

# Can the number of veto players measure policy stability?

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## **Abstract**

Ever since the publication of George Tsebelis's "Veto Players", political scientists have constructed measures of policy stability on the basis of his theory's implications. In this research note, we indicate two possible mistakes in this use. The first mistake is the failure to distinguish between weak and strong implications of veto player theory. In the second mistake, scholars ignore information about the preferences of veto players in different settings. Focusing on the sheer number of veto players is particularly dangerous when constructing measures that are applied to draw inferences from cross-sectional comparisons. After explaining the nature of these mistakes using a stylized example, we provide evidence from two popular datasets proposing measures of policy stability that have been used in political science journal publications. We also develop a set of best practices for using the number of veto players as a measure of policy stability.

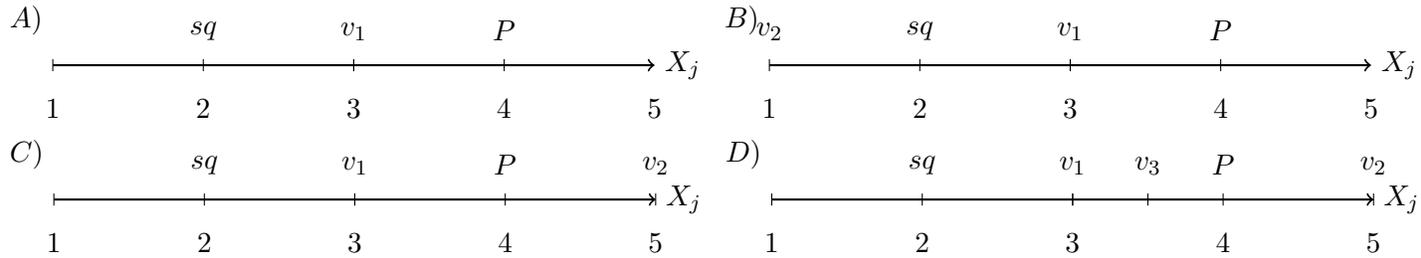
# 1 Introduction and example

According to George Tsebelis (2002)'s prominent book, every political system can be characterized by the number, preferences, and proposal power of its veto players. Consider, for simplicity, a uni-dimensional policy space. Given a status quo policy  $x$  and a set of veto players characterized by ideal points in this policy space, one can define a *winset*,  $W(x)$ , which is the intersection of the sets of policies that each veto player would prefer to the status quo,  $x$ . One can show that in order for a policy to be implemented over  $x$ , it must belong to  $W(x)$ . Because adding veto cannot expand, but can shrink, the winset, Tsebelis predicts that adding veto players to a political system will never decrease but can only increase or leave unchanged policy stability.

This allows for a more general, and arguably more useful, classification of political regimes than that into parliamentary, presidential, and semi-presidential. Unfortunately, most political scientists who apply this theory to measure political stability focus exclusively on the number of veto players, ignoring the other two characteristics of proposal power and preferences. This leads to inappropriate uses of the measure, particularly in cross-sectional settings. This article discusses the consequences of this omission using as examples the measures CHECKS and POLCON from two influential datasets: *The Database of Political Institutions* (Beck, Clarke, Groff, Keefer & Walsh 2001) and the *Political Constraints Index* (Henisz 2002).

We use a stylized example to motivate our argument. Consider part A of figure 1 below, showing the ideal point of a single veto player,  $v_1$ , alongside an exogenously given status quo,  $sq$ , and an exogenously given proposal,  $P$ . At the bottom of each panel, we have included cut points from 1 to 5 for ease of gauging spatial distance. If  $v_1$  is the only veto player in this setting, proposal  $P$ , equidistant from  $v_1$  and  $sq$ , will be accepted (as will any proposal between  $sq$  and  $P$ ). Consider next the addition of another veto player, whose ideal point is labeled with  $v_2$ , as represented in panel B of figure 1. Now proposal  $P$  would get rejected. In fact any alternative proposal to  $sq$  would be rejected. The addition of veto player  $v_2$  has increased policy stability in line with Tsebelis's theory. Suppose, however, that veto player  $v_2$  had been added not at position "1" but at position "5", as illustrated in example C. Now, the addition of veto player  $v_2$  has no effect on policy stability relative to example A. It is still the case that any proposal between  $sq$  and  $P$  will be accepted by these two veto players. Thus, whether or not adding veto players actually affects policy stability depends on

Figure 1: How veto players affect policy stability: stylized example



the preferences of veto players relative to the status quo and to those of other existing veto players in the system.

