concerning the “absorption” rule—if a political party controls two institutional veto players, there is only one veto player in practice. In Tsebelis (Forthcoming), he revises this formulation. He argues that the first difference between the two systems concerns who is the agenda setter. In parliamentary systems the agenda setter is the government, while in presidential systems the agenda setter is the legislature on non-financial bills (only). He quickly adds that the actual agenda-setting power of the executive may vary considerably, but he uses the differentiation as part of his working definition of the two types of systems.⁸ The second difference concerns veto player cohesion. Collective veto players, and particularly political parties, are more disciplined in parliamentary systems than in presidential systems. This discussion has important implications for the counting of veto players. He contends that “the fact that parties lack discipline in presidential systems makes it difficult or even impossible to identify the origins of particular votes. As a result, it is difficult to identify partisan veto players in presidential systems. Whenever this is the case, we will be confined to the study of institutional veto players (128).”

Taken together, the theory that Tsebelis has developed over the last seven years is impressive. Tsebelis is certainly correct when he emphasizes that the veto player approach allows one analyze seemingly unrelated theoretical literatures in political science. This is an important point. As Tsebelis (1995, 292) emphasizes, “one important contribution of such an approach is that by permitting a simple and conceptually consistent method of making comparisons across systems, it helps to resolve a pervasive problem of comparative politics: small sample size.”

Additional strengths of the theory include its parsimony and its ready application to empirical work. All of Tsebelis’ written work is clear how to test his arguments, and where there is any question he addresses the potential glitch in his next paper or book. The one potential weakness comes from the distinction between presidential and parliamentary democracy in Tsebelis’ latest work (Forthcoming). The recommendation that he makes empirically is to count partisan players across parliamentary systems based on the number of parties. For presidential systems, must first consider party discipline, *then* aggregate at the party level only if party discipline is high. Given that he his argument is that presidential systems usually have lower party discipline, his expectation is that this will rarely be the case. In the United States, his example of the applicability of the absorption rule in practice is the party discipline one found during the first 100 days of the Roosevelt Administration

⁸ As Tsebelis (Forthcoming, 127-128) notes, “some presidential systems may provide so many agenda setting powers to the president that they look parliamentary, and some parliamentary systems may take away so many agenda setting initiatives from the government that they look presidential.”

(120). This is a high standard to meet. Admittedly, this addition as applied to presidential systems makes the theory more realistic. As the next section of the paper will indicate, there has been little comparative work applying veto player theory to presidential systems, and one reason for the lack of work could be that the previous incarnation of the theory is less relevant for presidential systems.

Yet the change in the theory concerning veto players in presidential systems does complicate matters. A study of veto players in Latin America, for example, will now need to consider party discipline in every country. To make matters worse, as far as I know there is no acceptable, comparative measure of party discipline in any event even if one reads Tsebelis (Forthcoming) and decides to use such a concept empirically. This also leads one to suspect that the theory can become non-falsifiable—if the results do not match theoretical expectations, one can always argue that one mismeasured party cohesion. I would prefer to keep the theory *as applied to presidential systems* as it was pre-Forthcoming. It may not explain as much variance, but it is up to alternative approaches to explain as much with as simple a framework. And this concern aside, there is little doubt that the theory it is parsimonious and it is testable.

Tsebelis is not, however, the only application of the veto player approach, and three further distinct arguments about the definition of veto players deserve discussion before moving on to the empirics. One argument limits the focus to institutional players only. For Shugart and Haggard (2001), such institutions are “veto gates.” A gate is any institution that is required to approve a change from the status quo. An assembly is a veto gate, for example, and, in a unicameral parliamentary system, it is the *only* veto gate. The addition of a second legislature or a president with veto power would add veto gates. The concept is therefore related to the Tsebelis definition—Tsebelis too requires the scholar to consider every relevant institution—but in this case there is not an emphasis upon party control. This line of work therefore rejects the use of the “absorption rule” to determine relevant players. Counting veto gates can consequently lead to different counting rules from Tsebelis. A parliamentary democracy like the Netherlands, for example, has just one veto gate according to Shugart and Haggard in spring 2002 but three veto players according to Tsebelis because of the presence of a three-party coalition. Similarly, there is not an “absorption rule” in practice. The United States has three veto gates under both “united” and “divided” government.

This leads to potentially different predictions about the likelihood and scope of change. The very presence of a veto gate represents a potential check to changes from the status quo. The reasoning for a focus on the institutions is best developed in Immergut’s (1992) discussion of the politics of health care. Veto points (or gates) provide the opportunity for interest groups to block changes.⁹ Tsebelis (1995) does discuss interest groups as potential veto players, but he does not discuss *how* they matter in practice and how the institutional structure of the policy process may offer a

⁹ To be clear, Immergut (1992) in practice discusses “veto points” instead of “veto gates,” but the concept is the same—one should pay attention to the number of veto points that a bill must traverse to become law.

given interest group a veto over policy change even if the group's assent is not formally required for a bill to become law.

Huber, Ragin, and Stephens (1993), whose definition of "multiple points of influence" approximates Immergut's "veto points," also derive different implications of the choice of electoral system for the effective number of veto players. If parties are well-disciplined, interest groups must lobby the whole party (see Huber, Ragin, and Stephens 1993, 722, footnote 10). Hence a (closed list) proportional representation system decreases the potential level of influence of interest groups. Tsebelis similarly would interpret the use of closed list PR as eliminating the "personal vote," and in his framework this makes political parties more cohesive. Yet, to the extent that such systems lead to multiple parties represented in parliament and decrease the probability of a one-party majority government, PR systems increase the number of veto players and, so long as they do not share the same ideology in all dimensions, they decrease the scope for change from the status quo.

