

The Assessment of Partisan Bias in Districting Plans Outside of the US Context

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This paper aims to contribute to the extensive scholarly literature on evaluating the political consequences of districting plans – also known as boundary limitation or constituency maps outside of the US – in single-member district elections. As anyone familiar with the word gerrymander (Martis 2008) knows, districting plans can impact the seat distribution between the competing parties in elections in interaction with the number of votes won. This interaction may create normatively objectionable seat distributions. Yet, this only becomes apparent to the naked eye when a different party wins the majority of the seats than the one that got the majority of the votes. In the political science literature of the last few decades the partisan symmetry standard became the commonly accepted tool for conceptualizing and measuring the more or less hidden partisan bias of a districting plan.¹ It is also commonly accepted that the quantification of such bias always requires some sort of comparison.² Either the counterfactual imposition of simulated alternative district maps on the same vote distribution, or the simulation of hypothetical vote distributions for the same districting plan are considered the best way to determine how seat distributions may be impacted by a districting plan (Johnston 2012). However, many of the commonly used methods raise severe methodological objections, and extensions to multiparty competition remain extremely rare and difficult (Jackman 1994; Monroe 1998). This is an obvious and major obstacle to use cross-national comparative evidence to explore important theoretical and practical questions in assuring the fairness of elections, like whether and under what institutional design features non-partisan vs. conflictual redistricting may help to reduce the partisan bias of districting plans.

This paper's main contributions are conceptual and methodological points, but the illustration of the advantages of the approach developed here also sheds new light on empirical issues surrounding a recently adopted national districting plan that is widely considered blatantly

¹ For important milestones in the development of the standard see Butler (1951, 1953); Tufte (1973); King and Browning (1987); Grofman and King 2007; van der Hout and McGann (2009); McGann (2015). For the negligible differences between slightly different measures of the same concept see Nagle (2015). Proposals for alternative measures keep coming up, especially in legal scholarship, mainly in response to making the concept easier to measure and understand by judges and the public (e.g. Goedert 2015; McGhee 2014; Stephanopoulos and McGhee 2015), but invariably fail simple tests of logical consistency or conceptual appropriateness (see especially McGann et al. 2016).

² Simpler methods not involving comparisons can give strong signals of the presence of partisan bias (Wang 2016), but not its size.

biased. The paper's probably most radical proposition is that for analyzing districting plans, in most party systems, and perhaps in all multiparty systems, it is analytically necessary and normatively desirable to make extreme counterfactual statements, which contemporary methodologists would warn us to stay away from (King and Zhen 2006; Goertz and Mahoney 2012: 121). I shall argue that the merits and demerits of extreme counterfactuals and estimates of statistical uncertainty look rather different from the perspective of the deliberations in non-partisan commissions charged with drawing up districting maps than from the perspective of US-style litigation of already established districting maps. Once we adopt the forward-looking perspective of such an independent commission trying to impose fairness on the electoral system, the most sophisticated toolkits of gerrymandering analysis used in contemporary political science appear less useful than they could and should be.

Indeed, extreme counterfactuals, both retrospective and prospective, are routinely used in the voluminous literature on British elections (see Johnston 2012 for a recent overview), without reflecting on the unusually high degree of uncertainty that can be attached to these statements. In addition, these analysis nearly exclusively use the so-called uniform swing method in spite of its unanimous condemnation in the methodological literature of the last quarter century (Gelman and King 1990a, 1994a; Jackman 1994; Monroe 1998), as well as overwhelming empirical evidence that the emergence of regionally differentiated swings and party systems in the UK since the 1970s made the use of uniform swing models highly suspect (Curtice and Steed 1982; Johnston and Pattie 2011).³ My argument about the conceivable needs of a non-partisan commission that may at some point be charged to draw unbiased plans for a multiparty setting may offer an explanation for this practice.

My explanation for the continued popularity of the much derided uniform swing method is not a justification though. As a second main contribution, I propose using extreme counterfactuals in a

³ Except for Gelman and King (1994a), Jackman (1994) and Monroe (1998), the only scholarly analyses of districting plan bias outside of the US and Britain that I am aware of also rely on either the uniform swing (see e.g. Brookes 1953, 1959; Johnston and Forrest 2009; Siaroff 2010) or the multiyear (e.g., Katz 1997; King 1990) or some indexing methods (Gudgin and Taylor 1979, 1980; Rydon 1957; Rydon and Soper 1958). The latter are even more compellingly criticized and rejected as acceptable methodologies in gerrymandering analysis than uniform swing itself (see Gelman and King 1994; Jackman 1994; McGann et al. 2016: 68; Monroe 1998). In the interest of brevity this paper avoids discussing the indexing and multiyear methods.

more nuanced and circumspect way, while also taking advantage of variable-swing methods for characterizing the respective probability of outcomes that seem feasible under one or another set of the various, conflicting but more or less reasonable model assumptions between which existing data do not allow empirically-based adjudication. I suggest that extreme counterfactuals are best discussed if a common practice in the extant literature was abandoned. The practice in question is to express the partisan bias of a districting plan in a particular election with a single number (and its corresponding statistical error), and to do likewise with various bias components stemming from malapportionment, turnout, and so forth. Instead, I argue that the possible partisan bias of a plan – and each of its components - should be considered as a multidimensional array. If they were to draw fair districting plans, non-partisan commission should seek to minimize probable bias across the whole array. Which elements of the array should be most important to minimize is obviously a political question, and it remains to be seen whether and how independent commissions can gain a mandate to make relevant choices when facing tradeoffs. However, I suggest that there is no point in simplifying the issues they actually face.

Third, the paper suggests a new statistical toolkit, based on the powerful and fast machine learning technique of Hainmueller and Hazlett (2014), for forecasting the probable range of district-level variation in future election results. I avoid using previously known methods for reasons that are commonly found across democracies, such as the non-existence of relevant data about how the geographic distribution of one or more non-ignorable party – or turnout, or the size for that matter – may change from one election to another, but the clear presence of spatial patterns in district level results. I show an empirical analysis that illustrates the potential usefulness of the toolkit for analyses of districting plans. I reckon that specialist of statistics could offer superior techniques instead of a few, probably barefoot solutions that I introduce at some points. Yet the fundamental idea behind the new toolkit seems worth exploring and offers relevant advantages over previously existing methods. To my knowledge, the present paper is, apart from an unpublished conference paper of Monroe (1998), the first to adapt variable swing modeling to the analysis of partisan bias in a districting plan under a multiparty systems, and this may also be proof of the method’s particular suitability for the task.⁴

⁴ Jackman (1994) reorganized Australian electoral data as if the Liberal and National Parties were the same party in order to run an analysis following Gelman and King’s (1990a) method for two-party

Fourth, I shall argue that analyses of the fairness of a districting plan in an election should model the expected district-level variation in election returns primarily with variables that were observable right after the preceding election for the same office. This modest requirement facilitates an exciting analytical distinction that is largely absent from the previous literature in spite of it appearing in legal arguments in the US (Grofman 1990; McGann et al. 2016). Namely, it allows the quantitative separation in the retrospectively apparent impact of the districting plan in a given election into (1) effects that can conceivably be attributed to unfair differences in the starting positions that the districting plan gave to the different parties; and (2) effects that arose because changing social, economic and political conditions – most notably the actions of the electoral contestants themselves – altered the geographic distribution of voter support since the initial conditions were set. I argue that fair-minded map drawers cannot be expected to anticipate type (2) effects and should only try to anticipate type (1) effects. The empirical analysis also illustrates the analytical value of this distinction for a better understanding of election outcomes in retrospect.

