



The perils and pitfalls of ignoring disproportionality's behavioral components



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ABSTRACT

Scholars commonly use a measure of the discrepancy between party vote shares and seat shares (observed disproportionality) as a proxy for the effects of electoral institutions. We illustrate the problems with doing so and, instead, recommend that scholars use more direct measures of institutional characteristics. Conceptually, we demonstrate that observed disproportionality cannot accurately capture institutional effects. Empirically, we show that (1) the variance of disproportionality is much higher when electoral rules are restrictive than when they are permissive, (2) the conclusions we draw about the effects of observed disproportionality differ substantially across samples of elections, and (3) replacing measures of observed disproportionality with more direct measures of electoral system characteristics such as district magnitude produces different and more reliable results.

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1. Introduction

Electoral institutions vary in the ways in which they translate votes into legislative seats. It is common to think about these differences in terms of the proportionality of the vote-to-seat translation, which scholars then expect to affect a wide range of other outcomes of interest such as turnout (Jackman, 1987; Blais and Dobrzynska, 1998; Perez-Linan, 2001; Fornos et al., 2004; Henderson and McEwen, 2010), voting behavior (Carrubba and Timpone, 2005; Arzheimer, 2009), representational congruence (Blais and Bodet, 2006; Golder and Stramski, 2010), external efficacy (Davis, 2014) and policy outcomes (Bortolotti and Pinotti, 2008; Falcó-Gimeno and Jurado, 2011; Wigley and Akkoyunlu-Wigley, 2011). To examine the effects of an electoral system's (dis)proportionality on these other outcomes of interest, scholars commonly employ a measure of what we refer to as observed disproportionality: the discrepancy between party vote shares and party seat shares in any given election. Our aim is to demonstrate that such measures do a poor job of capturing the effects of electoral institutions and can lead scholars to draw faulty conclusions about the effects of electoral rules on other outcomes of interest. This is because any measure of observed disproportionality will

invariably reflect the degree of strategic behavior of voters and political parties and will, therefore, capture the effects of the reactions to electoral rules. This is especially problematic under restrictive electoral rules, such as single-member-district plurality (SMD-p) systems, which can produce very low levels of disproportionality, very high levels of disproportionality, or anything in between, depending on the behavior of voters and parties.

In this paper we investigate the usefulness of observed disproportionality as a measure of institutional effects. We begin with a conceptual discussion of disproportionality as it relates to electoral institutions, where we demonstrate that observed disproportionality does a poor job of capturing the concept(s) of disproportionality. We then illustrate the empirical problems associated with employing measures of observed disproportionality as measures of institutional effects. Specifically, we demonstrate that: (1) measures of observed disproportionality have an inherently heteroskedastic relationship with electoral institutions, and that the degree to which they reflect institutional characteristics is typically quite weak; (2) measures of observed disproportionality are strongly determined by behavioral factors, which means that the conclusions we draw about the effect of disproportionality can differ across different samples of elections; and (3) substituting measures of electoral system characteristics (e.g., district magnitude) for measures of observed disproportionality can lead to very different conclusions about institutional effects.

We encourage scholars to think carefully about whether they are interested in the effects of electoral institutions or in the observed level of disproportionality in any given election. Our read of the

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literature suggests that scholars are often concerned with the effects of electoral institutions, and in those cases we generally advise against the use of measures of observed disproportionality as stand-ins for the effects of electoral institutions. Instead, we recommend the use of more direct measures of electoral system characteristics that affect the proportionality of the vote-to-seat translation.

2. Conceptualizing disproportionality

Our survey of the relevant literature suggests that it is common for scholars to include a measure of observed disproportionality as an independent variable used to capture some sort of institutional effect (e.g., Anderson and Beramendi, 2002; Arzheimer and Carter, 2006; Arzheimer, 2009; Blais and Bodet, 2006; Bortolotti and Pinotti, 2008; Carrubba and Timpone, 2005; Crepez and Moser, 2004; Dahlberg, 2013; Ezrow, 2007; Jackman, 1987; Karp and Banducci, 2007; Kittilson, 2011; Kittilson and Schwindt-Bayer, 2010; Wigley and Akkoyunlu-Wigley, 2011).¹ It is usually difficult to discern the precise conceptual foundations of this measure because very few discuss the concept they seek to capture in any detail. In our read of the literature, when scholars ask about the relationship between disproportionality and other outcomes of interest they are often asking about the effects of the electoral institutions themselves: specifically, the mechanical effects of electoral institutions on the vote-to-seat translation. For instance, Bortolotti and Pinotti (2008) include a measure of observed disproportionality to capture the “barriers to entry imposed by different electoral systems (p. 339).” Carrubba and Timpone (2005) use disproportionality to measure “how disproportionately an electoral system translates votes into seats”, which they explicitly refer to as an “institutional control (p. 268).” In his study of support for radical right wing parties, Arzheimer (2009) refers to disproportionality as an “institutional feature” and states that “disproportionality is of particular interest because the existing research seems to disprove the common wisdom that less proportional systems help to ‘keep the rascals out’ (pp. 263–264).”² Anderson and Beramendi (2002) use a measure of observed disproportionality to capture “institutional differences” in the “disproportionality of the electoral system (p. 723).” And Dahlberg (2013) appears to view disproportionality and district magnitude (an institutional feature) as interchangeable, stating “the degree of proportionality or the size of the electoral districts often has dramatic consequences for how parties organize and compete (p. 673),” which is then measured using an index of observed disproportionality. Furthermore, disproportionality is sometimes employed as a control variable to capture institutional effects (e.g., Falcó-Gimeno and Jurado, 2011; Pacek et al., 2009; Whiteley, 2011), and in these cases there is typically even less attention paid to the conceptual underpinnings of the measure.³

¹ In addition, observed disproportionality is often included as an institutional component in Lijphart's index of majoritarian-consensus democracy (Lijphart, 1994; see also Anderson and Guillory, 1997; Tavits, 2004; Vatter et al., 2014).

² See also Arzheimer and Carter (2006).

³ In other instances, it is less clear whether scholars are referring to institutional effects or observed disproportionality. For example, in Jackman's (1987) analysis of turnout he includes a measure of observed disproportionality to capture “the degree of proportionality in the translation of votes into seats in the lower legislative house (p. 407)”, which could be interpreted as both the actual, observed disproportionality of the system or the disproportional nature of the electoral institutions themselves. Without further clarification of the concept at issue, it is unclear whether there is an institutional component to this definition of disproportionality. This lack of clarity is found throughout the literature on voter turnout (for a relevant discussion, see Blais and Aarts, 2006). In our view, both observed disproportionality and the institutional effects of electoral systems are likely to impact voter turnout.

While there is good reason to expect the proportionality of the vote-to-seat translation under a set of electoral institutions to affect a wide range of political outcomes, there is little reason to believe that observed disproportionality is a good measure of these effects. This is because electoral institutions have both mechanical effects on the vote-to-seat translation and also psychological effects on the vote distribution. Duverger (1954) coined the use of these terms in discussing the tendency of single-member-district plurality (SMD-p) systems to reduce the size of the party system to equal two. The mechanical effects of electoral systems occur as a result of the way in which votes are translated into seats. The psychological effects of electoral institutions occur when voters, potential candidates, and other political actors observe or anticipate the mechanical effects of electoral systems and adjust their behavior to support only viable political parties.⁴

Under SMD-p rules, only the candidate with the largest share of district votes will win a legislative seat, which means that many candidates and parties contesting the election inevitably fail to gain representation due to mechanical effects.⁵ As a consequence, voters who sincerely prefer a candidate with little chance of winning, but who are unwilling to waste their vote, will instead vote strategically for their most-preferred major party candidate. Candidates and small parties will either join with larger parties or avoid entering the contest if they are not electorally viable. According to Duvergerian logic, these strategic behaviors reduce the number of parties in a district to two: the number of viable contenders for one legislative seat (Cox, 1997).