The second argument calls for separating out the effects of partisan and institutional players while keeping the fundamental division in place. Birchfield and Crepaz (1998) and Crepaz (2002) make the distinction between "collective" and "competitive" veto points, with the latter Tsebelisian institutional players and the former partisan players respectively in practice.¹⁰ The predicted effects of the two differ from Tsebelis. The assumption is that "collective" players interact with each other constantly, and they tend to pass legislation through internal logrolling. One therefore expects potentially more change with more collective players. Increases in "competitive" veto players, however, are likely to lead to gridlock. Moreover, which party controls which competitive player is not a concern.¹¹ This approach therefore is consistent with elements of the veto gate literature, but it adds an assumption about the role of "collective" [or partisan] that is contrary to what Tsebelis argues.

Tsebelis' aggregation of partisan and institutional veto players also troubles Strøm (2000). He contends that it is misleading to aggregate institutional and partisan veto players. Partisan veto players may have motive to veto something but lack the opportunity, while institutional veto players may have opportunity but no motive. He notes that Tsebelis agrees with the latter—this is why party control of two chambers can be aggregated—and Tsebelis should add the former as well. Ganghof (2001) has a similar criticism. A veto player may be close in a policy space to another, so that in practice it should agree readily to changes in the status quo with the second player. Yet it may have motive to block changes in the status quo that the other veto player proposes. The key player he is someone Tsebelis rarely mentions, namely the voter—a party may have an incentive to differentiate itself in the eyes of voters, which means that it will not accept a position that is readily identified with another player even if ideologically the parties are more or less identical.

¹⁰ Crepaz' (2002) competitive players include "federalism, bi-cameralism, central bank independence, and rigid constitutional structures (183)."

¹¹ Wagschal's (1999a, 1999b) analyses have a similar coding.

Tsebelis (Forthcoming, 130-132) responds directly to Strøm's criticism. He argues that it does not make sense for veto players to have motive but no opportunity. Political parties in government work hard to negotiate coalition agreements, and they try to enact most of their agreement. If they disagree with a change to the status quo they can bring down the government. It therefore is not logical that a veto player could have motive but no opportunity.

Theoretically the difference between the two authors may be a difference in conceptualizations about how (parliamentary) governments form and how they function in everyday affairs. Strøm's (2000) argument is consistent with Laver and Shepsle's (1996) view of parliamentary politics. Coalition formation is a game to determine party control of portfolios. Once a minister is selected, that minister determines policy in that ministry. This means that the minister's party selects the policy in a given dimension. Other parties have no veto power in practice after portfolio distribution. Crepaz (2002) would add that the partisan players then logroll their preferred policies, and in the end spending increases. The deal among the parties is that they respect the autonomy of each other's ministers. Tsebelis' response would be that the parties collectively agree upon a coalition agreement that spells out the government's position in every policy dimension. Moreover, a party can pull down the government if a minister does something the party does not like.

Which argument is right ultimately may be an empirical question. Hallerberg (2002a) argues that the degree of minister autonomy within a government is a variable. In his study of budget policy, he contends that there are cases of "fiefdom governance" where the individual ministers have fairly wide latitude to set policy. Italy's "pentapartito" governments in the 1980's are good examples. There are also cases where the coalition partners negotiate detailed "fiscal contracts" that set budgets and policy positions for the expected life of the coalition. This type of "commitment governance" has been the norm in the Netherlands since 1982. The parties negotiate expenditure levels in every ministry for the upcoming four years. Only after parties have agreed upon the fiscal contract do they discuss portfolio allocation. The job of the minister is then to execute the contract in her ministry. More generally, and in Tsebelis' defense, coalition agreements have become more detailed, and have covered more policy areas, in recent years, as Strøm and Müller's (2000) edited volume convincingly demonstrates. The Tsebelis argument certainly has more applicability in the last two decades.

While Americanists rarely discuss "veto players" as meant in this paper, there is a rich literature on "divided government" that deserve mention before moving on to the empirical section.¹² "Divided government" exists whenever different parties control the presidency and at least one of the houses of Congress. It augments the constitutional separation of powers (Cox and Kernell 1991). Moreover, divided government generally means that less gets done. Alesina and Rosenthal (1995) make an argument very much in the spirit of Madison—voters intentionally support different parties for the different offices because they want to increase the checks on

¹² Many thanks to Christopher Carman for help wading through the Americanist literature.

the policies that any one party would implement on its own. This argument is in general broadly consistent with Tsebelis; an increase in the number of parties that control at least one of the three institutions at the federal (i.e., national) level leads to greater preservation of the status quo. The one difference is that Americanists are generally not willing to give too much credit to the idea of “absorption” of parties. Separation of powers still matters because the three institutions are elected differently (Hansen 1998).

Finally, there is a loose literature that counts simply the number of parties whose assent is needed to pass a bill (Roubini and Sachs 1989; Hallerberg and Basinger 1998; Franzese 2001.) The idea is that the fragmentation of government increases the transaction costs to changing the status quo. While the approach is friendly to Tsebelis’ work, it says nothing about the ideological distance among veto players.