Given this complex agenda, I keep the empirical analysis as simple as possible. For one, I do not illustrate the complexity and multidimensionality that the arrays of bias estimates can take, just mention some of the possibilities that are technically straightforward to implement with the toolkit discussed below. Also, I pick a relatively easy case of multipartism, which, with a very minor simplification of facts, could also be analyzed as a straight two-party competition for seats and features the same parties facing up each other in every district. This allows adding a comparison of the present results and those obtained with the best established and reputable

systems. Calvo and Rodden (2015) use a multi-year method that ignores district-level data – except for the generation of one Gini coefficient for each party per election – for generating estimates of expected seats under majoritarian bias for each party in each election given its size and geographic concentration (measured by the Gini coefficient). The standard criticism of multi-year methods apply, plus Johnston et al.'s (2016) remark about the implausibility of assuming identical spatial patterns across the UK. Linzer's (2012) estimates and their standard errors only yield bias estimates under some circumstances. Their chief defect is though that they are generated, in fact, by residual deviations between observed district-level vote share and mixtures of multivariate normal distributions that attempt to model them. Since there is no reason to think that under a given districting plan – that may have a built-in bias or any number of other purposeful features that reflect on intricate local circumstances like administrative boundaries – district-level vote shares would follow any regular distribution, there is no theoretical reason to consider his estimates anything else but white noise. Blau (2001) deals with the votes and seats won by third parties only as influences on two-party vote-seat ratios, which served as a key inspiration for my present paper. However, his bias estimates rely on a novel version of what Monroe (1998) calls indexing methods. Discussing multi-year and indexing methods falls outside of the present paper's scope. See footnote 3 too.

variable-swing method in the trade as implemented by Gelman and King's (1994a) *JudgeIt* software.⁵ Once again, applying the present toolkit to more complicated multiparty systems would not require changes in the methodology except for adding one set of rather routine variable transformations and choosing a strategy to deal with partial contestation – i.e. election returns from districts where one or more relevant party did not field a candidate, which is an ever-present, tedious problem for gerrymandering analysis but is thankfully absent from the data used here to an exceptional degree.

I also cut the discussion about the data context to a minimum. The paper uses empirics from recent Hungarian elections partly because of personal interest in the case, but above all because the 2011-12 redistricting in Hungary was surrounded by many of the usual smoking guns signaling an intentional gerrymander (Renwick 2012; Scheppele 2014; Tóka 2014). Local analysts and the independent press concluded that the adopted new map of constituencies was indeed putting the left-wing opposition at a massive disadvantage in future elections, and the expert respondents of the Electoral Integrity Project's 2014 global survey (Norris et al. 2016) gave the fourth highest score to Hungary after the US, the Philippines and Malaysia for how unfair its districting plan is (Martínez y Coma and Lago 2016). Recent Hungarian election results are so lopsided, however, that the authors of *JudgeIt* would probably conclude that – given the difficulty of telling how geographic patterns in the vote may change if the left-wing vote searched to match the vote total of the government – nothing can be said about partisan bias in this country with any degree of confidence. Hence the Hungarian data are particularly apt to illustrate the conceptual and methodological challenges that this paper focuses on, and to examine whether the toolkit proposed here can identify gerrymander where most likely there was one, and whether it produces size estimates that look plausible in a comparative perspective given the reputation of the new Hungarian system. I should emphasize, however, that this is not a case study of redistricting effects under the complex mixed-member electoral systems of Hungary. Instead, the methods and data discussed in the paper only concern first-past-the-post elections in a single-member districts. Extensions to more complex systems are possible, and would in fact be easy in the Hungarian case. They, however, would not advance in any way the

⁵ This comparison is not yet added to the current version of the paper. Comparison of the present results to preliminary analyses with *JudgeIt* reported in Toka (2013) is not conclusive because of differences in the elections providing the data.

conceptual and methodological agenda of the paper, and are therefore avoided here, as is a detailed discussion of the Hungarian case itself.

The plan for the paper is as follows. I start with a brief and simple review of the concept and measurement of partisan bias, and discuss why in most party systems it has to be conceived as an array that includes extrapolation to extremely counterfactual scenarios that could not be observed in the reasonably recent past. Next I make some admittedly casual suggestions about how the evidence required from electoral analysis by non-partisan commissions in charge of drawing electoral district maps may differ from the kind of evidence prioritized by scholarship in the United States – i.e., in the vast majority of technically sophisticated work on redistricting plans to date. The third section discusses how the evidence non-partisan commissions may want to require from electoral analysis might influence the range of predictor variables for vote shares in statistical models paving the way to districting plan assessments. Section four outlines how a particular machine learning technique can be used to meet the requirements identified in the previous sections and illustrates the results that can be obtained with an empirical example. I conclude with a discussion of further work and research prospects along the lines discussed in the paper.

1. Bias as an array and its estimation method families

Districting plans establish the geography of electoral districts, but cannot directly impact the seat distribution between the competing parties. Once, however, at least some votes were cast for candidates of a party, they may earn a smaller or higher number of seats depending on how they were distributed across districts. The same absolute number of votes can earn more seats in districts that have fewer voters, and/or a larger percentage of votes cast for ignorable also-run parties, and/or small margins of victory for the party in question than its rivals (see e.g. Johnston 1976, 2002; Johnston et al. 2001). It is only through the relative stability of voting patterns across space, i.e. the over-time correlation between the vote share of the same party in the same geographic aggregates, that districting plans influence seat shares. But in the presence of such over-time stability the impact that the districting plan exercises on a party's chances to win seats via the free future decision of individual voters, is, in principle, somewhat predictable.

Figure 1 about here

The difficulty with observing which parties are favored over some others by the districting plan stems from the fact that majority rule in single-member districts does not lead to any natural, automatic relationship between the vote and seat shares of the parties except for usually giving more seats to a party with more votes (Tufté 1973). Consider the left-wing opposition's share of the combined single-member district vote for the left and the governing Fidesz in the 2014 Hungarian elections, which heralded the arrival of the new districting plan examined in this paper, displayed in Figure 1.⁶ With their 37.8 percent of the two-party vote, left-wing candidates got over the 50 percent mark and won the seat in just 10 – i.e., 9.4% – of the 106 newly drawn districts. The partisan symmetry standard, which is based on the requirement of neutrality towards political alternatives (McGann et al. 2015) and has gained wide acceptance in the political science literature (Grofman and King 2007), reckons that we cannot tell if this was a fair, an exaggerated or an unjustly diminished percentage of the seats unless we can establish whether the rival side would have gained the same number of seats had it won the same fraction of the popular vote jurisdiction-wide. The application of the bias concept to multiparty systems where some parties may have an asymmetric role – e.g. represent the alternative to different major parties in the different parts of the country – may require caution and raise important concerns (Gelman and King 1996) but this can only be dealt with according to what is sensible in a local context and is not directly relevant for the empirical material considered in this chapter.