Similar, but weaker, mechanical and psychological effects exist in proportional representation (PR) systems. Low district magnitudes, high electoral thresholds, and the electoral formula can all pose mechanical barriers to small parties entering the legislature and, as a consequence, create disproportionalities in the vote-to-seat translation (Taagepera and Shugart, 1989; Gallagher, 1992; Cox and Shugart, 1996; Cox, 1997; Benoit, 2000). Voters avoid voting for candidates and party lists that have little chance of entering the legislature, as well as for the obvious winners. Electorally nonviable candidates and parties – those that would not overcome the electoral threshold or do not expect to obtain any district seats – either refrain from contesting the election or coalesce with other parties (see, for example, Reed (1990) and Cox (1994) for the analysis of single non-transferable vote systems, Cox and Shugart (1996) for the analysis of strategic voting under proportional representation). Thus, one can think of the mechanical and psychological effects of electoral systems as a continuum, ranging from the highly disproportional and restrictive vote-to-seat translation of SMD-p systems to an almost perfectly proportional representation system, with most systems falling in between these two extremes.

When voters and candidates behave strategically, their observed behavior differs from the behavior that we would see if they acted sincerely. This means that the psychological effects of electoral systems can produce levels of observed disproportionality that deviate substantially from the mechanical disproportionality of the electoral rules. To aid our conceptual discussion we draw on two idealized electoral scenarios, which we present in Fig. 1. In the first

⁴ Although originally Duverger talked about the national party systems, recent studies analyzed the mechanical effects and psychological effects at the level of electoral districts (see Reed, 1990; Cox, 1997, 1999; Singer and Stephenson, 2009; Singer, 2013).

⁵ The winner-take-all nature of SMD-p also means that a candidate can win a seat even with a small lead in terms of votes, which creates some nonlinearity in the translation of major parties' vote shares into their seat shares at the national level. This effect is especially pronounced if there are only two major parties in the system and these parties are competitive throughout the country (e.g., Theil, 1970).

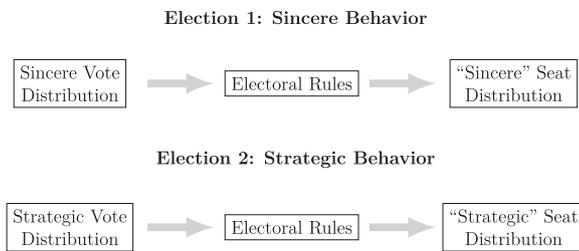


Fig. 1. The electoral sequence with sincere and strategic behavior.

scenario – Election 1, all political actors behave sincerely and support the party they prefer, producing what we refer to as a sincere vote distribution. The sincere vote distribution will not only reflect the sincere preferences of voters, but will also reflect the sincere distribution of political parties; in other words, the constellation of parties that would exist if electoral rules were perfectly proportional. If we then take this sincere vote distribution and translate the votes into seats using some set of electoral rules, we end up with what a sincere seat distribution would look like under a given set of party preferences and electoral rules. In the absence of any strategic behavior, the difference between the sincere vote distribution and the sincere seat distribution will capture the mechanical effects of the electoral rules in this particular election, which we could label “mechanical disproportionality.”

In our second electoral scenario – Election 2, we begin with the same sincere distribution of party preferences, but then assume that all political actors behave perfectly strategically and anticipate the mechanical effects of the electoral system. All potential candidates only join parties with a chance of winning representation. All voters support only parties with a viable chance of winning a legislative seat. In this scenario, we can expect there to be minimal differences between the strategic vote distribution and the strategic seat distribution it produces in this particular election, because all political actors have anticipated the mechanical effects of the electoral rules and adjusted their behavior accordingly. Thus, in our second, strategic electoral scenario, the mechanical effects of the electoral system have become virtually invisible. We can think about these psychological effects of electoral institutions as the difference between this strategic vote distribution and the sincere vote distribution in the first electoral scenario, which we might label “psychological disproportionality.”

We can combine these two scenarios to think about the full effect that electoral institutions have on the vote-to-seat translation. Considering both mechanical and psychological effects, we can conceptualize the total potential effects of electoral institutions as the difference between the sincere vote distribution of the first electoral scenario and the strategic seat distribution of the second electoral scenario. We might think of this difference as the total disproportionality produced by electoral institutions, since it reflects the difference between the counterfactual sincere vote distribution that would occur under perfectly proportional electoral rules and the strategic seat distribution that the electoral rules promote through both their mechanical and psychological effects.

Scholars have used a variety of measures of disproportionality in their empirical models, such as those developed by Rae (1967), Loosemore and Hanby (1971), Gallagher (1991), and Lijphart (1994). Although these measures are formulaically different, they all calculate the differences between party vote shares and party seat shares using observed electoral data. Thus, we refer to these as measures of observed disproportionality. Are these measures of observed disproportionality good measures of the mechanical and/

or psychological types of disproportionality described above? The clear answer here is no. The reference point for all three concepts of disproportionality – mechanical, psychological, and total – is the sincere vote distribution in a particular election, which can tell us how far off the election results are from what they would be under perfectly proportional electoral rules. Without a measure of this counterfactual vote distribution, it is impossible to know whether we are accurately measuring any of the three types of disproportionality.

So what will measures of observed disproportionality capture? In the complete absence of strategic behavior, observed disproportionality would equal mechanical disproportionality, and would therefore be a good measure of the mechanical effects of electoral institutions. However, this comes with a very important caveat. In a purely sincere electoral scenario, observed disproportionality would capture the mechanical effects of electoral institutions only for that particular set of sincere preferences. Under a different set of sincere preferences, the mechanical effects of the same set of electoral institutions could actually be quite different. Take, for example, elections held under SMD-p rules in two different countries. The first country is quite homogeneous and has a natural demand for only two political parties. In this case the mechanical effects of the SMD-p system will not be very visible, since sincere preferences will keep the number of parties low irrespective of the electoral institutions. The second country, however, is fraught with politically salient social divisions. Sincere preferences in this country call for a large number of political parties, but if support for all parties is roughly even across electoral districts the SMD-p rules will strongly discriminate against all but the largest of these parties. The mechanical effects of the SMD-p rules will be quite strong and apparent in this scenario. All of this is to say that, even if political behavior was perfectly sincere at all times and in all places, observed disproportionality would still not be a desirable measure of the mechanical effects of electoral institutions. The mechanical effects can differ across elections even when electoral institutions are held constant.⁶

The empirical reality is that political behavior rarely – if ever – conforms to our idealized “sincere” electoral scenario. At least some voters, parties, and other political actors are likely to be short-term instrumentally rational and to support political parties that have a chance at securing legislative representation. While this strategic behavior will reinforce the mechanical effects of electoral institutions and increase total disproportionality, it will also reduce the observed mismatch between votes and seats in any given election, thereby minimizing observed disproportionality.⁷ Thus, it is when the effects of electoral rules are at their strongest, affecting both the vote-to-seat translation and the behavior of political actors, that we will be least able to capture these effects by using measures of observed disproportionality.

We suspect that scholars often turn to measures of observed disproportionality because they appear to be handy summaries of the disproportionalities inherent in the vote-to-seat translation.⁸ As we have demonstrated, this is simply not the case. We cannot expect measures of observed disproportionality to accurately

⁶ We are thankful to Matt Golder for pressing us to think about this point.

⁷ The notion that the psychological effects of electoral systems minimize observed electoral disproportionality has been established in the literature (Taagepera and Shugart, 1989; Best, 2010), but has not always been recognized by scholars that use measures of observed disproportionality to stand in for the effects of electoral institutions.