Taken together, a common theme that emerges is that increases in veto players usually leads to less policy change (the exception here is Crepaz 2002). The next section reviews literature examining whether this is the case in studies done to date.

Empirical Application of the Veto Player Approach

This section reviews empirical work using the veto player approach to explain differences in policy outputs. I want to stress that it can be only a partial review; Tsebelis (Forthcoming) does a terrific job in summarizing empirical work that finds support for veto players, and an article-length treatment cannot be as comprehensive. Instead, I would like to highlight what policy fields, and for what parts of the world, the approach has been most successful, and also where it has been less often tested but where theory tells us it should be examined further.¹³ This way of reviewing the work is constructive because it illustrates where more work can, and should, be done. The discussion below indicates that there are common themes, such as legislative passage of bills and the composition of budgets, where scholars have used the approach. Political economists in the US and in Germany in particular have tested the theoretical implications. Moreover, the data set that analysts use has usually been composed of industrialized democracies and usually (though not always) parliamentary democracies. One way to tell that the approach is growing more mature is that there are increasing disagreements among the scholars who do this type of work about how best to apply it. The application of veto player theory to developing countries as well as presidential systems appears less frequently, and the results are more mixed.

Legislatures in Parliamentary Democracies

One branch of veto player theory examines legislative outputs broadly defined. Little work has been done in this area outside of Tsebelis and his students, but the reason for the lack of empirical work has as much to do with the state of legislative studies

¹³ Another way to do this type of review is to break down the work according to the somewhat different definitions of veto players that appear in the literature.

as with the use (or non-use) of veto players in broadly comparative studies. There are simply few legislative studies that are both comparative and quantitative.¹⁴ Tsebelis (1999) does apply his framework to the production of important laws across 15 European democracies during the time period 1981-91. Increases in the ideological distance between the most extreme veto players in a one-dimensional space lead to decreases in the number of important bills that pass parliament. A student of Tsebelis, Kreppel (1997), focuses on legislative output in Italy. She finds that increases in the number of political parties leads to decreases in legislative output.¹⁵

Political Economy Outputs in Developed Countries

Much of the veto player literature in practice has been applied to political economy outputs in developed countries. The initial works focused on the composition of budgets, and it looked at the number of players only in time series-cross section analyses of OECD countries. The classic article in budgeting is Roubini and Sachs (1989). They argue that one-party majority governments consistently maintained tighter fiscal policy than coalition governments with 2-3 parties, while the latter governments performed better than majority governments with 4-5 parties. The worst performers were minority governments. While the authors do not use the term “veto players,” their results are consistent with arguments that focus on the number of parties whose assent is required for changes from the status quo. Their empirical results, however, have been questioned both because of their use of an ordinal variable instead of separate dummy variables (Edin and Ohlsson 1991) and because of many miscoded values for their political variables (De Haan and Sturm 1994 and 1997; Beck et al.,). Based on a data set of OECD countries during the period 1955-1990, Franzese (2001) found that increasing the fragmentation of the government made change from the status quo stickier—government with many players in government tended to maintain either high budget deficits or low budget deficits. Volkerink and De Haan (2000) find that increased fragmentation of the cabinet in terms of the number of parties leads to higher budget deficits. Interestingly for a comparison with Tsebelis, both Franzese (2001) and Volkerink and De Haan (2000) test ideological distance explicitly but do not find evidence that the ideological distance among coalition partners matters; only the actual *number* of veto players is statistically significant.¹⁶

In his own work with Chang on budgets, Tsebelis and Chang (2001) found the opposite result, namely ideological distance is statistically significant and leads to less change in the budget as distance increases while the absolute number of veto

¹⁴ Exceptions include the edited volumes of Herbert Döring (Döring 1995 and Döring and Hallerberg 2002).

¹⁵ See also Zucchini (1998).

¹⁶ Volkerink and De Haan (2001) operationalize distance as “political fragmentation,” and they use the same formula as Franzese (2001) in one case and, following Tsebelis (1995), they also examine the maximum ideological distance between parties in coalition.

players is not significant. They examine the degree of change in different categories of the budget with a data set of 19 OECD countries for the period 1973-90. A potential criticism of their work concerns their coding of ideological distance. As explained in the previous section, they average three measures of the location of political parties on a simple left-right scale, then they average this average with Laver and Hunt's scoring of the ideological position of a given party's foreign policy position vis-à-vis the Soviet Union. One question concerns whether party positions change over time; the indices that Tsebelis and Chang (2001) use assume that party positions remain constant over time. The second question concerns the use of foreign policy. The rationale in Tsebelis and Chang (2001) is that foreign policy captures truly a second dimension, but one can ask whether foreign policy positions have any relation to budget outcomes.

Bräuninger (2000) takes seriously these criticisms, and his examination of changes in budget policy represents a rigorous test of the argument. He uses data from the party manifesto project to classify the ideological positions of parties in every dimension. He then runs similar regressions on central government expenditure data from 14 OECD countries during the time period 1973-1997. He finds that the simple one-dimensional coding of ideological distance performs better than the more complex versions that take account of policy positions in multiple dimensions. His results are reassuring to scholars who use simpler measures of veto players.