To discover bias, one could probably generate at random a large number of alternative aggregations of precincts into electoral districts and see if the average across of left-wing seats across these random – and hence, we may think, unbiased – plans is significantly lower or higher than the 10 observed under the district map in effect in the election (see e.g. Chen and Rodden 2015). The problem with this solution is that some parties may just have it in their nature to attract geographically more or less concentrated voters, and thus be advantaged or disadvantaged by most districting plans even in the absence of any favoritism intended (Chen and Rodden

⁶ While these two alliances only collected a bit more than 70 percent of the total vote in this election – their lowest combined share since 1998 –, they did win all the single-member district seats, just like they did in every parliamentary elections since 2002 except in ten out of the 528 individual races in all three combined. It is thus not much of a simplification if for a moment we consider the 2014 election in the single member districts as a two-party contest.

2013). However, it can be shown that there always is a possibility to find an unbiased map, it is just that sometimes they are rather rare among a large number of randomly chosen maps complying with other preset criteria like contiguity and so forth (McGann et al. 2016: chapter 4).

Alternatively, one might look for bias by examining the ratio of seat shares to vote shares across a large number of elections under the same electoral and party system. This could be done with the usually rather small set of real-world elections that used the same districting plan (Tufte 1973; King and Browning 1987; King 1990; Katz 1997). Yet this “multi-year” method runs into severe methodological objections that will not be revisited here (see footnote 3). It turns out to be far more instructive to do the same with results from hypothetical elections simulated using arbitrarily set shifts in the jurisdiction-wide percentage of the vote going to each party and adding them to empirically informed expectations about where, irrespectively of the jurisdiction-wide breakdown of the vote, each party can be expected to do better and worse than average and how much. This is also the approach taken by variable-swing methods including the one used in the present paper and discussed in more detail below. In this section, however, I only mean to suggest that partisan bias is best conceived as an array rather than a point estimate. To illustrate this point it is enough to introduce the simpler and methodologically inferior uniform swing method.⁷

Uniform swing estimates D_{ijk} – the difference between Party j 's fractional or percentage share of the (either two-party or total) votes in district i and Party j 's average share across all electoral districts in the jurisdiction in hypothetical elections k – in the simplest and most intuitive way possible, namely by calculating it from observed election results. Thus the shape of a distribution like that one shown in Figure 1 is assumed to remain absolutely unchanged as the vote share of the party increases above or drops below the level where it was in the observed election. We can then estimate that under this uniform swing model the left share of the seats in the 2014 election would have been 40.6% and that of Fidesz 58.5% (with a single seat going to a third party

⁷ The uniform swing method was initially developed by David Butler (1951, 1953) to study swing ratios. It was adopted for the analysis of partisan bias by British expat Ralph Brookes in New Zealand (1953), from where it returned to the UK with Ron Johnston twenty years later (1976). Since the 1980s uniform swing has been by far the pre-dominant method of districting plan analysis in the UK mainly because it proved more comprehensible to political scientists than the indexing methods previously championed there by geographers (Johnston 2012).

candidate) had the left and Fidesz had an equal share of the votes nationwide and the vote share of all third party candidates remained as they were observed in the actual event of the 2014 election. This gives us a first point estimate of partisan bias in the districting plan, which – calculated, with no preference meant, from the left’s perspective throughout this paper, meaning that negative numbers signal bias that helps the right-wing rivals - is minus 17.9. This is, in absolute value, not too far from the highest equivalent figures calculated for any US state in recent elections by McGann et al. (2015, 2016) and are -17.6 for Virginia, -19.7 for Pennsylvania, -19.0 for Ohio, -19.9 for North Carolina, -22.5 for Missouri, -22.6 for Mississippi, -19.0 for Louisiana, and -25.3 for Alabama (negative numbers signal pro-Republican bias).

Similar bias estimates can, however, be calculated for any values of V_j (jurisdiction-wide vote share of Party j). We could, for instance, compare the 13.2% of seats that the Hungarian left is postdicted by this model to have obtained with a 40% share of the two-party vote in 2014 to the 11.3% of the seats that the same model gives to Fidesz candidates under the assumption that they won a 40% share of the two-party vote. The bias estimates therefore turn positive (pro-left) for the 40% benchmark, making it quite pointless to characterize the conceivable impact of the districting plan with a single point estimate across the whole range of values. The reason for this is that – apart from differences across districts in the number of people voting for the two parties for which we carry out the bias calculus and the impact of locally strong third party candidates – the only reason why a districting plan may favor one major party at the expense of the other if the former wins its seats with smaller margins of victory than the latter at vote distributions of substantive interest. Yet a close look at Figure 1 immediately reveals that if the districting plan helps a party as it appears (under uniform swing assumptions) to help Fidesz when the two parties have a similar vote shares, then that is because the other party has more extremely safe seats for itself that it will win even with a very low jurisdiction-wide vote share. Thus, at some particularly low and at some very high benchmark values of V_j , the party favored by the plan at the 50% threshold will be hurt and earn a smaller number of seats than its main rival, which has a

less even distribution of its voter support across districts given how the electoral map was drawn.⁸

Figure 2 about here

As an illustration, Figure 2 shows the estimates obtained with uniform swing applied to the data shown in Figure 1. Note that the values of partisan will remain rather symmetrically distributed around the 50% mark throughout this paper. This, however, is not a mathematical necessity but a mere empirical happenstance of third party candidates very rarely threatening to take seats away from either left-wing or Fidesz candidates in the 2010 and 2014 Hungarian elections.⁹ Most importantly though, it is a mathematical necessity that as we move from very low to very high values of V_j in the course of simulating hypothetical election outcomes, the bias estimates change both size and sign except in the highly unlikely case of a perfectly neutral districting plan that produces an impeccable series of zeros throughout the entire theoretical continuum for V_j .¹⁰

The reason why the literature on US elections is content with using point estimates for bias is clear and unproblematic though. First, under relatively balanced two-party competition it is perfectly enough to focus on what may happen in the continuum from $V_j=.45$ to $V_j=.55$, and the variation of partisan bias that can be observed in this narrow range is often seem no more than mere noise or irrelevant detail. In fact, it should rarely if ever make a difference in substantive interpretation whether one measures bias with the exact estimate for $V_j=.5$, or averages multiple values from the $V_j=.45$ to $V_j=.55$, or estimates bias from the intercept of a linear or logistic regression of S_j (the seat share of party j) on V_j (Tufte 1973; Gelman and King 1994; Grofman et al. 1997; Nagle 2015).

