# State of the Art Article

# Promises and Pitfalls of Veto Player Analysis

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### Abstract

Veto player approaches have come to occupy a central role in comparative politics. This article critically reviews the literature, focussing especially on veto player explanations of policy outputs and outcomes. The review highlights three problems empirical veto player studies have to face: 1) identifying the relevant veto players, 2) establishing equivalence between veto players, and 3) specifying (theoretically or empirically) veto players' policy preferences. The article concludes that empirical veto player analyses advance our understanding of political institutions and their effects, but that they should deal more systematically with the three above mentioned problems.

*Keywords:* *veto players, veto points, comparative public policy, intentional explanation*

### Introduction[1]

Veto point and veto player approaches have come to occupy a central place in comparative politics, especially in the fields of comparative public policy and political economy. Virtually every policy area has been studied within at least one of the various approaches, and the relevant literature grows at a fast pace. The most elaborate and prominent approach, George Tsebelis' veto player theory (Tsebelis 1995a; 2002), moves well beyond the explanation of particular policy outputs or economic outcomes and tries to provide a unified theoretical

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perspective on political institutions in a wide variety of political systems. Tsebelis' theory systematically relates veto players to the potential for policy change in a political system, which is in turn linked to important system characteristics such as regime stability, government stability or bureaucratic independence. Tsebelis' theory can thus be seen as the main (theoretically based) competitor to Lijphart's (empirically based) distinction between majoritarian and consensus democracy (Lijphart 1999).

This article has three related goals. First, I want to review the recent literature on veto points and veto players, focusing especially on explanations of policy outputs and economic outcomes. In doing this, I will for the sake of simplicity use "veto player *approaches*" as a general label for studies that highlight the importance of institutional veto power and refer to Tsebelis' work as veto player *theory*. Given the volume of the relevant literature, my aim is not to enumerate each and every veto player (hereafter: VP) study but to give the reader an idea of how this literature has developed. This point leads me to my second goal. While different VP approaches are often perceived as being close relatives, I want to emphasize important differences between them. To this end, I will highlight a few analytical dimensions along which VP approaches differ and classify VP approaches accordingly. This classification provides the basis for my third goal: highlighting important pitfalls of VP analysis. I will try to show that different VP approaches struggle with similar basic problems, but that the precise form in which these problems become virulent differs.

The article is set up as follows. The next section outlines the basics of VP analysis and suggests distinction which can be used to classify VP approaches. The following three sections then discuss three different VP literatures: 1) (comparative) case studies, 2) quantitative studies that make *assumptions* about players' substantive preferences and 3) quantitative studies that try to *measure* players' preferences. The latter of these three sections focuses on Tsebelis' veto player theory and its applications. The final section concludes with a list of questions which should be kept in mind in reading empirical VP analyses.

## Conceptual basics and problems of veto player approaches

The basic idea common to all VP approaches is simple: if some individual or collective actor has veto power (that is, under unanimity decision rules), she will use it to further her interests. More specifically, she will veto policies that go against her interests. Since veto power is a fact of political life, VP explanations of legislative processes and outcomes have a high *a priori* plausibility. The importance of veto players (hereafter: VPs) follows directly from the conception of legislators as *intentional* actors. Whenever we understand behaviour as

intentional action, we presume that actors are at least minimally rational and that their preferences and beliefs have an underlying structure that is at least temporarily "fixed". This is obviously true for VP arguments as well. If preferences were assumed to change constantly – and if we could not in some way take account of this change – we would never be able to say whether some actor accepted a policy proposal because she lacked the power to achieve a better outcome or because she changed her mind about what is the best outcome. VP arguments *as such* are thus not about how institutions shape policy preferences, but about how institutions influence policy output given actors' policy preferences.

The implications of the basic VP argument are straightforward: The shape of legislative policies is influenced (only) by VPs; and if many players have substantially different interests, they will likely find it difficult to agree on a change of the status quo policy. This insight is of course plain common sense. What characterizes VP approaches in modern comparative politics, however, is that they try to *specify* the common sense in ways that unify our conception of political systems, increase our explanatory and predictive leverage, and make VP arguments amenable to systematic empirical analysis. In pursuing these goals, empirical VP studies have to deal with three types of problems:

1. *Problem of identification*: Scholars have to distinguish real VPs from other potentially influential actors. On the *vertical* dimension the question is to what extent sets of individuals can be treated as collective VPs (e.g., parties versus party factions). On the *horizontal* dimension the question is whether particular powerful actors, such as courts, are really VPs.

2. *Problem of preference measurement*: Once the relevant VPs are identified, their preferences have to be determined (however roughly). Most particular predictions or explanations depend crucially on such preference attributions.

3. *Problem of equivalence*: Closely related to the problems of identification and preference measurement is the problem of equivalence. Are the relevant VPs really similar in all respects (other than their policy preferences), or is it necessary to distinguish different types of VPs?

These three problems are important in all VP analyses, but the way in which they come to the fore differs depending on the basic methodological approach adopted. Therefore, two additional distinctions become important. One is between qualitative and quantitative studies. In qualitative studies the three problems are closely intertwined and the main problem is how to justify the particular solutions adopted. Quantitative studies have to be further distinguished between those that make *theoretical assumptions* about actors' preferences and those that try to *measure* them. The following three sections deal

with these three main approaches in turn. Each section first gives an overview over the literature and then moves on to discuss how – and how successfully – different studies deal with the three problems outlined above.