There have also been a few articles that examine budgetary policy in a given country instead of across a set of countries, although as far as this author knows the analyses have been restricted to one country, namely Germany. Bawn (1999) examines German spending patterns for the time period 1961-89. She classifies spending items according to whether they are SPD items, such as educational grants and loans, labor market policy, and sport, whether they are CDU items, such as defense, roads, and housing, and whether the (party) benefits of a given spending item are ambiguous. She finds support for a veto player's explanation of government spending. Spending for SPD items, for example, increased in 1967 when the SPD first affected the budget after forming a Grand Coalition with the CDU in 1966, but, as a veto player argument would anticipate, spending did not change in 1970 after the party formed a coalition with the FDP. König and Tröger (2001) look at the composition of German budgets for a somewhat longer period, or 1961-98. There are some differences between their study and Bawn's. They treat the government as a potentially separate actor from the majority parties in the Bundestag. The government has agenda-setting power on the budget, and, since the Bundesrat's approval is not necessary for the annual budget, the game is restricted to the government and to the Bundestag as institutional actors. They also do not assume that the FDP always wants lower spending across the board as Bawn does; instead, there are some budget categories, such as spending on research and development, where the FDP would like more spending. Even with these changes, their results broadly are in line with those reported in Bawn (1999). It should be noted that each article takes seriously ideological distance, and so support Tsebelis' empirical application of the argument.

Huber, Ragin, and Stephens (1993) look at the composition of one part of the budget, namely spending on the welfare state. They use generalized least square techniques to estimate the effects of “constitutional structures” on “welfare state effort,” which they measure as either social security transfers as a percentage of GDP, social security benefits as a percentage of GDP, or total revenue as a percentage of GDP (733). They find that an increase in the number of constitutional structures reduces welfare effort. Crepaz (2002), examining a factor that size of the welfare state presumably affects, namely income inequality, arrives at much the same conclusion. An increase in his “competitive” veto players, which largely overlap Huber, Ragin, and Stephens’ “constitutional structures,” increases income inequality. His findings on “collective veto players,” however, contradict much of the previous literature. He contends that increases in partisan veto players leads to higher government spending and lower income inequality. The previous studies that look at budgets more generally all assume some level of gridlock with more players so that spending should not increase.

There has also been work that examines the other side of the budget, namely taxation. Hallerberg and Basinger (1998) look at the reaction of OECD countries to the 1986 tax reform in the United States. This reform lowered the marginal tax rate on corporations as well as the top marginal rate on personal incomes. Using a cross-section regression of 18 countries, they find that countries with more than one veto player made smaller changes to their marginal tax rates than countries with one veto player during the time period 1986-90. While they count the players according to Tsebelis (1995), they look at the number only, not the ideological distance between them. Their conclusions, however, have not gone uncontested. In a “Controversy” section of the journal *Politische Vierteljahresschrift*, Ganghof (1999) criticizes the article on several counts.¹⁷ While Hallerberg and Basinger (1999) respond to the criticisms directly, and interested readers in the debate are asked to read both articles, I freely admit that Ganghof (1999) has a point—there are alternative ways to formulate the regression equations and to code some of the cases, and under some alternatives the veto player effects disappear. With an n of just 18 one can put only so much confidence in the results.

Basinger and Hallerberg (2002) attempt to address two weaknesses they saw in Hallerberg and Basinger (1998) as well as some of Ganghof’s (1999) criticism. First, in the original article there was no underlying model how veto players matter in an interactive fashion. That is, the authors assume that countries see a cut in the US, make a decision at home, and ignore what the rest of the world is doing. Second, the empirical analysis in the original article is rather primitive. In this paper, they begin with a formal model of tax competition. The model’s key insight is that political costs that accompany tax reforms have both direct and indirect effects on the likelihood and scale of tax reforms. While countries with higher political costs for reforming are less likely themselves to reduce taxes, these costs also reduce competing countries’

¹⁷ Ganghof (1999) also critiques earlier work by Uwe Wagschal (1999a) on the connection between veto players and tax reform; see Wagschal (1999b) for his response.

incentives to reform regardless of their own political costs. In veto player terms, the model predicts that countries consider the likelihood of reform in competitor countries based upon their competitors' veto player structures when making decisions about their tax rate on a mobile factor. In their empirical work, they examine the tax burden on capital in 20 OECD countries over the period 1980-97.¹⁸ They find that decision-makers are indeed sensitive to the likelihood of change in competitor countries based upon their competitors' veto player constellations. They also find evidence in a time series-cross sectional framework for the original Hallerberg and Basinger (1998) thesis. Increasing the ideological distance of domestic veto players decreases the sensitivity of the government to actual changes in tax burdens abroad in competitor countries in the previous year.

Finally, two works roughly a decade apart use a case study approach to examine how veto points affect the development of specific policy areas. Immergut (1992) finds that increasing the number of veto points increases specialized interest group influence over health care reform, and they can stall reforms. The number and location of veto points also influence the *strategies* that interest groups use in their lobbying of the government. Bonoli (2001) focuses on pension reform. He finds that increases in the number of veto points leads to concessions to key (narrowly defined) constituencies in countries like Switzerland, while few veto points allows the government to pass its reform more or less as-is and not to target specific constituencies.

As this summary indicates, the application of the veto player approach to political economy outputs in developed countries is becoming fairly sophisticated. Different methodological approaches, be they case studies, cross-sectional regressions, or time-series cross sectional work, all find support for the core contention that increasing veto players leads to increasing policy stability.

The next subsections turn to cases where institutions, be they political parties, presidents, or legislatures, tend to be less well-defined, and less cohesive, than in the developed world. *A priori* one would therefore expect to find less evidence in support of the approach.