⁸ For the same reason, gerrymandering – unless it manipulates only the expected number of votes for the two parties in the comparison combined – always carries some risks that requires prudent hedging (Owen and Grofman 1988; Grofman and Brunell 2005; Friedman and Holden 2008; Puppe and Tasnádi 2009).

⁹ For example, given the equal success rate of third-party candidates independently of whether the left or Fidesz won 40% of the two-party vote, the bias estimate in Figure 2 is an identical 1.9% for both the 40 and 60 percent benchmark, it is just that the same figure is obtained as 87.7 (left seat share) – 85.8 (Fidesz seat share) in the second and as 13.2 - 11.3 in the first case.

¹⁰ A minor caveat applies for a districting plan where malapportionment and/or turnout and ignorable party vote shares differences across the districts are the only source of partisan bias. Under such a plan, the sign of the partisan estimates can stay non-negative (or non-positive) for all possible values of V_j , but will still change in size.

What is far less obvious is how analyses of partisan bias in multiparty systems can proceed without conceptualizing partisan bias as an array rather than a point estimate. In such systems, substantively relevant comparisons involve, almost as a rule, parties of highly unequal vote shares, say the UK Conservatives at 36.1, Labour at 29.0 and the Liberal Democrats at 23.0 percent of the vote (the numbers are their actual vote shares in the 2010 general election), to mention an example that is far from extreme. Producing a single partisan bias estimate along the above lines – or maybe one for each party pair – would dramatically simplify and indeed distort the true landscape of highly variable bias estimates, often of different sign, obtained while different swings are applied to, say, the Liberal Democrat and Conservative votes to estimate their respective seat shares at identical benchmark values of V_j . Single point estimates obtained with the Borisyuk et al. (2008, 2010) methodology¹¹ favored in most recent analyses regarding UK elections are, in particular, guaranteed to be uninformative at any, and highly misleading for probably most of the time.

As is probably obvious already, the normative assessment of districting plans in multiparty settings require extreme counterfactual inferences in the sense given to the word by King and Zhen (2006), i.e. that there is nothing in the way of usable data to consider as relevant evidence to judge how things may look like if the current UK Liberal Democrats receive an equal vote share with the Conservatives. However, without making such extremely counterfactual inferences there is no way to accomplish an analysis for which there appears to be a strong demand for, as the staggering amount of works on partisan bias in the UK's districting plans, as well as the considerable media coverage and policy interest (ADD REFERENCES) that they raised witness. What I am suggesting here is that this call should be answered with presenting not a single bias estimate as the one and only that can emerge under a districting, but an honest and comprehensive exploration of a range of alternatives that can obtain under various

¹¹ This method tries to apply uniform swing to the three-party case. It creates symmetry between the three parties in the vote shares that they obtain in hypothetical elections by applying uniform swing to the three-party breakdown of the vote to calculate seat distributions of all possible combinations of the three above party names with the actual vote shares of these parties in an election of interest. I.e. it may first attribute the observed Con vote share to Lab, the Lab share to Lib Dems, and the LD share to the Cons, and calculate what seat distributions would obtain (with uniform swing showing what breakdown of the vote would obtain in each district). Then it proceeds to combine the same national vote shares with the same party names in the five other possible ways. Total partisan bias from the perspective of each Party j is then calculated by averaging j 's seat share across the six hypothetical elections.

distributions of the popular vote, and probably under different sensible assumptions about how swing patters may vary across districts. Where possible, the estimates presented should include rigorously assessed confidence intervals too, without however creating the impression that these reflect anything more than some uncertainty that will remain even if national vote distributions are of a particular sort and certain assumptions that go beyond observable data happen to hold true. Presenting bias estimates as a complex multidimensional array that covers the widest possible range of conceivably feasible and plausible, empirically informed scenarios is the best way to communicate the model uncertainty that is involved in extreme counterfactual inferences.

The much greater differences between observed jurisdiction-wide vote shares between relevant parties j in a multiparty than in a balanced two-party system are, however, just one reason why single point estimates are not suitable for estimating partisan bias in a districting plan for a multiparty system. A second quite trivial reason is that the seat breakdown between Party A and Party B at a given jurisdiction-wide breakdown of the two-party vote between them will depend on the jurisdiction-wide vote share of Party C, and maybe Party D and E and ... Z, depending on how many parties have a realistic chance to pick up seats in the system. For a sensible analysis bias estimates need to be calculated separately for each combination of party vote shares that are deemed to be of relevance in mapping the outcomes of substantive interest in, for instance, designing a new districting map. A possible simplification of the resulting task will be illustrated below in the empirical analysis. The dimensionality of the array of bias estimates can however be increased to an arbitrary extent by exploring the dependence of model estimates on assumptions about the underlying variability of election results, regional patterns in swing, and so forth. By now the reader may well wonder who and how can cope with such a bewildering overload of complex information, and thus I move on to my next point.

2. How a non-partisan commission may think about partisan bias

Many countries that employ single-member districts for national elections put non-partisan technocratic bodies in charge of drawing districting plans for the country (Grofman and Handley 2008). It would be unwise to expect that such bodies can always operate in a truly non-partisan way in countries with low quality of governance, high corruption, extensive partisan patronage,

and/or extensive politicization of the civil service and the public sphere. It may be even less wise to charge such bodies with making fundamentally political decisions like how much racial gerrymander should be applied, what overrepresentation of some regions' population – like those of Scotland and Wales in the UK Parliament or the vast Northern territories in the Canadian – should prevail, and so forth.

The previous literature on redistricting also provides compelling evidence that merely non-partisanship or bipartisanship and the lack of effort and intent to create bias are no sufficient guarantees that a plan will not produce partisan bias (Rallings et al. 2008; Johnston et al. 2012). Clearly and conscientiously following their usual guidelines and criteria like compactness, contiguity, respect for communities of interest, equal population size, and so forth, boundary commissions are probably more likely to draw plans with some partisan bias than purely unbiased plans. At the minimum, legislation need to set demonstrated unbiasedness as one of the criteria that districting plans must meet before non-partisan commissions would even be allowed to pay heed to this consideration (McGann et al. 2016: chapter 4). But since large parts of the democratic world lay their hope for fair districting plans in non-partisan bodies (Grofman and Handley 2008), it is worth to consider what information they may require and be able to process and act upon if they were called to draw districting plans that minimize the expected partisan bias.

First, given the usual professional profile requirements, members of such bodies are likely to display uncommonly high levels of numerical literacy (Rossiter et al. 1999). Hence the recurrent emphasis of the American redistricting literature on the need for simple measures that can be used in courts probably does not apply here. Conceptual clarity, precision, methodological rigor and transparency presumably remain, however, as important as ever. A boundary commission member can be expected to understand that future election outcomes are uncertain, and that a districting plan should be well suited to meet fairness targets under numerous alternative futures. It is highly unlikely that members of such a commission would be greatly impressed by the simplistic, highly deterministic forecasts obtained with uniform swing models that highlight only one and therefore rather improbable future instead of mapping alternatives and providing plausible confidence intervals.