## Qualitative studies

VP arguments have of course long been made in process analyses of legislative decision-making. The systematic comparative study of the role of institutional veto power on legislative processes and policy outputs can, however, be traced back to Immergut's (1992) study of health politics in the US, France, and Switzerland. In the meantime, many (comparative) case studies have adopted a VP approach. Topics and case selections are diverse, including, e.g., economic reform in transition economies (Hellman 1998), pension reform in Europe (Bonoli 2000), or investment decisions in developing countries (MacIntyre 2001).[2]

The particular arguments advanced by qualitative studies are quite different, however, depending on the subject matter as well as authors' (often implicit) theory of actors' preferences. For example, while Immergut emphasized how veto points provide opportunities for interest groups to *block* health care reform, Bonoli finds successful pension reform in all of his countries, but also governments making more concessions in multi-veto point systems; and Hellman even contends that more parties in governments meant *more* substantial policy change. As these examples already suggest, the critical aspect of these studies is not their reference to veto power, but to their specific arguments about who the relevant actors are (*identification*) and what these actors want (*preference measurement*).

This insight leads me to discuss the pitfalls of qualitative VP studies. The main problem is that as long as scholars are "flexible" enough in their attribution of preferences and beliefs to actors as well as their (dis-)aggregation of collective actors, a VP explanation can virtually *always* be developed. For example, Stewart (1991) explains US tax policy in terms of veto and agenda-setting power, but fails to justify his reconstruction of the relevant policy space as well as his attribution of policy preferences within that space. As Bradford (1991) shows, there are good reasons to doubt Stewart's story. Similarly, when Bonoli (2000: 84) finds that the powerful British government did seem to make significant concession in the face of strong opposition to their original pension reform plan, he disaggregates the British executive into separate players, so that the VP explanation still works.

My point is not so much that the above discussed explanations are misleading (though they may be). Rather, I want to highlight the danger that *ad*

[2] See also Haggard and McCubbins (2001), Haverland (2000), Mulé (2000), Ames (2001), Kaiser (2002) and Ganghof (2003).

*hoc*-decisions about which preferences to attribute to actors or which groups of actors to treat as VPs can lead to empty or shallow explanations. In the pension reform example, it could be that the changes that the British government made to its pension reform plan can also be described as the formation of more adequate beliefs about the short-term *economic or electoral costs* of pension reform and that these costs kept *all* governments from pursuing more ambitious reforms. If this were the case, the VP explanation of the British government's concessions would be too "shallow" to be explanatory (Miller 1987: 102-4).

My suggestion, then, is not to focus too much on the vetoing part of VP explanations. Since the importance of veto power is self-evident, authors' main task is to justify their identification of VPs and the "measurement" of these players' preferences. The difficulty is not to show that some VP explanation *can* be developed, but to try to *confirm* the particular explanation advanced. Most importantly, authors have to justify the preferences and beliefs they attribute to VPs. This is far from easy because these preferences and beliefs can never be measured directly but have to be inferred from the very behaviour that is to be explained; there is thus a permanent threat of circularity. While I cannot discuss justification strategies here, I believe that they should be a central concern of case study researchers (Bartelborth 1999; Ganghof 2003: ch. 2).

## Quantitative studies that *assume* VPs' preferences

This section discusses quantitative VP studies that make theoretical assumptions about VPs' substantive preferences rather than to measure these preferences. In political science the best-known literature of this kind analyzes various aspects of welfare state effort or redistribution, both in phases of expansion and retrenchment (Huber *et al.* 1993; Huber and Stephens 2001).[3] A literature of comparable size, written largely by economists, focuses on budget deficits (Roubini and Sachs 1989).[4] Many other literatures are emerging, some of which cover large sets of countries (Beck *et al.* 2001). Important outcome variables include, e.g., investment and economic growth (Henisz 2000a; 2000b), inflation (Treisman 2000; Keefer and Stasavage 2002) or the "rule of law" (Andrews and Montinola 2004).

Since VPs' preferences are typically *assumed* in this literature, it is obvious that different VP explanations may have very little in common. What

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[3] The literature on the role of VPs in welfare state expansion and retrenchment is enormous. See also Kittel and Obinger (2003), Schmidt (1996; 2001), Swank (2002) and the literature cited in these studies.

[4] A recent overview over the literature is given by Volkerink and de Haan (2001). See also Franzese (2001).

characterizes particular studies is their specific theory of preferences as well as the way in which they identify and distinguish VPs.[5] These aspects are closely related because distinguishing and counting VPs is the *only* way in which theoretical assumptions about preferences can be operationalized. For example, Huber *et al.* (1993) (implicitly) distinguish two types of veto players or points: parties in government are assumed to matter only insofar as they influenced the ideological orientation of the government, while institutional veto points such as strong second chambers or federalism are assumed to hinder welfare state retrenchment. Crepaz and Birchfield (drawing on Lijphart's work) make a related argument (Birchfield and Crepaz 1998; Crepaz 2001; see also Swank 2001; 2002). They agree to the point about institutional veto points made by Huber et al. but argue that parties in government and parliament are "collective" VPs, a higher *number* of which increases redistribution. Both groups of authors differ in the way in which they identify the two types of VPs.

Here we get to the potential problems of this literature: if authors differ in the way in which they identify and distinguish VPs as well as in their underlying theory of these actors' preferences, we would expect authors to carefully explain both their theory and how it relates to the used VP indices. After all, regression results in economics and political science – and especially in comparative political economy – have to be seen as certain types of observations rather than causal inferences (Hoover 1994). They don't *provide* explanations, they *invite* them. Hence, we would expect authors to discuss in some detail why their theory helps to explain statistical findings. This, however, is often not the case.