⁸ Wigley and Akkoyunlu-Wigley (2011) explicitly say that “[t]he advantage of using [observed] electoral disproportionality is that it is more sensitive to the variety of factors, other than just electoral formula, that define each country’s electoral system (p. 599).”

capture the mechanical effects of institutions. Moreover, our discussion suggests that scholars interested in assessing the effects of electoral institutions cross-nationally should be wary of any measure based upon electoral returns, since these will reflect differences in both the degree of sincere or strategic behavior and the underlying demand for political parties. Instead, we recommend using measures of the electoral institutions themselves, such as district magnitude, the electoral threshold, and the electoral formula, which will not capture political behavior.

To be clear, observed disproportionality is sometimes more appropriately used as a dependent variable (e.g., Jones, 1993; Anckar, 1997; Barkan et al., 2006; Carey and Hix, 2011) or as an independent variable measuring voters' expectations about observed disproportionality (e.g., Wessels and Schmitt, 2008). There are also scholars who recognize the psychological effects captured by measures of observed disproportionality and the problems and limitations with using these measures to capture institutional effects (e.g., Blais and Aarts, 2006; Gallego et al., 2012; Grofman and Selb, 2011). However, it is also common for scholars to treat measures of observed disproportionality as measures of the mechanical effects of institutions, or recognize that these measures are only rough proxies for such effects and yet use them anyway. We have already outlined the conceptual problems with employing measures of observed disproportionality. In the following sections we highlight the potential empirical problems of doing so.

3. Empirical analysis of observed disproportionality

Following from the above discussion, it is clear that the mechanical effects of electoral institutions only determine the potential range of disproportionality we might observe in any given election. Exactly where observed disproportionality falls within this range will be determined by other factors, most notably the psychological effects of the electoral institutions.⁹ The potential range of observed disproportionality will be large under restrictive electoral rules, where variations in strategic behavior can produce very high disproportionality, very low disproportionality, or anything in between. In contrast, the degree of strategic behavior will matter much less under highly proportional electoral rules, where the vote-to-seat translation is capable of keeping observed disproportionality low irrespective of the number of electoral parties. What this means is that the relationship between institutional disproportionality and observed disproportionality is heteroskedastic in nature. Observed disproportionality can vary much more in SMD-p systems than it can in very proportional systems, with moderately proportional electoral rules falling somewhere in between. From an empirical perspective, this implies that the reliability of observed disproportionality as a measure of mechanical effects will be lower if the sample is dominated by observations of more restrictive systems.

To illustrate the empirical problems associated with measures of observed disproportionality, we draw on data from 26 old and new democracies over the post-WWII period (1945–2013) and calculate

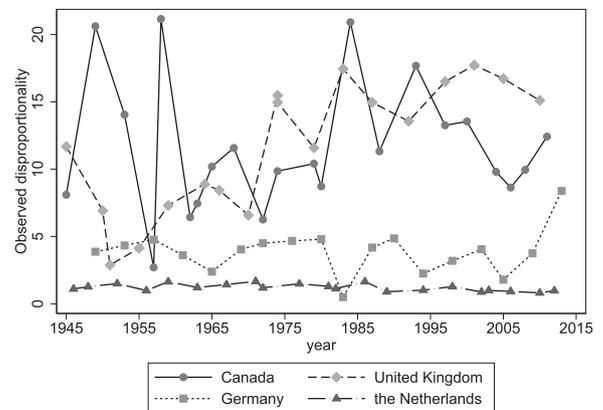


Fig. 2. Dynamics of observed disproportionality in four countries.

Gallagher's (1991) least squares index of disproportionality,

$$\sqrt{\frac{\sum (v_j - s_j)^2}{2}}$$

where v_j is the share of votes obtained by a party nationwide and s_j is the share of seats allocated to it. This index has become almost a standard among observed disproportionality indexes within contemporary literature.¹⁰

As a result of strategic behavior in SMD-p systems, it is possible for parties to receive roughly proportional shares of legislative seats despite the fact that the mechanical effects of the electoral rules are highly disproportional. For example, in the most recent elections to the U.S. House of Representatives (2012) the Republicans managed to secure a majority of seats even though the Democrats received a plurality of votes (certainly not the most proportional outcome on record). Nonetheless, the level of disproportionality in this election was 5.0 according to Gallagher's (1991) measure, which is substantially lower than the level of disproportionality observed in the 2010 United Kingdom election (15.1) where the same SMD-p electoral system is used. It is also lower than the level of disproportionality observed in the 2013 German election (8.3) and in the 2010 Hungarian election (11.7), both of which were held under mixed-member-proportional (MMP) electoral rules that impose weaker restrictions on small parties.

Fig. 2 shows the variation in the observed disproportionality in four countries: Canada, the United Kingdom, Germany and the Netherlands. Among them, Canada and the United Kingdom use SMD-p systems, while Germany holds elections under a Mixed-Member Proportional system, and the Netherlands uses a single-district proportional representation system. Notice the spread in the values of observed disproportionality under SMD-p: after the Second World War, observed disproportionality ranged between 2.7 and 21.1 in Canada and between 2.9 and 17.7 in the United Kingdom. The variance of observed disproportionality among the

⁹ Importantly, the distribution of the vote across geographic districts can also produce variation in observed disproportionality within a given set of electoral rules. Since our purpose is not to provide a full and complete model of observed disproportionality, but simply to argue that measures of observed disproportionality are inappropriately used as measures of the mechanical effects of electoral institutions, we find it convenient to focus on the psychological effects of electoral institutions that are captured by measures of observed disproportionality. However, in the empirical sections of the paper we talk about the variance in disproportionality that is unexplained by institutions as a result of "behavioral" factors. We acknowledge that there are other behavioral factors at work here, such as shifts in the geographic distribution of the vote or changes in sincere preferences.

¹⁰ Our set of countries includes Australia, Austria, Belgium, Bulgaria, Canada, Croatia, Czech Republic, Denmark, Estonia, Finland, France, Germany, Greece, Hungary, Iceland, Ireland, Israel, Italy, Japan, Latvia, Lithuania, Luxembourg, Malta, the Netherlands, New Zealand, Norway, Poland, Portugal, Romania, Slovakia, Slovenia, Spain, Sweden, Switzerland, and the United Kingdom. Although we rely exclusively on Gallagher's least squares index in our empirical analyses, our discussion of the problems associated with using measures of observed disproportionality to capture institutional effects applies to all disproportionality measures that are based upon election results — i.e. discrepancies between votes and seats. Replication files for our analyses can be found at <http://andreizhirnov.weebly.com/replication-materials.html>.

Canadian and British elections is about 320 times as large as the variance of disproportionality in the Netherlands and about nine times as large as the variance in German elections. Despite the difference in variances, the lower boundary of the observed disproportionality is close in all four cases: it is 2.7 in Canada, 2.8 in the UK, 0.5 in Germany, and 0.8 in the Netherlands. Since the electoral rules in these countries are constant under the time period examined, this observation tells us that observed disproportionality in Canada and the United Kingdom is more sensitive to the fluctuations in behavioral patterns.