Application in Developing Countries

Scholars have employed the veto player approach less frequently to the developing world, although the results so far are broadly encouraging. Like in the developed world, some of the work has been case study in nature and some large n. Because large n studies usually have mostly developing countries as cases even when developed countries are included in the analysis, I include such studies in this subsection as well.

¹⁸ The calculation of tax burdens is based on the data set provided in Carey and Tchilinguirian (2000). Such calculations are not unproblematic, and Hallerberg and Basinger (2002) should be humble about how much they can read into their results. Ye they do use what is "best practice" among the different measures available.

In an empirical chapter in the Haggard and McCubbins (2001) edited volume, Heller and McCubbins (2001) explain how different configurations of veto gates affected the development of the regulation of electric utility regulation in Argentina and in Chile. They argue that “an increase in the number of veto players, while making no other changes, will be more biased towards preserving the reversionary policy (232).” Consistent results with a more Immergutian conceptualization of veto points come from Henisz and Zelner (2001). They examine electricity investment in a panel data set of 78 countries. They find that more veto points leads to more overinvestment in “white elephant” projects, or projects that concentrate political benefits but are not justified based upon their costs to the general population. They contend that there are more “white elephant” where there are many veto points. They conclude that policy-makers are better able to withstand interest group pressure when there are few veto points.¹⁹ This work follows Henisz (2000a, 2000b), where he similarly looks at a large n data set that covers most of the world to argue that veto points affect economic growth and multinational investment. Increases in the number of veto points leads to higher economic growth and higher investment as it becomes harder for governments to change the economic rules of the game.

MacIntyre (2000) similarly looks at investment decisions, but, unlike Henisz, he hypothesizes a non-linear relationship between veto players and investment levels. In his view, public policy is the intervening variable between underlying political institutions and the level of capital investment in a given country. Many veto players can lead to stark policy rigidity, which makes governments too slow in responding to crises, while few veto players can lead to excessive policy volatility. Both extremes can scare capital. Instead, an intermediate level reduces volatility and increases rigidity, and the level fosters investment. Empirically, he uses an analytic narrative framework to examine fluctuations in the level of investment in four countries that suffered under the East Asia crisis during the period 1996-98.

Treisman (2000) examines changes in inflation in 87 countries during the 1970s and 1980s. He argues that federalism allows more veto players into macro-economic policy, and it tends to “fix” the level of central bank independence at either high or low (I will discuss central banking in more detail below). States with more veto players similarly tend to “lock in” their previous inflation rates. If the rates were high they remain high, while if they were low they remain low.

In terms of *doing* empirical work with developing country data sets, Beck et al.’s (2000) data set makes it possible to do much more than has been accomplished to date. It includes an impressive 177 countries for the time period 1975-95. They do code the prime minister’s party as well as the three largest parties in government, which would allow one to construct a variable for the number of parties in government. They also code the policy preferences of parties for many countries on five dimensions. This coding is obviously rough, but it nonetheless allows the

¹⁹ Their index for veto points runs from 0 to 1. It considers party fragmentation within specific institutional veto players, their congruence, and whether or not there are strong courts. The original index is presented in Henisz (2000).

creation of an ideological distance variable.²⁰ They also code whether the parties of the executive control the lower house of the legislature, and, “with this information, one can calculate which of the main veto gates (legislature, executive) in a system have changed hands in any year (18).” Another relevant variable is their replication and expansion of the coverage for Roubini and Sachs’ (1989) “political cohesion” variable, which looks at whether the same or different parties control the legislature and the executive in presidential systems while in parliamentary systems there are one, two, or three-plus parties in a majority government or whether this is a minority government. This variable allows a good approximation for the number of players. Finally, they also include a variable for the number of institutional checks in the system.

The results for developing countries, whether as case studies or as large n studies, are all as a veto player approach would expect. Unfortunately, there are too few of them. It may be that there is much less faith in institutional approaches among those who do work on developing countries. Yet this lack of faith should not discourage rigorous testing of a theory that has proven its use in the developed world, and that has encouraging evidence in the studies that have used it to date.

Transition Countries

Transition countries are located in Eastern Europe. They all experienced the collapse of some form of Communist regime around 1989-1990, and they are transitioning from a command economy to a market economy. They are therefore in need of significant changes to their status quo. The new democracies would seem to constitute a good data set to test the effects of veto players.

Yet the research that has been done to date on this part of the world has not confirmed the veto player approach. Indeed, Hellman (1998) seems to find the opposite effect. He argues that increasing the number of parties in government *increases* the degree of change from the status quo. Short-term winners from the initial transition process, such as enterprise insiders who benefit from subsidies and who strip away remaining assets, bankers who thrive because of arbitrage opportunities, etc., fight further moves toward consolidation. They insist that the status quo remain so that they can continue to collect their rents, to the great disadvantage of the population at large. Reform then stalls where government coalitions have narrow bases. Where government coalitions are broad-based and include many parties, parties against further reforms are watered down. In his empirical analysis, he finds strong empirical support for his argument.

²⁰ They do not allow party positions to evolve over time. They also begin by categorizing parties based on the party’s name (Beck et al, 14.) While they double-check whether the name makes sense where they can, this method is obviously problematic. Portugal’s Social Democrats, for example, could easily be coded as center-left based on their name when they are in practice the mainstream right party. Yet I do not mean to be too critical; it is quite a task to code 177 countries over 20 years, and it is unreasonable to expect more than a rough cut when the authors are collecting 113 variables per country.