Consider the two examples mentioned above. Huber et al. (1993) carefully explain their theoretical logic, according to which veto points are access points for special-interest groups that oppose welfare state expansion. However, there is no discussion about how this theory relates to the actual "constitutional structures" index used in the regressions. This 7-point index simply *adds* the various veto points, implying that the seventh veto point in one country has the same effect as the first. What is more, for bicameralism and federalism Huber et al. distinguish between weak and strong forms, with the effect that the strong forms have twice the weight of the other types of veto points, presidentialism

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[5] In part of this literature, the effect of veto players is indirect. For example, many authors have argued that the number of veto players determines the independence of central banks which in turn affects inflation. Even in these kinds of arguments, however, implicit assumptions are made about the preferences of veto players. For example, if all veto players were assumed to have the same preferences as the central bank itself, their number should be irrelevant to inflation. Essentially the same is true for the argument that many veto players increase growth and investments by creating a "credible commitment" not to intervene into markets.

and referendums.[6] Both of these coding decisions not only lack a clear theoretical rationale but are also very important because they effectively increase the veto point scores of the US and Switzerland – the two cases that are most supportive of the authors' arguments (cf. Obinger and Wagschal 2000).

Similar problems plague the distinction between collective and competitive VPs made by Crepaz. First of all, his theory of actors' preferences is rather vague, for he merely mentions various reasons why "collective" VPs could lead to more redistribution and higher expenditures, e.g., "inclusion of interests", "logrolling" and "collective action problems" (Crepaz 2002: 174). This seems insufficient as a theoretical basis of coding decisions. Logrolling can hardly explain redistribution, and without additional assumptions it can also not explain government expansion. For instance, disciplined parties can form preferences about the size of government and design institutions that solve their collective action problem. Secondly, the relationship between Crepaz' theory and his empirical indicators are not clear. These indicators imply, e.g., that a pivotal opposition party in Denmark's unicameral parliament is a "consensual" player, while a pivotal opposition party in Australia's second chamber (the Senate) is "competitive". There might be a good reason for this, but Crepaz does not give one.

The basic danger is thus similar to that in the qualitative literature. Unless coding decisions are systematically guided by theory, there is a danger that quantitative VP "explanations" are too easy to construct. In fact, they risk becoming mainly exercises in data mining. Since there has been a proliferation of VP indices – with the included types of VPs ranging from two to ten (e.g., Schmidt 2000: 352; 2001: 40; Swank 2002; Kittel and Obinger 2003) – and since the ranking of countries is quite different in the various indices, it is likely that at least one of them is "significantly" correlated with some dependent variable of interest. The careful establishment of the links between theory and measurement should therefore be a major concern of quantitative VP studies.

## Quantitative studies that *measure* VPs' preferences: veto player theory

This section focuses on Tsebelis' (1995; 2002) VP theory. This theory uses the notion of VPs to build a comprehensive rational-choice institutionalist theory of

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[6] The original index Huber et al. (1993) also includes a veto point for the electoral system, which serves as a proxy for party discipline. This item of the index is also weighted more heavily. In their later work, however, Huber and Stephens (2001: 372, n. 10) eliminate this item from the index, because electoral systems are only one of various factors that influence party discipline.

comparative political institutions. VPs are individual or collective actors whose agreement is required for policy decisions. VPs can be both parties (e.g., as part of a coalition government) and institutions such as a second chamber.

There are two main reasons why Tsebelis' theory fits well into this section. First, in contrast to most of the quantitative approaches discussed above, Tsebelis eschews assumptions about the *content* of VPs' preferences. Getting at these preferences is for him mainly a question of empirical measurement. Second, however, he does make very strong assumptions about the *general nature and shape* of actors' preferences. Most importantly, Tsebelis goes beyond the standard assumption in spatial models, which is that actors have a unique ideal point in some policy space and that their policy utility decreases continuously and symmetrically with the policy's distance from this point. He assumes that when the relevant policy space has two or more dimensions, each dimension is *weighted equally* ("Euclidian preferences") and there are no side payments. Without this assumption virtually nothing general could be said about the importance of VPs, because even many VPs could easily agree on policy change if they cared about different policy dimensions. The formal structure that Tsebelis' assumptions add to the commonsensical VP argument allows Tsebelis to derive precise hypotheses as well as to deal with the *problems of identification and equivalence* in a theoretically guided manner. Tsebelis is committed to formulating an empirically testable comparative theory and hence uses his assumptions to derive rules for how to code VPs in comparative perspective.

Tsebelis' more general claims can be summarized succinctly: a political system's potential for policy change – or, conversely, its *policy stability* – is a function of only three variables: 1) the number of VPs, 2) the distances between these players' policy ideal points (congruence) and 3) VPs' internal cohesion. As this formulation makes clear, more specific hypotheses can typically only be derived once one knows more about the location of actors' preferences and how the decision-making process is structured – both between players and within collective players. A few general hypotheses can be stated, however. Increasing the number of VPs tends to increase *policy stability* within a system, and it will never decrease it. If the ideal point of a new player is located in the Pareto Set of the existing players (the set of policies that could not be changed without leaving at least one player worse off), however, this player has no effect on policy stability – in Tsebelis' terms, the players is "absorbed". Via their effect on policy stability VPs are assumed to affect a number of important characteristics of political systems. High policy stability reduces the importance of players' agenda-setting power, because there are not many agreeable policies from which agenda-setters can choose. Policy stability may also lead to government instability in parliamentary systems, because governments may resign if they cannot get anything done. For similar reasons, it may lead to regime instability

in presidential systems. Finally, high policy stability may lead bureaucrats and judges to be more active and independent from the political system.