The above discussion leads us to expect a heteroskedastic relationship between electoral institutions and disproportionality, with restrictive electoral institutions having a higher variance in observed disproportionality than permissive electoral institutions. While Fig. 2 provides preliminary support for this expectation, we undertake a fuller investigation of the relationship between three institutional variables and observed disproportionality. Our institutional variables include the average district magnitude, the presence of a decisive and compensatory upper tier, and the legal electoral threshold.¹¹ To compute the average district magnitude of the decisive tier, we follow the logic of Lijphart (1994) and Gallagher and Mitchell (2005). When there is only one electoral tier, average district magnitude equals legislative size divided by the number of districts. For adjustment-seats PR and mixed-member compensatory systems, we treat the upper tier as decisive if its size exceeds $1/(2 * M + 1)$, where M is the magnitude of the lower tier (Gallagher and Mitchell, 2005). We also treat the upper tier in remainder-transfer systems as decisive if it is used to adjust the distribution of all seats (both those already allocated in lower tier districts and those that are left to be allocated by the upper tier) to the distribution of all votes (those “used” in the allocation of seats in the lower tier and those “unused”) and the size of upper tier satisfies Gallagher and Mitchell’s (2005) numeric criterion. The average district magnitude is then calculated as legislative size divided by the number of districts in the decisive tier. We follow convention in the literature by taking the log of this variable to capture the decreasing marginal effect of district magnitude.

We record an electoral system as having a compensatory and decisive upper tier if the upper tier was determined to be the decisive tier by the criteria outlined above, i.e. the electoral system was a mixed-member compensatory system, a proportional representation system that allocates a fixed number of adjustment seats, or a remainder-transfer proportional representation system where unallocated seats are used to adjust the total distribution of seats to match the distribution of all votes, and the size of the upper tier satisfied Gallagher and Mitchell’s (2005) numeric criterion.

The legal threshold is the formally specified vote share that determines whether parties gain entry to the legislature. Such a threshold can be specified either as a share of the valid votes cast nationwide or as a share of valid votes cast in an electoral district. In order to make district-level thresholds comparable to national thresholds, we recalculated the thresholds defined in terms of the district valid votes to reflect the share of valid votes cast nationwide that a party needs in order to participate in the division of seats.¹²

We plot observed disproportionality against each of three institutional variables in Figs. 3–5: average (decisive) district magnitude, the national legal threshold for representation, and the

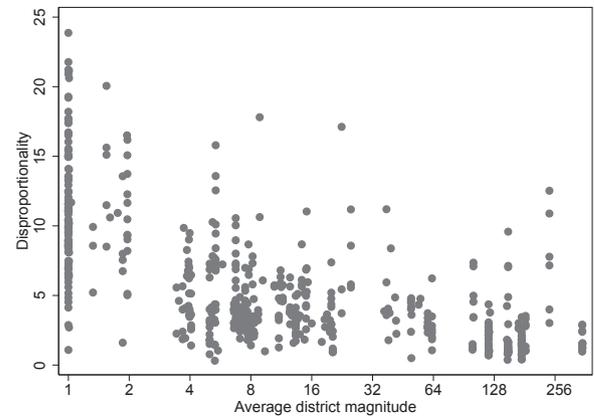


Fig. 3. Disproportionality and average district magnitude (decisive tier).

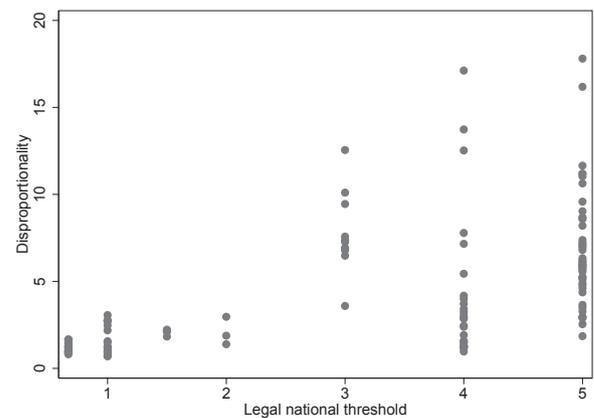


Fig. 4. Disproportionality and national legal threshold.

presence of a decisive and compensatory upper tier. All three figures demonstrate that the variance of disproportionality grows with the permissiveness of the electoral system. In Fig. 3, we see that countries with a low average district magnitude have, on average, greater variance in observed disproportionality, while countries with larger district magnitudes have a more compressed range of observed disproportionality. Similarly, in Fig. 4 we see that countries with higher legal thresholds have a higher range of disproportionality than countries with lower legal thresholds. In both figures the more restrictive electoral institutions are associated with greater variance in observed disproportionality.

Fig. 5 shows the distribution of observed disproportionality in electoral systems with and without a decisive upper tier. The former, presumably, adjusts the distribution of seats in the legislature to more proportionately reflect the distribution of votes across parties. From Fig. 5, we can see the variance in observed disproportionality is lower in countries that have a compensatory upper tier and higher in countries that do not, again following our expectation that the variance will be higher under more restrictive electoral rules.

We can infer from the figures that there are significant variations in the degree of strategic behavior among voters and parties that translate into variations in observed disproportionality. In single-member-district plurality systems, disproportionality ranges from about 1 to well over 20. Since the electoral rules are constant, this variance must be due to behavioral factors. We can generally expect the level of observed disproportionality in an election to respond to factors that affect the willingness or ability for political

¹¹ These three variables are major features of electoral systems that are known to affect the disproportionality of the vote-to-seat translation. In the Appendix, we also examine the effects of the electoral formula on observed disproportionality.

¹² Specifically, district-level thresholds are multiplied by the number of valid votes cast in the district with largest district magnitude and divided by the number of valid votes cast nationwide.

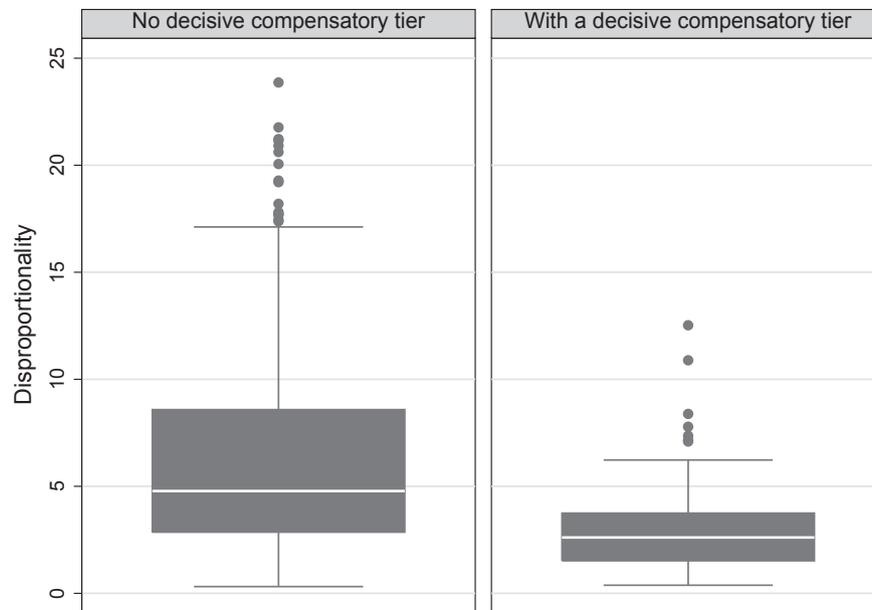


Fig. 5. Disproportionality and compensatory tiers.

actors to behave strategically. As an illustration, let us focus on the conditional effect of one of such factor: democratic experience. It is typically argued that voters and candidates in new democracies have not had time to get accustomed to the operation of democratic institutions in general and electoral rules specifically. As a consequence of having less information about electoral rules and their effects on the viability of political parties and a less institutionalized party system, we can expect strategic coordination around the “right” number of political parties to be lower in new democracies than in old democracies. This means that, on average, observed disproportionality should be higher in new democracies compared to old democracies, even when electoral rules are similar.

As a first step toward evaluating this expectation, we again plot observed disproportionality against average district magnitude in Fig. 6, this time splitting our sample between old and new democracies. As expected, observed disproportionality is generally higher among new democracies than old democracies at the same values of district magnitude. Democratic experience generally seems to lead to lower levels of disproportionality, irrespective of electoral institutions. It should be also noted that the effect of district magnitude on observed disproportionality in new democracies appears to be much weaker than it is in old democracies.