Kitchelt (2001) is not a true test of the veto player approach, but the weak to non-existent results he finds for constitutional design measures reinforce the perception that institutional differences have little practical effect in these countries. He measures constitutional design as the relative strength of the president as well as whether there is parliamentarism. His empirical results provide weak support for the effects of constitutional design on market liberalization in his statistical analysis but no effects on institutional economic reform. Yet further analysis as well as detailed case knowledge makes him “somewhat skeptical (39)” even of these results.

Bodenstein (2002), in an examination of the determinants of trade liberalization in transition countries, finds no relationship at all between Beck et al.'s (2000) checks and balances and a negative impact of strength of the presidency. Like Kitchelt (2001), he does not test the Tsebelis argument directly, but the results nevertheless are consistent.

Do these three articles together constitute an indictment of the veto player approach, at least as applied to transition countries? On the face of it, no. Political parties are simply not cohesive in any East European country, with the last incarnation of the party “Solidarity” in Poland almost an absurd extreme. Indeed, one can tell a Tsebelisian story even with the results in Hellman (1998). The narrow party coalitions Hellman discusses arise to protect the initial change during the transition process, and they have no interest in change from the status quo. A large coalition with loose party discipline, however, may very well increase the size of the winset and make greater change possible. If a reformer is the agenda-setter, then that change will occur.²¹ Political institutions are similarly fragile and subject to change. The best one can say is that the jury is out concerning the application of the approach to transition countries.

Conditional Effects on Institutions

There is also a growing literature that looks at how veto players determine the relative effectiveness, and the choice, of institutions. Tsebelis (Forthcoming) emphasizes that the ideological distance between/among veto players determines the amount of discretion that certain institutions have in a given policy area. For example, large ideological differences among parties in government would allow the bureaucracy relatively greater reign to do what it wants than a government with just one veto player. Tsebelis applies this framework to the checking power of referenda on the government (Hug and Tsebelis 2001; Tsebelis Forthcoming, Chapter 5), legislative veto players on judges (Tsebelis Forthcoming, Chapter 5), and the Council of Ministers on European Commission proposals (Tsebelis Forthcoming, Chapter 11). Others have made similar arguments concerning the relative independence of central banks. The application of the theory to central banking is more developed than to other institutions, and Tsebelis (Forthcoming) does a good job of summarizing the

²¹ Indeed, an interesting extension of Hellman (1998) would be to look at the agenda-setter in the various governments.

effects of veto players on other institutions, so I will focus especially on the central banking literature here.²²

Lohmann (1998a), Bernhard (1998), Keefer and Stasavage (2000), and Moser (1999, 2000) all consider how increases in the number of some type of “veto player” increase the discretion of the central bank to determine monetary policy. It becomes harder for the government to unite to overturn the decision of the central bank when there are more veto players.

Who these “veto players” are, however, and the exact role that they play differ. Lohmann’s (1998a) focus is on Germany, and she notes that the discretion of the Bundesbank increases when the government’s parties do not hold a majority of seats in the upper house of parliament, the Bundesrat. Discretion therefore increases when Germany experiences periods of divided government; the number of partisan players *in* government is not consequential. Bernhard focuses primarily on the dichotomy between one-party and multi-party governments. States that have coalition governments are more likely to have independent central banks. Keefer and Stasavage (2000) come closer to the original Tsebelis coding of veto players. Like Tsebelis they include partisan players for parliamentary systems, but they count the number of veto points in presidential systems depending upon whether or not the same parties control the different institutions. They also make a series of adjustments depending on whether a country uses an open or closed list proportional representation system and whether they judge the electoral system as being competitive.²³ They find that the effectiveness of central banks in pursuing low inflation increases with an increase in the number of (logged) veto players. In the empirical results they include 78 countries.

Bernhard (1998) considers a combination of institutional and partisan veto players. He codes countries with “strong bicameralism” and indicates that such states are more likely to have independent central banks. In practice those states are Australia, Germany, Switzerland, and the United States. He also creates a “punishment index” where increases in the lead to greater independence. This index includes polarization, parliamentary or congressional committee strength, and whether or not a country has a coalition or minority government. Neither of his variables match completely the Tsebelis definition for veto players—strong bicameral states are coded the same way whether one party or more than one party controls them, while the punishment index codes coalition governments and one-party minority governments the same way—but Bernhard’s (1998) results are consistent with an argument that increases in veto players increases independence of the bank.

Moser (1999, 2000) in contrast, focuses on the level of institutional checks and balances for a cross section of 22 OECD countries rather than partisan control. He creates dummy variables for countries with strong checks and balances, weak

²² The next two pages received inspiration from a shorter summary of the central bank literature in Hallerberg (2002).

²³ They also take the log of the value before they include it in regressions.

checks and balances, and no checks and balances. Countries with bicameral chambers that have equal power and that have different rules to elect them to assure different “compositions” are coded “strong;” those where the lower chamber dominates but where there is still a different composition are coded “weak;” and all others are coded with no strong checks and balances. One should note that “composition” is meant to represent in most cases the *potential* for different partisan majorities to control the two houses, which makes the argument distinct from Tsebelis (1995 and 1999) who counts only *actual* differences in partisan control.²⁴ Moser (1999, 2000) produces results to argue that states with strong institutional checks and balances have lower inflation rates than states without them even controlling for the level of central bank independence. He also emphasizes that it is harder for governments with strong checks and balances to change bank charters that grant banks independence, and he finds that states with more checks and balances have more independent banks.