The great appeal of Tsebelis' theory stems from its potential to *unify* our understanding of political systems. The theory cuts across long established categorizations of political systems and it systematically analyzes institutions such as direct democracy or presidentialism, which are not easy to deal with in Lijphart's (1999) approach (Hug and Tsebelis 2001; Tsebelis 2002: 109-12). For instance, the difference between presidential and parliamentary systems is treated mainly as an issue of legislative agenda control. Tsebelis argues that the parliamentary system gives most legislative power to the government, while the prototypical presidential system gives agenda control to the parliament. While considerable controversy exists in the literature about how much agenda setting powers presidents effectively have in different presidential systems (Tsebelis 2002: 112-14), Tsebelis' focus on agenda control seems nevertheless useful to analyse the similarities and differences of presidentialism and parliamentarism.

What about *empirical* performance? Most of the studies discussed in the previous two sections are broadly consistent with Tsebelis' basic propositions. However, there is also an increasing number of self-proclaimed tests or applications of VP theory – although not all of these actually measure VPs' preferences. For example, Kreppel (1997) looks at Italian legislative output over time, Hallerberg and Basinger (1998) analyze changes in corporate and personal income tax rates in OECD countries, Bawn (1999) examines German spending patterns over time, Tsebelis (1999; 2002: ch. 7) examines labor law production in European democracies, and Tsebelis and Chang (2001) as well as Bräuninger (2003) analyze the structure of budgets in OECD countries. All of these studies find the main propositions of VP theory corroborated.

How convincing are these tests? Can they really be seen as corroborations of the theory? And if so, what do they corroborate; only the basic claim that intentional actors' use their veto power to pursue their preferences, or more specific claims of Tsebelis' formal theory? Answering these questions turns out to be quite difficult. What I want to do here is to highlight a number of problems seldom discussed by protagonists of VP theory. I will first deal with issues of preference measurement and then with the question of identifying VPs and establishing their equivalence.

## The problem of preference measurement

Tsebelis assumes that actors have *fixed* policy preferences. This assumption is reasonable because it underlies the commonsensical VP argument. To understand its problems, however, we have to be clear about the concept of "policy preferences" – which Tsebelis is not. Policy preferences refer to actors'

ranking of (potential) legislative projects. However, legislators don't care about such projects as such. Policies are *means* rather than *ends*. Policy preferences are thus derived from actors' more basic goals or preferences as well as their *beliefs* about how these are related to policies. I find it useful to distinguish two types of basic preferences. First, VPs can be assumed to care about certain *outcomes* in the world (e.g., economic growth); they have *outcome preferences*. Second, at least partisan VPs (political parties with veto power) may also care about being re-elected and getting into government offices; they have *positional preferences*. The policy preferences that VP theory focuses on are thus derived from more basic outcome and positional preferences; they are "final" preferences in the sense that they are not anymore mediated by actors' beliefs about how to pursue their *basic* goals.[7]

Ideally, therefore, empirical tests of VP theory would have to measure these final policy preferences. Yet this is virtually impossible. Available measures of collective actors' preferences – whether they are based on expert surveys, party manifestos, or what have you – can generally not be seen as measures of final policy preferences. Whether and to what extent this is the case depends on the dependent variable under consideration. For example, if a study tries to explain an aggregate outcome variable such as total government spending or taxation, Laver and Hunt's (1992) expert scores of parties' positions on an "increase taxes versus cut spending" scale may be considered a proxy of parties' policy preferences. However, the same measure seems ill-suited as a proxy for parties' policy preferences on, say, corporate tax rates. If the measure of policy preferences does not fit to the dependent variable studied, a mechanistic application of VP theory may generate misleading theoretical expectations and problematic empirical tests.

Consider two examples. Given its assumption of fixed policy preferences, VP theory argues that exogenous shocks are difficult to handle by multi-VP systems. This is true as far as it goes, but exogenous shocks may also *change* VPs' policy preferences by changing their beliefs about the mapping of policies onto outcomes. It is precisely this problem that plagues Hallerberg and Basinger's (1998) comparative application of VP theory (see also Wagschal 1999b). The US tax reform of 1986 was widely believed to be a strong shock for other OECD countries, forcing them to reduce both their corporate and personal tax rates. Using this shock as a starting point, the authors assume that the "demand" for reform was roughly constant across countries, everything else being equal, so that the effect of the number of VPs on reform "supply" could be measured

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[7] Of course, the translation of policy preferences into legislative action is mediated by actors' beliefs about the allocation of veto and agenda-setting power, but these beliefs are generally assumed to explicable in terms of the *actual* allocation of institutional power.

by the magnitude of policy change in the adjustment period after the shock. They use a dummy variable to distinguish systems with one and more VPs. According to Tsebelis (2002: 203-4), the use of a dummy variable is consistent with VP theory because "in a single dimension what matters is the ideological distance among coalition partners. While single-party governments have by definition range [sic] of zero, the range of two or multiparty governments is not necessarily related to the number of partners". Hallerberg and Basinger find, as expected, that more VPs reduced the magnitude of tax cuts. Here is how Tsebelis interprets these results:

> [T]he possibility for single-party or small-range government to change the status quo significantly may enable a country to adapt more easily to exogenous policy shocks.… Once the United States under Reagan reduced taxes for companies and individuals in the highest personal income bracket, other industrialized countries followed. Rates were adjusted by larger or smaller amounts. Among those that made large adjustments were the single-party labor governments of New Zealand and Australia. These governments were leftist (although moderate), and in principle they were not advocates of tax reductions for the rich. Once they decided to decrease taxes, however, partisanship was immaterial: The reductions were comparable to those of Thatcher's Conservative government in the United Kingdom. (Tsebelis 1999: 604)