To get a more precise estimate of the relationship between electoral institutions and disproportionality, we estimated several models of observed disproportionality that we present in Table 1. In Model 1 we present the results of a simple linear regression where disproportionality is modeled as a function of the average (decisive) district magnitude and the legal threshold.¹³ Model 2 adds dummy

variables for electoral formula to this specification (the complete set of results for models with dummy variables for electoral formula, can be found in Table A.5 in the Appendix). In Models 1 and 2, both institutional variables have statistically significant effects on observed disproportionality, with more permissive institutions (i.e. a higher district magnitude and lower threshold) producing lower levels of disproportionality and more restrictive institutions (i.e. a lower district magnitude and higher threshold) producing higher levels of disproportionality. These are the relationships between electoral institutions and observed disproportionality that are commonly expected in the literature. However, these two institutional features together account for only 44 percent of the variation in observed disproportionality. When we add dummy variables for electoral formula the R-squared increases to 0.60, which means that we still have not explained 40 percent of the variation in observed disproportionality.¹⁴ A sizable portion of the variation in disproportionality cannot be explained by these electoral institutions. Following from our argument presented above, we suspect that most of this remaining variation is due to behavioral factors.

In Models 3–4, we estimate linear regressions with multiplicative heteroskedasticity, which allow each institutional variable to affect both the expected mean level of observed disproportionality and its variance. We expect to see that more permissive and proportional electoral rules – those featuring higher average district magnitude and lower thresholds – will lead to lower average levels of observed disproportionality, but, at the same time, will also reduce the variance of observed disproportionality. The results of Models 3–4 suggest that the average district magnitude and the legal threshold have a significant effect on the variance of observed disproportionality in addition to the mean, although the statistical significance of this effect varies across model specifications. Wald

¹³ As a robustness check, we also estimated our models using the specification employed in Cox (1997), Amorim Neto and Cox (1997) and Golder (2003), which includes both the average magnitude of the lower-tier districts and the size of upper tier in the analysis. In addition, we also calculated the “effective district magnitude” discussed by Taagepera and Shugart (1989, 117) and included it in our models as our measure of district magnitude. Neither alternative specification changed the inferences that we drew from our analysis. The reader can find the estimates with the alternative specifications of the district magnitude in Tables A.3 and A.4 in the Appendix. These include the earlier described method borrowed from Cox (1997), Amorim Neto and Cox (1997) and Golder (2003), as well as Taagepera and Shugart’s (1989) “effective” district magnitude.

¹⁴ We suspect that this may be an overestimate of the percent variance explained, since some of the electoral formulas are used so infrequently that their dummy variables likely capture country-specific effects. This is especially the case regarding the single-member-district run-off system used in France. We also note that the R-squared is slightly lower in the models presented in Table A.3, where we use two alternative measures of district magnitude.

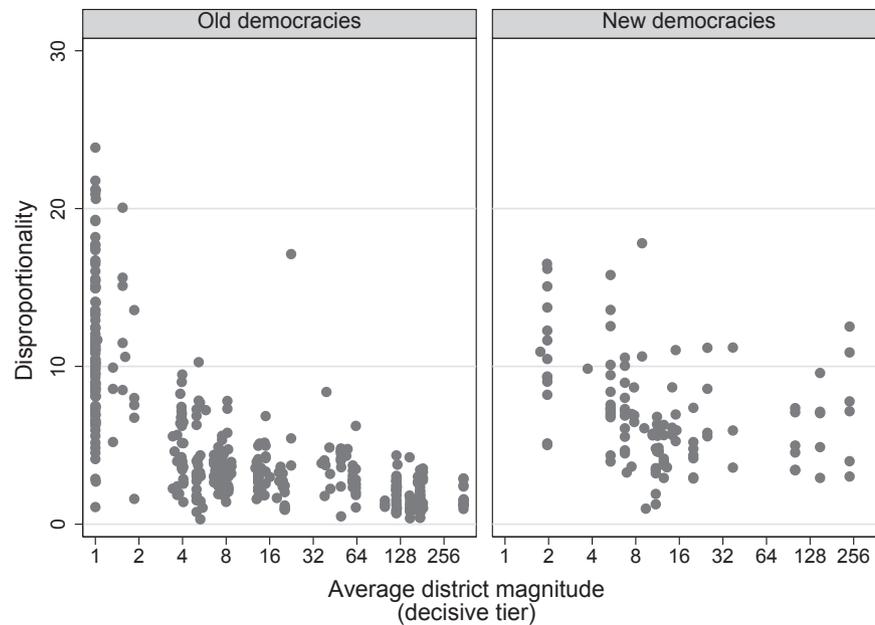


Fig. 6. Disproportionality and average district magnitude in old and new democracies.

Table 1
Institutions and observed disproportionality.

	Full sample					Old democracies	New democracies
	1	2 ^a	3	4 ^a	5	6	7
Estimated effect on mean							
(ln)District magnitude (decisive tier)	−1.73** (0.09)	−0.97** (0.11)	−1.27** (0.08)	−0.77** (0.09)	−1.70** (0.10)	−1.87** (0.10)	−0.87** (0.28)
Legal threshold	0.55** (0.09)	0.56** (0.10)	0.51** (0.09)	0.32** (0.08)	0.38** (0.11)	0.75** (0.16)	0.12 (0.14)
New democracy					1.19** (0.42)		
Constant	9.35** (0.32)	11.65** (0.76)	7.96** (0.28)	11.65** (0.75)	9.15** (0.35)	9.44** (0.35)	8.87** (0.80)
Estimated effect on ln(variance)							
(ln)District magnitude (decisive tier)			−0.44** (0.12)	−0.23 (0.14)			
Legal threshold			0.17 (0.09)	0.28** (0.09)			
Constant			3.09** (0.20)	3.11** (0.24)			
Observations	536	536	536	536	536	422	114
R ² /VWLS R ²	0.44	0.60	0.39	0.53	0.45	0.50	0.10

Notes: Dependent variable is Gallagher's least squares index of disproportionality.

Models 1–2, 5–7 are linear regressions; models 3–4 are linear regressions with multiplicative heteroskedasticity.

Robust standard errors are in parentheses, ** $p \leq 0.05$ (two-tail test).

^a Dummies for electoral formula were included in the model, but are omitted from the table. See Table A.5 in the Appendix for the estimates.

tests for the dependence of the model variance confirm this result: they reject null hypotheses for the log of the average district magnitude ($\chi^2 = 13.6$ with 1 d.f. in Model 3; $\chi^2 = 2.9$ in Model 4) and legal threshold ($\chi^2 = 3.4$ with 1 d.f. in Model 3; $\chi^2 = 10.6$ in Model 4). Furthermore, the signs on the coefficients in the variance component of the models suggest that more restrictive electoral institutions are associated with a higher variance in observed disproportionality, with lower district magnitudes and higher thresholds being associated with a higher variance in observed disproportionality.¹⁵

It is instructive to compare the estimated effects of the district magnitude on the mean of the observed disproportionality in the models with and without the variance parameterization (e.g., Model 1 against Model 3). The direction and the significance of the

effects of the district magnitude on the mean stays intact, while the magnitude of the predicted effect changes. The influence of average district magnitude is attenuated if we take into account the effect of these variables on the variance. For example, a one-unit increase in the log of average district magnitude leads to a decrease of 1.7 in disproportionality if we look only at the mean of disproportionality (Model 1), but only a 1.3 decrease if we take into account the difference in variance (Model 3). Likewise, the effect of the legal thresholds is attenuated if we take into account the variance component.