Another way to see the same point is to compare example C with example D. In C, there are just two veto players:  $v_1$  and  $v_2$ . Yet, C has just as much policy stability as does the system in example D, where there are three veto players. This aligns with Tsebelis's theory that the addition of veto players will not decrease but can only increase policy stability. However, many empirical works in political science cite the veto player theory when backing expectations that increasing the number of veto players ought to *strictly increase* policy stability. For the purpose of this note, we refer to this error as *mistake I*.

Note, further, that when Tsebelis talks about the addition or reduction of veto players, he has in mind the addition or subtraction of veto players only relative to the same structure. His theory does not imply that claims about policy stability can be made through the comparison of numbers of veto players in different preference structures. That systems with more veto players do not necessarily have more policy stability is illustrated by comparing example B to example D. While D has three veto players and B has two, D has less policy stability than B.

We will refer to this error of drawing inferences on policy stability from the cross-sectional comparison of the numbers of veto players alone as *mistake II*.

## 2 Use of Veto Points Theory in Empirical Political Science

15 years ago, Steffen Ganghof (2003) anticipated difficulties with the application of Tsebelis's theory, identifying three problems: the failure to identify the true veto players, to measure these players' preferences, and to identify different types of veto players. The first problem is associated with fo-

cusing on institutional veto players without accounting for informal veto players. As an example, consider a cabinet in a parliamentary regime that is a coalition made up of three parties. A measure of policy stability would need to count each member of the ruling coalition as a veto player. Furthermore, if one of the parties making up the ruling coalition were to be divided into factions, the measure would also have to reflect the number of these factions. Scholars also have to determine if other powerful actors such as courts are veto players. The second problem boils down to what we identify above as mistake I. Failure to account for veto player preference may lead scholars to predict a reduction in stability where Tsebelis's theory, noting that the additional veto player whose ideal point falls in between veto players with extrem ideal points does not impact the range of proposals preferred to the status quo, predicts no change. As an illustration, compare a pair of transitions in figure 1. The transition from A to B is a veto player addition that decreases policy stability, while the transition from C to D is a veto player addition that has no effect on policy stability. A scholar who fails to account for the difference between preferences represented by  $v_3$  from D relative to C and those represented by  $v_2$  from B relative to A commits mistake I, or the second pitfall Ganghof identifies. The third problem is arguably a problem with Tsebelis' theory itself, which does not distinguish between types of veto players. A constitutional court has the same status as does a leader of a party faction, even if the latter is completely informal and even if his veto intervention capacity can only be mobilized under special circumstances. Because both of the datasets (that use veto players to measure policy stability) discussed here include other variables allowing scholars to account for regime type, we do not elaborate on Ganghof's third error.

Despite Ganghof's early and significant contributions, many scholars have continued to measure policy stability using the number of veto players.

Yet, even if scholars correctly identify all political actors with formal or informal veto powers, adequately represent their preferences (so avoiding mistake I), and control for regime type, they may still make mistake II, a mistake even Ganghof failed to acknowledge. Avoiding all of the pitfalls Ganghof lists does not shield scholars from making unwarranted predictions based on cross-sectional comparisons of veto player number. Whether or not scholars use readily-available veto player data correctly or incorrectly depends on the specific statistical method they choose.

The remainder of this note will focus on the use of two measures from datasets popular in IPE and CPE. We identify how many articles use these measures correctly, and present data on how

often the use of these measures entails mistakes I and II. We also develop a set of best practices for the correct use of measures of policy stability using the concept of veto players.

