

Veto Points and Veto Players: A Skeptical View

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If some policies are better than others, if different parties share the understanding of the constraints, and if voters learn from experience, then most of the time parties of different stripes will follow similar policies. Even if parties have different ideal points in the space of outcomes, they will have proximate ideal points in the space of policies if their politicians believe that they face trade-offs between objectives.

-- Adam Przeworski¹

1 Introduction²

Developing a skeptical view on veto point or player (hereafter generally : VP) arguments is difficult for two reasons. The first is that VP arguments have a high a priori plausibility. Veto power is an observable fact of political life, and the generic VP argument simply combines this fact with a conception of individual and collective actors as rational agents that have more-or-less consistent preferences over policies. If veto power exists, VPs are likely to make some difference. The second difficulty is that the literature on VPs really consists of various literatures that make different kinds of arguments and use different research methods. A critical review therefore has to deal with the specific limits and problems of each of these literatures. To deal with these two difficulties, the rest of this introduction develops a simple conceptual framework, which helps both classify the existing literatures and to locate each literature's "weak spots".

Well-crafted arguments about the effects of VPs on policy output are extremely convincing because the institutional effect is located at the *end* of a long causal chain. At their core, most VP arguments are not about how institutions *shape* policy preferences, but about how institutions influence policy output *given* actor's policy preferences. Therefore VP arguments are typically not arguments about the importance of veto power (such arguments would be trivial), but arguments about

- who the veto *players* are, or where the veto *points* are located; and about
- what the nature of these *player's* preferences is, or which interests are likely to be channeled through these veto *points*.

The first question – Which VPs matter? – implies both the identification of the actors that have veto power and the level at which sets of individual actors can usefully be treated at a more aggregate level as collective actors. Here lies also one of the main differences between the veto *point* and the veto *player* perspectives. If sets of individual actors cannot be treated as collective actors, and if individual preferences are difficult or impossible to measure, it seems straightforward to focus on the *location* of veto *points* and on how this location influences decision-making processes and interest group strategies (Immergut 1989; 1992). On the other hand, if collective actors with measurable preferences can be identified, more far-reaching expectations about policy outputs can be formed (Tsebelis 1995a; 2002). The second question – about VP's preferences – can refer to specific policy preferences ("more or less

¹ Przeworski (2001: 333).

² I thank André Kaiser, Bernhard Kittel, Matthias L. Maier, and Fritz W. Scharpf for helpful discussions.

welfare state spending”), but also to the general characteristics of veto player’s preferences: for example, whether they relate only to *Policy* (as opposed to *Vote* and *Office*) or whether they are “Euclidian” (equal weighting of issue dimensions) or not.

The specification of these two questions leads to a first important distinction. Many quantitative studies of specific policy outputs or outcomes (e.g., welfare state effort, budget deficits, growth) that include VPs as a central explanatory variable decide both of the above questions theoretically rather than empirically. Most importantly, they do not measure VPs’ preferences. Instead, they either assume certain VP preferences (e.g., “against welfare state expansion”), certain patterns of veto player interaction (“logrolling”), or specific second-order effects of policy stability (“growth-enhancing credible commitment). In these literatures, the problem of discriminating between different types of VP arguments as well as between institutional and non-institutional variables becomes largely a statistical problem.

Other – quantitative and qualitative – VP studies are committed to measuring rather than assuming VP preferences. This form of empirical “disaggregation” reduces the demands on the data analysis, but shifts the empirical challenge to finding valid measures of VPs preferences. At this point, it is important to introduce a straightforward but often neglected conceptual distinction between *outcome preferences* and *policy preferences* (see, e.g. Przeworski 1998: 143; 2001: 333). VP arguments are logically predicated on “fixed” preferences. If preferences change – and if this change is not in some way controlled for in the empirical analysis – the effect of the institutional veto power is difficult to ascertain. For “rational choice” scholars, this goes without saying. They have a general preference for treating preferences as fixed and behavioral changes as the result of changed *strategies*. The problem with this approach is that the translation of basic preferences about *outcomes* – which may usefully be treated as being exogenous – into legislative output (*policies*) is a two-step procedure. First, political actors translate their *outcome preferences* into *policy preferences*, based on their beliefs about the behavior of societal/economic actors; then they translate these policy preferences into a legislative strategy, based on the allocation of veto and agenda-setting power as well as their beliefs about other veto players’ strategies.

This distinction between outcome and policy preferences highlights an important “access point” for non-institutional variables (e.g., “ideas” or “culture”): veto power combines with *policy* preferences. Therefore, *if* scholars are able to measure policy preferences more-or-less directly, their explanations will often be very convincing. This does not mean that they will also be arresting, because much of what is interesting might only be found if the causal chain is extended backwards (cf. Kitschelt 2002). More importantly, protagonists of VP arguments often *cannot* measure policy preferences directly but only indirectly via indicators of outcome preferences or partisan ideology. This measurement problem is important in both qualitative and quantitative research, and I will show that protagonists of VP arguments frequently underestimate these problems, which can lead to misleading causal inferences.

The second problem that “disaggregated” VP studies have to solve is that of identifying the relevant VPs. Here lies one important difference between quantitative and qualitative studies. The latter can often ascertain the importance of particular VPs empirically, while the former cannot. However, Tsebelis’ VP theory, the most ambitious VP approach, tries to systematically derive coding and counting rules from basic assumptions about the general nature of VPs preferences. Tsebelis’ VP

theory is based on the assumption of policy-seeking VPs with fixed Euclidian policy preferences. Based on this assumption, he is able to make coherent arguments about how to count veto players, e.g., in situations of minority governments or oversized coalitions.

Tsebelis assumption of policy-seeking VPs with fixed policy preferences highlights a second “access point” for non-institutional variables. Tsebelis essentially assumes parties whose preferences are induced by voters’ (policy or outcome) preferences and who are “Downsian” in the sense that they have no problems converting policy gains in the legislative arena into electoral gains. “[P]olicy is the only convertible vote-buying currency” (Huber 1999: 276). If this assumption is relaxed, VPs’ vote- and office-seeking behavior may exert a separate influence on policy outputs (or lack thereof) and non-institutional variables can enter the analysis, e.g., the configuration of party competition and issue ownership or the procedural preferences of voters.

In what follows, I will discuss three main VP literatures in turn. Section 2 discusses the qualitative literature, which was the starting point for a systematic comparative analysis of VPs (Immergut 1989; 1992). Section 3 discusses the more “aggregated” quantitative literatures that are organized around particular dependent variables. The best-known and most mature literature of this kind focuses on welfare states and was strongly influenced by the qualitative work. (Huber/Ragin/Stevens 1993). Sections 4 and 5 mainly discuss Tsebelis’ VP theory and related studies. Tsebelis uses quantitative methods to establish the empirical link between VPs and policy stability – his main proximate dependent variable – but tries to *measure* VPs preferences. Section 4 focuses on Tsebelis’ assumption of fixed policy preferences and the problems of measurement. Section 5 discusses his assumption of “Downsian” parties and makes a case for the explicit analysis of VPs vote- and office-seeking. Section 6 concludes with a brief discussion of the literature’s relevance for constitutional engineering.

2 Qualitative studies

Qualitative studies that rely on some sort of VP argument vary from single case studies guided by systematic *theoretical frameworks* (see, e.g. Scharpf 1997; Cox/McCubbins 2001; Haggard/McCubbins 2001a) to “tests” of a VP argument using ordinal comparison (Immergut 1992).³ Case studies are well-suited to reveal the importance of VPs because they can *ascertain empirically* both the level at which sets of individuals can be treated as collective actors and these actors’ policy preferences. Immergut’s (1989; 1992) seminal study of veto points in *Health Politics* is an excellent example. Its combination of (i) a systematic analysis of the underlying substantive policy conflict, (ii) historical analysis, and (iii) comparative analysis allows her distinguish empirically between actors’ – deputies’ and interest groups’ – policy and outcome preferences and their legislative strategies (see Immergut 1992: 79).

³ I will not try to list all studies making VP arguments. See, e.g., Cowhey and McCubbins (1995), Rockman and Weaver (1993), Eaton (2000), Haverland (2000), MacIntyre (2001), Bonoli (2000), Maioni (1998), Ames (2001), Castiglioni (2001), and Hellman (1998). The veto point concept has also been adapted for the analysis of institutional change (Kaiser 1997; 2002).

However, many qualitative VP analyses make no systematic effort to measure policy preferences and to account for the potential effect of belief changes. In fact, some authors exclude the possibility of belief changes almost a priori, partly because allowing policy preferences to change allows one to explain almost anything. This wariness is well-grounded, but I believe the same wariness is in order in making assumptions about the dimensionality of the policy space, the location of ideal points, issue salience, or the level at which individuals are treated as collective actors.

Some studies assume fixed policy preferences but make no effort to determine these preferences empirically. For example, Stewart (1991) analyzes US tax policy in the 1980s using the tools of (RC) institutionalism – veto players, agenda control, reversionary outcome. In stark contrast to the conclusions of other in-depth analyses (e.g., Conlan/Wrightson/Beam 1990) Stewart dismisses changes in economic beliefs out of hand. Yet at the same time he openly acknowledges that his analytical construction of a two-dimensional policy space and his attribution of policy preferences to the actors are merely illustrative. As a result, while the logic of Stewart’s analysis is impressive, it is entirely unclear whether it “explains” anything. Bradford (1991), a mainstream tax economist and previous staff member of the US Treasury, finds that changes in economic beliefs played a crucial role in the changes of US tax policy, and that Stewart’s reconstruction of the policy space neglects crucial dimensions.

Even if scholars try to determine policy preferences empirically, they can use the disaggregation of actors to downplay the importance of belief changes. Consider Bonoli’s (2000; 2001) analysis of how differences in the number of veto points affect the magnitude and type of concessions that the opposition and trade unions can extract from the government with respect to pension reform. While he generally finds, not surprisingly, majoritarian policy-making and few concessions in the UK, he notes one important instance in which the British government did seem to make a significant concession in the face of strong opposition by dropping an important part of its reform plan. Bonoli (2000: 84) argues that dropping this reform element was not due to popular opposition, but to “an element of power fragmentation” within the executive. The Treasury was strongly opposed to that reform element because it feared its short-term budgetary consequences. According to Bonoli (ibid), “[t]he position of the Treasury, and its capacity to block the implementation of unwanted legislation, can be conceptualised in terms of a veto point”.

