

Chapter 2

Empirical Applications of Veto Player Analysis and Institutional Effectiveness

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Introduction

Following the initial publications of Immergut (1992), Huber et al. (1993), and, especially Tsebelis (1995, 2002), the veto player approach has justifiably received increasing attention during the last decade. It is a good theory according to the standards of political science. It is parsimonious, it is testable, and, as Sartori (1970) would want to know, it travels well. Indeed, one of the principal appeals of the approach is its claim that it allows one to compare the effects of institutions in a wide variety of settings, be it presidential or parliamentary democracy, one-party or multi-party government, or Spain and South Korea.

However, despite recent empirical advances as well as clear theoretical claims, we do not know how widely applicable the approach is in empirical work. This essay begins with a discussion of the definition of “veto players.” It relies 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 chapter 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 third part of the chapter extends the analysis to the effectiveness of institutions. Certain institutions work best under specific veto player configurations. If the veto player configuration changes but the

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institutions remain the same, one would expect that the institutional effectiveness would decline. This section illustrates the argument with examples from the fiscal policy realm. The final section concludes the discussion.

Veto Players: Definitional Issues

The veto player approach in comparative politics is young; indeed, it is difficult to find work older than two decades, which explicitly considers “veto players.” Immergut (1992) and Huber et al. (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) wrote a key article that introduced the term “veto player.” Since his 1995 article, the term has come into vogue. Yet, as Tsebelis (1995, 2002) 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” show up at least as early as Montesquieu and this argument motivates Madison 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’s 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 two-third 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.¹ 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 his 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 and they

¹ Note that, in the consideration of the 2002 immigration bill, the German President, Johannes Rau, did serve as a true veto player. Yet such occasions are quite rare in the German system.

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 who constitute the collective actor (Tsebelis 1995, 311).

How should one apply this argument in empirical work? Tsebelis (1995) focusses especially on how to define and to count relevant veto players. What one does ultimately depends on 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.² 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, for example, 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 (under President Obama’s Administration from 2009) 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 or not does the government have a majority status in the 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 that would have existed under a majority government with the same parties.³

Tsebelis (1999), Tsebelis and Chang (2004), and Tsebelis (2002) refine the application of the model further. The emphasis during the first two works is much more on the ideological distance among veto players. In his article, Tsebelis (1999) 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 who 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 greater the ideological distance between the veto players the lower the probability that there will be significant change from the status quo. Moreover, the larger the alternation the greater the probability of change, because governments that are much different from previous governments will want to move away from the status quo. Tsebelis (1999) places empirical focus on significant law production in 15 European parliamentary democracies during 1981–1991. To compute his two independent variables, he averaged the standardized values of three measures of partisanship: Warwick (1994),

² 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.

³ See Ganghof and Bräuninger (2006) for a spatial analysis and application to Denmark in particular that disputes this view.

Castles and Mair (1984), and Laver and Hunt (1992). He found that both “distance” and “partisanship” have significant effects on the number of laws produced in these democracies.

Tsebelis and Chang (2004) took the analysis to the budgetary realm. They found that increase in “distance” lead to smaller changes in budget composition across 19 advanced industrial countries during 1973–1995.⁴ 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 the article by Tsebelis and average this figure with ideological distance in the foreign policy dimension provided by Laver and Hunt (1992), which is the given party’s position vis-à-vis the Soviet Union. To determine the location of the previous government’s status quo to calculate the measure of “alternation,” Tsebelis and Tsebelis and Chang (2004, 457) 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 in the previous government as a proxy for the position of the status quo.”

Finally, Tsebelis (2002) addresses most completely the issue of cohesion of collective veto players. In Chap. 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, whereas 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. . . (63).”

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 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 who have different policy preferences.

Tsebelis (2002) also considers differences between parliamentary and presidential systems. In his article, Tsebelis (1995) 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. Tsebelis (2002)

⁴ 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*.

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, whereas in presidential systems the agenda setter is the legislature on non-financial bills (only). He 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.⁵ 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 (85).” A further refinement comes in Alemán and Tsebelis (2005; see also Tsebelis and Alemán 2005). The focus is on the agenda-setting powers of the president across Latin America. One institutional feature that especially strengthens the role of the president is the ability to amend legislation that Congress already has passed. In practice, these “positive” powers of amendment are more powerful than the more “negative” role of a straight veto.