Second, boundary commissions work on plans for the future rather than judge the past. In an interesting way, therefore, they are likely to find value in statistically insignificant numbers. They may find uniform swing models overtly simplistic and yet consider even a small probability of substantial bias, as long as it is convincingly derived, sufficient to reject a plan. Courts cannot do that, and may even demand robust demonstration of intent in deciding whether a plan was gerrymandered or not. But none of that is a relevant concern for boundary commissions.

The fundamental reason for the weaker standards of evidence that map-drawers can live with than judges is that at the point when they consider a proposed or merely possible plan, the social costs of rejecting it and starting to consider another plan are easy to calculate, simple (mostly just additional work time) and extremely close to zero. In comparison, the social costs that courts legitimately keep in mind when considering the annulment of an existing districting plan can be complex, hard to calculate and often very substantial, particularly in terms of some values that the judicial system is meant to uphold and may be dearer than egalitarian fairness to conservative judges.

All in all, the cult of statistical significance is more likely to prevail in courts than in boundary commission meeting rooms. And yet, boundary commissions are likely to be more receptive to variable swing than uniform swing models. They may not need crystal clear, “statistically significant” evidence of future bias, just a reasonably clear demonstration that under some set of plausible assumptions about the future, there is a non-negligible risk of bias in a plan, and can move on to search for promising alternatives.

3. Modeling the impact of the districting plan

Simulation sets obtained with uniform swing models cover only an infinitely narrow segment of the outcomes that could conceivably occur under the same districting plan in a given election: what happens when each district votes exactly as in an observed election except that all district experiences the same aggregate swing between parties j . By effectively attaching a zero statistical estimate to their bias estimates, they even deprive themselves of the ability to distinguish systematic partisan bias from sheer randomness in election results that can even

occur because of the uncoordinated spontaneous hesitation of swing voters on the day of an election, not to mention during the campaign (Gelman and King 1994). Consider the following example as illustration of how deep this problem runs. In the 2014 Hungarian election, panel data was collected about an N=1,500 cross-section of the public, interviewed about their voting intentions in the days before the election and about their actual vote a few days after the vote. Though the vast majority voted as intended, a few changed party compared to their prior intention on (or by) election day. Their movements cancelled out each other's impact and made virtually no difference in the aggregate breakdown of the vote in the sample. By plugging in the probabilities of various intention changes obtained with the survey data in the district level election outcomes and making a few assumptions – such as the existence of the same patterns of party switching across districts, and independence of choices among swing voters nested in the same district – we can calculate if this short-term volatility of voting behavior could affect the seat shares of the major parties on election day. The results of 1,000 simulations suggests that the entire pro-left bias that the uniform swing method claims to find in the event of the left receiving about 38% percent – the actual figure in 2014 - of the two-party vote (two of the 106 seats, see Figure 2 for the estimate) was within the 95% confidence interval of the variation that could emerge in vote shares in the election merely because of the uncoordinated behavior of relatively few swing voters in the last few days of the campaign and election day.¹²

The example underlines the need to use methods for generating expected vote shares that allow for variation in proportion of the actual uncertainty that concerns election outcomes. In other words, this means a call for the use of variable-swing models of the kind introduced by Gelman and King (1990, 1994a) in the redistricting literature. There are two generic features of these methods that are indispensable, and some that are peculiar to one or the other of Gelman and King's two different models and will not be followed here for reasons that I explain later below.

The generic features of variable-swing models are that they construct hypothetical elections from three additive components and only treat one of them as a constant. The first and third components are much the same as in uniform swing models except that no uniform swing application known to me attempted – though in principle they could just as variable-swing models routinely do – to use multiple estimates of the first to characterize the existing

¹² ADD DATA SOURCE AND DETAILS OF THE ANALYSIS.

uncertainty about it. I shall call this component $E(D_{ijk})$, which stands for expected deviation of district i from the jurisdiction-wide average of relevant quantity V_{ij} in hypothetical elections k , and where the relevant quantities j are:

V_{ij} if $j=1$: the percentage of total electorate assigned to the district;

V_{ij} if $j=2$: district turnout;¹³

V_{ij} if $j=3$: the fraction of all valid votes cast for other candidates than the top three;

V_{ij} if $j=4$: the fraction of all votes for the top three candidates that went to the one who was not representing either of the top two parties (i.e., the governing Fidesz and the oppositional left-wing alliance, respectively);

V_{ij} if $j=5$: the left's share of the votes cast for the candidates of the top two parties.

Note that more D_{ijk} quantities could and should be added to this list of prospective dependent variables in more complex party systems, where the candidates of the top two parties do not always end up among the top three candidates in every single district, or there is some substantive interest in distinguishing between the various third parties, probably because third parties do actually pick up some seats in the election in question. However, following the logic applied in forming the above quantities, it is always possible to create the desired set of district deviations from the jurisdiction-wide average of every D_{ij} quantity such that there is no necessary logical link between them as there is in the case of vote percentages that collectively sum up to 100. Therefore separate regression models can be estimated for each D_{ij} variable as long as any empirically occurring correlation between them is satisfactorily modelled through the choice of independent variables and parametrization. Section four below discusses the reason why I avoid using the more customary approach of estimating all V_{ij} proportions with a single equation after a log-ratio transformation of percentage figures calculated from the same base.¹⁴

Once a model was estimated, the fitted values and the variance-covariance matrix of coefficient estimates can be used to generate N different estimates for each D_{ij} variable to capture the underlying uncertainty about its expected value conditional on the given model parameters and

¹³ To simplify matters I count invalid and blank votes as abstention.

¹⁴ This customary approach follows Aitchison (1986) and can rely on a variety of estimators specifically developed for multiparty electoral data (Katz and King 1999; Honaker et al. 2002; Jackson 2002; Mikhailov et al. 2002; Tomz et al. 2002; Mebane and Sekhon 2004; Yamamoto 2010).

assumptions. The N estimates for each $E(D_{ijk})$ can be matched with each other at random to calculate, for instance, the expected proportion of all registered voters supporting the left-wing or the Fidesz or the best third-party candidate in each district i in each hypothetical election k given the expected size, turnout, and vote distribution of the district electorate in that election and in that district, and hence a full set of $E(V_{ijk})$ proportions.

The third additive component of hypothetical election outcomes for variable swing models (and the second and last component for uniform swing models) is a series of constants chosen to move the jurisdiction-wide average value of a quantity of interest – above all $E(D_{i5k})$, the split of the two-party vote – to different benchmark values. Traditional uninformed swing models use these constants as the only source of difference between hypothetical election outcomes, while in variable-swing models it becomes merely the cross-district mean of variable local electoral swings (King 1989). For the key innovation of variable-swing models is that vote shares change compared to a previous election in different ways in the different districts. Hence hypothetical election results – N times as many as the number of benchmark values for which extrapolations are made – are created by adding up the values of an $E(D_{ijk})$ variable, a suitable constant to arrive at a benchmark value of jurisdiction-wide swing, and a local swing component that has an average value of zero but varies across districts to an extent that can be set in accordance with, for instance, the estimated district-level variability of $E(D_{ijk})$ values from one election to the next.