This explanation is instructive because it is so obviously question-begging: Why was partisanship immaterial? Why did left-wing parties decide to cut tax rates although they were not generally in favour of tax reductions for the rich? The only satisfactory answer, outside of VP theory's conceptual map, is that Finance Ministers' *beliefs* about the mapping of tax rates onto desired policy outcomes changed and led to a convergence of policy preferences across countries. The economic shock must have been so strong (relative to the costs of national policy adjustment) that ideological differences between left and right generally became unimportant. If this is acknowledged, however, another question arises: If *agenda setters*' policy preferences converged across countries, shouldn't we expect *VPs*' policy preferences to have converged within countries so that the number of VP becomes irrelevant? Elsewhere I present qualitative and quantitative evidence that this was indeed the case (Ganghof 1999; 2003). The Australian case mentioned in Tsebelis' quote turns out to be an example for this within-country preference convergence, because there *was* an additional veto player: the Senate.[8]

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[8] Tsebelis and Money (1997: 624) incorrectly state that the Australian Senate is not a veto player with respect to money bills. While the Senate can neither initiate nor amend money bills, it can veto them (Evans 2001).

One could object to this line of reasoning on the grounds that Hallerberg and Basinger should have measured the "ideological distance" between VPs rather than their number. This would not make much of a difference, however, because data on players' *policy* preferences is simply not available. For example, to measure the partisan orientation of governments, the authors used Castles and Mair's (1984) expert judgements about parties' positions on a left-right scale. This data would obviously be ill-suited as a proxy of policy preferences (after the exogenous shock); it is at best a proxy of outcome preferences (before the shock).

A second example that highlights the problem of measuring policy preferences is Tsebelis' (1999; 2002: ch. 7) own analysis of law production in the area of labor regulation. Tsebelis uses a data set (constructed by Herbert Döring and his collaborators) which identifies significant legislation in a comparable manner. He estimates the effect of the "ideological distance" between VPs on the number of laws produced. He finds a statistically significant effect, suggesting that the ideologically most diverse government on average produced about one labor law less than a single-party government. Much could be said about the robustness of these results (or the lack thereof). For example, his regression results are strongly driven by four exceptional governments – in Belgium, Sweden, Greece, and the United Kingdom – three of which are characterized by a very long duration in office (which gives laws more time to accumulate). If these four cases were dropped, the relationship between VP "range" and law production would not be significant (Tsebelis 1999: 604; 2002: 179). Thus, one may wonder whether Tsebelis' empirical results are a very impressive corroboration of his theory.

My question however is a more basic one: Can his results count as corroboration of his theory *at all*? After all, corroboration means non-falsification and thus presupposes that falsification is possible. But could VP theory really be falsified? I doubt that because such falsification would in my view demand fairly good measures of VPs' *policy* preferences. Tsebelis does not have such measures. To be able to measure the ideological distance between VPs at all, he has to make strong assumptions (that the policy space is one-dimensional, but that the status quo is nevertheless not on this dimension) and rely mainly on crude and time-invariant expert judgments about parties' positions on a general left-right scale. Therefore, had VP theory failed Tsebelis' test, would we consider the theory falsified or would we rather reject the auxiliary assumption that Tsebelis' measure of *policy* preferences was adequate? I find the latter option more likely, which means that Tsebelis' test could neither falsify nor corroborate his specific theory.

If this argument is along the right lines, VP theory might be better considered a very coherent theoretical framework rather than an empirically testable theory.

Testable claims would then depend on further theoretical assumptions about the content of players' policy preferences, or about how various measures of players' "positions" on some scale actually relate to final policy preferences.

If the specifics of Tsebelis' VP theory are difficult to corroborate through statistical tests, such tests may also not be a good basis for judging the adequacy of Tsebelis' coding rules. This makes it important to understand how these rules follow from his assumptions about preferences. Providing such understanding is the task of the next two sections. I will start with the *problem of identification* and move on to the *problem of equivalence*.

## The problem of identification

To establish whether some actor is a VP or not, at least two questions have to be answered: 1) Does this actor have effective veto power? 2) How likely is this actor's ideal point located in the Pareto Set of other (already identified) VPs ("absorption")? The second question brings us back to the distinction between outcome and policy preferences. Since Tsebelis largely ignores it, he may overestimate the extent to which some types of actors are in fact absorbed. Constitutional review is a case in point. Tsebelis argues that when judges are appointed by partisan VPs, they will be selected by the partisan players for their competence and "(known) policy position" (Tsebelis 2002: 227). As a result, they will usually be absorbed. In practice, however, judges may often be rather selected for their *outcome* preferences – either because this type of selection is cognitively less demanding for the partisan players or because judges' policy preferences are unknown. In this case, judges may frequently be outside of the partisan VPs' Pareto Set in the space of policies, even if they are in this set in the space of outcomes. The reason is that politicians and judges face very different constraints in translating outcome into policy preferences.

German tax policy provides a good example. In Germany the constitutionally protected right of equality is also applied to taxation. Parties and judges alike embrace the basic principle of taxing different types of incomes or activities equally. However, in translating this basic principle into operative policy preferences, parties and judges face very different constraints. Parties have to trade-off tax equality against budgetary, electoral, and efficiency goals and thus almost always form policy preferences for discriminatory taxation. By contrast, the Constitutional Court has to consider legally defensible alternatives and may therefore form policy preferences for equal taxation. The policy preferences of judges and parties may diverge strongly, so that courts become crucial VPs in the area of taxation. This is precisely what happened in German tax policy (Ganghof 2003: ch. 8).