Hallerberg (2002) examines the joint choice of exchange rate regime and the level of central bank independence.²⁵ He argues that two types of veto players matter, partisan veto players and the presence or absence of sub-national veto players in the form of federalism. A crucial issue for partisan players is whether voters can readily identify the benefits, and the costs, of the manipulation of the economy with the partisan players. A second issue concerns the veto player’s controllability of a given type of policy.

According to Hallerberg (2002), “identifiability” and “controllability” vary systematically across the four possible combinations of veto players. Under standard Mundell-Fleming conditions and the assumption that capital is mobile states must choose either fiscal or monetary policy autonomy. In one-party unitary governments identification and control are clear, and parties where such governments are common prefer flexible exchange rates and the monetary policy autonomy that accompanies it as well as dependent central banks. In multi-party coalition governments in unitary systems identification is traditionally difficult, and the ability to target benefits to specific constituencies under fiscal policy makes this option more attractive for coalition governments. Such governments prefer central banks that are politically independent, although they value central banks that finance government debt. Under federalism, in contrast, party(ies) that constitute the federal government have less control over fiscal policy because sub-national governments compose much of total government involvement in the economy, and they prefer to have flexible monetary policy. Sub-national governments generally do not support a dependent central bank that gives more power to the federal government. His data set includes most OECD

²⁴ Moser (2000) does note that, in cases where the coding is unclear, he looks at partisan control and rules a country as having no checks and balances if the same parties always control both houses. He does this in his coding for three countries—Belgium, Italy, and Japan.

²⁵ The paper will appear as part of a special issue of *International Organization* on the joint determination of exchange rate regime and the level of central bank independence in Fall 2002 co-edited by William Bernhard, Lawrence Broz, and William Roberts Clark. Readers interested in how a joint consideration of the two institutions affects one’s analysis should read the special issue.

countries post-1973, and the analysis includes both simple “eyeball” tests (do the countries fall into 2 X 2 tables as predicted) as well as a logit analysis.

Hallerberg either alone or with Jürgen von Hagen in other work (Hallerberg and von Hagen 1998, 1999; Hallerberg 2002) has also examined how the number of veto players affects the fiscal institutions that parliamentary democracies in the European Union put in place to make decisions on the budget. He argues that there are two ways to solve common pool resource problems, which arise in all governments because decision-makers on spending rarely have an incentive to consider the full tax burden of their decisions. One way to solve this problem, which they term “delegation,” involves delegating strategic problems to one central player who considers the full tax burden. In practice, this player is usually the finance minister. In contrast to this discretionary approach, “commitment” involves clear rules on the budget. Coalition partners sign what amount to “fiscal contracts” for the life of the coalition, and in the process of the negotiations they internalize the tax externality. The discretionary approach works only in countries with one veto player in government, or where the policy positions of the players are close. The reason is that coalition partners would not trust a strong finance minister where ideological distances are larger. Instead, countries with veto players where the ideological distance is large prefer explicit fiscal contracts that all parties can monitor. Their data sets are composed of the 15 European Union members, and their empirical work covers 1980-2000.

Empirical Issues Related to the Veto Player Approach

This review has indicated that authors have in practice used somewhat different formulations of “veto players” in their empirical applications. There are two issues that arise. The first is whether one should count the number of players or the ideological distance between them. The second is whether the key distinction is between the number of parties that must approve a change in the status quo (through the absorption rule) and the number of institutions. Given the critical role of Tsebelis’ work to this research agenda, I want to review his comments on these distinctions. In Tsebelis (Forthcoming), as well as at several conferences, he has emphasized that the proper way to use the approach is to rely on ideological distance. As his absorption rule indicates, the number of players matters only if the players have ideologically distinct positions. On the number of institutions, in Tsebelis (2002) he admits that one may need to focus on the number of institutions in presidential systems where party discipline is weak, but in practice it is usually the partisan players, rather the number of distinct institutional veto players, that matter. Once again, the absorption rule plays the key role.

Moreover, Tsebelis has argued that one way to test the competing formulations of veto players is through empirical analysis. The number of veto players should lose statistical significance whenever ideological distance is included in the same direction. In theory, there is nothing wrong with this recommendation. Yet in practice, this recommendation is problematic if the two are highly correlated with one

another. Indeed, Figure 1 (reproduced from Basinger and Hallerberg 2002, where it appears as Figure 2) indicates that the two tend to trend together. A change in the average number of veto players usually results in a change in the average ideological distance among them in the same direction. The exception is the early 1990's, when the absolute number of players increased while the ideological distance decreased. This suggests that the effects of one measurement dominating another are based on how well one or the other performs over just a few years.

[FIGURE 1 ABOUT HERE]

This discussion leads to three conclusions. First, looking at regression analyses to see which variable drops out will not solve the issue. Second, to the extent that the better theoretically developed argument should win when two arguments are highly correlated, the ideological distance argument as developed by Tsebelis (Forthcoming) is better anchored in theory, and it should win. That leads to a third conclusion—a transaction-cost approach requires more theoretical development by proponents if it is to be used more widely.