I shall return to discussing the estimation of variable district-level swings in section four below. Let's now deal with a prior issue that explain the choice made regarding the measurement of local swing. The question is what independent variables should be included in the models generating empirically informed expectations about hypothetical election results if our purpose was to assist the work of a boundary commission that is expected to minimize the risk of partisan bias in some future election, or aims at determining how a particular map may have contributed to the generation of partisan bias in the seat allocation in an already observed election.

Contemporary simulations suggest that, at least for two-party settings, it is always possible to draw an unbiased district map that complies with reasonable criteria set in terms of human and physical geography (McGann et al 2016: chapter 4). Success in this or sheer chance may thus create fully equal starting positions for the parties, at least in terms of the districting plan, at the beginning of an electoral cycle. Yet, changing party appeals to social groups, strategic

mobilization efforts, economic development and an infinite number of other factors can conspire to create, by the time of the next election, a partly new electoral geography that, under the given districting plan, may unfairly help some parties over others.

As an illustration, consider the Hungarian data displayed in Figures 1 and 2 again. They appear to suggest a small pro-left bias in the map in case the left was very far behind or far ahead of Fidesz in the popular vote, and a much stronger pro-Fidesz bias in case the competition for votes turned out to be relatively closely tied. But how do we know that this was indeed the result of the districting plan? Cannot it be that the excess number of seats won by the left at a relatively low vote total was due not to how the new district boundaries were drawn in 2011-12 but to the fact that they had a much reduced media access, financial resources and presence in local office outside of a few urban strongholds, where they then picked up a surprisingly number of seats in 2014 compared to how poorly they did in the countryside? And cannot it be that the superior efficiency of the Fidesz vote distribution derived from the extreme resource advantages over the opposition that the party could use in a targeted and strategic way throughout the 2010-2014 electoral cycle to adapt its vote distribution to the new district map? Indeed, observers of the country could conceivably come up with quite a few explanations like these and could, in principle, explain away some or all the bias pattern apparent in Figure 2 as a result of events and strategic activities that occurred after the districting plan was adopted.

Support for such explanations would still leave us with the impression that the observed interaction between the geographic distribution of the vote and the district boundaries slightly helped the socialists in winning a few more seats in the 2014 elections than might be expected otherwise, but would have helped the governing Fidesz mightily if the election became more competitive. Yet our soundest explanation for this would not credit the manipulative intent of the map-drawers with this outcome. It would instead propose that an extremely resourceful governing party is much better able to adjust the geographic distribution of its electoral support to the needs of winning a close election than an ailing opposition struggling with uncountable logistical issues and a diminished, geographically concentrated and immobile activist basis under an increasingly authoritarian government. The implication would be that what mattered was not how the districting plan was drawn, but rather that one side to the partisan conflict had massive

resource advantages that came handy in majoritarian elections and could equally be relied on independently of the districting plan.

The morale of these arguments is thus that models that assess the performance of a districting plan in an election should not include variables like incumbency or campaign spending that are likely partly endogenous to the expected showing of parties in the given election. They should also avoid predictors like the results of elections for other offices within a legislative cycle because these would already reflect electorally relevant developments since the legislative cycle started. In particular, they should not add part of the residual to the expected values derived from a model as an adjustment for the impact of unobserved independent variables. In other words, they should avoid, with the key exception of vote distributions in elections prior to the adoption or retention of the districting plan, most of the independent variables that Gelman and King (1994a) recommend for inclusion in the models that assist in forming expected district-level differences of vote shares for the analysis of districting plans. This change in the commendable range of independent variables arises from the change of perspective that we experience when we move from the world of litigation as a method of adjudicating claims regarding unlawful bias to committee rooms filled with non-partisan civil servants, geographers, data scientists and the like charged with drawing unbiased districting plans for a jurisdiction.

A key question is how far back in time one should go in the search for appropriate predictors. A suitable solution may be to treat every electoral cycle as a new competition that starts with the establishment of a starting position for each party, which is either the way they fared in the last election or what future expected vote is promised to them by a newly drawn electoral district (if there are newly drawn districts laid out for the next election). Parties and candidates then start their efforts to improve their chances compared to this starting position. The model that should be used to assess the impact of the districting plan itself should only include information that is already observable at the time when the starting positions are clarified to the actors.

That is, district-level variation in the election of interest in terms of campaign expenditure, candidate attributes like incumbency, elections for other offices that occurred since the last election for the same should indeed be interesting variables in exploring what shapes election results and a party's seat share in a given election year. King and Gelman's (1991) analyses of incumbency effects and how they suppressed much of what could have been a strong pro-

Republican bias in US congressional elections from WW2 to the end of the 1980s is an unrivalled demonstration of how much insights can be obtained from such analyses. However, the partisan fairness of a districting plan must be evaluated against the circumstances at the time of its adoption, or (implicit) retention for a next round of elections following the announcement of the results for a previous election. Whatever happens afterwards to change the geographic distribution of the vote – and hence emerging patterns of partisan bias in the interaction of a districting plan with a given spread of the vote across the jurisdiction – till the next election is partly the result of social and political change that district boundaries cannot be held responsible for. Even if some of these changes are the responses of political actors to the opportunities and constraints created for them by the existing districting plan, the responsibility for the prudence and effect of those choice must be borne by the actors or external circumstances beyond the districting plan. After all, one would not want to blame the design of the running tracks in an athletic stadium just because one runner reached the finishing line faster than the others.

Incidentally, there is an additional conceptual advantage in thinking of each electoral cycle as a race to improve one's starting position set partly by past electoral history and partly by a districting plan that was either just adopted or (implicitly) retained for another election, and attempt to assess the fairness of the districting plan with reference to this starting position instead of the eventual election outcome that the political process arrives at after its usual twists and turns a few years later. This advantage concerns how this perspective can deal with tactical voting, which is common cause of concern in applying analysis of partisan bias to multiparty settings (see most recently the discussion of the British case in McGann et al. 2016). The problem, however, is also present in two-party settings. What if, for instance, turnout drops in some districts, or the local minority party does not even field a candidate because the local majority is expected to have an overwhelming dominance in the election? If this reduced the nationwide vote share either – through reduced turnout – of the locally dominant party, or – through not fielding a candidate – of the local minority, how should this change be counted in the aggregation of vote shares across the jurisdiction for the purposes of determining which party has the more efficient geographic distribution under the districting plan? Can a party become more favored by a districting plan just because some of its voters do not show up at the polls? That does not sound right, but it is also not very appealing to require some more counterfactual estimates of how many people may have voted or abstained tactically to the already complex

task of estimating partisan bias. The above perspective suggests that the ideal way of proceeding is to evaluate the fairness of the districting plan with respect to past election results without discounting for the effects of tactical behavior by candidates and voters that may have occurred in them. The behavioral patterns established by strategic acts in the past are part of the existing starting position against which parties will try to improve their lot in the next electoral cycle. The question is how fair a particular districting plan is to the parties given the starting position, and that starting position may include the need that some parties would need to convince supporters who tactical defected or abstained in the past that next time there will be a point in acting differently than what they may have already got used to doing in their non-competitive home district.