This interpretation is problematic because Bonoli had initially focused on the executive as the collective actor (with more-or-less fixed policy preferences). When he finds that the initial reform plans were significantly changed, he further disaggregates this player, focusing on the Department of Health and Social Security (DHSS) as the “real” origin of the policy proposal.⁴ Obviously, there are two possible competing interpretations that are to a large extent “observationally equivalent”. Bonoli treats both DHSS and the Treasury as veto players with more-or-less fixed preferences. A different plausible interpretation would treat the intra-executive interactions (including the Prime Minister) to a large extent as a process of belief formation about the economic and political costs and benefits of different

⁴ I do of course not wish to argue that disaggregation of a collective veto player should be avoided. Especially in single-party majority governments, much of the interesting politics is likely take place within the collective veto player (Mulé 1997; 2000: 138; Laver 1999). However, the case for disaggregation has to be made explicitly and carefully.

reform plans. From this perspective, the initial reform proposal was not blocked by a veto player, but the executive (as a whole) formed more adequate beliefs about the *electoral* costs and the short-term *economic* transition costs.

What is crucial is that pursuing these competing explanatory variables would *not* necessarily lead to a *less systematic* explanation. It is *only* via changes in beliefs about the costs and benefits of various policy strategies that most systematic factors other than veto (and proposal) power can enter the analysis. Economic constraints and trade-offs have to be *perceived*, policy blueprints (“ideas”) have to be *evaluated*, and the electoral costs and benefits of policies have to be *estimated* by the VPs. The possibility of belief change therefore has to be allowed for in order to recognize these and other systematic factors.

The importance of these factors is illustrated by other analysts who have identified the electoral costs of retrenchment policies as important explanatory variables for two of Bonoli’s cases, Britain and France (Pierson/Weaver 1993: 131-35; Pierson 1997; Vail 1999), and pursuing this explanatory path could lead one to a systematic analysis of the structural conditions under which electoral considerations are more or less constraining for protagonists of welfare state retrenchment (Kitschelt 2001). Similarly, a systematic study of the economics of pension system transition might lead to the conclusion that British government’s (the DHSS’s) initial reform plan had been unrealistic from the start, because short-term budgetary constraints – created by the “double-payment” problem of shifting to a funded system – were too tight. Economic constraints rather than institutional features would then figure more prominently in the analysis.

I am not able to judge which interpretation is more adequate in the case of British pension reform, but the two examples highlight two straightforward but often neglected problems. First, a focus on player’s *policy* preferences (as opposed to outcome preferences) and the presumption that these are fixed strongly biases the analysis towards an institutional explanation. Once policy preferences are formed, few non-institutional factors can possibly enter the analysis. Second, arguments about the dimensionality of the policy space, the salience of different issue dimensions, the location of player’s preferences in this space, and the level at which individual actor’s can be treated as collective actors are as vulnerable to adhocism as are arguments about changes in the economic or technical beliefs the link outcome preferences to policy strategies. This last problem of the adequate level of treating individual actors as collective actors is especially important, because belief formation of a *collective* veto player may be difficult to distinguish empirically from pure veto politics *within* this player. This problem seems especially relevant in studies on political systems for which the identification of VPs cannot rely (exclusively) on formal rules (cf. Gould 2001: xx).

3 Quantitative studies

While case studies are well-suited to reveal the importance of VPs, they are of course less well suited to gauge the *average causal effect* of VPs on some dependent variable. As a result, qualitative and anecdotal evidence has triggered several quantitative literatures that are organized around specific dependent variables (or sets of dependent variables) and that highlight VPs as one important explanatory variable. These literatures essentially test to what extent specific VP arguments found in

the qualitative literature can be generalized and how large the average effect of VP is on some dependent variable. These VP arguments are specific in the sense that they typically do not link veto players to abstract characteristics of policy output (frequency, pace, etc.) but to substantive policy outputs or outcomes. In political science, the best-known and “oldest” literature of this kind focuses on various aspects of “welfare state effort” – both during welfare state expansion and during retrenchment (Huber/Ragin/Stevens 1993; Huber/Stephens 2001).⁵ A comparably large literature, largely by economists, focuses on budget deficits (Roubini/Sachs 1989).⁶ Of course, many other literatures exist (or are emerging), focusing, for example, on investment and economic growth (Henisz 2000a; 2000b), inflation (Keefer/Stasavage 2000), or various aspects of taxation (Ashworth/Heyndels 2001; Baker/Gould 2002; Basinger/Hallerberg 2002).

Obviously, it is impossible to discuss all of these and other literatures in detail. Yet I believe that most of them share some basic problems. As a consequence of extending the number of cases, “measurement” becomes a serious problem (Adcock/Collier 2001). Measures of policy preferences are difficult or impossible to come by and identifying veto players on an empirical basis becomes tedious. As a result, many explicit and implicit assumptions have to be included in order to specify the relationship between VPs and the dependent variable. The causal chains linking independent and dependent variables are often long and contingent, and the link between theoretical concepts and indicators is often fairly weak.

As a result, the problem of discrimination between different VP arguments as well as between institutional and non-institutional variables becomes almost entirely a statistical matter. The demands on the data analysis increase strongly. Yet, as is well-known, the sample size in comparative politics often does not provide enough information to “let the data decide”. The pooling of cross-section and time-series data does often not help much in this respect because VP constellations usually do not change much over time and because few political science hypotheses are about short-term adjustment processes. The important variance is typically that between the cross-sections, but if the cross-sections are restricted to the set of “advanced OECD countries”, we usually have too little information to discriminate empirically between competing causal chains.⁷ It follows that VP “explanations” of some variance on the dependent variable are most convincing if (i) the sample size is large, and if (ii) the causal chain between VPs and the dependent variable is not too long, and if (iii) the nature of the causal link does not depend strongly on the actual preferences of all or some VPs.

Henisz’s (2000a) work on economic growth is a good example of a study that approximates these conditions. In his analysis, the link between VPs and growth is not the *assumed preferences* of some specific VPs but policy stability *as such*, the underlying causal mechanism – “credible commitment” –

⁵ The literature on the role of VPs in welfare state expansion and retrenchment is enormous. Among other important works are Armingeon (2002), Birchfield and Crepaz (1998), Crepaz (2001; 2002), Kittel et al. (2000), Kittel and Obinger (2002), Schmidt (1996; 1997; 2001; 2002), Swank (2001; 2002).

⁶ A recent overview over the literature is given by Volkerink and de Haan (2001). See also Sturm and de Haan (1997), Franzese Jr. (2001) and Beck et al. (2001).

⁷ Of course, the small N problem in regression analysis only exacerbates the general problems of regression analysis based on weak theory. See Leamer (1983), Western (1996), and Shalev (1997).

seems fairly robust, and his analysis covers up to 88 countries (see also Henisz 2000b; Beck/Clarke/Groff/Keefer/Walsh 2001). On the other hand, the large sample size raises other serious methodological concerns. One question concerns the stability of the effects in various sub-samples, which is a well-known problem in the cross-sectional growth literature. Another problem is measurement validity and multicollinearity. Gerring and Thacker (2001: 34) note that Henisz' three Political Constraints indices correlate with the Freedom House indicator of Political rights at $r=0.69-0.84$. In these authors' analysis of corruption, the various "VP" indices of both Henisz and Beck et al. (2001) are not robust to the inclusion of the Political Rights variable.

The literature on welfare state or redistributive effort, on the other hand, is a good example for the problematic combination of contingent causal pathways, lacking data about preferences, and small sample size. This literature is mainly driven by a generalized assumption about the preferences of specific VPs as well as about the dynamics of VPs' interactions (e.g., "logrolling"); and the decision about which VPs to include and which VPs to lump together into one index are partly derived from these assumptions about preferences. However, many different and partly contradictory assumptions exist and we lack a theory about the relative importance of the various causal chains. As a result, most authors have found "statistically significant" effects of VPs, but the various measures of VPs could hardly be more diverse. For example, some authors have come to focus on federalism and bicameralism as the main ("countermajoritarian") VPs (Swank 2001; Kittel/Obinger 2002) while others use additive indices with up to ten potential VPs, including central banks or the organizational structure of the social security system (Schmidt 2000: 352; 2001: 40). This literature thus not only raises important concerns about the "conceptual equivalence" of the various VPs (Kaiser 1997; 1998), but it has also contributed little to our *theoretical* understanding of the effects of VPs. While the literature supports the result of qualitative studies that "VPs matter", no consensus has emerged on *which* VPs matter (most), and *how much* of a difference VPs make.⁸

As already noted, this lack of theoretical and conceptual convergence in the literature on veto points and welfare state effort is *not*, in my view, a sign that this literature is flawed, but simply reflects the fact that there are many plausible causal chains linking a crudely measured independent variable to a highly aggregated dependent variable. For example, in the original formulation of the veto point argument by Huber, Ragin and Stephens (1993), single-member district electoral systems (SMD systems) were seen as crucial veto points. Huber et al. carefully explained the underlying logic, drawing on (the draft versions of) Immergut (1992) and Maioni (1998). The argument was that SMD systems tended to lead to a lack of party discipline, which made it easier for special-interest groups to use parliament as a veto point; and since special-interest groups were assumed to be opposed to welfare state expansion, SMD systems were associated with *less* welfare state effort.⁹ Unfortunately, there is

⁸ Of course, the diverging results of different authors working with different VP indicators are also due to different estimation techniques. Most of these studies use pooled cross-section time-series data, and the adequate way to deal with this type of data in different situations is still controversially discussed in political science (see, e.g., Achen 2000; Huber/Stephens 2001: 57-62; Kittel/Obinger 2002). In what follows I will not enter into this discussion.

⁹ Note that the direction of the causal effect is found to be reversed during periods of retrenchment. The absence of veto points is found to facilitate retrenchment (Huber/Stephens 2001: 309).

another plausible causal chain that runs in the opposite direction (as noted by one of Huber et al.'s reviewers, p. 722, n. 10): SMD systems tend to lead to fewer coalition parties, which reduces veto points *within* the executive, so that welfare state effort could *increase* (at least if parties *are* disciplined).