With Models 5–7, we illustrate how non-institutional factors can influence observed disproportionality by looking at the effect of democratic experience. First, we include a dummy variable marking the new democracies; second, we split our sample into the subsamples of old and new democracies. According to the estimates, disproportionality in new democracies is about 1.2 percentage points higher in new democracies than in old democracies. Moreover, the effect of institutions appears to be weaker in new

¹⁵ These effects are more apparent in the models that use different specifications of the district magnitude (see Table A.3 in the Appendix).

democracies, similar what we observed in Fig. 6. For instance, the estimated effect of the log of the average district magnitude in the decisive tier is noticeably weaker (but still significant), while the estimated effect of the legal thresholds loses its significance. Similar conclusions can be drawn from the comparison of the estimated effects of lower tier district magnitudes and the effective district magnitudes. This suggests that observed disproportionality will be lower in countries where voters and parties have had enough time and/or information to coordinate around a set of viable parties and higher in newer democracies where the degree of coordination around a set of viable parties is lower.

In this section we have demonstrated that: (1) there is a good degree of variation in observed disproportionality that cannot be attributed to electoral institutions; (2) there is a heteroskedastic relationship between electoral institutions and observed disproportionality; and (3) democratic experience, which we use as a proxy for the degree of electoral coordination, conditions the effect of electoral institutions on observed disproportionality. This means that we should be skeptical of any conclusions drawn about the relationship between electoral institutions and other outcomes of interest when institutions are measured by observed disproportionality, since the findings will likely vary across different samples of elections.

4. Replication

In this section we demonstrate the implications of our arguments for an actual empirical study by replicating Blais and Bodet's (2006) analyses of observed disproportionality and several dependent variables: the diversity of legislative party positions, the mean party distance from the median voter, the number of parties in government, and government distance from the median voter. According to the institutional argument made by Blais and Bodet (2006), under proportional representation parties have incentives to distance themselves from other parties and, as a result, from the median voter. Although each individual party is expected to be more remote from the median voter in PR systems, such parties tend to form more diverse governments that are, on average, as close to the median voter as those operating under SMD-p systems. The authors proposed the following hypotheses about the influence of electoral institutions:

- “1. PR produces less centrist parties and thus greater diversity.
2. PR increases the number of parties in government.
- < ... >
5. PR has no net effect on the distance (congruence) between the government and voters.” (Blais and Bodet, 2006, 1246-7)

They then estimated a series of ordinary least squares models using data from the Comparative Studies of Electoral Systems (CSES). Four of these models investigated the three hypotheses presented above by using Gallagher's Least Squares Index of disproportionality as their measure of electoral system proportionality.¹⁶ We replicate these regressions in the first row of Table 2.¹⁷ The original analysis, as well as its replication on the original sample, confirmed all four hypotheses. The authors found a negative relationship between observed disproportionality and the diversity of party positions in the legislature, the mean party distance from the median voter, and

the number of parties in the government. They did not find any relationship between disproportionality and government distance from the median voter.

The original analysis used data from the first two waves of the Comparative Study of Electoral Systems (CSES). The discussion above suggests that the sample of elections may influence the conclusions drawn about the effect of disproportionality on other outcomes of interest. To investigate this possibility, we estimated the same models using data from the third wave of the CSES (presented in the second row of Table 2).¹⁸ If observed disproportionality accurately captured the effects of electoral institutions, different samples of elections should retain the effect. However, we find important differences between the authors' original estimates and ours that use the third wave of the CSES. For the third wave of the CSES, the estimated coefficients in the “diversity” and “mean party distance” regressions become statistically insignificant.¹⁹

As discussed above, democratic experience is likely to shape the degree of strategic behavior, which means we are likely to find differences in the effect of observed disproportionality across old and new democracies. We also estimated separate analyses for new and established democracies, the results of which are also presented in Table A.3. With respect to established democracies, our findings for three of the dependent variables – “diversity”, “mean party distance”, and “number of parties” – largely follow the patterns found in the original analysis. Regarding the fourth dependent variable – “government distance” – the estimated coefficient on observed disproportionality is negative and significant, which runs counter to Blais and Bodet's (2006) expectations: the theory suggests that we should not find any relationship.

The results of our analysis of new democracies are strikingly different from the original estimates. In new democracies, we observe positive and statistically significant relationships between observed disproportionality and three of the dependent variables: “diversity”, “mean party distance”, and “government distance”. While the coefficient on observed disproportionality in the number of parties regression is also positive, it fails to reach conventional levels of statistical significance. Should we base our conclusions solely on this subsample, we would have to reject all three directional hypotheses and could not claim that disproportionality has nil effect on government distance. In fact, our conclusions about the effects of observed disproportionality in new democracies are almost precisely the opposite of our conclusions about the effects of observed disproportionality in old democracies.

The differences here are important. Depending on the sample we use, we draw varying conclusions about the relationship between observed disproportionality (or, by implication, electoral institutions) and our dependent variables. Due to variations in behavior, no measure of observed disproportionality will be a reliable measure of institutional effects. Thus, we recommend the use of direct measures of electoral institutions when scholars are interested in capturing institutional effects. As an illustration, we substitute average district magnitude in place of observed disproportionality in the last row of Table 2.²⁰ In both statistical and substantive terms, the only dependent variable for which we find the same effect of observed disproportionality (in the original estimates) and district magnitude is the number of parties in

¹⁶ The diversity of legislative party positions and the mean party distance from the median voter were both used as dependent variables to test whether disproportionality produced greater diversity and less centrist parties, respectively.

¹⁷ For simplicity, we present only the coefficients for disproportionality. All models were also estimated with a dummy variable for “old democracy.”

¹⁸ We excluded presidential republics.

¹⁹ We find results quite similar to the original estimates when we pool all three waves of the CSES.

²⁰ The full set of analyses with district magnitude as the independent variable can be found in Table A.6 in the Appendix.

Table 2
A re-estimation of models from [Blais and Bodet \(2006\)](#).

	Dependent variable			
	Diversity of legislative party positions	Mean party distance from median voter position	Number of parties in government	Government distance from median voter
Effect of disproportionality				
Original sample <i>N</i> = 31	–1.55** (0.72)	–6.52** (1.58)	–13.13** (4.65)	–3.07 (2.24)
Third wave CSES <i>N</i> = 48	–0.84 (0.96)	–4.54 (2.79)	–11.52** (4.49)	–1.05 (2.15)
New democracies <i>N</i> = 28	3.93** (1.51)	11.15 (5.70)	8.16 (9.73)	8.65** (3.77)
Old democracies <i>N</i> = 51	–1.84** (0.43)	–7.93** (0.87)	–15.49** (3.56)	–3.49** (1.29)

Notes: Table entries are OLS coefficients of the effect of Gallagher's least squares index of disproportionality and the logged average district magnitude. Estimates of the effect of the old democracies dummy and the constant were omitted from the table. Cluster-robust standard errors are in parentheses; ** $p \leq 0.05$ (two-tail test).

government, where higher district magnitudes (i.e. more proportional institutions) lead to greater numbers of parties in government. We do not find any significant effects for the other three dependent variables.

5. Conclusion

Scholars have often used measures of observed disproportionality to capture the mechanical effects of electoral institutions. As we have demonstrated, observed disproportionality also captures the reactions of political actors to the proportionality of the vote-to-seat translation, making it a poor measure of these institutional effects. The level of disproportionality we observe in any given election is an outcome of the electoral process that signals the willingness or ability of parties and voters to coordinate around a set of viable political parties. In addition, the potential range of observed disproportionality is much higher in restrictive electoral systems than permissive electoral systems.

