Chapter 1
Veto Player Theory and Policy Change: An Introduction

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Some months ago I had the honor to be invited to Mannheim for a conference organized by Thomas Koenig and Marc Debus. The subject matter was "Reform Processes and Policy Change," and the organizers thought that my book *Veto Players* would be a good starting point for their study. I thought that the conference was an excellent idea, particularly since the reputation of Mannheim on policy studies is outstanding. Little did I know that this would be only a first step, because they now have received an interdisciplinary multimillion grant from the German Government (SFB 884) to "Political Economy of Reforms" (http://reforms.uni-mannheim.de/english/startpage/index.html).

I was excited to see the work of a number of colleagues on topics and methods related to my own research. I think I speak not only about myself, but about most of them when I say that I (we) came out of these meetings knowing and understanding a lot more about a series of topics related to the political economy of institutions and policymaking. This is why the organizers and I decided to bring this work to the attention of the scholarly public. I am contributing the introduction to this edited volume, and the organizers (along with their individual contributions) will draw the conclusions.

This Introduction will address issues raised both in *Veto Players* and in the works of the participants of that conference. The reader will be able to study this book without having read *Veto Players* previously and will be able to understand that, besides the common interests in substance and methodology which the authors of this book share, and aside from the appreciation of each other's work and contribution, we also have disagreements. And it is the combination of appreciation and disagreement that is essential to the growth of knowledge. We read each other's work and are fascinated by it, but we express our disagreements openly and sincerely, trying to persuade each other about the validity of our arguments, and argue that our expectations are corroborated by the data.

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This introduction has two parts: the first introduces the reader to elements of *Veto Players* that will be addressed by the subsequent chapters; the second points out the differences between the specific chapters and the original book. These differences range from disagreements in some auxiliary assumptions that lead to different conclusions, to the significance and applicability of the model to different phenomena, to the methods applied in the chapters, to evaluating the preferences of actors, to estimating the accuracy of predictions, etc.

Arguments Made in *Veto Players*

This book connects political institutions to policy outcomes. Focussing on policy outcomes is important because the impact of public policies on the life of citizens is overwhelming. If we think about it, the most important events in our life (besides significant developments in the immediate family like deaths, births of children, etc.) are decisions made within the political system to expand or contract healthcare, or unemployment benefits, or educational privileges, or taxes etc.

While individual citizens are interested in specific policies, and have strong preferences in one dimension (like tax reduction) or another (such as education), the way one can address all of these potential changes in a systematic comparative way is to assess the possibility of changing the status quo policy (no matter what the status quo is). This is the goal of *Veto Players.* Most of the propositions developed in the book regard the impact of particular institutional settings on "policy stability," that is, the difficulty of making a significant change to the status quo. The book argues that policy stability is the effect of a constellation of "veto players" (vp): individual or collective actors whose agreement is necessary for the change of the status quo.

From these two definitions, a series of propositions follows, some of them known through folk knowledge for millennia. One analytical truth connecting veto players with policy stability is that as the number of vp increases, policy stability does not decrease (a change of the status quo does not becomes easier, though it may not become more difficult). I call this proposition an analytical truth–because it is the consequence of the definition of the terms alone–and it is logically inconceivable that it will be otherwise, no matter what additional assumptions one would make. For example, if a veto player decides to veto some of the policies that others accept for changes to the status quo, then the player's existence will make the change to the status quo more difficult, while if s(he) agrees with everything that the others agree to, this player's addition will not change anything in terms of policy outcomes. What is not possible under any set of conditions is to add a veto player and make changes in the status quo easier.

Another analytic truth concerns the ideological distances of veto players. What is required for this proposition are two different assumptions: First, common knowledge of the location of the different veto players in the policy space, and second, that they exercise their veto only on the basis of their policy preferences, not on the basis of other considerations (say electoral goals, appeal to their constituency, etc). If a vp is located in the Pareto set (the set of outcomes that makes no actor worse off than the status quo) of the rest of the vp, (s)he will be "absorbed" (i.e., will not exercise veto powers). As I show in the original book, there is no position accepted by all other veto players that will not be approved by this particular one if (s)he is located within the Pareto set of the others. I argue that in this case, the absorbed player will not count as an additional vp. A trivial application of this statement is that if there are two vp with identical preferences, they will act as though they were a single vp, because they will always agree on any policy change. Another application is that if the policy positions of three vps are on the same straight line, then the one in the middle is absorbed and, as a result, what matters is the distance between the two extreme vps, not the location of the one in between. Again, as a consequence, in a one-dimensional setting of vps, the *only* thing that matters is their "range" (the distance of the two most extreme vps), not their number. Given the analytic status of this proposition, if some empirical study finds that the number of vp matters and not the range, the *only* possible inference is that the single dimensional assumption is not a reasonable approximation of reality in the case under study.