Consider next the question of when VPs do have effective veto power. This is partly a formal question, which is easy to answer. For instance, to decide

whether some (non-absorbed) second chamber has veto power on some issue, it is usually sufficient to look into the country's constitution. However, some cases are more difficult to decide. The ones that have received most attention in the literature are oversized coalitions and minority governments. Tsebelis (1995b; 2002) holds that what is easier to count is also correct: opposition parties are *no* (potential) VPs in the case of minority government, but *all* coalition parties are VPs in the case of oversized coalitions. This position stands in contrast to the views of other comparativists. Strøm (2000: 280; Strøm and Müller 1999: 259-61) argues that particular parties can and frequently are outvoted in oversized coalition governments and many authors consider opposition parties to provide powerful checks on minority governments (e.g., Laver and Shepsle 1991; Powell 2000).

To show how Tsebelis' position on these issues follows from his basic assumption about actors' preferences, I will focus on the case of minority governments.[9] Tsebelis not only assumes that actors have fixed policy preferences but also that the more basic positional preferences matter *only* via the formation of policy preferences. He views policy platforms as being optimally designed to balance actors' more basic goals. Therefore, *within the legislative arena* veto players behave as *pure policy-seekers*.

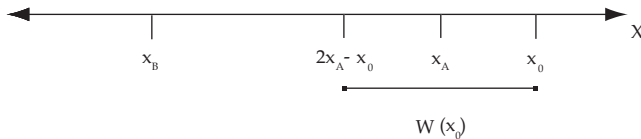**Figure 1:** *Illustration of the basic veto player model in one dimension*



Figure 1 illustrates, for a one-dimensional policy space, what this assumption implies. The horizontal line represents the policy space $X$. An actor $i$ has a unique ideal point in that policy space, here denoted by $x_i$. The utility of actor $i$ is assumed to be a function of the policy $x$. Each actor's policy utility is highest at her ideal point and all that actors are concerned about is to move the status quo as close as possible to their own ideal point. Players A and B consequently will vote for any policy proposal that is contained in their policy-based *preferred-to-sets*: the set of policies that are closer to their ideal point than the status quo ($x_0$). Assuming that A and B are VPs, the intersection of their

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[9] The following draws on the more comprehensive analysis by Ganghof and Bräuninger (2003).

preferred-to-sets, the *winset of the status quo*, $W(x_0)$, contains all points that both players prefer to the status quo. In Figure 1, the preferred-to-sets of A and B consist of all policies in the intervals $[2x_A\text{-}x_0]$ and $[2x_B\text{-}x_0]$, respectively. The winset of the status quo is equal to actor A's preferred-to-set. Even though A and B agree on the direction of policy change they disagree on the location of the policy $x \in W(x_0)$ that could replace the status quo. Standard VP theory assumes that the location of the status quo replacement depends on the actors' agenda setting power rather than on their electoral or parliamentary strength. If A has the power to make a take-it-or-leave-it offer to B, she will propose her own ideal point $x_A$ and B will accept.

The crucial assumption is that an actor *always* accepts even very small gains as long as they exceed transaction costs – and if her agenda-setting power does not allow her to extract larger gains. This implication drives Tsebelis' treatment of the identification problem in the case of minority governments. Tsebelis argues that minority governments, especially one-party minority governments, are located in the centre of the multidimensional policy space. As a result, on each major policy dimension, the situation is supposed to look similar to that depicted by Figure 1. Let A be a one-party minority government and B the opposition party. If the two actors are pure "policy-seekers" within the legislative arena and if the government controls the agenda, A will propose its ideal point and B will accept this proposal because she only cares about whether or not $x_A$ is better in policy terms than the status quo. More specifically, she does not care about how much *potential* policy gain she sacrifices by accepting policy $x_A$ rather than $2x_A\text{-}x_0$ – the winset alternative that is closest to her ideal point. The preferences of the opposition party do not seem to matter, for the government is free to choose its preferred policy. Hence, while the opposition party has *formal* veto power, it does not seem to have *effective* veto power.

Tsebelis' reasoning is not only predicated on certain assumptions about the location of minority governments and their effective agenda-setting powers, but it is also based on the view that actors' positional preferences do not have any *direct* impact on actors' utility. This view stands in contrast to arguments advanced in the qualitative literature on legislative policy-making. Many studies have argued that even if actors could find a compromise, they may frequently have an incentive to eschew compromise and try to lay the blame for the consequent policy failures on each other (e.g., Sundquist 1988; Scharpf 1997; Huber 1999; Zohlnhöfer 1999). The underlying assumption is typically that many voters reward parties for "getting things done" and that governing parties are in better position to claim credit for policy change than opposition parties.

**Figure 2:** *Illustration of the sacrifice ratio in one dimension*
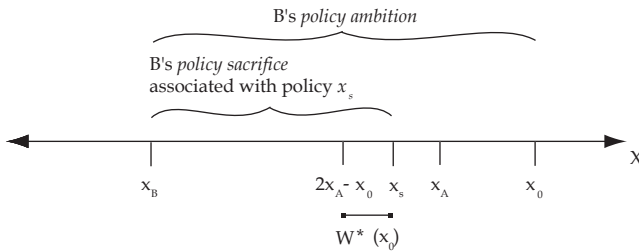


What happens if we include such considerations into the basic VP model? One way to think about this is illustrated in Figure 2.[10] If the opposition party B believes that its electoral prospects will be worsened (relative to the other parties) by helping the government to get things done, this consideration will reduce B's utility from changing the status quo. However, this disutility is unlikely to be constant. Rather, it will itself depend on *where* the new policy lies. If the policy is close to B's ideal point, the party is unlikely to suffer from collaborating with the government. If it is far away, however, the (expected) electoral losses may be quite substantial. This reasoning can be expressed in terms of an actor's *sacrifice ratio*, which puts the maximal *policy sacrifice* (the distance between her ideal point and the new policy) an actor is willing to make in relation to her *policy ambition* (the distance between her ideal point and the status quo ante). In Tsebelis' theory, actors are willing to agree to policies that are associated with very small policy gains (as long as these are bigger than transaction costs); their sacrifice ratio is one. By contrast, if electoral considerations are allowed to have a direct impact on actors' utility function in the way described above, actors' sacrifice ratios can be below or above one, depending on the way in which the institutional and non-institutional context in which they are embedded shapes their positional considerations.