On a more technical level, the rather strict diet with respect to the choice of independent variables suggested by the above approach may well result in an increase in the uncertainty estimates throughout the entire estimation procedures, and eventually spill over into bias estimates to. The above argument suggests that if this happens, then it happens for the right reason. However, someone may be concerned that far too many gerrymandered district plan may show statistically insignificant bias and pass under the radar as a result. Anyone with such concern should, however, recall that I am not commending this approach for arguments in court, and while a $p=.30$ estimated probability of “no bias” may indeed be enough for a court to conclude that an a legislation-approved districting plan “is not proven guilty”, the same result may look perfectly sufficient for an independent commission to reject a plan on the ground of far too high a risk of bias.

Last but not least, any drop in the accuracy of estimates caused by avoiding within-cycle explanatory factors among the predictors if two potentially powerful but hitherto largely untapped contemporary resources are put in the service of redistricting analyses. The first of these resources are geolocation data about electoral districts, which can assist an effective mapping of spatial patterns and regional variation in election returns. The second are modern machine learning techniques that allow a far more extensive and inductive use of non-linear effects and complex interactions between the predictors than the conventional regression models used in redistricting analysis to date. Machine learning techniques can be optimized to assist out of sample predictions without overfitting models to the existing data and do not expect

researchers to be able to tell in advance what interactions or polynomials of the predictor variables need to be included in the models to achieve the best possible reproduction of systematic patterns in cross-district differences in election returns that may be expected to keep being reproduced in future elections unless the twists and turns of politics change the underlying social fabric. The key disadvantage of machine learning models is that they rarely make it easy to interpret the results with narratives about what exactly is the estimated influence of particular independent variables in a model. This, however, is not a relevant handicap in the present context, where these models are only needed to do what they in fact are meant to do: detect systematic patterns in the data for an estimation sample – in this case, data that could be observed in 2010 - that allow efficient forecasting (for 2014, in our case). Table 1 shall provide some evidence that they do so.

4. Data and empirical results

This section gives a brief illustration of the analytical potential of the approach proposed above. I reckon that my statistical apparatus may not sufficiently address some technical issues, but I trust that specialists of statistics can find superior solutions for these problems. I merely intend to show here a potentially rewarding direction for methodological efforts.

The ultimate goal is to derive partisan bias estimates for the new districting plan adopted by the government side of the Hungarian parliament in 2011-13. The geographic shape of the districts in this plan is rarely unusual, and mostly respects municipal boundaries as one might expect. Otherwise, though, there are many signs suggesting a gerrymandered plan. The plan was developed in such complete secrecy that not a single participants' name or any detail of the proceedings became public knowledge, and no proposal became public before submission to a short and merely formalistic debate in parliament. The plan was passed through parliament in spite of opposition protest and demonstrations over, evidence of a spectacular increase in a positive correlation between left vote share and district size showing partisan malapportionment, and without any public consultation or even justification regarding district boundaries (Tóka 2014). As I pointed out in the introduction, the reputation of the plan is that it is strongly biased, and this appears to be borne out by the methodologically problematic uniform swing analysis of

the actual single member district results of the first and so far only election organized under the new plan (see Figure 2 and above).

The question is whether the approach proposed here can shed some light on whether this apparent bias was really the result of the actual shape of the new districts or just apparent. Maybe both the apparent bias and the bad reputation of the plan was due to some other features of the political context, neatly summed up in the “free but not fair” characterization of the 2014 elections by Scheppele (2014)? Even if the bias was at least partly real, one may also want to see if the regular random variability of electoral geography from one election to another election. I hope to capture this natural and to some extent foreseeable variability with a variable-swing forecast of 2014 electoral geography. This forecast is an extrapolation based on the 2010 starting position on the one hand, and machine learning estimates of how 2002-2006 election data could predict the 2010 electoral geography.

The dependent variables in the estimation phase are district level single-member election results in 2010. The independent variables include 2006 and 2002 election returns, based on votes cast in a completely different set of 176 single-member districts and under different rules (see Renwick 2012). The precinct-level election results of the 2002, 2006 and 2010 elections in the first round of competition in the single member districts were aggregated into the 106 new electoral districts created in 2011 by opposition activists Viktor Szigetvári, Csaba Tordai and Balázs Vető (2011). The aggregation was done manually for about a quarter of the nearly 11,000 precincts (themselves drawn somewhat differently for the different elections) located in municipalities split between multiple new districts. The aggregation is known to be imprecise for a few hundred precincts that were split between two or more new electoral districts in unknowable proportions. The Szigetvári et al. (2011) data and their simple and very critical analysis were widely discussed in political polemics in Hungary but no criticism was raised against data quality.

The reason why, in spite of this excellent data situation, modeling possible electoral continuity from 2010 to 2014 is somewhat challenging is that the third and fourth biggest party in the 2010 elections were, for all practical purposes, entirely new formations without prior electoral

history.¹⁵ Partly to compensate for this, and partly to model any systematic spatial pattern in election returns that may reasonably be expected to prevail in subsequent elections, the average altitude and latitude coordinates of the 2014 polling stations belonging to each new districts were added among the predictors.¹⁶ It is expected that the impact of latitude and altitude coordinates on the vote is not linear, and forms complex interactions with other variables (see also below). To prevent the estimates wrongfully generating expectations of similarity between proximate urban and rural districts, I also added to the predictors a variable showing the average log electorate size of the municipalities in the district.¹⁷ For each of the five dependent variables, the lagged dependent variable was added to the predictors alongside the 2002 vote share of Fidesz, MSZP (the close equivalent of what the left-wing alliance was in the 2014 election), and two smaller parties that have been discontinued since, but whose 2002 vote shares serve, for plausible reasons, as modestly good predictors of 2010 Jobbik and LMP votes.

A possible weaknesses of my estimation strategy for the post-2010 expected district-level deviations from national vote percentages include the separate generation of expected values for the five D_{ij} variables mentioned above. This runs flatly against the state-of-the-art in the analysis of multiparty electoral data (see footnote 14), even though the usual problem of mutual dependence between the individual variables due to them summing up to 100 (percent) is completely eliminated by how exactly I formed the dependent variables in the analysis (see section 3 above). I believe that is a price worth to pay in exchange for exploiting the full potential of geolocation data about the districts to capture spatial regularities in the 2010 Hungarian election returns – something that traditional regression models would not allow to anywhere near the extent as the KRLS method proposed by Hainmueller and Hazlett (2014a) and its implementation with a corresponding R package (Hainmueller and Hazlett 2014b) permitted me. KRLS, however, does not allow either multinomial dependent variables or estimating

¹⁵ Caveat: Jobbik, collecting 17% percent of the vote in 2010, was in fact founded in the early 2000s but remained virtually unheard of by most of the public until about 2008. It did run a joint list in the 2006 election with a much better known long-established far right party, and the joint list did receive 2 percent of the vote. However, their district-level results from 2006 and 2010 show a negative (and at -.08 anyway weak) correlation.