## 2.1 CHECKS1 as a measure of policy stability

Beck et al. (2001) propose three policy stability measures—CHECK1, STABS, and STABNS—of which CHECK1 is predominantly used in the political economy literature. We will focus on CHECKS1 in this paper. The variable counts the number of veto players in a political system but tries to correct for preference alignment determined by three factors: (1) the number of parties competing in elections; (2) the electoral system; and (3) the party affiliation of the veto players. These adjustments are made by mechanically adding one to or subtracting one from the number of veto players.<sup>1</sup> While Tsebelis's theory only predicts that stability will not decrease with an increase in veto player number when veto player preferences are unaccounted for, this measure predicts a strict increase in policy stability in this scenario, thereby committing mistake I.

Mistake I will be committed whenever such an inference is drawn regardless of whether scholars are making cross-sectional *or* time series comparisons; if the inference is drawn on the basis of cross-sectional comparisons, scholars will additionally be committing mistake II. For example, suppose a scholar were to use CHECKS1 in a time series regression to show that increasing the number of veto players makes land reform less likely to occur in a given year. Further, suppose this scholar were to find that CHECKS1 has no significant effect in predicting the probability of land reform. Were this scholar claim their finding to be inconsistent with veto players' theory, they would be committing mistake I, as a lack of change in policy stability with the addition of a veto player is consistent with veto players' theory. However, were this scholar to pool the data cross-sectionally and run a regression, finding again no significant effect of CHECKS1, they would additionally be committing mistake II.

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<sup>1</sup>Specifically, in presidential systems, one is added for the president and for each chamber, except when the president's party is the largest legislative party in the closed list system. In parliamentary systems, one is added for each party in the governing coalition, including the prime minister's party, except for when the "prime minister's party is the largest in the legislature in a closed list system, in which case it is reduced by one."

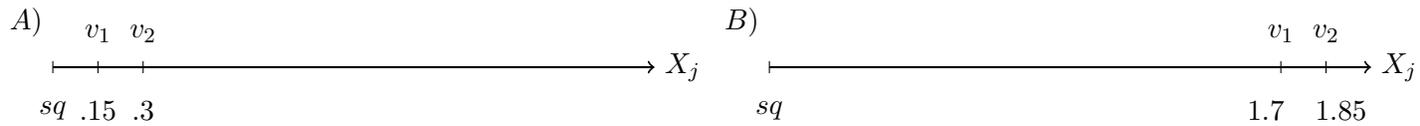
## 2.2 The Political Constraints Index

The second prominent dataset proposing a variable based on the number of veto players to measure political stability is the Political Constraints Index (Henisz 2002). In order to construct the variable (*POLCON*), Henisz first extracts from political science databases the number of independent branches of government (focusing on the executive and lower and upper legislative chambers, as these presumably have veto power over policy). Next, he assumes that the preferences of each of these branches are independent and identical, distributed over a unidimensional policy space. For instance, for just two veto players, this distance is  $\frac{1}{4}$  because  $\frac{1}{(n+2)} = \frac{1}{4}$  for  $n = 2$  (Henisz 2002, 381). The assumption that the preferences of each veto player are identical and independent is a strong one, especially in parliamentary regimes, where the executive is a direct outcome of legislative elections and must be aligned, at least to some extent, with the preferences of the legislative median party. Aware of this, in the next step, Henisz adjusts the measure to take into account the partisan affiliation of the executive, the size of the legislative majority, and the fragmentation of the legislature (Golder 2006). Since *POLCON* accounts for directionality (the side of the existing veto players to which additional player falls), we concede that the addition of veto players within the same country over time is measured well. As a result, it should work well for time series analysis within the same country and mistake II is avoided.

In other words, it is warranted to use this measure to claim that: “Countries are more likely to implement land reform as they become more authoritarian than as they become more democratic” (Albertus 2011), where becoming more authoritarian and more democratic are operationalized by a decrease and increase in the number of players (as measured by *POLCON*), respectively. It is not, however, warranted to claim that “countries with more veto players are more likely to implement land reform than countries with fewer veto players.”