However, once the executive is disaggregated into partisan VPs, it becomes questionable whether “interest group access” should really be seen as the main driving force in all VPs’ “preference formation”. Skepticism in this regard has motivated Crepaz’ distinction between “collective/consensual” and “competitive” VPs (see Birchfield/Crepez 1998; Crepez 2001; see also Swank 2001; 2002). Crepez argues that due to “inclusion of interests”, “logrolling” and “collective action problems” (Crepez 2002: 174), “collective VPs”, that is, coalition parties and pivotal opposition parties under minority governments, lead to *more* redistributive effort and higher expenditures. This conjecture is plausible, too, but theoretically underspecified. For instance, “logrolling” as such – that is, without additional assumptions about exogenous shocks etc. – can hardly explain government expansion. Disciplined parties can form preferences about the size of government, and they can design institutions that solve their collective action problem (see, e.g., Roubini/Sachs 1989; Hallerberg 2002). In addition, if the theoretical analysis moves away from an argument about “interest group access” and towards a “partisan” logic, it is not obvious why strong second chambers will always be “competitive”. If opposition parties under minority governments can participate in expansive logrolls, so could second chambers, especially if they are controlled by left parties.

In short, different authors make very different, and partly contradicting, arguments about (i) which VPs matter and (ii) what these VPs’ preferences are. The problem is that we lack a theory about the relative importance of the various causal pathways.¹⁰ As a consequence, different studies typically single out one particular causal chain and let the data “speak for itself”. Unfortunately, with a sample size of below 20, the data does not speak very clearly. All of the very different VP arguments are backed up by some “statistically significant” coefficient estimate, but, in my view, this only shows that the concept of statistical significance becomes toothless when the same types of data for the same small set of countries are analyzed repeatedly, and when the pooling of time-series obscures the “true” uncertainty about the long-term cross-sectional effects.

To get an impression of the uncertainty of the causal inferences, and of the importance of measurement details, let us take a closer look at the measurement of the only two VPs whose inhibiting effect on welfare state effort (during welfare state expansion) seems to be acknowledged by all authors: federalism and bicameralism. Virtually all authors seem to base their measurement of federalism and bicameralism on Lijphart (1984). As a result, both types of VPs are not simply coded as existing or being absent, but as “absent”, “weak”, and “strong”. On the one hand, relying on an already existing coding scheme is a prudent decision when theoretical guidance is lacking. On the other hand, Lijphart’s trivalent coding should at least be consistent with the basic theoretical logic. I am not sure that this is the case. Weak and strong bicameralism (as used in this literature) differ with respect to the

¹⁰ On the problem of various causal pathways linking independent and dependent variables in quantitative research, see also Swank (2001) as well as Gerring and Thacker (2001).

“congruence” of the second chamber (Lijphart 1984: 99).¹¹ Yet if the basic logic is one of interest group access, then one could expect only the veto power of the second chamber to be important. On the other hand, if the causal mechanism is not one of interest group access but partisan veto, then Tsebelis’ absorption rule should probably be applied (see Hallerberg’s paper) and the substantive effect of the upper house should be expected to depend on its partisan composition.

Similarly, the distinction between weak and strong federalism seems to be based on the central government’s tax share (Lijphart 1984: 178).¹² Again, I do not understand how this relates to interest group access, and if the tax share matters, then it should probably be given priority over the formal distinction between “federal” and “unitary” systems. In this case, additional VPs would have to be counted in Denmark, Finland, Norway, Japan, and Sweden (*ibid.*). The trivalent coding of federalism and bicameralism is also somewhat inconsistent with many author’s bivalent coding of presidentialism. From a VP perspective, it would be more intuitive to distinguish the very indirect veto power of the French and Finnish president from the strong power of the US president, rather than to distinguish between weak and strong forms of federalism and bicameralism.

How important are these kinds of coding details? Figure 1 shows the bivariate relationship between Huber et al.’s (1993) original VP (“constitutional structures”), index – which includes presidentialism, referenda, SMD systems, bicameralism and federalism as “veto points” – and the average social security benefits (as percentage of GDP) in the period 1970-79.¹³ The scatterplot shows the influence of the cases of Switzerland and the US. Both countries’ VP score is increased by the trivalent coding of federalism, bicameralism as well as SMD systems. The two countries are coded as having six and seven veto points, respectively.

In Figure 2, the VP index is changed by dropping the SMD variable – for the above-mentioned reasons and in line with Huber and Stephens (2001: 372, n. 10) – and by including all other VPs as simple dummy variables. The effect of this recoding is severe. Countries like New Zealand, UK, Japan, Canada and Australia “loose” between 1 and 3 veto points and the negative effect of VPs is now more strongly driven by Switzerland and the US. This is obviously problematic, because only 4 of 18 countries now have two or more VPs. In addition, it is plausible to assume that the marginal effect of each additional VP decreases with the total number of VPs (*cf.* Hallerberg/Basinger 1998).

FIGURES 1 – 3

In my view, Figure 3 therefore better represents the state of our theoretical knowledge. In this scatterplot, the VP variable only distinguishes three groups of countries, depending on whether they have “no”, “one”, or “more than one” VPs. The effect of VPs is now considerably smaller. However, the location of the cases with below-average “welfare state effort” is much more consistent with the

¹¹ According to Lijphart’s (1984: 99) original definition, weak bicameralism also included systems with formally weak but “incongruent” second chambers.

¹² The coding of federalism is different in Lijphart (1999b).

¹³ The original data comes from Huber et al. (1997). The period average was used in Kittel et al. (2000). I thank Bernhard Kittel for sharing it with me.

available qualitative evidence (see Obinger/Wagschal 2000): the VP explanation works very well for the US and Switzerland (Huber/Ragin/Stevens 1993: 741-42), less well for Australia and Canada (Noel 1999; 2000; Siegel 2000), and not at all for New Zealand and Japan (Seeleib-Kaiser/Thränhardt 2000).

In sum, investigating the effects of VPs on highly aggregated dependent variables is a very ambitious enterprise. If the preferences of important VPs cannot be measured, if hypotheses depend strongly on the nature of these preferences, and if the number of cross-sections is small, results are likely to be fragile. While this conclusion does not deny the importance of VPs, it might “temper enthusiasm about accomplishments” (to cite the conference organizers).

Remedies are difficult to come by. I believe that more could be learned if the *exploratory* nature of most data analyses in very small samples was more openly acknowledged and if the empirical and theoretical level of aggregation was reduced. Empirical disaggregation (decomposing indices) might enable us to learn more about the causal mechanisms underlying different VP arguments and how VPs interact with other (institutional and non-institutional) variables. For example, I find it surprising that the “party discipline” variable, which figured prominently in the works of Immergut (1992) and Maioni (1998) and strongly stimulated the quantitative work, has vanished completely from the VP literature on welfare state effort. It might be possible to find other proxies for this variable and to analyze its effects on interest group access in welfare provision at a less aggregated level (cf. Carey/Shugart 1995). A more disaggregated approach might also facilitate closer links between theoretical concepts and statistical indicators (see, e.g., Gerring/Thacker 2001).

4 Tsebelis’ veto player theory

This section and section 5 largely discuss Tsebelis’ (1995; 2002) codified veto player theory (VPT) as well as those quantitative studies that explicitly apply Tsebelis’ coding rules.¹⁴ I speak of “codified” veto player theory (hereafter: VPT) because Tsebelis relies on essentially the same tools and concepts as other RC institutionalist scholars (e.g., Haggard/McCubbins 2001b). However, Tsebelis does not content with building more-or-less flexible theoretical framework, but tries to move towards a middle-range theory of comparative political institutions that can systematically guide operationalization decisions in quantitative research. Tsebelis’ theory goes much beyond the geometry of unanimity rule. He uses VPs as core concept of a unified and systematic approach to comparative political institutions, which uses all the theoretical tools that RC institutionalism has to offer in order to replace names by variables (see Hallerberg’s paper). The resulting theory is an admirable achievement and may well become “the basis of an institutional approach to comparative politics” (Tsebelis 2002: 432).

VPs are at the core of Tsebelis’ RC institutionalist approach, because VPs are systematically related to policy stability, and it is policy stability that, according to Tsebelis, links VPs to many

¹⁴ See, e.g., Kreppel (1997), Hallerberg and Basinger (1998), Bawn (1999), Tsebelis (1999), and Tsebelis and Chang (2001). With respect to the “divided government” literature in the US, see especially Mayhew (1991) and Binder (1999).

second-order characteristics of political systems, such as government or regime stability.¹⁵ The focus on policy stability, an abstract characteristic of policy output, also links Tsebelis' theory to the quantitative literatures discussed in the previous section. However, in his empirical work Tsebelis tends to avoid some of the methodological problems discussed above by pursuing a less ambitious goal. His theoretical focus is mainly on the short causal chain between policy preferences and policy output (or lack thereof), and he is committed to *measuring* rather than *assuming* preferences. The focus on a small causal chain also makes it easier for him to choose "X-centered" research designs, which do not try to find a "good" model for the variance on the dependent variable but "only" to make unbiased causal inferences about the link between VPs as well as their preferences and policy output (cf. King 1991; Gerring 2001: 137).

An analysis of the limits of codified VPT must focus on this theory's answers to the two basic questions highlighted in the introduction: (i) who are the VPs, and (ii) what is the nature of their preferences. In the case of Tsebelis, the second question is the more basic one, because coding rules about which players are to be included in the analysis are to a large extent derived from the assumptions about preferences. Only the question of VP (party) cohesion is mainly treated as an empirical matter. Tsebelis acknowledges that lacking party discipline or party cohesion – two concepts which he seems to use interchangeably – may force one to disaggregate the analysis and use different modeling tools. The problem of party discipline also seems to be one of the reasons why all existing quantitative comparative has a strong focus on European parliamentary democracies.

Tsebelis makes three crucial assumptions about VP preferences:

- All VPs' are pure *policy-seekers*. That is, *partisan* VPs are Downsian parties that treat policy gains as directly convertible into electoral gains.
- VPs' have fixed *policy* preferences.
- These preferences have an Euclidian shape.

The last assumption – Euclidian preferences – is a technical necessity. If VPs were allowed to weight different issue dimensions differently (and if information on issue salience is lacking), hardly any general theoretical arguments could be made about policy output and policy stability (for a general critique of spatial preferences Milyo 2000).¹⁶

The first assumption – Downsian parties – plays a crucial role in some of Tsebelis' coding decisions. Most importantly, Tsebelis' argues that, as a general rule, opposition parties are *not* veto players under minority governments and that *all* coalition parties are veto players in surplus majority coalitions. I will deal with the issue of vote- and office-seeking VPs in more detail in section 5. Here it

¹⁵ Related approaches have also focused on other abstract criteria of policy output, e.g., public-regardedness versus private-regardedness of policy, social welfare (Kaldor-Hicks improvements), or intertemporal stability (Scharpf 1991: 56; 1997: 89-93; Cox/McCubbins 2001).