Figure 1.1 gives a graphic representation of the argument above. Veto player B is located between A and C, and there is nothing that A and C can agree to (the intersection of their preferences is the thin lens in the picture) that B would disagree to (his/her preferences over the status quo include this thin lens). It is easy to verify that as long as B is located between A and C, this statement will be true. The reader can experiment by herself and verify that if B is not on the line AC, there will always be some parts on the lens that are not preferred by B to the status quo.

A last empirical application is in two dimensions: if a vp is located inside the triangle generated by three others, (s)he will be absorbed. Again, if under these conditions an empirical application finds that this particular vp exercises a veto it is because one of the initial assumptions is violated: either the vp exercises veto on

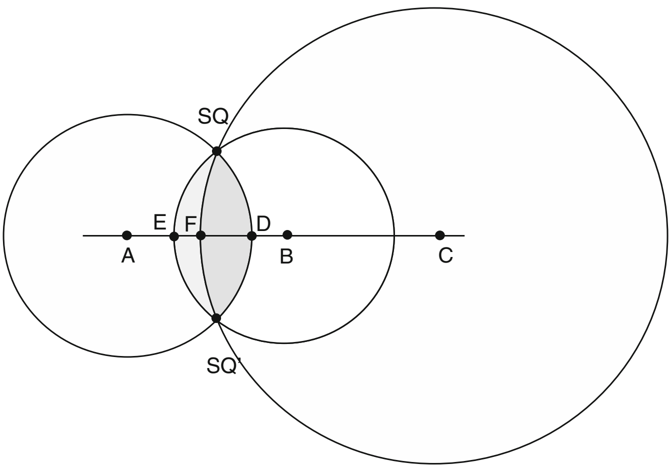

Fig. 1.1 Winset of VPs A and C is contained within Winset of VPs A and B (B is absorbed)

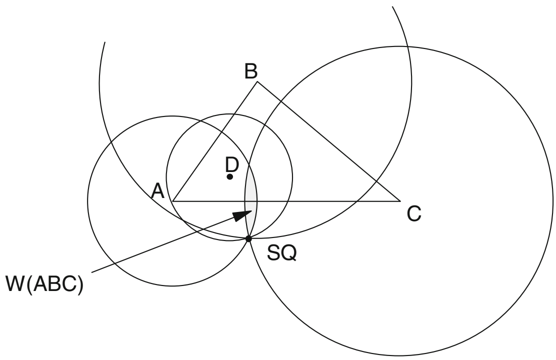

Fig. 1.2 Winset of VPs A, B, and C is contained in Winset of D (D is absorbed)

the basis of motives other than policy preferences (say electoral considerations) or s(he) is not located inside the triangle of the three others (say s(he) is outside the plane generated by them).

Figure 1.2 gives the graphic representation of the argument. D is a veto player located in the triangle created by the positions of vp A, B, and C. Consequently, anything that A, B, and C prefer to the status quo (the area W(ABC) in the picture) is preferred by D to the status quo. Again, the proposition will not be true if D is located outside the triangle (or outside the plane).

These propositions are mentioned here because they are discussed in some of the chapters of this book. They do not exhaust the implications of the theory of veto players, but, as I said, this chapter is not a summary of the book but of the parts of the veto players theory that are relevant to what follows.

Another part of the veto players book that will be discussed is the counting rules of vps. The first step in mapping the vp constellation of a country (a necessary step in assessing the impact of political institutions on policy outcomes) is to establish the number and identity of its institutional veto players by determining how many institutions are required to agree for a new policy to be adopted (stipulated within the constitution of the country). For example, in the United States, there are a minimum of three institutions whose agreement is needed (President, House, and Senate), in Germany most of the time two need to consent (Bundestag and Bundesrat), while just one institution must agree to policy changes in France (National Assembly) and the UK (House of Commons).[1] Once we have identified the institutional vp of a country, we can focus on the political conditions inside each institutional that determines the partisan veto players of the system. For example, if a stable coalition prevails inside a legislature, then the parties members of this coalition are the vp. Figure 1.3 illustrates this argument.

Think of a legislature composed of five parties where three of them are required to form a majority. Suppose that it is a parliamentary system with cohesive parties, and three of them are in government (say A, B, and C). In this case, one can

[1] I will omit here a discussion of constitutional courts as vps (see Tsebelis 2002).