In Figure 2, assume that actor A has a sacrifice ratio of one (pure policy-seeking), while actor B has one of around 0.6. This ratio is expressed by the policy $x_s$. At this point, B's *electoral losses* of collaborating with the government are exactly offset by the *policy gain* associated with replacing the status quo with policy $x_s$. Actor B's *preferred-to-set* is now smaller than in Figure 1, leading to a smaller winset, $W^*(x_0)$. The governing party A has to propose at least $x_s$ to get B's approval. A will make this offer because the party prefers $x_s$ to the status quo and knows about B's sacrifice ratio. Party B's electoral considerations increase its

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[10] The following provides the intuition behind an extended veto player model suggested in Ganghof and Bräuninger (2003).

"bargaining power" because they make its threat to veto any policy to the right of $x_s$ credible. Hence, with a sacrifice ratio significantly below one, opposition parties may have *effective* veto power, providing powerful checks on minority governments. Empirical evidence is in line with this view (e.g., Damgaard and Svensson 1989; Green-Pedersen 2001).

As this example shows, Tsebelis' rules for identifying VPs are disputable because they are based on disputable assumptions. Scholars applying VP theory have to make a decision whether or not they embrace these assumptions. If they want to neither embrace Tsebelis' coding rules nor reject Tsebelis' theory altogether, they face the task of specifying alternative assumptions on which coding rules can be based.

## The problem of equivalence

One conclusion that can be drawn from the above discussion is that rules for identifying VPs should be permissive: all *potential* VPs should be counted. Such a counting rule has indeed been suggested by Kaiser (1998: 213) and has inspired Schmidt's (2000: 352) very comprehensive VP index.[11] The problem with permissive counting, however, is that it becomes increasingly difficult to assume that all VPs have a roughly equal average effect on policy stability. This raises the *problem of equivalence*: Is it useful and possible to distinguish different types of VPs?

Kaiser (1997: 436; 1998: 213-214) actually suggests distinguishing between four veto points on the basis of their intended effects: consociational, delegatory, expert, and legislatory. However, this suggestion is mainly typological and not linked to particular hypotheses about how different types of VPs affect policy stability. Kaiser does not aim at a theory of VPs' behaviour (Kaiser 1997: 437). In contrast, Wagschal (1999b; 1999a) is willing to assume differential effects of different types of VPs on policy stability. Inspired by the qualitative literature on VPs, he suggests to distinguish between competitive and consensual VPs. Wagschal differs from Crepaz in that he does not want to advance a theory of VPs' *substantive* preferences which links certain VPs to particular outcomes like redistribution. However, neither does Wagschal offer a theory of what makes some VPs' competitive and others consensual. Like Kaiser, he seems to treat VP behaviour mainly as an empirical matter. He argues that the same players may sometimes be competitive and sometimes consensual (Wagschal 1999a: 630), so that the coding of players has to be partly based on how they actually *use* their veto power (Wagschal 1999b: 239).

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[11] For a critical discussion of permissive veto player counting, see Fuchs (2000).

I believe that if our goal is to *explain* legislative behaviour and output, coding decisions should be based on a clearly specified theoretical logic. If we want to motivate a distinction between "weak" and "strong" VPs, we should explore how Tsebelis' theoretical model has to be changed or extended. In the remainder of this section, I want to sketch some ideas and highlight some difficulties of such an enterprise. The starting point is the two characteristics of VPs recognized by Tsebelis: the internal *cohesion* of VPs and the distance between their policy preferences (*congruence*). Any theoretical extension of VP theory has to either modify the theoretical treatment of these two variables or specify additional characteristics. I want to give brief examples for all three options.

1. The *cohesion* variable could in principle provide a basis for distinguishing different types of VPs in quantitative empirical analyses. It has often been argued that under conditions of divided government in presidential systems policy change is facilitated by not too high levels of party discipline (e.g., Sartori 1994: 94; Mainwaring and Shugart 1997: 418-421; Tsebelis 2002: 84-85). A similar argument could be made about the German *Bundesrat*, where Länder governments do not always follow the party line. Less than perfect party discipline may thus be one reason to consider some VPs "weaker" than others in quantitative analyses.[12]

2. As to VP *congruence*, one argument could be that this variable is partly *endogenous* to different VP constellations. More specifically, one can argue that the policy preferences of coalition parties that share government responsibility converge somewhat, while opposition parties controlling legislatory veto points are keen on setting themselves apart from the government. Such an argument would certainly move beyond the rational choice framework. In my view, the endogeneity of policy preferences is best conceptualized in terms of *motivated reasoning* (Kunda 1990). When VPs update their beliefs about the mapping of policies onto outcomes, they do not engage in unbiased Bayesian belief updating. Rather, "motivation may affect reasoning through reliance on a biased set of cognitive processes: strategies for accessing, constructing, and evaluating beliefs" (Kunda 1990: 481). Actors are not only driven by accuracy goals, which motivate them to try and reach a "correct" conclusion, but also by *directional goals*, which motivate them to justify a particular (preselected) conclusion. VPs' government status may thus systematically affect their beliefs about the mapping of policies onto outcomes, either hindering or facilitating policy change.