Taken together, the theory that Tsebelis has developed is impressive. Tsebelis is certainly correct when he emphasizes that the veto player approach allows one to 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 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’s written work is clear on how to test his arguments, and where there is any question, he addresses the potential glitch in his next chapter or book. The one potential weakness comes from the distinction between presidential and parliamentary democracy in Tsebelis’s latest work (2002). The recommendation that he makes empirically is to count partisan players across parliamentary systems on the basis of the number of parties. For presidential systems, one must first consider party discipline and *then* aggregate at the party level only if party discipline is high. Given that 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 that was found during the first 100 days of the Roosevelt Administration (78). This is a high standard to meet. Admittedly, this addition, as applied to presidential systems, makes the theory more realistic and the focus on agenda setting in presidential democracies by Alemán and Tsebelis (2005) suggests a more fruitful way to go for empirical work.

⁵ As Tsebelis (2002, 84) 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.”

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 the article by Tsebelis (2002) and decides to use such a concept empirically.

Tsebelis is not, however, the only application of the veto player approach and three further distinct arguments about the definition of veto players deserve some discussion before moving on. 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 on 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 (2001) but three veto players according to Tsebelis because of the presence of a three-party coalition. Similarly, there is no “absorption rule” in practice. The United States has three veto gates under both “united” and “divided” governments.

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 by Immergut’s (1992) discussion of the politics of health care. Veto points (or gates) provide the opportunity for interest groups to block changes.⁶ 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 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 et al. (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 et al. 1993, 722, footnote 10). Hence a (closed list) proportional representation system decreases the potential level of influence of interest groups. Tsebelis would similarly interpret the use of closed list proportional representation (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

⁶ 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.

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.⁷ 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. Therefore, potentially, one expects more change with more collective players. Increase in “competitive” veto players, however, is likely to lead to gridlock. Moreover, which party controls which competitive player is not a concern.⁸ 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’s 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, whereas 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 (2003) has a similar criticism. A veto player may be close in a policy space to another, so that in practice he should agree readily to changes in the status quo with the second player. Yet, he may have motive to block changes in the status quo that the other veto player proposes. The key player here is 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, although ideologically the parties are more or less identical.

Tsebelis (2002, 86–88) 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 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 a 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

⁷ Crepaz’ (2002) competitive players include “federalism, bi-cameralism, central bank independence, and rigid constitutional structures (183).”

⁸ Wagschal’s (1999a, b) analyses have a similar coding.

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's response would be that the parties collectively agree on 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.

Ultimately, which argument is right may be an empirical question. [Hallerberg \(2004\)](#) 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 1980s 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. The parties negotiate expenditure levels in every ministry for the upcoming 4 years. This type of "commitment governance" has been the norm in the Netherlands since 1994. Only after parties have agreed on the fiscal contract do they discuss portfolio allocation. Then the job of the minister is to execute the contract in his/her ministry. Generally, and in Tsebelis's defense, coalition agreements have become more detailed, and have covered more policy areas, in recent years, as [Strøm \(2000\)](#) 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 chapter, there is a rich literature on "divided government" that deserves to be mentioned before moving on to the empirical section.⁹ "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 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 2002](#)). The idea is that the fragmentation of government increases the transaction costs to changing the status quo.

⁹ See also [Cox and McCubbins \(2005\)](#). Many thanks to Christopher Carman for help wading through the Americanist literature.

Taken together, a common theme that emerges is that increase in veto players usually leads to less policy change (the exception here is [Crepaz 2002](#)). The next section reviews the literature to examine 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. It can only be a partial review; [Tsebelis \(2002\)](#) does a terrific job in summarizing empirical work that finds support for veto players, and a chapter-length treatment cannot be as comprehensive. Instead, I would like to highlight for which policy fields, and in which parts of the world, the approach has been most successful and also where it has been tested less often and where theory tells us it should be examined further.¹⁰ 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 indication that the veto player 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 the approach 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 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.¹¹ [Tsebelis \(1999\)](#) does apply his framework to the production of important laws across 15 European democracies during 1981–1991. Increase in the ideological distance between the most extreme veto players in a one-dimensional space leads to decrease

¹⁰ 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.

¹¹ Exceptions include the edited volumes of Herbert [Döring \(1995\)](#) and [Döring and Hallerberg \(2004\)](#).

in the number of important bills that pass parliament. Kreppel (1997) similarly focusses on legislative output in Italy. She finds that increase in the number of political parties leads to decreases in legislative output.¹²

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 focussed 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, whereas the latter governments performed better than majority governments with 4–5 parties. The worst performers were minority governments. Although 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, 1997; Beck et al. 2000). On the basis of a data set of Organisation of Economic Co-operation and Development (OECD) countries during 1955–1990, Franzese (2002) 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 (2001) 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 (2001) 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.¹³

In his own work on budgets with Chang, Tsebelis and Chang (2004) found the opposite result, namely ideological distance is statistically significant and leads to less change in the budget because distance increases while the absolute number of veto 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 1973–1990. 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 and then they average this average with Laver and Hunt's scoring of the ideological position of a given party's foreign

¹² See also Zucchini (1998).