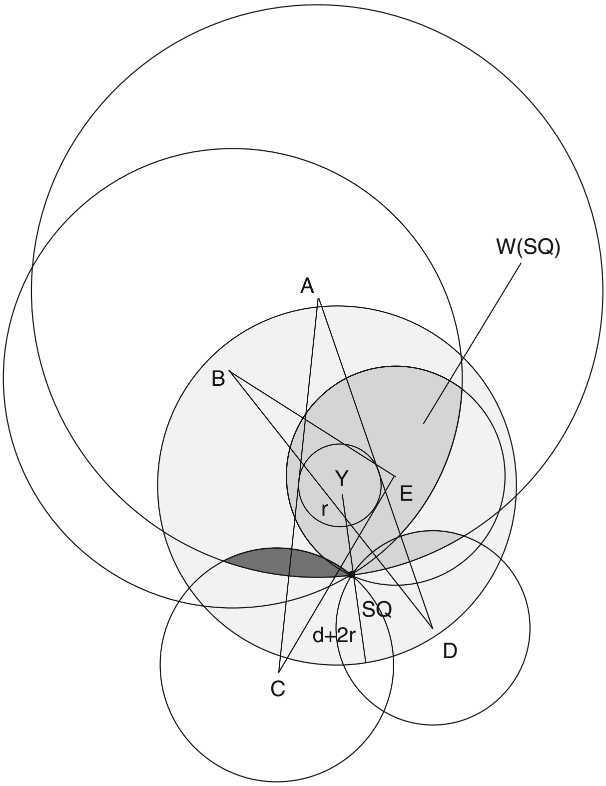

Fig. 1.3 Circle (Y. $d + 2r$) contains the Winset of the status quo of collective VP (ABCDE)

predict the outcomes that can replace the status quo quite accurately: it is the heavily shaded lens in Fig. 1.3. If, however, parties can form multiple coalitions, then there are a number of coalitions that can form to replace the status quo, and the prediction cannot be very accurate. Policy outcomes could lie within any one of the three lenses that are lightly or heavily shaded. Tsebelis (2002) demonstrates that this irregular shape can be included inside a circle (with centre Y and radius $d + 2r$ in the figure), so that one can think of this parliament as a collective vp and approximate its preferences. If parties are cohesive but different majorities prevail in each chamber of a bicameral legislature, then one would have to include the required parties for concurrent majorities in both chambers. For example, in Germany in 2004, there was a majority coalition of the Social Democrats and the Greens in the Bundestag, but the addition of the Christian Democratic Party was necessary to achieve a majority in the Bundesrat, as a result there were three veto players in Germany.[2]

[2] Actually the situation is slightly more complicated because for some laws the Bundesrat does not have veto power, which will mean that for these laws, the only veto players are the Social Democrats and the Greens, while in the most significant legislation, the agreement of the Bundesrat is required.

It is not enough, however, to simply count the number of partisan vp (parties required to achieve a simple majority) within the institutional vp: we must also pay attention to significant additional details of the political system. For example, it is necessary to determine the decision-making rule inside each institutional vp as this determines who the partisan vp are (as the vote required to adopt a that proposal increases the number of partisan vp may also increase as additional actors are necessary to meet the voting threshold).[3]

Measuring the number of veto players is relatively easy: one has to identify institutional vp, and then open each of them (see who the partisan vp are as a function of the decision-making rule, the existence or not of stable coalitions, and the cohesion of parties). Assessing the ideological range of the particular vp constellation is a more challenging matter.

First, one has to identify the number of dimensions of the underlying policy space. In the original book, I use two examples (in Chaps. 7 and 8): one on labor legislation where I assume that the issue is highly correlated with the left–right axis, so I use this as the single policy dimension; the other is on the budget where I use a two-dimensional approach (as a demonstration of how one can increase the number of dimensions in the analysis). The number of dimensions is usually ignored in empirical studies, and a one-dimensional assumption is adopted without adequate concern about whether it is a reasonable simplification of the phenomenon that is studied. The more complicated the issue, the more appropriate is the adoption of a multidimensional model.

Second, one has to select a dataset from which to extract ideological positions. There are three methods that have been used in the past: expert survey assessments that do not introduce very much variation over time, the manifesto project that assesses policy positions of parties in each election, and, more recently, computerized programs that are based on any documents the researcher would like to analyze. Several of the chapters in this book deal with the issues of measuring the underlying policy space, so the reader will be immersed in this methodology below, and throughout the book.

This is a summary of the issues relevant to veto players and policy stability. However, there are implications of veto players for the formation of coalitions and for structural characteristics of countries, like the importance of bureaucracies and the judiciary. Since coalitions are studied and discussed in several chapters, let me present a summary of the veto players approach to coalitions.

Given that both range and number of vp affect policy stability, the expectation is that whenever a country is faced with a significant exogenous shock (like an increase in oil prices, a financial meltdown, or prolonged social strife around a particular issue such as unemployment, healthcare, or education), a government with multiple distinct vp will not be able to produce policies to overcome the crisis, and consequently (in a parliamentary system) will have to resign, and be replaced by another coalition government. There are two implications of this analysis (though only the

[3] For the details of analyzing institutions that decide by qualified majority, see Tsebelis (2002).

first is included in the book): The duration of governments with large ranges or multiple vp will be shorter; the second is that, given that government duration of coalitions with a large ideological range and many vp is shorter, this will affect the composition of governments. While this is an important and interesting phenomenon in itself, I will not discuss this issue any further.