The difference between partisan and institutional players is the second empirical dimension where there is disagreement. Indeed, the main difference between the two camps may concern who matters in politics. For Tsebelis, the central players in practice and across large data sets are political parties. For Immergut (1992) and Huber, Ragin, and Stephens (1993), critical players also include interest groups. Indeed, in Immergut's case studies of health care reform, she provides detailed examples how interest groups composed of health professionals were able to block reform where they could gain ready access, and the more veto points the more access they had. It should be stressed that there is no necessary contradiction between the two approaches; party cohesion is critical for both arguments, and in the same way—it reduces the number of veto players or veto points. Once again, we are left with the need for a satisfactory measure of party cohesion/discipline. In the meantime, I suggest that authors test the theories as they are. In this case, there is indeed enough variance. One statistical problem that one should remember is that constitutional structures rarely change. One could be conflating results for constitutional structures with other country-specific attributes.

Conclusion

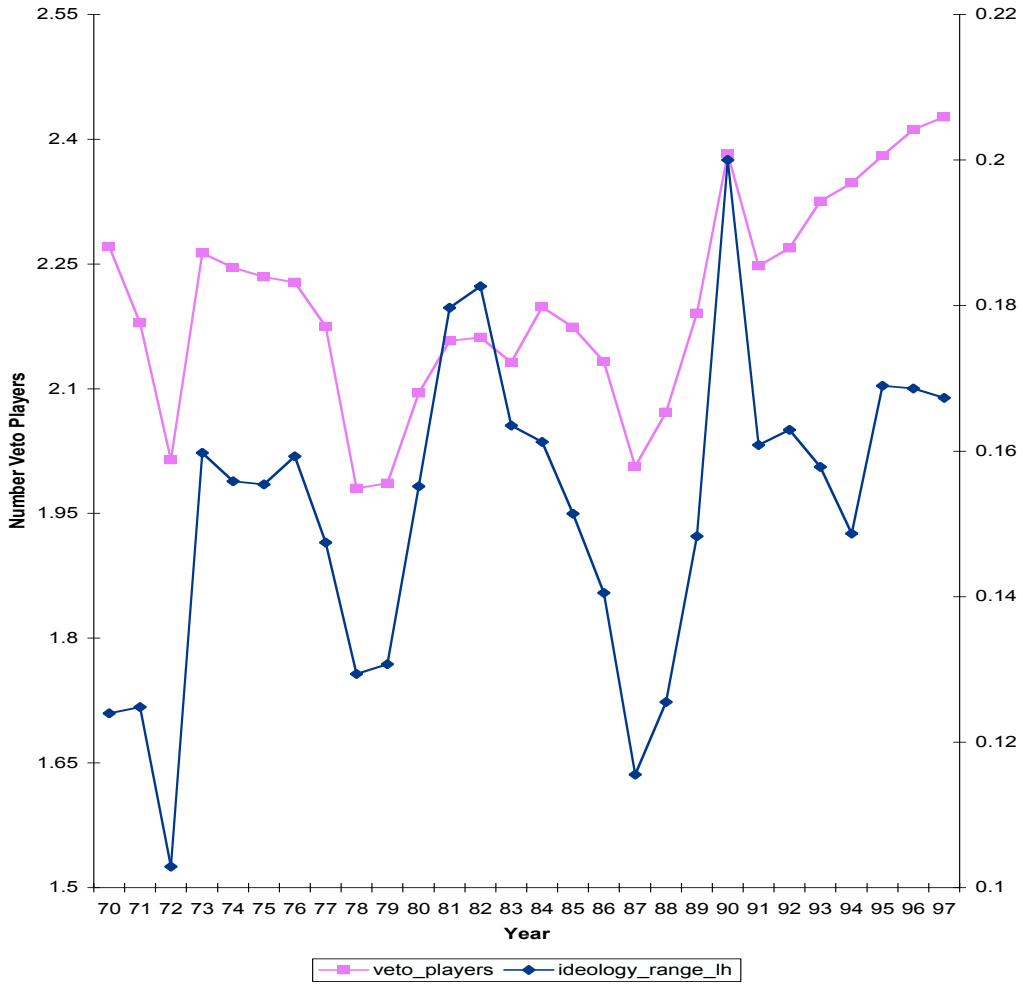
This paper began with a consideration of the strengths of the theory based on Lave and Marsh's (1975) classic undergraduate text. In the course of this essay, it has become clear to the reader that the veto player approach constitutes a good theory. It is beautiful because of its relative simplicity. It also has some truth. Of course, the empirical discussion indicates that it is especially "truthful" when applied to political economy outputs in developed parliamentary democracies. Yet the applicability to the rest of the world so far, while relatively thin, has been encouraging. Moreover, the relative paucity of work outside of the developed world has less to do with explicit

evidence that the theory is false than with the fact that the theory has yet to be tested. There is so much more than can be, and should be, done in the future.

The veto player concept also increases the researchers' ability to compare across seemingly incomparable systems, be they democracies, non-democracies, single- or multi-party systems, or presidential and parliamentary systems, as the original Tsebelis piece (1995) argues forcefully. The approach therefore furthers our knowledge of comparative politics.

Finally, Tsebelis in particular reminds us of where to begin any analysis. One to focus on actors whose assent is required to change the status quo. One must ask the question, who matters? This in itself is important. Too many approaches, and too many authors, forget this guiding question. Without knowing who matters it is hard to explain anything.

Figure 1: Aggregate Veto Player and Ideological Distance, 20 OECD Countries, 1970-97



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