¹³ 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.

policy position vis-à-vis the Soviet Union. One question concerns whether party positions change over time; the indices that [Tsebelis and Chang \(2004\)](#) use, assume that party positions remain constant over time. The second question concerns the use of foreign policy. The rationale in [Tsebelis and Chang \(2004\)](#) is that foreign policy captures truly a second dimension, but one can ask whether foreign policy positions have any relation to budget outcomes.

[Bräuninger \(2002\)](#) addresses 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's spending patterns during 1961–1989. 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 Free Democratic Party (FDP). [König and Troeger \(2001\)](#) look at the composition of German budgets for a somewhat longer period, from 1961 to 1998. There are some differences between their study and that of 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, because 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 are broadly in line with those reported by [Bawn \(1999\)](#). It should be noted that each article takes ideological distance seriously and hence supports Tsebelis's empirical application of the argument.

[Huber et al. \(1993\)](#) 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, for example, income inequality, arrives at much the same conclusion. An increase in his “competitive” veto players, which largely overlaps Huber’s, Ragin’s, and Stephens’s “constitutional structure,” increases income inequality. His findings on “collective veto players,” however, contradict much of the previous literature. He contends that increase 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 found that countries with more than one veto player made smaller changes to their marginal tax rates than countries with one veto player during 1986–1990. Although they count the players according to Tsebelis (1995), they look only at the number, 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.¹⁴ While Hallerberg and von Hagen (1999) respond to the criticisms directly, and interested readers in the debate are asked to read both articles. As one of the co-authors of the original article, 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, under some alternatives, the veto player effects disappear, whereas under others, the veto player effects are clear. With an n of just 18, one can put only so much confidence in the results.

Basinger and Hallerberg (2004) 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 mattered in an interactive fashion, that is, the authors assume that countries see a cut in the United States and 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 chapter, 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’ 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 on the basis of 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 during 1980–1997.¹⁵

¹⁴ 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.

¹⁵ 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 (2004) should be

They find that decision makers are indeed sensitive to the likelihood of change in competitor countries on the basis of 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 in competitor countries in the previous year.

Finally, two works roughly a decade apart used 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 \(2001\)](#) focusses on pension reform. He finds that increase in the number of veto points leads to concessions to key (narrowly defined) constituencies in countries like Switzerland, whereas few veto points allow 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 it 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 it 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.

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

humble about how much they can read into their results. Yet they do use what is "best practice" among the different measures available.

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 overinvestment in “white elephant” projects or projects that concentrate political benefits but are not justified based on their costs to the general population. They contend that there are more “white elephants” where there are many veto points. They conclude that policymakers are better able to withstand interest group pressure when there are few veto points.¹⁶ This work follows [Henisz \(2000a, b\)](#), 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 multi-national investment. Increase 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.¹⁷

[MacIntyre \(2001\)](#) 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, whereas 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 1996–1998.

Satyanath’s solo or co-authored work with David LeBlang suggests that veto players are especially problematic during economic crises. In response to the East Asian crisis ([Satyanath 2006](#)) and in causing currency crises in the first place ([LeBlang and Satyanath 2006](#)), increasing veto players meant an inability to act quickly to address the crisis. His more general finding that gridlock may be “good” in normal times but “bad” in a crisis is an interesting extension of Tsebelis’ arguments.

[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). Similarly, states with more veto players tend to “lock in” their previous inflation rates. If the rates were high they remain high, whereas if they were low they remain low.

The results for developing countries, whether as case studies or as large n studies, are consistent with the veto player approach. In comparison to research on developed countries, however, there are comparatively fewer articles. It may be that

¹⁶ 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 \(2000\)](#).

¹⁷ See also [Henisz et al. \(2005\)](#), where they find that increased veto players make it more likely that small investors come to a country because they have greater confidence that the policy environment will remain stable. In this way, veto players may make certain types of changes *more* likely – the possibility of stability makes reforms that open up markets feasible.

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 has had encouraging evidence in the studies that have used it, to date.

Transition Countries

Transition countries are located in Eastern Europe. They all experienced some form of collapse of Communist regime around 1989–1990 and 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 been less confirmatory of 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 towards consolidation. They insist that the status quo remains, 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.

[Kitschelt \(2001\)](#) test 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 between the assessment of [Beck et al.’s \(2000\)](#) checks and balances and a negative impact of strength of the presidency. Like [Kitschelt \(2001\)](#), 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, the answer is 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.¹⁸ Political institutions are similarly fragile and subject to change.

Moreover, to the extent that presidents are relevant in former Communist countries, Tsebelis and Rizova (2007) find similar evidence for the strength of the president in roughly one-third of countries in the region where the president has the same power to amend legislation as is found in some Latin American countries.