While it is clear in parliamentary systems who the veto players are, this is not true about the identity of agenda setters. In a general way, the agenda setting institution in parliamentary systems is "the government," but this statement is very vague. First, it does not identify the real agenda setter except in the rare case of single party governments. Second, it does not identify the exact powers of the government. Let me deal with each one of these issues in turn.

With respect to the first issue (who within a government is the real agenda setter), I explicitly and purposely adopted the position that all the parties of the coalition participate in the process. Other analyses on this point make a different assumption: they assume that it is solely the prime minister, or the corresponding minister, or underline the contribution of the finance minister who sets the legislative agenda. I do not doubt that in some cases one can find significant contribution from the prime minister or the corresponding minister in shaping a particular policy. However, I believe that none of these individuals would have exclusive jurisdiction, because by virtue of being a veto player, if ministers have a significant policy difference with a member of the coalition, either the policy will not go forward or the coalition will collapse because they will use their veto power.

With respect to the second issue (agenda setting power of different governments), this is a new independent variable in the study of parliamentary systems. The best analysis of this subject was conducted by Döring (1995) and I have consequently adopted his analysis. As we will see, several of the chapters in this volume deal with this issue, and we will address it again in the second part of this introduction.

To conclude the discussion of the relevant aspects of veto players theory, the veto players' analysis is based on a partial equilibrium assumption: as long as the government is in place, it will have a series of properties determined by its vp configuration. If faced with a shock, the government may survive or may change and then we will have a different government with similar or different properties. This statement ignores elections, as well as other exogenous shocks. Should we include them in the analysis in order to produce a more general theory? Of course, we should. However, it is unclear at this point how this can be done in a systematic way. In addition, all the reasoning in the analysis is based on complete information models – that is, the model assumes that parties know each other's positions in a multidimensional space. I think this is a reasonable assumption, but should we be restricted by it for everything we study? No, and the reader will find that several chapters remove this assumption and proceed further in the analysis. I have been assuming single bill consideration by the political system on the basis of policy preferences. No side payments were used, no log rolling could occur. Is it because I believe that these phenomena never occur? No, but I did not allow for these options in the original book because if you allow for these possibilities everything becomes possible, and you cannot test theories on the basis of this assumption. Again, the reader will see that this argument is disputed in this book in a meaningful and creative way. There have been a series of simplifying assumptions in the original book, and most of them are challenged in the pages that follow.

Arguments Made in This Book

It is time now to apply the arguments developed above to the studies presented in this book. Mark Hallerberg first presents an overview of the veto players literature. He has done an exhaustive study that covers legislative politics as well as political economy issues (to which he has widely contributed). He argues that institutions designed by a specific configuration of veto players may not function as well with a different constellation. I have two points of disagreement to raise with his thorough analysis.

The first is that he (rightfully) considers not only my approach to the analysis of political institutions but a number of other approaches that speak to the policy consequences of institutions. He identifies in them different counting rules and argues that empirical evidence will show which one is better. Let me give to empirical evidence what is due to empirical evidence, but let me also point out that logical consistency is a necessary condition for the development of a theory. In the absence of logical consistency, corroboration (no matter how rigorous the tests and no matter how strong the results) *does not* matter. Take, for example, the issue of veto players in the way it was developed in the original book, and veto points or veto gates, that is, the veto players determined by the constitution alone. The difference between the two concepts is significant, and the difference arises from two causes. First, some institutional veto players may be absorbed. For example, two chambers of a bicameral legislature may both have veto power, but they may have identical compositions (Italy). If this is the case, in veto players theory, one is absorbed by the other, while they both constitute equally important veto points, or veto gates. Conversely, a single institutional veto point or veto gate may be composed of multiple partisan veto players (see discussion surrounding Fig. 1.3), where one single institutional veto player can be composed of three partisan ones.

The point I am making here is that the veto points or veto gates analyses are inconsistent if one takes into account the preferences of the different political actors. Of course, if such positions are unknown or unknowable (think of studies covering hundreds of countries for centuries, like some analyses of economic growth (Henisz)), then veto points are the best approximation of veto players, and as such it should be used. However, when we can assess the preferences of the potential veto players, we should use that information.

So, veto players and veto points are not two different ways of counting wherein one can use whichever method s(he) prefers, or test them and see which is more accurate. One logically supersedes the other, and if both veto points and the ideological composition of the groups that compose those points are available, the veto players' framework should be used; if this information is not available, veto points are probably the best approximation. I do not need to develop the point any further. The reader will verify that most chapters in this volume use both institutions and preferences, and some of them go to great lengths to develop means of assessing one or the other or their combination.