An illustration of this common mistake from political science is provided by Mansfield, Milner & Pevehouse (2007). The authors of this article analyze country dyads who have the opportunity of entering a Preferential Trade Agreement (PTA), and find that the probability of a PTA forming declines with the number of veto players in each country. Consider three countries, corresponding to examples A, B, and D from Figure 1, and assume that  $P$  represents the spacial location of the PTA agreement. Veto players’ theory predicts that it would be easier to form a PTA between countries A

Figure 2: The assumption of the uniformly distributed status quo



and D than between countries A and B, but D has 3 veto players while B has only 2. Hence, holding constant the number of veto players in country A, the country with *more* veto players ought to be entering a PTA with higher probability than the country with *fewer* veto players, the opposite of what (Mansfield, Milner & Pevehouse 2007) expect to find.

Henisz also develops another measure, POLCON V, which follows the same logic as POLCON III but also accounts for two additional veto players: the judiciary and sub-federal entities. Though this measure considers potential veto players more thoroughly, it should also not be used for cross-sectional comparisons..

The following example in figure 2 illustrates why the measure cannot be used cross-sectionally. The distance between veto players in example A and in example B is  $.15$ ; from the point of view of POLCON, then, their structures are identical. If the status quo is to the left of both veto players (for instance, at  $0$ , where  $sq$  is located at  $0$  in A and B), the addition of veto players in example A is much more likely to occur to the right of both veto players, which is unlikely to affect policy stability (note that the intersection of winsets will be the interval  $[0, 1.5]$  regardless of the inclusion of additional veto players). However, in example B, the addition of veto players is much more likely to occur to the left of  $v_1$  and  $v_2$  and be consequential for stability: every addition to the left of the left-most veto player in example B will further shrink the winset. This is why the two structures, even though treated as identical by Henisz's measure, are not the same. In a nutshell, Henisz's measure accounts for relative distances between existing structures of veto players, but does not take into account their moderation or extremity. This is particularly relevant for uses of POLCON in comparative politics, where regime types ranging from inclusive democracy to harsh dictatorship are plausible interpretations of such divergent structures as shown in figure 2.

Before providing aggregate evidence of the uses and misuses of veto player-based measures of policy stability, we mention two other particularities of the Political Constraints Index. First, it ignores the judiciary as a distinct type of veto player. This is particularly significant for the work of

comparativists. Stalling decisions made by constitutional courts are just as consequential for policy stability as a presidential veto or the failure of an upper chamber to corroborate a lower chamber decision. However, in many contexts, constitutional courts cannot veto legislation unless explicitly requested to do so by a political actor and must justify any vetoes with constitutional provisions. Ignoring the specificities of courts decreases the reliability of the Political Constraints Index. A second problem is the failure to distinguish between presidential and parliamentary regimes. Even if the president is from the same party as a majority of the legislature, their separation of origin and survival can make them uneasy bedfellows (Samuels & Shugart 2010). For anecdotal evidence one need look no further than the struggles of Donald Trump to muster a Republican majority for repealing Obamacare. A prime minister leading the party holding a legislative majority no doubt has an easier time getting the support of his party's members in the legislature.

It should be noted that Henisz does not, in his article presenting the dataset, encourage scholars to apply it cross-sectionally; indeed, as previously mentioned, he takes care to account for veto player preferences, recreating the party composition of each legislature and adjusting alignment with the fractionalization index to reflect how far apart veto players are from one another. Yet, even leaving these nuances aside, the inconvenient conclusion of the paragraphs above is that although POLCON may be used for assessing the relative stability of the same unit over time, it cannot be used for making cross-sectional inferences.