¹⁶ Issue salience may, however, be the source of a *systematic* difference between different VP constellations, e.g., if coalition parties put more emphasis on the issue dimensions their ministers occupy. For a radical version of a similar argument, see Laver and Shepsle (1996).

suffices to say that Tsebelis' argument does not necessarily hold if VPs are not pure policy-seekers.¹⁷ Whether or not Tsebelis' coding rules about minority governments and oversized coalition (as well as courts, see below) bias causal inferences depends on whether or not departures from pure policy-seeking can be assumed to "average out". In a small-N cross-section regression that covers only one or a few instances of policy output (or lack thereof) in each cross-section, "omitted veto player bias" may be a significant problem.

The rest of this section deals with the second assumption – fixed policy preferences. Despite their central importance assumptions about preferences are not discussed in any detail by Tsebelis. In general, he clearly talks about VPs' or deputies' *policy* preferences, although he seems to use various terms interchangeably. The distinction between outcome and policy preferences is not systematically discussed.¹⁸ He does discuss *uncertainty* about the relationship between policies and outcomes, but more as an exceptional case, e.g., in his discussion of the effect of exogenous shocks on government formation (p. 314-17). However, in this discussion Tsebelis switches to a conception of actors (VPs) that have preferences over outcomes, so that the exogenous shock can be modeled as a movement of the status quo (or reversionary outcome) rather than as a change of policy preferences. The lack of distinction between outcome and policy preferences can have important consequences for both coding rules and empirical results. Both problems are discussed, in exemplary ways, below.

4.1 Coding rules

Tsebelis' treatment of constitutional courts that have strong review powers is probably the best example for the importance of assumptions about preferences. In contrast to scholars such as Stone Sweet (2000) or Volcansek (2000; 2001), Tsebelis (2002: 331) argues that judges are usually "absorbed" because appointment processes eliminate extreme "positions". He realizes that if judges were *always* absorbed they would *never* veto legislation, which is not the case. He solves this "puzzle" by noting that (i) although judges are selected for their competence and "(known) policy position", "[s]ome of their positions may be unknown because they have not deliberated on every issue" and that (ii) the court may only be indicating to the government that a "particular way of reaching its goal violates the constitution, so a different course of action is necessary" (Tsebelis 2002: 331-32).

I believe that Tsebelis strongly underestimates both the extent to which most political struggles are about means rather than ends and to which the *policy* preferences of the "median judge" are unknown in advance. Even if parties and judges are close to each other in the space of outcomes or with respect to basic principles, they may often be far apart in the space of policies. The reason is that politicians and judges face *very different constraints* in translating outcome into policy preferences. In

¹⁷ Many other quantitative studies in comparative politics do treat opposition parties under minority governments as powerful VPs. See, e.g., Powell (2000).

¹⁸ Tsebelis also is agnostic as to whether deputies and VPs "have direct preferences over policies..., or whether they care simply about reelection...". In the first case it would be politicians that have to form theories about how policies relate to outcomes; in the second case it would be voters, based on their "tastes" (cf. Laver/Hunt 1992: 3).

fact, *legal constraints* may be so tight that judges' preferences have to be seen as discrete rather than continuous and spatial (van Hees/Steunenberg 2000).

Consider an example. In Germany, the constitutionally protected right of "equality" is also applied to taxation. Virtually all judges have a preference for equal tax treatment, and virtually all parties embrace equal taxation as a basic principle. However, in translating this basic principle into operative policy preferences, parties and judges face very different constraints. Parties have to trade-off equality against budgetary, electoral, and efficiency goals and therefore choose discriminatory taxation all the time (Hettich/Winer 1999). By contrast, judges' *policy* preferences are restricted to the set of legally defensible alternatives. Thus, policy preferences of judges and parties may diverge strongly and courts can become crucial veto players on almost every aspects of taxation, which is what happened in Germany (see, e.g., Mäkinen 2001: 27-30).

Hence, with the distinction between outcome and policy preferences in mind, Tsebelis' argument about judges becomes much less convincing. More specifically, the adequacy of ignoring judges in empirical analyses will depend strongly on the set-up of the empirical analysis and especially on the dependent variable. If the dependent variable measures particular policy outputs over a limited time period (rather than aggregate outcomes), "omitted veto player bias" may be severe.

4.2 Empirical results

The distinction between outcome and policy preferences is also important for empirical veto player analyses. As discussed in the introduction, there is indeed every reason to expect a strong relationship between VPs' *policy* preferences and policy output (policy stability). The main problem is one of measuring VPs' policy preferences. In fact, measures of policy preferences generally do not exist. Even if indicators of "partisan ideology" go beyond a simple left-right distinction and gauge parties' positions on particular issues, they do not measure *policy* preferences.

Consider an example. One important measure of "partisan ideology" is the Laver-Hunt scores on increasing services versus cutting taxes, ranging from 1 to 20. Even if the dependent variable in a veto player analysis were the overall level of taxation or spending (which is of course an *outcome* variable), the parties' general ideological differences would not necessarily translate into fiscal policy differences. If, for example, a small country with an already high tax burden and high unemployment has to deal with large budget deficits, short- and medium-term fiscal policy preferences of parties are likely to converge strongly (see the Finnish example below). In addition, the Laver-Hunt scores generally tell us little about the tax mix or tax structures that different parties prefer.

Of course, in many standard empirical research designs, the resulting problem is probably merely one of *random* measurement error, which attenuates the effect of VPs. In fact, measurement error might be one of the reasons why the estimated effect of VPs' in Tsebelis' regression analyses are fairly small (Tsebelis 1999; Tsebelis/Chang 2001; Tsebelis 2002: chap. 7). For example, in his study of labor

legislation, Tsebelis (1999: 604) finds that the ideologically most diverse government on average produced about one law less than a single-party government.¹⁹

Beyond random error, however, the lacking distinction between policy and outcome preferences may however entail misleading expectations about the effect of veto players – in both quantitative and qualitative analyses. The best example is probably the effects of exogenous shocks, which figure prominently in the literature on divided government and veto players (cf. Alt/Lowry 1994; Alesina 1995). Given fixed policy preferences, exogenous shocks may indeed be difficult to handle by systems with many ideologically diverse veto players. On the other hand, exogenous shocks are also likely to *change* VPs' *policy* preferences, because they change both the actual relationship between policies and outcomes and VPs' beliefs about this relationship (see also Milyo 2000). Depending on the ir precise nature, exogenous shocks may therefore render veto players almost irrelevant. Consider a quantitative and a qualitative example.

Hallerberg and Basinger (1998; 1999) had the great idea of using an *actual* international economic shock to design the first cross-national quantitative test of the VP argument. The US tax reform of 1986 was widely believed to be such a shock for the other OECD countries, forcing them to reduce both their corporate and personal tax rates. Using this shock as a starting point solved two problems at the same time. First, the *direction* of policy change could be assumed the same for all countries; second, if the shock was strong, one could assume the *demand* for policy change to be roughly constant across countries, so that the effect of veto player on the *supply* could be measured by the magnitude of policy change in the adjustment period after the shock (cf. Fiorina 1996: 89). Hallerberg and Basinger find, as expected, that countries with only one VP cut corporate and top personal tax rates much more then countries with more than one VPs (similarly, Wagschal 1999b). 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 State 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.

Yet this explanation only begs the question: Why *was* partisanship immaterial? Why *did* left-wing parties decide to cut tax rates? The only satisfactory answer – outside VPT's conceptual map – is that *beliefs* about the mapping of tax rates and policy *outcomes* changed and led to a convergence of governments' policy preferences across countries. In other words, the economic shock must have been so strong (and policy adjustment so inexpensive) that “ideological differences” became unimportant.

¹⁹ Tsebelis (1999: 604) argues that the estimate only “appears” small because one would have to aggregate across different areas of legislation to find the “overall” effect. I do not understand this argument. What ultimately matters is the number of laws prevented by ideological divergence *relative* to the “demand” for laws. Since aggregation over areas of legislation increases both the number of prevented laws and the demand for policy change, the “overall effect” of VPs should only increase if the estimate for labor legislation happens to be a particularly small estimate.

Indeed, with respect to corporate taxation the Australian and New Zealand governments are the prototypical examples of this kind of preference reversal, because their former beliefs about tax policy tradeoffs had led them to *increase* corporate tax rates immediately before the shock.²⁰

However, once the underlying causal mechanism (belief change) is acknowledged, another question comes up immediately (Ganghof 1999): if agenda setters' policy preferences converged *across* countries, why did not veto player's policy preferences converge *within* countries. My own quantitative and qualitative research on this economic shock, which focuses on *general* rather than *central* government tax rates, strongly suggests that VPs policy preferences did indeed converge in corporate taxation (Ganghof 2001): the US reform in 1986 marked the beginning of a new "policy regime" (Przeworski 2001: 324)²¹ so that neither partisanship nor the number of veto players had a general effect on the size of corporate tax cuts.²² The Australian case mentioned by Tsebelis is one example for this within-country preference convergence, because the government did not control a majority in the second chamber. But also the Nordic and continental European countries with multi-party minority and majority coalitions responded swiftly to the economic shock. The effect of the US tax reform on corporate tax *rates* therefore is not an example for how domestic institutions can mediate economic pressures, but for how "strong but narrow" (cf. Mosley 2000) economic pressures can almost nullify institutional differences.

As a second, qualitative, example, consider the case of Finland. It is quite difficult to tell a standard veto player story about Finnish politics in recent years. Finnish government coalitions are usually oversized and the ideological differences between coalition partners, as measured by the standard indices, are quite large (see Tsebelis 1999: 606-07). In fact, since 1995 Finland has been governed by the so-called Rainbow coalition including both the far left (post-communists) and the far right (Conservatives). According to the standard scores of party ideology, the "ideological range" of this coalition is large: 5.5 on a ten point left-right scale (Huber/Inglehart 1995) and almost 11 on the Laver-Hunt twenty-point taxation scale (Laver/Hunt 1992: 179). Given these "ideological differences" VPT would probably predict government instability and an increased likelihood of policy deadlock, especially since Finland experienced a very strong economic shock (recession) in the early 1990s, the adjustment to which took the rest of the 1990s. Minimum winning coalition would have been possible

²⁰ New Zealand even seems to be a case of changing *outcome* preferences. Partly in response to a number of external economic shocks, and under the influence of a small coterie of market-liberal reformers, New Zealand's Labour Party became an altogether different party during the 1980s, moving from a 3.8 on a ten-point left-right scale to 13.8 on a twenty-point taxation scale, thus *leapfrogging* the National Party (Castles/Mair 1984; Laver/Hunt 1992). Only after 1995 did the Labour Party return to its center-left stance on economic issues (Brechtel/Kaiser 1999).