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[12] Note that a more extensive discussion would have to clearly distinguish between the concepts of "cohesion" and "discipline". The former – on which Tsebelis' theory focuses – refers to the location of individuals' policy preferences, the latter to these individuals' willingness to act upon the commands of the leaders of the collective player.

3. An example for a newly added variable would be players' *sacrifice ratio* sketched above. The sacrifice ratios of different types of players may differ in systematic ways. Once again, government status would seem to be the crucial variable. Coalition parties, especially minor ones, that are keen on being and staying part of governing coalition, may have a sacrifice ratio of above one. That is, since they receive *positional gains* from "getting things done", they may be willing to accept (minor) *policy losses*. Conversely, opposition parties may often have a sacrifice ratio of significantly below one, because the government parties are in better position to claim credit for policy change. As a result, opposition parties may demand significant *policy gains* in order to compensate *positional losses*. The notion of the sacrifice ratio may thus also provide a basis for distinguishing between "weak" and "strong" VPs (Ganghof and Bräuninger 2003).

While these three examples show that there are indeed ways to distinguish different types of VPs on a theoretical basis, they also suggest that this is not an easy exercise. One reason is that different theoretical ideas may point in opposing directions: An oppositional majority in the German Bundesrat may be a "weak" veto player in that it is not very "disciplined" but a "strong" one in that it has an electoral incentive to keep the government from getting things done.[13]

Another reason for the difficulty of distinguishing VPs is that one might have to know quite bit a about particular systems in order to adequately gauge the "strength" of particular VPs. Consider the case of the Australian Senate. One point on which authors like Wagschal and Crepaz agree is that strong second chambers controlled by opposition parties are "competitive" VPs. In the case of the Senate, however, this is clearly a disputable claim. Since 1962 (with the exception of the period 1976 to 1981) the pivotal position in the Australian Senate has been controlled by minor parties and/or independents, most notably the Australian Democrats, which have virtually no chance of gaining representation in the House of Representatives. The reason is that since 1949 the Senate has been elected under the Single Transferable Vote system, while the House continued to be elected under the Alternative Vote system. These pivotal players have very different vote- and office-seeking incentives compared to the major parties. If the *major* opposition party (or parties) controlled the Senate, its electoral incentives could turn them into an especially "strong" veto player. By contrast, minor parties have little incentive to block legislation in order to hurt the government because they have no chance of winning government office themselves. Their main partisan purpose is to review and modify government policies, and their electoral incentives actually tend to make them more rather

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[13] For VP analyses of German bicameralism, see Bräuninger and König (1999) as well as König (2001).

than less accommodating. Conventional opinion is that the idea of "responsible government" confers a mandate to govern on the majority party in the House of Representatives and "[m]inor parties are only too aware of this conventional opinion" (Young 1999: 16). Minor parties are thus careful not to be charged with obstructionist behaviour. In sum, it may be quite misleading to describe the Senate as a "competitive" VP.

Summing up, there can be no doubt that he *problem of equivalence* raised by Kaiser and Wagschal is of major importance for quantitative VP studies. If such studies have to work with fairly small samples, it may make quite a difference which VPs are treated as being equivalent. Much work remains to be done, however, to put different coding rules on a solid theoretical footing.

## Conclusion

VP approaches have greatly advanced our understanding of comparative politics and political economy. VP approaches in general, and Tsebelis' veto player theory in particular, provide for conceptual and theoretical unification of the study of political institutions and their effects. This unification as such increases scientific understanding. Conceptual and theoretical unification is one thing, however, empirical analysis quite another. My review has highlighted three basic problems of empirical veto player analyses: 1) How do we conceptualize and measure veto players' preferences? 2) How do we identify the relevant veto players? 3) How do we establish equivalence between different players? While these questions are central to empirical veto player analyses, they are not always answered convincingly. My main conclusion is therefore that further progress in comparative veto player analysis partly depends on a more explicit and more systematic tackling of these three problems.

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### Potentiale und Probleme der Vetospieleranalyse

Vetospieleransätze spielen mittlerweile eine zentrale Rolle in der vergleichenden Politikwissenschaft. Dieser Artikel gibt einen Überblick über die Literatur, wobei der Schwerpunkt auf Vetospieler-Erklärungen

von Policy-Entscheidungen und Ergebnissen liegt. Die Diskussion betont drei Probleme empirischer Vetospielerstudien: 1) die Identifizierung von Vetospielern, 2) die Feststellung der Äquivalenz unterschiedlicher Vetospieler und 3) die (theoretische oder empirische) Spezifizierung der Policy-Präferenzen von Vetospielern. Die wesentliche Schlussfolgerung des Artikels lautet, dass empirische Vetospielerstudien das wissenschaftliche Verständnis politischer Institutionen vertiefen, dass jedoch die drei genannten Probleme systematischer behandelt werden sollten.

**Potentialités et problèmes  de l'analyse des "veto players"**

L'analyse des "veto players" a pris une place importante  dans le domaine de la politique comparée. Cet article se propose de passer en revue cette littérature de manière critique en mettant l'accent sur l'impact des "veto players" sur les "outputs" et les "outcomes" des politiques publiques.Cette discussion met en évidence trois problèmes auxquels est confrontée l'analyse empirique des "veto players" : 1) l'identification des "veto players" pertinents, 2) l'établissement d'une équivalence entre les "veto players", 3) la spécification (théorique ou empirique) des préférences  de "veto players". L'auteur conclut que l'analyse empirique des "veto players" est un outil important pour la compréhension des institutions politiques et de leurs effets, mais que cette analyse devrait se pencher systématiquement sur les problèmes mentionné ci-dessus.

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