This does not mean that any scholar working with panel data incorporating a time series is automatically "off the hook" for committing mistake II. In order to ensure that the effect of veto players is estimated separately for each country, scholars have to include fixed effects in their regressions. Implementing random effects or panel corrected standard errors takes care of heteroscedasticity issues but does not ensure the correct use of the number of veto players. The final section of this note presents the results of a meta-analysis of recently published political science articles citing either of the two databases discussed above in a use of veto player numbers in their empirical analysis, and classifies them into (1) papers that use the CHECKS1 measure and commit Mistake I, (2) papers that use CHECKS1 measure and do not commit mistake I, (3) papers that use the POLCON measure incorrectly and commit Mistake II, and (4) papers that use the POLCON measure correctly.

### 3 Meta-analysis of political science articles

Table A1 in the Appendix lists papers that used the CHECKS1 measure.<sup>2</sup> To see whether a paper committed Mistake I, we verify if the following 3 conditions are met: (1) CHECKS1 is used as a proxy for policy stability/flexibility or political constraints, (2) CHECKS1 turns out to be insignificant in empirical analysis, and (3) the author explicitly or implicitly points out that the insignificance of CHECKS1 means that veto players/policy flexibility/political constraints have no effect on the outcome variable of interest. All three conditions must be satisfied in order for mistake I to occur.

Among the 12 papers using CHECKS, only 2 clearly committed Mistake I. In the other 10 papers, the veto players measure was significant, rendering condition 2 inapplicable and immunizing the article to Mistake I.

Since the construction of the CHECKS measure does not account for veto player preferences, articles that used CHECKS as a measure for political constraints are still liable to commit Mistake II, even if they did not make Mistake I. Only Crespo-Tenorio, Jensen & Rosas (2014) do not make either mistake; their use of CHECKS to measure clarity of responsibility and not policy stability protects them from either pitfall.

Table A2 focuses on Mistake II, listing all articles using POLCON III that meet our selection criteria. Column “DV scale” indicates whether the dependent variable is dichotomous, continuous, categorical, or treated as if continuous. A DV is categorized “as if continuous”, if although discreet in nature, it can take up many values. Among articles listed in Table 2, 22 commit Mistake II while 24 avoid it. Those committing Mistake II typically either use a dichotomous or categorical dependent variable. Mistake II was unavoidable in articles that, due to data limitations (too short a time series), had to rely on cross-sectional comparisons.

No papers in our sample relied on a time series alone, though ten relied exclusively on cross-sectional comparisons. These papers, for obvious reasons, could not include fixed country effects. Yet, even many of the POLCON papers that analyzed panel data subjected themselves to mistake II by refraining from fixed effects. The avoidance of fixed effect analyses is most likely attributable to one of two reasons. First, if the dependent variable is not continuous and shows no variation in some countries, fixed effects would lead to throwing those countries out of the panel. Thus, papers

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<sup>2</sup>More details on the search results are provided in the Appendix.

that use dichotomous and categorical DVs may be especially unlikely to use fixed effects. Articles with continuous DVs may face a similar problem if their DVs vary little within the same countries. Second, and relatedly, panels with short time series but wide cross-sections will lose relatively many degrees of freedom when including a dummy for every country. Again, scholars worried about losing degrees of freedom may settle on random effects or panel corrected standard errors, both of which make them vulnerable to Mistake II. Since these considerations are important, we decided to include in Table A2 information about the DV as well as the length of the time series. Detailed explanations of why a particular paper was classified in a certain way are provided in the electronic appendix.

## 4 Ways forward

These results indicate that the incorrect use of veto players as a measure of policy stability is relatively common in empirical political science work. The mistakes are not universal or impossible to avoid, however. As a rule, scholars using panel data should include fixed effects in their regressions to ensure that countries with different veto player structures are not being compared to one another. If the length of a time series is too short to warrant such an operation, or if there is no variation in the dependent variable at the level of some countries, and scholars want to avoid throwing out these countries from the panel, they must look beyond veto player number to measure policy stability.

An alternative fix would use not the number of veto players, but rather the proportion of the policy space covered by the veto players whose ideal points are furthest away from one another—in other words, the convex hull of the veto player set. It is easy to show (as we do in the Appendix), that policy stability increases with the size of the convex hull in relation to the overall policy space. Therefore, measures that rely the convex hull could be used fruitfully for cross-country comparisons.

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