Empirically, the situation is not any better. Although indexes of observed disproportionality consume fewer degrees of freedom than a battery of actual institutional variables, their validity is questionable at best. These measures reflect both observable and unobservable behavioral characteristics and do so in ways that make it hard to disentangle institutional and non-institutional characteristics during estimation. Furthermore, the conclusions drawn about the effect of observed disproportionality can differ quite dramatically across different samples of elections. As we have shown, relying on such measures can lead researchers to draw faulty conclusions about the relationship between electoral institutions and other outcomes of interest. When scholars are interested in examining the effects of electoral institutions, our general recommendation is to include measures of the electoral institutions themselves (e.g., district magnitude) rather than to rely on measures of observed disproportionality. Data on electoral system characteristics has become widely available and easy to include in cross-national analyses.²¹

If institutional data are unavailable, or if other reasons prevent the use of institutional variables, we recommend proceeding with caution with respect to using measures of observed disproportionality. Firstly, our analyses demonstrate that observed disproportionality will differ across elections when there are differences in the degree of strategic behavior. The more strategic behavior varies across a set of elections or countries, the more any

measure of observed disproportionality will vary as a result of behavioral – rather than institutional – factors. Researchers should seriously consider the likely extent of behavioral variation as well as how this variation will obfuscate the institutional effects of interest. For example, we have shown here that there is a substantial amount of variation in strategic behavior between new and old democracies. Thus, scholars should be especially wary of using measures of observed disproportionality when their sample of countries is diverse, or in any case where we might expect substantial differences in the degree of strategic behavior. Secondly, we encourage scholars to be thoughtful when interpreting findings based on a measure of observed disproportionality and recognize that the observed level of disproportionality in an election may vary even when electoral rules are constant. If exploring the effects of disproportionality on individual behavior, for example, these effects should be explained in terms of the level of observed disproportionality in each election, rather than as a result of electoral institutions.

We can imagine some instances where scholars may be theoretically interested in the effects of observed disproportionality. For example, the disproportionality of the votes-to-seats translation observed in a given country in past elections might be a good proxy for the disproportionality observed in future elections, and hence – it may affect elite calculations about whether or not to change the electoral rules and/or voters' perception of the marginal value of their vote. In these circumstances scholars should surely include a measure of observed disproportionality in their models, since this variable is a theoretically motivated and accurate measure of the concept at hand. As a first step, we encourage scholars to think carefully about whether it is observed disproportionality or, rather, the effects of electoral institutions that they intend to measure. Generally, the disproportionality we observe in any given election can be viewed as an outcome of the interaction between political behavior and electoral institutions. Thus, measures of observed disproportionality may be best regarded as political outcomes that deserve explanation (i.e. dependent variables), rather than proxies for institutional effects.

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²¹ See, for example, [Bormann and Golder \(2013\)](#).

Appendix

Table A.1
Summary statistics for the sample of estimation.

Variable	Mean	Std. Dev.	Min	Max
Disproportionality	5.604	4.432	0.314	23.866
Average district magnitude (d) (decisive tier)	45.123	75.210	1	350
Average district magnitude (l) (lower tier)	17.76	36.05	1	150
Size of the upper tier	0.119	0.212	0	1
Effective district magnitude	27.014	46.389	1	
Legal threshold	0.772	1.61	0	5
New democracy	0.213	0.41	0	1

Notes: $N = 536$. The sample covers democratic elections in Australia, Austria, Belgium, Bulgaria, Canada, Croatia, Czech Republic, Denmark, Estonia, Finland, France, Germany, Greece, Hungary, Iceland, Ireland, Israel, Italy, Japan, Latvia, Lithuania, Luxembourg, Malta, the Netherlands, New Zealand, Norway, Poland, Portugal, Romania, Slovakia, Slovenia, Spain, Sweden, Switzerland, United Kingdom between 1945 and 2013. The number of observations per group by electoral formula is as follows: plurality rule (40), majority with run-off (32), alternative vote (27), LR-Hare (65), LR-Droop (12), d'Hondt divisor method (together with the Hagenbach-Bischoff method) (223), Sainte-Lague divisor method (classical and modified) (50), SNTV (18), STV (36), mixed-member majoritarian (19) and reinforced PR (14) systems. Systems with a hierarchy of tiers are classified by the uppermost tier.

Table A.2
Pairwise correlation of variables in the sample of estimation.

	1	2	3	4	5	6	7
1. Disproportionality	1.000						
2. (ln) Average district magnitude (decisive tier)	-0.632	1.000					
3. (ln) Average district magnitude (lower tier)	-0.566	0.697	1.000				
4. (ln) Effective district magnitude	-0.657	0.923	0.619	1.000			
5. Size of the upper tier	-0.013	0.314	-0.217	0.330	1.000		
6. Legal threshold	0.009	0.280	0.302	0.053	0.126	1.000	
7. New democracy	0.160	0.037	0.113	-0.004	0.340	0.548	1.000

Table A.3
Institutions and observed disproportionality.

	1a	1b	2a ^a	2b ^a	3a	3b	4a ^a	4b ^a
<i>Estimated effect on mean</i>								
(ln) District magnitude (lower tier)	-2.15** (0.13)		-0.95** (0.14)		-1.31** (0.06)		-0.98** (0.08)	
Size of upper tier	-3.96** (1.00)		-0.59 (1.07)		-2.56** (1.08)		-0.63 (0.96)	
(ln) Effective district magnitude		-1.92** (0.11)		-1.18** (0.16)		-1.32** (0.10)		-0.78** (0.12)
Legal threshold	0.58** (0.10)		0.54** (0.11)		0.85** (0.13)		0.46** (0.10)	
Constant	9.45** (0.36)	9.76** (0.33)	11.65** (0.76)	11.65** (0.76)	7.00** (0.24)	8.16** (0.31)	11.65** (0.75)	11.65** (0.75)
<i>Estimated effect on ln(variance)</i>								
(ln) District magnitude (lower tier)					-0.96** (0.07)		-0.74** (0.13)	
Size of upper tier					0.69 (0.59)		0.29 (1.12)	
(ln) Effective district magnitude						-0.45** (0.14)		-0.44** (0.17)
Legal threshold					0.42** (0.07)		0.35** (0.12)	
Constant					3.39** (0.16)	3.19** (0.24)	3.11** (0.24)	3.11** (0.24)
Observations	507	536	507	536	507	536	507	536
R ² /WVLS R ²	0.39	0.43	0.58	0.57	0.48	0.36	0.62	0.51

Notes: Dependent variable is Gallagher's least squares index of disproportionality. Models 1–2 are linear regressions; models 3–4 are linear regressions with multiplicative heteroskedasticity.

Robust standard errors are in parentheses, ** $p \leq 0.05$ (two-tail test).

^a Dummies for electoral formula were included in the model, but are omitted from the table. See Table A.5 in the Appendix for the estimates.

Table A.4
Institutions and observed disproportionality in new and established democracies.

	Full sample		Old democracies		New democracies	
	5a	5b	6a	6b	7a	7b
(ln) District magnitude (lower tier)	-2.16** (0.13)		-2.29** (0.13)		-1.43** (0.36)	
Size of upper tier	-5.52** (0.90)		-10.90** (1.18)		-0.64 (1.01)	
(ln) Effective district magnitude		-1.92** (0.11)		-1.95** (0.11)		-1.52** (0.47)
Legal threshold	0.22 (0.11)		0.30 (0.17)		0.16 (0.14)	
New democracy	2.86** (0.41)	1.71** (0.33)				
Constant	9.33** (0.36)	9.39** (0.35)	9.95** (0.38)	9.46** (0.36)	9.59** (0.88)	10.25** (1.04)
Observations	507	536	398	422	109	114
R ²	0.43	0.46	0.50	0.49	0.19	0.16

Notes: Dependent variable is Gallagher's least squares index of disproportionality. Models are linear regressions.