²¹ "Policy regimes are situations in which major parties, regardless of their partisan stripes, propose and implement similar policies." (Przeworski 2001: 324)

²² According to my analyses, top personal tax rates were not subject to a strong shock. Therefore, VPs made some difference for their setting. Left-wing VPs tended to increase top rates, right-wing VPs tended to decrease them. However, in this respect opposition parties under minority governments, which are no veto players according to Tsebelis, were as important as coalition parties. In addition, in one country, Germany, the Constitutional Court was a crucial veto player. (Ganghof 2000: 611-13; 2001).

(and were predicted (Mattila 1997)), because the previously existing supermajoritarian elements of the Finnish constitution were abolished by 1995.²³

However, none of these predictions materialized. Since 1983 no government has been prematurely terminated, and there are no obvious signs of frequent policy deadlock. How can this puzzle be explained? Part of the answer, I believe, lies in the vote- and office-seeking incentives of the partisan veto players (see section 5). However, another part of the explanation is simply that *policy* preferences converged strongly due to the tight economic conditions. Finland is a small, high-tax country. If such a country has to deal with huge (shock-induced) budget deficits and high unemployment, medium-term policy preferences can converge strongly. For example, despite general ideological differences on taxation and the size of the public sector (captured by the Laver-Hunt scores), all Finnish parties in 1995 agreed that taxation levels had to be maintained and spending had to be reduced. Hence, all that parties could thus about on taxation were marginal issues of tax mix and tax structure (Jungar 2000: chap. 6; 2002). Put bluntly, the “exogenous shock” diminished rather than increased the importance of veto players and their ideological differences. As the leader of the Finnish Conservatives, Sauli Niinistö, put it after the Rainbow coalition’s first term:

Four years ago, I used to say the government has an easy time because the nation had difficulties. Now it seems the nation has easier times, so the risk that the government will have difficult times is increasing.²⁴

In summary, I am prepared to believe that the link between VPs’ policy preferences and policy stability is strong enough to use it as the basis for a theory of comparative political institutions. If this is true, attention shifts to Tsebelis’ specific propositions about second-order effects of VPs (but which are not subject of this paper). VPs are of course also important variables in efforts to explain particular policy outputs and outcomes: as key explanatory variables, control variables, and parts of interaction terms (cf. Schmidt 1996; Imbeau/Pétry/Lamari 2001). Yet since we cannot measure operative policy preferences, the linkage between one’s measure of preferences (or ideology) and one’s measure of policy output has to be carefully considered. While this insight might seem trivial, it is often neglected in actual empirical work.²⁵

²³ Constitutional rules allowed one-third of all MPs to postpone the final adoption of an ordinary law by two to four years by voting it to lie dormant (Anckar 1992). In addition, a law introducing a new state tax and lasting for more than one year also had to be passed with a two-thirds majority (Nousiainen 2000: 269).

²⁴ *Financial Times* Survey, <http://specials.ft.co./ln/ftsurveys/country/q298e.htm>.

²⁵ I have not discussed the question whether hypotheses derived from or consistent with VPT perform better empirically than other, closely related, hypotheses. The problem is that lacking degrees of freedom and disagreement about estimation techniques do not seem to allow any clear conclusions. For example, Tsebelis (1999; 2002: chap. 7) finds that the negative relationship between “ideological range” and the number of significant labor laws is strongest in minimum winning coalitions, less strong in oversized coalitions, and virtually absent for minority governments. While this throws doubt on Tsebelis’ coding rules for the latter two types of government (see section 5), the sizes of the sub-samples are too small to draw any clear conclusions. Similarly, in the study of fiscal policy and budget deficits, VP hypotheses (focusing on the number of and/or ideological distance between coalition parties) have many contenders (e.g., hypotheses that focus on coalition parties’ bargaining strength or that focus on governments’ majority status). Unfortunately, for the above-

5 Partisan goals in comparative veto player analysis

Most VP analyses, and especially Tsebelis' VPT, focus on policy-seeking VPs. However, in all of the discussed literatures, there have been voices arguing that this focus is too narrow. Many studies of legislative decision-making – which do often not work with an explicit veto point or veto player framework – have highlighted the importance of vote-seeking behavior in explaining legislative outputs (or the lack thereof). For example, Sundquist's (1988) seminal article on the consequences of divided government in the United States argued that even if VPs can find a policy compromise (that is, if the winset of the status quo is non-empty), they may frequently have an incentive to eschew compromise and try to lay the blame for the consequent policy failures on each other. This line of reasoning has been further developed and refined by various authors (Gilmour 1995; Goodin 1996; Quirk/Nesmith 2000; Groseclose/McCarty 2001).

Very similar arguments have been made for other countries as well. For example, Tsebelis (2002: 131) believes that “in Germany if the Bundesrat is controlled by the opposition the situation is not politically different from a grand coalition: legislation that is not approved by both major parties will not be accepted”. Many German authors would strongly disagree. For them, the two situations are *numerically* similar, but *politically* very different. It has long been argued that vote-seeking and “position-taking” (Mayhew 1974) incentives can turn an opposition-controlled Bundesrat into a “competitive”, obstructionist veto player, especially with respect to “significant” legislation (Lehmbruch 1978; Scharpf 1995; 1997; Zohlnhöfer 1999; 2000). Similarly, a number of authors have analyzed how opposition parties under minority government oscillate between “consensual” and “competitive” strategies, depending on their perception of the electoral as well as economic costs and benefits of policy compromise (Huber 1996; 1999; Boston 1998: 81-84; Green-Pedersen 2001).

These arguments have also left their mark on the quantitative literature. Section 3 discussed Crepaz's distinction between “consensual” and “competitive” VPs. While this distinction largely grew out of the literature on “welfare state effort” and focused on the *substantive* effects of the two types of veto players (more redistribution or spending v. less redistribution or spending), Crepaz' also made a more general argument and pitted his distinction against Tsebelis' assumption that VPs are all created equal (for a reply, see Tsebelis 2002: 132-35). In their original argument for distinguishing different *types* of VPs, Birchfield and Crepaz (1998: 181-82) emphasized that consensual veto points entail “face-to-face” interaction as well as “shared responsibility”. This has to be interpreted as a hint at either the endogeneity of policy preferences – that is, convergence of technical beliefs between consensual players, divergence between competitive players – or at the differential effects of vote-seeking incentives on different types of VPs.

Wagschal (1999b; 1999a) also makes a *general* distinction between consensual and competitive veto players that is not focused on a particular mechanism (like “logrolling”) or a particular dependent variable. While there are significant differences in the coding schemes of Crepaz and Wagschal, *partisan* veto players (both in government and in parliament) are seen as “consensual”, *institutional* veto players (presidents, second chambers) as “competitive”.

mentioned reasons this literature as a whole is inconclusive with respect to the relative performance of various theories (for a recent overview, Volkerink/de Haan 2001).

All of these arguments suggest that partisan VPs' vote- and office-seeking behavior can have systematic effects on policy output (or the lack thereof), and that therefore the institutional and non-institutional context in which VPs are embedded should be brought into the analysis. However, there have been no efforts to develop a comparative and theoretical perspective on partisan VPs with multiple and partly contradictory goals. In-depth studies lack a comparative perspective, while the competitive-consensual distinction lacks a clear theoretical foundation.

I have no pretensions about being able to provide such a systematic comparative perspective. In what follows, I merely want to highlight that various country-specific studies have made very similar arguments. In addition, to make an empirical case for the importance of VPs vote- and office-seeking behavior, I will briefly analyze three cases that represent particular VP constellations and that VPT has difficulty to explain. Like Tsebelis, I focus on parliamentary democracies – Denmark, Finland, and Australia – that are characterized by generally high levels of party discipline and for which data about parties' "ideology" is available.

5.1 *Relaxing the assumption of pure policy-seeking*

Authors that relax the assumption of purely policy-seeking ("Downsian") VPs usually focus on VPs' vote-seeking. More specifically, the following assumptions are given up. First, many authors allow partisan VPs' *policy preferences to diverge from those of their own voters*, or at least of some important constituencies. This divergence is to a large extent a result of the fact that politicians have a better understanding of the *tradeoffs* involved in translating outcome preferences into policies. For example, welfare state retrenchment is not popular *policy*, but might be seen as inevitable in order to deliver favorable *outcomes* (e.g, higher employment) at the end of the electoral term (Kitschelt 2000: 162).

Allowing for diverging policy preferences between parties and their voters leads to the basic distinction between two types of issues, those where preferences of parties and their voters are aligned and those where they are not. Quirk and Nesmith (2000) call the latter "autonomy issues". On autonomy issues, the logic of veto player interaction is often one of blame avoidance (Weaver 1986) rather than credit claiming. This has two important consequences. First, the absence of additional veto players may become a liability rather than an asset for the agenda setter. Second, if additional veto players exist, the bargaining outcome is difficult to predict. In both cases, non-institutional factors (e.g., the configuration of party competition or issue ownership) have to figure prominently in the explanation of policy stability or change (recall the discussion of Bonoli's analysis in section 2).

The second assumption that many authors relax is that all veto players have the same capability of "converting" policies into votes (either positively or negatively). Huber (1999: 276-77) nicely describes the point:

If a party's role in implementing a policy is unclear to voters, then it will be difficult for the party to translate its policymaking role into electoral success. Therefore, the extent to which parties can pursue votes by pursuing policies will depend on the extent to which it is clear to voters where responsibility for policy outcomes lies. The degree to which vote-seeking parties are Downsian, then, depends on the ways in which institutional arrangements shape voters' perceptions of the policymaking process.

The typical assumption is of course that the VPs that form the government are responsible, while opposition parties (under minority governments or when controlling institutional veto points) have greater difficulty in claiming responsibility. Thus, the way vote-seeking incentives play out in VP behavior may differ systematically across countries. However, institutional arrangement may not be the only factors that shape “voters’ perceptions of the policymaking process”. Cultural traditions or the normative underpinnings of a political system may also be important (see the discussion of the Australian “*Washminster*” system below).