Another difference between the veto players and veto points counting mechanisms arises from the issue of "enabling" and "encompassing" veto players. As I explained in the previous section, it follows from the definition of veto players that adding veto players necessarily leads to policy change becoming equally or more difficult. It does not matter what one chooses to call them. If the argument is that policy change becomes easier as the term "enabling" suggests, then, the argument is that parties in government are not veto players, and this argument should be presented and assessed.

The second point that Hallerberg makes (and it is connected to his own work) is that some veto player configurations may generate results that are better under the initial configuration than under the subsequent alternatives. I have tried to avoid normative implications of veto player throughout my work on the theory. I never said that few or many veto players are better. On the contrary, my argument was that reasonable people may disagree and the best approximation for whether one prefers more or fewer veto players is their attitude towards the status quo. I made this argument even discussing the economic growth argument that multiple veto players are better than few for growth, because they create policy stability and enable the market to fully develop. While policy stability may be a good condition for growth, it may also be the case that the ability of the political actors for change policy quickly may improve economic results (think of the Obama stimulus package). Finally, different actors may have different assessments of policies and as a result of the institutions that promote or inhibit those policies. Therefore, normative assessments about veto players will be equally controversial.

Jahn (Chap. 3) proceeds through a careful reconstruction of all existing indexes (both veto player and veto point) incorporating all available information about changes in institutions and in preferences. Although he shares my theoretical assessment of veto points in comparison to veto players, he nevertheless proceeds in a careful reconstruction of all indexes in order to be able to construct a correlation matrix of all of them.

There are two important contributions that Jahn introduces in his analysis, both of them depend on the time dimension. Preferences of actors change over time. This change was not included in my original work, so Jahn clearly proceeds one step further. However, this analysis will also soon become dated because, as we will see in the discussion of the subsequent chapter, we are today at the verge of major methodological discoveries with respect to preference assessment. As we will see, preferences can be assessed not only by voting in legislatures (as is the case in the US congress) but also by textual analysis of party documents or parliamentary speeches, or laws, and consequently, the manifesto approach that measures policy position only before elections and inter- or extra-polates in between will be soon be set aside.

The other significant change that Jahn introduces is the change in institutions. This second change does not produce a significant empirical result (the indices, Jahn 1 and 2, are correlated at 0.98) but it is nevertheless a significant contribution.

I do not know how "medium"-strength chambers are included in the index in his measure. While (as Jahn cites) I have argued that any second chamber has influence in the legislative output, it does not mean that it is a "veto player." For example, both the House of Lords and the French Senate can be over-ruled by the lower chambers, and consequently their agreement is not necessary for a modification of the status quo. It is essential that we keep the same rules across countries when we construct summary indices, otherwise it will not be clear what differences can be attributed to institutions and what to changes in rules. Similarly, the President of the French Republic is not a veto player (whether under cohabitation or not) because he cannot veto a bill (he can send a bill back to the National Assembly for reconsideration, but it is re-examined under the same rules). Now, the dynamics of the French government are very different if the elected President is the leader of the government coalition, or if he has to form a government from opposing parties. In the first case, he is the indisputable leader of the country; in the second, he falls back into the position of a President of a parliamentary democracy (with the most significant difference that he can dissolve the national Assembly whenever he wants). But in either case, he is not a distinct veto player.

One final point of disagreement with Jahn's conclusions is his statement that "[a]nalyses in which Henisz' indicator is applied and in which results are interpreted according to Tsebelis' theory are invalid." While I do not for a moment dispute Jahn's findings that the index I calculated correlates with "Crepaz 1" at 0.51 and with Henisz's "polcon 3" at only 0.40 I find the intellectual, conceptual, and methodological affinity with the latter to be much stronger (and I have used it when the time period or the spread of countries was much larger than my own dataset).

The Koenig et al. chapter presents a new methodology for the estimation of preferences of political actors. Compared with the preference specification I used in the veto players book, this methodology is several steps ahead. It uses legislation to identify verbal expressions relevant to different ministries and traces these expressions back to party documents. Then, with a computerized method like wordfish, they approximate the ideological conflict that generates these words and produce the ideological positions of the different parties. The method is impressive: it updates the information constantly (as a function of laws enacted (or if one wants simply debated)), it produces outcomes in multiple dimensions, and the dimensions are correlated with the different government ministries as opposed to decisions of some researcher.

The results of the analysis are remarkable: one can see the movement of the FDP from left to right as a coalition partner of the SPD first, and the CDU-CSU later. All of these movements are lost in the analyses in my book. The authors are kind to point out that in the last period when the government parties are the closest together they produce no legislation (against the veto players theory); the result can be explained by the fact that the status quo was presumably very close to the parties to begin with (which is consistent with the theory).