Robust standard errors are in parentheses, ** $p \leq 0.05$ (two-tail test).

Table A.5

Institutions and observed disproportionality.

	Effect on mean			Effect on mean			Effect on ln(variance)		
	(2)	(2a)	(2b)	(4)	(4a)	(4b)	(4)	(4a)	(4b)
Majority	0.78 (1.23)	0.78 (1.23)	0.81 (1.23)	0.60 (1.23)	0.09 (1.30)	0.40 (1.25)	0.32 (0.36)	0.42 (0.37)	0.36 (0.37)
Alternative vote	-2.32** (0.90)	-2.32** (0.90)	-2.32** (0.89)	-2.32** (0.89)	-2.32** (0.89)	-2.32** (0.89)	-1.31** (0.41)	-1.31** (0.41)	-1.31** (0.41)
d'Hondt	-5.32** (0.84)	-5.67** (0.90)	-4.42** (0.91)	-5.87** (0.80)	-5.68** (0.82)	-5.71** (0.84)	-1.30** (0.47)	-0.57 (0.51)	-0.28 (0.57)
Saint-Lague	-5.71** (0.89)	-7.23** (0.89)	-5.03** (0.91)	-5.82** (0.85)	-6.28** (0.83)	-5.90** (0.87)	-2.03** (0.65)	-0.72 (0.50)	-0.82 (0.57)
LR-Hare	-4.84** (0.96)	-6.92** (0.97)	-3.90** (1.10)	-5.76** (0.87)	-6.73** (0.84)	-5.66** (0.96)	-0.77 (0.71)	-0.23 (0.76)	0.42 (0.80)
LR-Droop	-3.46** (1.18)	-4.05** (1.25)	-1.92 (1.12)	-3.27** (1.15)	-3.04** (1.25)	-2.94** (1.09)	-1.67** (0.84)	0.12 (0.91)	-0.17 (0.63)
LR-Imperiali	-4.49** (0.89)	-4.52** (0.89)	-4.20** (0.90)	-4.77** (0.87)	-4.49** (0.87)	-4.76** (0.88)	-1.58** (0.46)	-0.89 (0.45)	-1.29** (0.48)
STV	-6.65** (0.85)	-6.67** (0.86)	-6.33** (0.86)	-6.96** (0.83)	-6.68** (0.83)	-6.97** (0.84)	-1.33** (0.40)	-0.58 (0.40)	-1.01** (0.43)
Mixed-member majoritarian	0.07 (1.16)	-0.28 (1.28)	0.20 (1.16)	-0.12 (1.13)	-0.25 (1.24)	-0.18 (1.12)	-0.33 (0.46)	-0.57 (0.69)	-0.21 (0.47)
Reinforced PR	-2.32 (1.22)	-2.23 (1.23)	-1.00 (1.19)	-1.98 (1.41)	-1.67 (1.48)	-1.69 (1.16)	-0.46 (0.62)	0.34 (0.64)	-0.01 (0.58)
Constant	11.65** (0.76)	11.65** (0.76)	11.65** (0.76)	11.65** (0.75)	11.65** (0.75)	11.65** (0.75)	3.11** (0.24)	3.11** (0.24)	3.11** (0.24)

Notes: Dependent variable is Gallagher's least squares index of disproportionality. baseline category is plurality rule. Models 4-6 are linear regressions. Models 4-4b are linear regressions with multiplicative heteroskedasticity. Electoral formulae were coded on the basis of the uppermost tier. Robust standard errors are in parentheses; **p ≤ 0.05 (two-tail test).

Table A.6

A re-estimation of models from Blais and Bodet (2006).

	Dependent variable			
	Diversity of legislative party positions	Mean party distance from median voter position	Number of parties in government	Government distance from median voter
<i>Effect of disproportionality</i>				
Original sample	-1.55** (0.72)	-6.52** (1.58)	-13.13** (4.65)	-3.07 (2.24)
N = 31				
Third wave	-0.84 (0.96)	-4.54 (2.79)	-11.52** (4.49)	-1.05 (2.15)
CSES				
N = 48				
New democracies	3.93** (1.51)	11.15 (5.70)	8.16 (9.73)	8.65** (3.77)
N = 28				
Old democracies	-1.84** (0.43)	-7.93** (0.87)	-15.49** (3.56)	-3.49** (1.29)
N = 51				
Full sample	-1.05 (0.56)	-5.31** (1.76)	-12.25** (3.45)	-1.82 (1.55)
N = 79				
<i>Effect of (ln) av. district magnitude</i>				
Original sample	0.031** (0.015)	0.126** (0.048)	0.054 (0.161)	0.042** (0.066)
N = 31				
Third wave	0.045 (0.025)	0.104** (0.043)	0.247** (0.084)	0.065 (0.036)
CSES				
N = 49				
New democracies	-0.013 (0.062)	-0.127 (0.133)	0.050 (0.182)	-0.209** (0.085)
N = 28				
Old democracies	0.045** (0.016)	0.145** (0.030)	0.180 (0.105)	0.090** (0.032)
N = 51				
Full sample	0.035** (0.016)	0.095** (0.040)	0.163 (0.095)	0.035 (0.039)
N = 79				

Notes: Table entries are OLS coefficients of the effect of Gallagher's least squares index of disproportionality and the logged average district magnitude. Estimates of the effect of the old democracies dummy and the constant were omitted from the table. Cluster-robust standard errors are in parentheses; **p ≤ 0.05 (two-tail test).

Table A.7

Summary statistics for the sample used in replicating Blais and Bodet (2006).

	Original sample, N = 31				Expanded sample, N = 79			
	Mean	St. dev.	Min	Max	Mean	St. dev.	Min	Max
Diversity of legislative party positions	2.03	0.53	1.00	3.20	1.97	0.52	0.67	3.20
Mean party distance from median voter	0.84	0.20	0.37	1.20	0.77	0.22	0.00	1.20
Number of parties in government	2.39	1.17	1	6	2.48	1.18	1	6
Government distance from median voter	2.35	0.72	1.44	4.35	2.29	0.61	1.35	4.35
Least squares index of disproportionality	0.05	0.04	0.01	0.16	0.05	0.03	0	0.17
(ln) Av. district magnitude	1.81	1.44	0	5.01	2.91	1.69	0	5.86
Old democracy	0.71	0.46	0	1	0.65	0.48	0	1

Notes: The original sample includes Australia (1996), Belgium (1999), Bulgaria (2001), Canada (1997), Czech Republic (1996, 2002), Denmark (1998), Germany (1998, 2002), Great Britain (1997), Hungary (1998, 2002), Iceland (1999), Ireland (2002), Israel (1996, 2003), the Netherlands (1998), New Zealand (1996, 2002), Norway (1997, 2001), Poland (1997, 2001), Portugal (2002), Slovenia (1996), Spain (1996, 2000), Sweden (1998, 2002), Switzerland (1999, 2002).

In addition to these cases, the expanded sample includes Australia (2004, 2007), Austria (2008), Canada (2004, 2008), Croatia (2007), Czech Republic (2006, 2010), Denmark (2001, 2007), Estonia (2011), Finland (2003, 2007, 2011), Germany (2005, 2009), Great Britain (2005), Greece (2009), Iceland (2003, 2007, 2009), Ireland (2007), Israel (2006), Italy (2006), Latvia (2010), the Netherlands (2002, 2006, 2010), New Zealand (2008), Norway (2005, 2009), Poland (2005, 2007), Portugal (2005, 2009), Romania (1996, 2004, 2008), Slovak Republic (2010), Slovenia (2004, 2008), South Africa (2009), Spain (2004, 2008), Sweden (2006), Switzerland (2003, 2007), Turkey (2011).

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