Once vote-seeking – and any other partisan goal for that matter – becomes partly divorced from policy-seeking, the conception of “policies” also changes. In the standard (RC) veto player model, each policy in the multidimensional space is defined by some (Euclidian) distance to each player’s ideal point, and VPs simply minimize this distance. Once other partisan goals enter the analysis, the Euclidian distance to the VPs’ ideal point is only one of several *attributes* of a particular policy (cf. Jones 1994; 2001). From this perspective, partisan VPs do not care about policies *as such*, but about various attributes of a policy, most importantly,

- its (Euclidian) distance to the VP’s ideal point (*Policy*),
- the signal it sends to the VP’s voters (*Vote*),
- the extent to which it helps to stabilize the VP’s *office* position,
- or the signal it sends to the party membership (*internal coherence and mobilization*)

One difficulty in modeling VPs with multiple goals is that the *Vote*, *Office*, and *Internal Coherence* attributes of a particular policy are itself a function of the policy’s location. It is interesting to note, however, that many country-specific analyses of legislative decision-making seem to rely on very similar intuitions. The basic idea is simple. Those veto players that have greater problems in translating policy gains into electoral gains will likely demand higher policy gains as compensation. As Quirk and Nesmith (2000: 574) put it in the case of divided government in the US:

[T]he president and Congress may adjust the division of a bill’s benefits between their respective party constituencies to even out electoral effects that would otherwise hinder agreement. If passing a tax cut will tend to help a Republican president, a Democratic congressional majority may demand that its constituency groups get the larger share of the cuts.

This view implies that the “electorally disadvantaged” players have to achieve a higher policy gain in order to agree to policy change compared to the case of pure policy-seeking.

However, if vote-seeking matters, each VPs’ willingness to agree to policy change will not only depend on their *policy gain* (how much the Euclidian distance between their ideal point and the status quo is reduced by the policy change), but also on their *policy ambition* (the distance between their ideal point and the status quo ante). Consider the example of the tax reform project of the conservative German government, which failed due to a veto of the German upper house (Bundesrat) in 1997/98. It was *the* major political project of this Kohl government and its failure triggered a fierce debate about the consequences of divided government in Germany (Silvia 1999). Many observers believe that the strategic situation in the tax reform negotiations can be described by Figure 4 (cf. Zohlnhöfer 1999; 2000).

Figure 4



Figure 4 shows a one-dimensional policy space ranging from low to high (effective and statutory) income tax rates. All parties want to reduce taxes compared with the status quo (sq). As explained by Tsebelis (1999), we can focus on the two most extreme players. Player **a** is the Social Democrats, who controlled the Bundesrat and wanted only a minor tax cut. Player **b** is the Liberals, the small market-liberal coalition party, who wanted a large tax cut. Assume that the other coalition parties (Christian Democrats) were somewhere between the points **b** and **t**. If VPs were only interested in *Policy* (as assumed by Tsebelis' VPT) the winset of the sq would be given by the interval $[t, sq]$. If the government had exclusive agenda setting power, the government would propose the point **t** and the Social Democrats would accept, because they are indifferent between **t** and **sq**. However, if Social Democrats believe that the coalition parties will be better in converting policy change into electoral gains, they have little incentive to accommodate the government. Assume therefore that Social Democrats announce that they will only accept their own ideal point **a** and that the government believes this threat to be credible because it knows the Social Democrats' vote-seeking incentives.

Now, one could expect that the governing coalition accepts **a**, because this policy change would increase all player's policy utility. However, while the coalition parties may be better at converting policy gains into electoral gains, they may also be skeptical about voters' ability to understand the game that is played in the legislative arena. Especially for the Liberals, player **b**, policy **a** may not be strong enough a signal to their voters, given the Liberal's ambition to achieve a large tax cut. Due to their incentive to send a clear signal to voters, Liberals may not care about their absolute policy gain associated with policy **a**, given by the interval $[a, sq]$, but about their policy gain relative to their policy ambition, given by the interval $[b, sq]$. Put differently, there is likely to be a limit to the policy sacrifice, here given by $[b, a]$, that the Liberals are willing to make.

This idea can be expressed by a *sacrifice ratio* which puts the maximal policy sacrifice a VP is willing to make in relation to this player's policy ambition. In Tsebelis' VPT, veto players are willing to agree policies that are associated with very small policy gains (as long as they are bigger than transaction costs). Thus, their sacrifice ratio can be close to one. If goals other than *Policy* are included into the player's utility function, their sacrifice ratio may be significantly smaller. In Figure 4, assume that the Liberals need at least policy **t** in order to send a clear signal to their voters, so that their sacrifice ratio is around 0.7 ($[b, t]/[b, sq]$). In this case, and if Social Democrats demand policy **a**, the winset of the status quo becomes empty, even though it would not be empty with Downsian parties. In the case of the Germany tax reform, Liberals had indeed good reasons to eschew compromise with the Social Democrats: (i) the tax reform negotiations came late in the electoral cycle; (ii) the party believed to "own" the tax issue (cf. Petrocik 1996); and (iii) its popularity at the time of the negotiations was

low. Thus, the Liberals had a clear incentive to block “a tax reform that was too far away from its own ideas” (Zohlnhöfer 2000: 721, my translation, SG).²⁶

The important lesson of the German tax reform example is that differences in VPs’ sacrifice ratios are not only driven by their institutional position (government v. opposition) and thus their ability to convert policy into electoral gains. The liberals *could* have claimed credit for a pulling tax policy in the “right” direction. However, given their relatively *extreme* policy position and their weak electoral position at the time, the achievable policy gain was not sufficient to send a clear signal to their voters.

I believe that the logic of a sacrifice ratio is well-suited to explain veto player’s oscillation between more and less accommodating behavior in many political systems. The most obvious examples are opposition parties under minority governments. For example, largely in response to the gravity of Sweden's economic problems, the Swedish Centre Party entered into a “parliamentary coalition” with the Social Democratic minority government in April 1995. However, deterioration in its opinion poll ratings during the period of the parliamentary partnership eventually prompted the party to reduce its level of support for the government during the latter part of 1997. The resulting decrease in the parties’ level of accommodation (its sacrifice ratio) was reinforced by the improved state of the economy. “As the urgency of the economic situation faded, there was every expectation that the Centre party would adopt a less accommodating approach. And this, indeed, is precisely what happened.” (Boston 1998: 84; see also Huber 1999).

One may argue that the factors that influence VPs’ sacrifice ratios (poll ratings, electoral cycle, issue ownership, economic pressures) are too erratic to be explicitly excluded in (formal) VP analyses, and that they average out over time and across space. However, a detailed comparative analysis of the vote- and office-seeking incentives of VPs’ may also identify systematic but frequently neglected sources of policy stability or change – and thus also of second-order variables such as government stability. The following brief case studies corroborate this expectation.

5.2 *Vote- and office-seeking VPs in comparative perspective*

Minority governments in Denmark

The performance of Danish minority governments is difficult to explain from a VPT, which does not treat opposition parties under minority governments as veto players (and therefore does also not associate minority governments with government instability). First, during certain periods, Danish minority governments were forced to make very significant concessions to opposition parties. Second, Danish minority governments accepted “alternative majorities”. The first three bourgeois governments under Schlüter in the 1980s lost 8 percent of the final votes in parliament (Nannestad 1997: 71). In fact, on non-economic issues, the government accepted so many defeats that in effect the opposition “governed” on many of these issues (Damgaard/Svensson 1989; 1992). Third, in the case of Denmark

²⁶ Of course, to argue that the tax reform failed because of VPs’ vote-seeking, plausible vote-seeking and position-taking incentives are a necessary but not a sufficient condition. Another necessary condition is that the size of the winset would indeed have been empty with purely policy-seeking veto players. Here I am agnostic as to whether the one-dimensional reconstruction of the policy space is adequate.

there seems to be a clear relationship between minority governments' difficulty to bring their policies through parliament and government instability (cf. Strom 2000b). Finally, the policy-making "strength" of Danish minority governments shows significant variation over time and thus can hardly be explained with the agenda-setting powers of the government (Green-Pedersen 2001).

Tsebelis' (1995b; 2002) argument that opposition parties under minority governments, especially one-party minority governments, are generally no veto players depends strongly on his assumption of Downsian parties with spatial preferences. His argument is essentially that because minority governments are, by their nature, located in the centre of the policy space, the situation on each major issue dimension looks similar to that depicted in Figure 4. Player **a** is the government, player **b** is the opposition party that is located on the same side of the status quo. Since a Downsian opposition party will agree to *any* policy that brings the status quo closer to its ideal point, Tsebelis' opposition party would bow its thanks to the government and agree to a policy near the government's ideal point.

Obviously, this argument does not necessarily hold if the opposition party has a "sacrifice ratio" of significantly below 1. In other words, if the opposition party believes that agreeing to a particular policy proposal will lead to electoral losses (or lack of gains) that outweigh the policy gain, they will reject the proposal. This leads to the first important factor influencing policy (and government) stability under minority governments: the *configuration of the party system*. It is reasonable to assume that the major opposition party tends to be non-accommodating towards the government. The government's bargaining power therefore increases if it can rely on other opposition parties, esp. those on *their side* of a particular issue, who are interested in influencing policy (not too small sacrifice ratio) *and* whose policy preferences are not too extreme.

The Danish experience is in line with this expectation. Several of the bourgeois minority governments under Poul Schlüter in the 1980s had large difficulties finding parliamentary majorities when they needed the support of either the Social Democrats (the major left-wing opposition party) or of the entire non-socialist block, consisting of Radical Liberals and the Progress Party (Green-Pedersen 2001). The problem was that the Progress Party had an extreme ideology (19.4 on the Laver-Hunt taxation scale) and at times lacked any interest in influencing policies. In combination these two factors made it "difficult [for the government] to negotiate with the Progress Party" (Green-Pedersen 2001: 59). For example, before 1989 the Progress Party voted against the budget as a matter of principle. The government could turn towards the left and try to make deals with the Social Democrats, but given this party's sacrifice ratio, deals on important pieces of legislation were typically only possible when the government proposed legislation that was close to the Social Democrats' ideal point. On a number of issues, agreement was only possible when the government was willing to pay a "very high price" in policy terms (Green-Pedersen 2001: 60-61). In short, the winset of the status quo seemed to be significantly smaller than it would have been in the case of purely policy-seeking VPs.

Policy-making was facilitated when the extreme players moved towards the center (with respect to their general ideology as well as their policy preferences) and got more interested in influencing policy. Especially the Social Democratic-led minority governments in the 1990s benefited from this ideological convergence. Both opposition parties on the left, Unity List and Socialist People's Party, moderated their stances on economic policies, so that the government's ability to find support for its policies increased.