Yet, all this precision comes at the loss of comparability. The reason that veto players has a negative coefficient for the law production function is not the difference in the ideological distance of parties in German government, but the fact that countries like Germany or Italy produce few significant laws, while Greece or the UK produce a lot of them. And this requires compatibility among the ideological range of different countries. The Koenig et al. approach is extremely significant for the study of each country, but the results cannot be compared cross-nationally, because the legislation varies, and the smart tags of one country are not relevant to the next.

This is a similar problem with the ideological positions of legislators in the US House and Senate on the basis of their voting records: given that different amendments are presented in each chamber, there is no comparability of voting in the two chambers. Different researchers have found different "anchors" to produce comparable results: some amendments may be identical and produce a (limited) way for making comparative judgments; some legislators move from one chamber to the other, so, assuming that they have the same positions (over time), can become the means of comparing one chamber with the other etc. Still, no method has been able to place the President in these ideological continuums, and this is the reason I have not been able to include the United States in my dataset (to respond here to Jahn's criticism).

So, Koenig et al. produce "country expertise" with their analysis, but they do not have the ability to make cross-country comparisons (yet). If they are able to make comparisons, they will achieve immediate recognition. But what they have already achieved is an immense step forward in the identification of policy preferences. This is not an achievement just related to veto players, but a big step forward in political and social sciences more generally. Because preference identification is a necessary step for the analysis of any rational behavior since by definition rationality is maximization of goal achievement, this methodological advancement is a step forward for the social sciences more generally.

The Mueller and Meyer chapter deals with the way coalitions over-rule parties in performing government business. They start from the well-known Shepsle and Laver model according to which ministers have exclusive jurisdictions over policy-making in their area and address a series of mechanisms by which coalitions are able to reign them in and make them promote the coalition platform as opposed to their partisan wishes, despite the fact that their commitment to the party is long-term, and their career payoffs are controlled by the party and not the coalition.

There are means by which coalitions can reduce the independence of ministers, most of which have already been described in the literature: ex ante mechanisms (policy agreement, coalition discipline, election rule, and screening of ministers), ex post mechanisms (monitoring by parliamentary committees, coalition committees, and watchdog junior ministers), and environmental factors (familiarity, bargaining complexity, parliamentary polarization, cabinet preference divergence, time until next election, prime ministerial powers, and additional veto players). The original contribution of the argument is not the identification of any particular mechanism of supervision but the combination of all of them and the demonstration that all these mechanisms perform the same duties, and are connected with each other so that if the environment provides a lot of them, only a few additional ones are needed. In addition to the theoretical argument, empirical analysis using some 260 coalition governments in 17 West European countries beginning with the first post-war (or current regime) cabinet until 1999, corroborates the theoretical expectations of the authors. The bottom line is that the best conceptual approximation of how coalition governments work is that the parties composing these governments are veto players – that is, exercise veto over each other's proposals, so that the government policies are approved by all coalition partners. So, what in my book was essentially taken for granted is now carefully analyzed and corroborated by empirical evidence. Beyond that, the chapter is an important contribution to the very wide literature on coalitions, and explains how the most important institution in parliamentary democracies (government coalitions) works.

Saafeld's chapter derives a series of five assumptions on government duration from veto players, and tests them with the same dataset as Mueller and Meyer. The arguments are sound, the methodology appropriate, and the conclusions are very nuanced: some of the expectations are corroborated, whereas others do not produce results at the traditional levels of significance. The major finding of this chapter is a negative one, in contradiction of both expectations of veto players theory as well as of previous empirical work from Warwick: the ideological distance of veto players in the right–left dimension does not significantly affect the longevity of governments, although the number of veto players does. Saafeld's argument is that the significance of the number of veto players could be attributed to transaction costs, and not to veto players. This is a plausible explanation, but this is not the only study that finds the number of vps more significant than their range in one dimension. For example, Franseze, analyzing the size of budgets in democracies found the number of veto players mattered more than their range, and concluded that the evidence corroborated the veto players' argument.

This is an important issue deserving more extensive discussion. *All* spatial models agree that the results of single dimensional analysis do not extrapolate to multiple dimensions. The basic difference is that in one-dimensional models, there is always an equilibrium (the median voter), whereas in multidimensional models, the equilibrium disappears (the most accurate statement is that it exists with probability 0). In addition, the core (set of points that cannot be defeated by application of the decision-making rule) may or may not exist as a function of the number of underlying policy dimensions. So, the number of underlying dimensions is of crucial significance for spatial reasoning. Actually, the reason that I produced the veto player theory and did not use the equilibrium of a one-dimensional model nor analyze the core of different political systems is that these concepts are not guaranteed to exist. Veto players theory has an advantage over such analyses that the results hold *regardless* of the number of underlying dimensions.