Veto Players and Institutions

There is also a growing literature that looks at how veto players determine the relative effectiveness, and the choice, of institutions. Tsebelis (2002) 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 2002; Tsebelis 2002, Chap. 5), legislative veto players on judges (Tsebelis 2002, Chap. 10), and the Council of Ministers on European Commission proposals (Tsebelis 2002, Chap. 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 (2002) does a good job of summarizing the effects of veto players on other institutions, so I will focus especially on the central banking literature here.¹⁹

Lohmann (1998a), Bernhard (1998), Moser (1999), and Keefer and Stasavage (2003) all consider how increase in the number of some type of “veto player” increases 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 focusses primarily

¹⁸ Indeed, an interesting extension of Hellman (1998) would be to look at the agenda-setter in the various governments.

¹⁹ The next two pages received inspiration from a shorter summary of the central bank literature in Hallerberg (2002).

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 (2003) 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 on 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.²⁰ 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 bi-cameralism” 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 increase in the index 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 bi-cameral states are coded the same way whether one party or more than one party controls them, whereas 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 an increase in veto players increases independence of the bank.

Moser (1999, 2000), in contrast, focusses 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 checks and balances, and no checks and balances. Countries with bi-cameral chambers that have equal power and 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, 1999), who counts only *actual* differences in partisan control.²¹ 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.

²⁰ They also take the log of the value before they include it in regressions.

²¹ 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.

Hallerberg (2002) examines the joint choice of exchange rate regime and the level of central bank independence. He argues that two types of veto players matter, partisan veto players and the presence or absence of subnational 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.

The Role of Veto Players in Institutional Effectiveness

Analyses of budget policy in the European Union (Hallerberg and von Hagen 1999; Hallerberg 2004) have also assessed 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. "Good" fiscal governance arises from packages of rules, norms, and institutions that centralize the budget process. In game-theoretic terms, all governments face a common pool resource (CPR) problem. This problem arises whenever the decision makers consider the full benefits of spending only part of the tax burden. A centralization of the budget process that forces the actors to internalize the full tax externality leads to lower spending and, over multiple periods, lower budget deficits. They argue that there are two ways to solve CPR problems that 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, "fiscal contracts" involve clear rules on the budget. Coalition partners sign such "contracts" for the life of the coalition and in the process of the negotiations 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.

The work discussed in Hallerberg et al. (2009) extends this analysis and is particularly relevant for institutional analysis and suggests some avenues for future work. They consider cases where the fiscal institutions remain in place but where the ideological distance of veto players' changes. For example, the Greek Communist–Conservative coalition in 1989 brought together two parties that had no trust on each other. Although the fiscal institutions in support of the Greek finance minister were weak to begin with, there was no political will to delegate the power to one central player to consolidate public finances, and fiscal performance deteriorated. Similarly, when the veto player distance narrowed in countries that were expected "contract" states, a "delegation" form of fiscal governance would have been more

appropriate and fiscal performance similarly worsened. An example here would be the Irish government in 1989 or the Dutch government in 1994 (see Chap. 4 for further details).

The point here is that veto player distance can affect both the choice of fiscal institutions as well as their effectiveness. When institutions are designed for the long-term average, but there is a short-term deviation from this average, the institutions will remain in place but they will not function well.

Conclusion

This chapter argues that the veto player approach is intuitive, testable, and transparent. A principal strength is that it offers researchers a theoretical tool to compare across seemingly incomparable systems, be they democracies, non-democracies, single- or multi-party systems, or presidential and parliamentary systems. The approach therefore furthers our knowledge of comparative politics. The discussion of empirical evidence indicates that the approach has already demonstrated its empirical value when applied to political economy outputs in developed parliamentary democracies. Yet, the applicability to the rest of the world so far, although 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 is yet to be tested. There is so much more that can be, and should be, done in the future.

This chapter also suggests that more work should be done on how veto players affect the design of other institutions. Most work puts some variable for “veto player” on the right-hand side as an independent variable and some outcome variable like economic growth on the left-hand side. As the example from fiscal policy illustrates, there may be institutions that also bear explanation. The configuration of veto players leads to different types of coordination problems within government. Under one-party governments, the issue is to make decisions where the base preferences are essentially the same but where policymakers have incentives to consider only the tax burden that their constituencies bear, and this burden is usually less than the burden on the whole party (or country). One central player in the form of a finance minister can then act as the central coordinator. As the ideological distance among veto players increases, however, the nature of the coordination game changes. Parties with clear differences in what they consider to be an ideal policy must nevertheless make agreements. In the fiscal policy case, fiscal contracts that set clear rules and that are transparent for all players are preferable to granting discretion to one player who is common when ideological distance is zero or low.

Tsebelis’s work also reminds us of where to begin any analysis. He expects that we focus on actors whose assent is required to change the status quo. One must begin with 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.

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