A second important factor that is likely to influence the effective size of the winset is the match between the government policy agenda and the structure of “issue ownership” (Petrocik 1996). The opposition parties’ sacrifice ratio will be lower on the issues on which they believe to have an electoral advantage. For this reason, the Danish Social Democrat-led government had significantly less problems finding agreement with the bourgeois opposition on welfare state retrenchment policies than on tax reform. The non-socialist opposition parties had little to lose in electoral terms by agreeing to the former, but much to lose by agreeing on the latter (Green-Pedersen 2001: 65).

Oversized coalition governments in Finland

I have already discussed the puzzle of Finland’s stable oversized coalition governments (since 1983) and especially of the “ideologically diverse” Rainbow government in office since 1995. A closer look at the Finnish case may inform the debate between Tsebelis and Strom about the usefulness of treating all members of an oversized coalition as veto players. As noted above, Tsebelis argues that all members of an oversized coalition government are indeed veto players. Strom (2000a: 280) rejected this view, arguing that some coalition parties can be and frequently are outvoted (see also Strom/Müller 1999: 259-61). In his rejoinder, Tsebelis (2002: 132) insists that policies are negotiated by all parties in the coalition agreement, and that new issues have to be addressed “in common” by all VPs. Otherwise “the government coalition will dissolve, and a new government will be formed”.

In my view, the debate is solved by including *Office* and *Votes* into VPs’ utility function. Strom is correct in emphasizing that VPs can be outvoted, Tsebelis is correct that VPs negotiate the coalition program and that they can leave the government if they are outvoted. The question is: under what conditions do VPs enter into a government, and under what conditions do they leave it? Once we allow *Office* (and *Votes*) to matter and give up the assumption of Downsian parties satisfying well-informed voters, it is easy to see why outvoted parties would not leave the government. If the outvoted party left, it would lose office and the controversial policy would probably be implemented anyway by the remaining veto players. The problem for the VP then mainly becomes one of gauging the electoral consequences of staying in the government and weighting costs against benefits. Hence, if veto players feel unable to predict voters’ responses, or if they believe that the oppositional role is always less profitable, they may well accept significant policy losses in return for maintaining office.

Consider the Finnish case. Experts on the Finnish political system generally describe the logic of recent coalition politics as being characterized by strong office-seeking, proximate *policy* preferences (despite significant general *ideological* differences), and accommodating VP behavior (Jungar 2000; 2002; Nousiainen 2000; 2001; Raunio/Tiilikainen 2002). Some authors go as far as characterizing Finnish parties as “cartel parties” rather than policy-seeking veto players (Aarnio/Pekonen 1999: 195-201).²⁷ The “equilibrium” can be described as follows: proximate policy ideal points of VPs and accommodating VP behavior allow for policy change and make the opposition role seem unattractive. The possibility of policy change leads to government stability, which further reduces the attractiveness of being in opposition, because opposition will endure the whole electoral term. This unattractiveness

²⁷ On “cartel parties”, see Mair and Katz (1995) as well as Kitschelt (2000).

of being in opposition, together with the difficulty of predicting electoral behavior, reinforces accommodating VP behavior.

What does accommodating VP behavior mean? Generally, it means that VPs are willing to accept significant policy sacrifices (high sacrifice ratio). Sometimes, this may even include accepting *policy losses* on important issues (sacrifice ratio of above 1). Of course, showing this type of veto player accommodation empirically is difficult, because all of the concepts we are dealing with (policy preferences, issue salience, sacrifice ratio) are unobservable. However, my interpretation is in line with Nousiainen's characterization of recent Finnish coalition politics. He speaks of a very high "policy elasticity" of Finnish VPs. "The leaders are willing to compromise on their objectives and to limit their decisive preferences to those principles that are of the greatest concern to their party." (Nousiainen 2000: 288).

The Finnish Green League is a good example. Like the other smaller parties, the Greens were deliberately included into the coalition in order to reduce the "blackmail potential" of each of the smaller parties. "No party would be able to require more than it was entitled to with respect to its electoral strength" (Jungar 2000: 317). Like all other parties, the Greens were keen on participating in the government not only because they wanted to get some influence on policy, but also because they believed to *increase their future vote share* by becoming a "credible" party. However, to get into office they had to make large policy sacrifices. As one Green politician put it: "The [government] programme is in many respects a catastrophe, but power is not often offered – when it is, one has to grab it" (cited in Jungar 2002: 74). Given this strong office-orientation, it is not too surprising that the Greens did even stay in the government in early 2002, after they were outvoted in cabinet decision to build a new nuclear power plant – even though the Green's rejection of this policy had been one of the few "non-negotiable" positions of any party before the elections (Aarnio/Pekonen 1999: 201).

Australian bicameralism

My final example for the systematic effects of vote- and office-seeking veto player behaviour is the Australian Senate in the recent one or two decades. The behaviour of this veto player is also somewhat of an anomaly for VPT (as well as for Crepaz' distinction between consensual and competitive veto players). According to VPT, the Australian system should be characterized by a significant degree of policy stability, especially when the government is a Liberal-National coalition. The reason is that the pivotal player in the Senate is typically the Australian Democrats (sometimes together with other minor parties), and the "ideological distance" between Democrats and Liberals (party leaderships) was estimated to be 7.6 on the twenty point taxation scale by Laver and Hunt (1992: 136). This ideological distance is only slightly smaller than, say, that between the German Bundestag and Bundesrat under divided government (Laver/Hunt 1992: 197).²⁸ In fact, VPT would expect policy

²⁸ For VP analyses of German bicameralism, see Bräuninger and König (1999) as well as König (2001).

stability to be *higher* in Australia than in Germany, because Australian parties in the Senate are more disciplined (coherent) than are party blocks in the German Bundesrat.²⁹

However, there seems to be no evidence that policy stability increases significantly under divided government, or when the government (House majority) moves to the right (Sharman 1990: 225; Galligan 1995: ch. 2; Emy 1997; Uhr 1998; Goot 1999; Nelson 1999; Sharman 1999; Uhr 1999; Young 1999; for decriptive statistics, see Elliott 1997). The size of the concessions governments have to make is more difficult to quantify, but examples of large concessions are hard to find. Some even describe the Senate as just “another clearing house for government legislation” (Farrell/McAllister 1995: 260).

Consider the case of the 1993 budget negotiations as an example. This case is somewhat of a least likely case for my argument, because the Senate did initially refuse to pass the budget, it did extract some policy concessions, and this incidence did stimulate public debates about “obstructionist” behaviour of the Senate. The question, however, is whether the Senate really behaved in a “non-accommodating” manner (small sacrifice ratio). Did the Senate really obstruct the government agenda or did they only extract the type of minor policy concessions that are everyday business under minority governments (cf. Hallerberg 2002)?

Young’s (1999) in-depth analysis of the budget negotiations strongly supports the second interpretation. If anything, the Senate was particularly accommodating. Had the Senate been a pure policy-seeker, it would have most likely rejected the whole budget because, according to Young, it was opposed the government’s twin goals of deficit reduction and income tax cuts. In the event, however, the governments broad policy goals remained “unchallenged” (Young 1999: 22):

The right of the government to maintain its commitment to the twin concerns of deficit reduction and income tax cuts were [sic] accepted as *immovable parameters* within which any policy alternatives ... would have to fit. (ibid., my emphasis, SG)

Instead of pursuing its own ideal policies, the Senate focused on subordinate policy dimensions and negotiated policy changes at the margins. How can this type of accommodating behaviour be explained? Again, it is useful to consider VPS’ vote- and office-seeking incentives and to include non-institutional variables. As already noted, the pivotal players in the Senate are usually the Australian Democrats as well as other minor parties and independents. These parties have virtually no chance of gaining representation in the House. 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. The introduction of proportional representation in the Senate not only led to differing majorities in both houses, but also established small parties, rather than the major opposition party in the House, as the pivotal players in the Senate since 1962 (with the exception of the period 1976 to 1981).

²⁹ It has been argued that the Australian Senate is *not* a full-fledged veto player with respect to *money bills* (Tsebelis and Money 1997: 64). This view is incorrect. While the Senate can neither initiate nor amend money bills, it can veto them. In addition, disputes between the two houses are eventually settled by a joint session in which the House majority has greater weight, but this joint session is preceded by double-dissolution and re-election of both houses (see Evans 1999).

Yet these pivotal players have very different vote- and office-seeking incentives compared to the major opposition party in both Houses. If the major opposition party controlled the Senate, its “position-taking” incentives would often make it less accommodating, thus increasing policy stability or forcing the government to make significant concessions (cf. Sharman 1999: 354). By contrast, minor parties have little incentive to block legislation in order to hurt the government or to pass better legislation after the next elections – simply because they can never get into office themselves. In addition, if a minor party holds the pivotal position in the Senate, “strategic gridlock” motivated by vote-seeking with respect to Senate elections is also unlikely, because their power to influence policy can hardly increase.

To the contrary, once non-institutional variables are brought into the analysis, vote-seeking seems to make the Senate pivot more rather than less accommodating. The reason is that voters prefer accommodating veto player behaviour. Many Australian voters clearly understand the character of divided government and welcome minor parties as a moderating force in Australian politics (Bean/Wattenberg 1998; Goot 1999). At the same time, voters want minor parties to accept the overall “mandate” of the government. The Westminster legacy of the Australian political system and the “norms of responsible government” (see also Uhr 1998: 108; Young 1999: 14, 16-18) clearly play role in this respect. “Conventional opinion” is that 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).³⁰ As a result, minor parties generally chose an accommodating stance in the negotiations with the government and try to avoid accusations of obstructionism and attacks on the political legitimacy of their action. Only when minor parties have a strong backing of the voters in opinion polls, or when the government reneges on its election pledges, can minor parties adopt a less accommodating stance (Goot 1999).

In sum, if the assumption of „Downsian“ parties is relaxed, and if the institutional and non-institutional is brought into the analysis, Australian bicameralism resembles one-party minority governments more than, say, German bicameralism. This is a problem for Tsebelis’ VPT as well as Crepaz’s distinction between competitive and consensual VPs. The former counts strong second chambers but not the opposition under minority governments as VPs. The latter treats opposition parties under minority governments as “consensual” and strong second chambers as “competitive”.³¹

³⁰ This principal-agent relationship between voters and minor parties is also fostered by the settlement mechanism between the two houses. Since this mechanism includes double dissolution and re-election of both houses. For a summary of the existing double dissolutions of the houses in Australia, see Evans (1999: section 3.1).