Let us now take Saafeld's results (number of vps matters, but not range in one dimension), and let us assume that three government parties were located in an equilateral triangle or a similar shape. Obviously, the ideological conflicts cannot be understood in one dimension. In this case, one would have to go to two dimensions, or, would have the number of veto players as a proxy for the existence of multiple dimensions. So, the significance of number of veto players may be an indication of multidimensionality of the underlying space. How many dimensions? Spatial theory tells us that with n veto players (i.e., parties in government plus additional institutional ones), the number of dimensions can be up to $n - 1$. If it turns out that the first two dimensions eliminate the argument that it is the number of veto players that matters for policy outcomes, the interpretation would be that it is not transaction costs. So, we could test Saafeld's conjecture against mine (actually, in the veto players book I have a two dimensional analysis that could be replicated for any number of dimensions).

The chapter by Bräuninger and Debus deals with a phenomenon that has not been studied before. Bipartisan policy proposals, that is, bills that have been proposed jointly by members of the government and the opposition are the subject of their chapter. The reason that the subject has not been studied is that the canonical configuration of parliamentary systems is government versus opposition, given that the discipline of parties in parliamentary democracies was considered "Prussian," political scientists started studying parliamentary voting only recently. Bräuninger and Debus go one step further, and argue that agenda setting is even less restricted than votes, and so focus on this unusual for parliamentary system phenomenon. They find out that there is a significant percentage of bipartisan bills (although the number of those that becomes law is limited; but then again, any non-government bill has low probability of success in a parliamentary democracy). What is the reason even the existence of such bills? The authors cannot explain bipartisan bills on the basis of the veto players' assumption (that parties are policy oriented and preferences are determined by policy goals alone) and so, modify the assumption: what if MPs are interested in their reputation and reelection? Then, they would have independent desire to create a certain image about themselves, and they would be proposing bipartisan laws more frequently before elections. Such bills would be on minor issues rather than major ones (which government would be interested in the latter), and should occur more frequently the larger the distance among parties in the government, and the smaller the distance between government and opposition parties. All these expectations are corroborated by an extensive dataset covering legislation in both Germany and Belgium.

What is interesting to notice in this chapter is the discovery of a new phenomenon, and the generation of an explanation that bridges what happens with what we know about how politics works, in other words, the creation of a bridge between realism and rationalism.

The König and Junge chapter deals with an important puzzle: while the member countries of the EU have disagreements in terms of policies, they tend to vote unanimously in favour of Commission proposals. Since this appears to be a contradiction, the authors have to justify their claim. They provide several pieces of evidence to support claims of divergent policy preferences among member countries, the most significant being that the member states have attempted several times to revise EU institutions because they perceive them as impeding decision making. Given these disagreements over policy, why do they vote in favour of (most) of

Commission proposals? One possible explanation is that the Commission chooses to present only (or predominantly) legislation that can gain the required support of the member countries. The second (and this is the subject matter of the chapter) is that members may be logrolling across bills in the same jurisdiction, or across bills in different jurisdictions that are decided at the same time. The authors present a series of simulations according to which a small number of such bills are enough to eliminate disagreements. This is a very original contribution because logrolling (like side payments) is a well-known explanation for the question of why veto players would not exercise their veto powers. Logrolling, because it increases the dimensionality of the underlying policyspace until the aggregate bill is preferred by all relevant actors, and side payments, because they make disagreeing players change their mind (if the price is high enough), are two explanations of how veto players can overcome the institutional hurdles of policymaking. The reason that such explanations are not discussed very much in the literature (including the veto players book) is because such explanations (if accepted) would explain everything: for every instance of deviant behaviour there is a price (either in money or in policy) that can explain the deviation, and consequently, any behaviour can be explained. This chapter, however, demonstrates that a *small* number of bills, either in the same jurisdiction or in the same time period, are enough to produce the observed result. The authors claim that logrolling over a large number of bills is unrealistic because of the associated transaction costs. What is more interesting, in my view, is that they present one case where logrolling does *not* work, which indicates that their argument is not trivial: in Fig. 8.2, one can see that logrolling in the area of Justice and Home Affairs does not work (I presume because the few bills that exist in their data set are too similar to generate the necessary benefits). So, this chapter has produced an impressive algorithm to test the possibilities of logrolling.

The Curini and Zucchini chapter is the first paper that uses roll call votes to investigate institutions in a parliamentary system. It goes through a painstaking analysis of different models elaborated for the US system, translates veto players into the necessary context (one-dimensional politics, identification of the pivotal voter(s), and identification of the agenda setter) in order to produce specific expectations for roll call analysis, and compares four different models using this methodology.

One point that needs to be underlined is that for their analysis the agenda setting powers of the government have to be included. However, as I said above (Sect. 1), I consider agenda setting power an independent variable and use Doering's analysis to provide summary statements for different countries. So, the chapter by Curini and Zucchini tests two alternative assumptions of the veto players model: one where the government has the ability to propose bills under closed rule and one under open rule.

They provide extensive analysis of models and predictions, finding that the model of veto players model under open rule has the best results overall. However, I would be unjust to their paper if I did not underline that they produce a much more detailed analysis of different governments and party system configurations in Italy, and nuance their results identifying better models for different cases.

This is a fine piece of work both in terms of deriving specific predictions from different models and analyzing Italian politics. I want to state these points clearly so that what follows is not perceived as criticism, but as suggestion for further research: I am not as comfortable as the authors with the single dimensional models. They find it reasonable that, given government control of the agenda, what is brought to the floor is what the government wants, so that if the government is centrist, extreme left and right can only say "no," meaning that sometimes there is no difference between opposition left and opposition right. However, they cite cases where legislation comes from MPs, not the government, and cases that enable them to tell the moderate left and right from the extremes. I think that, had they not applied the W-Nominate scoring method, and had they not separated governments, they would have produced a multi-dimensional underlying space that would be more credible. But, as I said, it is only because I have read this creative analysis that I have these questions and look forward to future research building on this piece of scholarship.

The chapter by Steunenberg deals with implementation of European policy by EU member states. More precisely, it examines the conditions under which different countries will implement exactly (or deviate slightly from) regulations approved by the EU legislative system. The problem is complicated, the diversity of domestic institutions enormous, so the model is significantly simplified – complicated institutions are assigned names like "national authority" in order to examine their role in a summary way. Steunenberg produces a series of necessary conditions for implementation at the national level to deviate from the status quo (which is the decision made by the legislative institutions of the EU). These conditions include agreement between the Commission and the (national) veto players. What is interesting to note is that while the previous statement may seem trivial, as Steunenberg argues it is not true in the case that one deals with a sequential game between the Commission and not simply one (as this model) but a series of countries: since any action with the Commission with respect to one country will be considered by the others as information about the preferences of the Commission in their subsequent implementation decisions. This model deals with a very significant and very specific EU problem, and as the author many times iterates, it is a first attempt to understand how the multiple layers of decision makers interact.

The chapter on bicameralism (Hug) persistently emphasizes the point that under lots of different plausible configurations, the two chambers of a bicameral legislature interact in a strategic way. Unless this interaction is modeled explicitly, the results from the study of one chamber alone are biased. This is a simple and clear point, and it has been ignored for a long time by the literature. One may think that this is not a significant problem, since after all only one-third of the legislatures in the world are bicameral and the number where the second chamber has a significant say in policy and has a different composition from the first can be counted on one's hands. Yet, most of the countries that produce studies of legislatures (United States, Germany, Switzerland, and EU) belong in this category, so, the misspecification is of consequence. So, Hug's point is a very significant one.

Having said that, here is a case where the underlying number of dimensions is of critical importance. Hug can make all the calculations of backwards induction in complete and (some cases incomplete) information models because he assumes a single dimension, and a median voter result in all cases. If one assumes a two or higher dimension underlying space, the median voter disappears and so does backwards induction, leading us back to the Krehbiel and Rivers (1990) argument according to which strategic voting should not be a major concern. Given the heuristic character of Hug's models, the assumption of single dimensionality of the underlying space determines the credibility of his analysis.

Finally, the Junge chapter comes in the intersection of two different literatures: the study of European institutions that, despite their recent creation (in comparative perspective), are the second most studied institutions (after the United States) and empirical analysis that, of course, has a prevalent value in contemporary social sciences. What Junge does is apply a method that replicates the multiple steps of the analysis of game theoretic models and generates a coefficient for each one of them through simulations that involve small deviations from the point predictions. This way, if the studied institution is the unanimity rule and one of the members fails to agree the mistake is not considered as large as if a whole group of members failed to agree, despite the fact that, strictly speaking, the theory predicts that both situations are impossible.

Junge is able to analyze a series of models of the EU, some of them predicting an important influence of the Parliament, others of the Commission, and still others of the Council. While at the theoretical level disagreements among the authors of these different models have existed for many years, it is refreshing and encouraging that empirical studies can now shed light on the empirical accuracy of different approaches (characterized by assumptions concerning game trees, preferences, permitted and prohibited moves, agenda setting, vetoes, etc.). To my knowledge this is the first time that the quantal response approach has been applied to institutional analysis, and it has the ability to generate a whole industry of empirical investigations comparing the accuracy of specific assumptions concerning institutions. In particular, with respect to the EU where the institutional structure is young and complicated (the reader is referred to the other chapters on the EU in this volume), the potential of this method is remarkable and the potential impact really impressive.

To conclude, I have tried my best to understand how institutions work, and a series of colleagues took the approach I developed and applied it to new phenomena, or disputed some of the assumptions, and produced new and interesting explanations – that is, they produced added value. Either they identified phenomena that we did not know about before reading their chapter or explanations of problems that we had not identified or we did not know how to resolve. The reader will verify that each chapter has something to offer along one of these dimensions, or maybe several of them.

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