³¹ My analysis of the role of VPs’ office- and vote-seeking behavior has retained the assumption of *fixed* policy preferences. However, it is clear that the same factors that may make VPs accommodating or non-accommodating may also have *systematic* effects on VPs’ technical beliefs about the mapping of policies and outcomes. The causal mechanism is probably simple. Elites, like voters, engage in “*motivated* political reasoning” about alternatives, constraints, and tradeoffs (cf. Lodge/Taber 2000). Their information processing is *directed*. In times of strong economic constraints and severe tradeoffs between highly valued goals, it is therefore possible that different VP constellations differ systematically in the extent to which they lead to a convergence or divergence of VPs’ operative policy preferences. Inquiring into these kinds of differences seems difficult but promising (see, e.g. Steiner/Bächtiger/Spörndli 2001).

6 Conclusion: VPs and constitutional engineering

This is a long paper already, so I will not summarize my arguments. Let me instead make a final remark about the relevance of my discussion for questions of institutional choice and constitutional engineering. Many authors – especially those relying on quantitative analyses – have argued that their work has important implications for institutional choices. Authors that link certain VP structures – or institutional structures more generally – *directly* to substantive outputs or outcomes (without measuring VPs preferences) often advocate those institutions that are found to be associated with “good” outcomes (see, e.g., Crepaz 1996; Lijphart 1999b; Armingeon 2002; Lijphart 2002; Schmidt 2002). Similarly, Tsebelis draws practical lessons from his analyses, which are of course contingent on constitutional engineers’ preferences for either policy stability or potential for policy change: “[D]epending on government composition (or on institutional structures that consistently produce single or multiple veto players), one can get either policy stability or the potential for policy change, but not both” (Tsebelis 2002: 260; see also Tsebelis 1999).

Based on the analysis in this paper, I am not convinced by either type of constitutional advice. The causal inferences of most statistical analyses are too uncertain to be the basis of strong statements about the effects of certain VP structures, especially if the underlying theories are not well specified. Tsebelis’ VPT is well specified but blinds out much of what is of interest for constitutional engineers. In fact, few would object to Tsebelis’ conclusion cited above. By assuming policy-seeking VPs with fixed policy preferences, VPT is bound to corroborate politicians’ conventional wisdom about VPs. VPT fortifies, very much by design, the widely held notion of a *linear* trade-off between “inclusiveness” as well as “checks and balances” on the one hand, and “accountability”, “flexibility”, and “policy coherence” on the other (see, e.g., Royed 1996; Andeweg 2001). Yet from a constitutional engineering perspective, there are good reasons to be less interested in the precise strength of this linear relationship, but in the cases that deviate from the regression line due to *systematic* but omitted factors.

Consider the case of Australia, which we have analyzed above. Lijphart (1999a) proposed to adopt PR for House elections – thus increasing the number of VPs – in order to turn Australia into a “kinder and gentler” consensus democracy. Crepaz might agree to that proposal but propose to abolish the Senate in turn, so that a “competitive” veto player is replaced by “consensual” ones. Sartori (1997: 189) would ideally also abolish the Senate, but since this is difficult to do in a federal system he would opt for the German model of an upper house that is composed of the executives of the member states. Tsebelis would side with Lijphart if Australians want policy stability; if they rather want potential for policy change, he would probably advise them to “absorb” the Senate by ensuring congruent majorities.

Australians do not seem inclined to pursue any of these paths (cf. Brennan 1999) – for good reasons in my view. Once the institutional and non-institutional context in which VPs act is brought into the analysis, many of the proposals become questionable. Introducing PR for the House or adopting the German *Bundesrat*-model for the Senate could easily *increase* the vote-seeking and position-taking behavior of the parties – thus reinforcing ideological differences and hindering efficient decision-making. Conversely, “absorbing” the Senate would increase the risks associated with too strong governments – risks that made New Zealand turn away from a one-VP model (Mulgan 1995). By contrast, the Australian status quo seems to combine a fairly strong and coherent executive with a Senate that is generally fairly accommodating, but that it is able to block legislation altogether if the government reneges on its promises or has lost the support of a large majority of the electorate. The

current Australian “equilibrium” may in fact produce governments that are “strong enough to govern effectively but weak enough not to be able to govern against important interests” (Przeworski 1991: 37).

This example suggests that better constitutional advice hinges strongly on further theoretical progress. I believe that Tsebelis’ strategy of theoretical systematization and empirical disaggregation (that is, measuring rather than assuming preferences) goes in the right direction. However, more variables have to be brought into VP analyses in order to be able to inform constitutional choices. There is every reason to continue the strategy of replacing names by variables, but we should probably not focus exclusively on those variables with which names are most easily replaced.

Figure 1

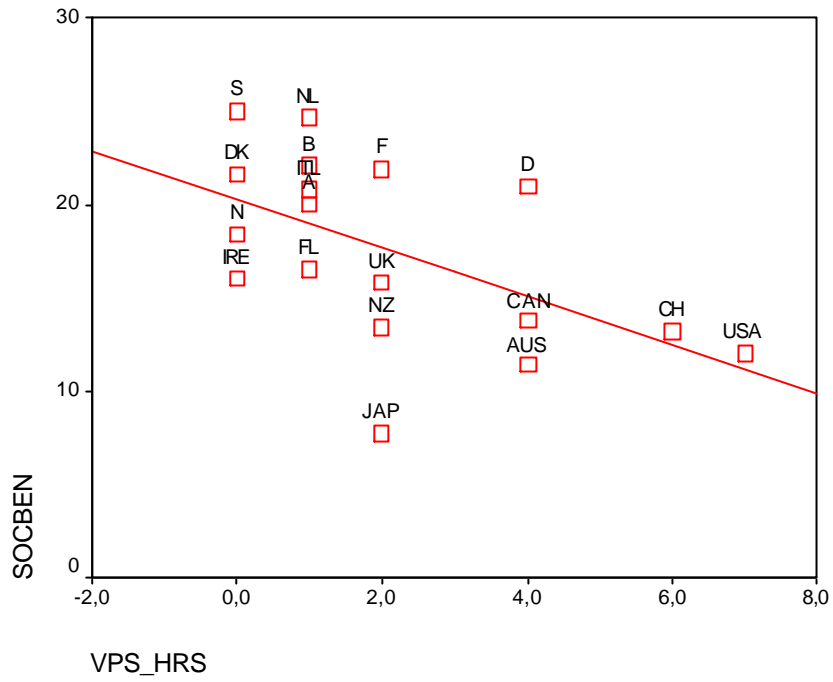


Figure 2

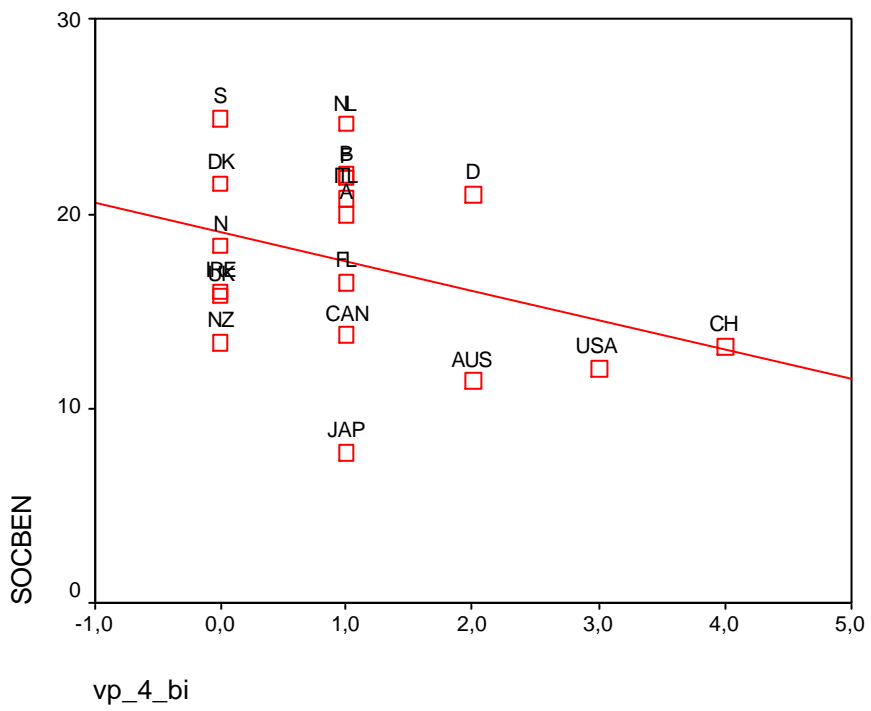
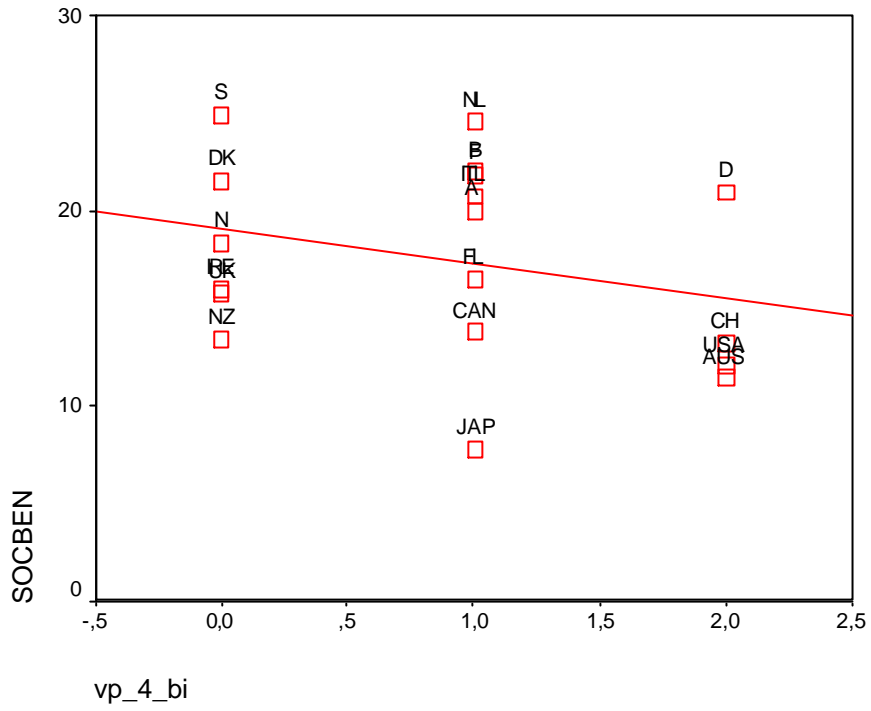


Figure 3



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