¹⁶ The weighted average – weighted by the number of locally registered voters who turned out on election day at each station – was used.

¹⁷ This average was also shaped by weights for the number of actual voters in the districts from each municipality.

seemingly unrelated models for multiple dependent variables. Thus, the usual methods of dealing with the boundedness of percentage figures and dependencies across quantities are not available in the KRLS framework. The very high predictive power of the KRLS model for each dependent variable should, however, take care of accurately modeling most empirical correlation between the dependent variables.

Table 1 about here

The model estimates obtained with the KRLS technique are of no substantive interest here, except for what is summarized by Table 1. The first two columns show, on the one hand, the multiple R statistics of model fit for the KRLS models estimated with the 2010 election data providing the dependent, and 2002-2006 data the independent variables, and, on the other, the same statistics derived with a simple linear regression model that features the same independent variables without interactions and polynomials. As expected, KRLS produces a better fit than OLS, but – given the strong continuity in some aspects of election results from the differences between district electorates ($j=1$) and district turnouts ($j=2$) to the differences between districts in the left share of the two-party vote ($j=5$) – the differences between OLS and KRLS only become big when, for a particular pair of elections, the lagged value of the dependent variable has diminished explanatory power (see the results with $j=3$ and $j=4$, for the ignorable parties' and the best-placed third party's share of the two, which are affected by the massive change in the identity of the third and fourth biggest party between the two elections). KRLS, thanks to its use of geodata and features of the algorithm, does all right even under these circumstances, while OLS can become even worse in explaining the dependent variable than predicting the future.

Columns 3 and 4 compare the predictive performance of the linear and the KRLS models. Here the correlation displayed is between the actually observed 2014 value of the given D_{ij} variable on the one hand, and an extrapolation obtained by using the coefficient estimates from the models estimated with 2002-2010 data but imputing the 2010 values to the independent variables. OLS does not do too badly here, but KRLS does notably better yet.

Recall from section 3 that the next step is to generate 1000 sets of $E(D_{ij})$ values (expected deviation of district i from the cross-district average of V_j in 1000 hypothetical elections with an as yet unspecified cross-district average value of V_j) from the fitted values and the covariance

matrix of the fitted values generated by the KRLS model. This is the first of three additive components of hypothetical election results at the district level.

The second component is the estimates for local swing, i.e. that component of the future deviation of V_{ijk} from the cross-district value of $E(V_{jk})$ that our model think is not predictable from past observations. The only thing that we can try to estimate is the shape and variance for the distribution of local swing. I do this by extrapolating from the KRLS models that tried to estimate the observed 2010 distributions of D_{ij} from prior data. The residuals from these models tend to be neither normally distributed nor homeoskedastic.¹⁸ This is no cause for concern in the case of KRLS models given their purpose and algorithm. However, it suggests that I should depart from Gelman and King's (1990, 1994) methods for estimating local swings via normal distributions. Instead, I calculate the residual for each D_{ij} variable, estimate its kernel density curve,¹⁹ and take a random sample of 1,000 estimates for local swing S_{ij} for each variable j in 1,000 hypothetical elections. The corresponding $E(D_{ijk})$ and S_{ijk} values are added up for each combination of i, j and k , respectively.²⁰ Alternatively, we could be more rigorous and pair each 1000 estimate $E(D_{ij})$ with each 1000 estimate of S_{ij} to form a million hypothetical elections. However, for the present illustration of the approach that I am proposing I stick with the simplified calculus yielding just 1000 elections.

Let's now stop for a second to take stock of what we have got at this point. The sum of $E(D_{ijk})$ and S_{ijk} values forms a three-dimensional array that contains 1000 D_{ij} values for each district i for each of the five quantities j for 1000 hypothetical elections k . Recall that D stands for the deviation of a district from the cross-district average of a given quantity, so the cells entries in the array run, theoretically, from -1 to 1, and, empirically, have an average value of 0 across districts i for each combination of j and k values. We just need to add an arbitrary constant – a

¹⁸ ADD INFO ON STATISTICAL TESTS.

¹⁹ ADD INFO ON IMPOSED PARAMETERS.

²⁰ When the resulting D_{ijk} value was such that V_{ijk} would have been greater than 1 or lower than 0 at some values of $E(V_{jk})$ that was to be used for generating the bias estimates for some benchmark values of $E(V_{5k})$, the D_{ijk} value was assigned to another randomly chosen district in the same election k , and replaced with that other district's initially assigned D_{ijk} value. This random reassignment process continued until all V_{ijk} values of interest fell within the 0, 1 bounds. This routine was only needed, however, for less than one percent of the estimates for D_{i3k} values (with $j=3$ referring to the ignorable party's share of all valid votes). To avoid this little tweak, one may consider running the KRLS models with the logit of the observed D_{ij} values as the dependent variable.

separate constant for each quantity j – that will transfer these values into the 0 and 1 range to properly form hypothetical election results V_{ijk} .

By adding the actually observed 2014 cross-district average of the V_{ij} quantities to D_{ijk} for all 1000 values of k , for instance, we get data for 1000 hypothetical district-level elections that all produced the same jurisdiction-wide distribution of the votes. What the 1000 hypothetical elections differ in is the distribution across the districts, and hence the expected breakdown of the seats between the parties. Figures 3 to 7 compare the kernel density distribution of the observed and the 1000 extrapolated values for the five quantities of interest, i.e. electorate size, turnout, the ignorable party's share of the vote, the best third party candidate's share of the three-party vote, and the left's share of the two-party vote. To compare the extrapolated distributions not merely to the observed 2014 distributions but also to the observed seat distribution, the latter was also subjected to a linear transformation shifting its mean to equal the mean of the 2014 distribution, i.e., by subtracting from the observed 2010 V_{ij} its own mean and adding the 2014 observed mean instead.

Figures 3 to 7 about here

What is notable that for all five quantities, the observed values fall outside of the 95% confidence interval for at least some of the value range.²¹ In other words, the 2014 reality was always at least slightly different from what one might have expected given the 2010 election results and what the 2002-2010 period taught us about how future election results will relate to past results and geography. Something changed in the social fabric of election results in 2010-2014 compared to the previous period. It falls outside of the scope of the present paper to test explanations for these differences, but as I suggested above there would indeed be plausible reasons to expect some changes – as they may always be when one electoral cycle is compared to the next.

Of particular interest for the expected seat distribution is Figure 7, which shows the distribution curves for the left share of the two-party vote. All distributions are skewed leftwards, which

²¹ This is particularly true for the ignorable and third party shares of the votes ($j=3$ and 4 , respectively). The poorer predictive performance for these quantities is not surprising given how the absolute novelty of the identity of the third and fourth biggest party in the 2010 election.

implies an anti-left bias when V_{ij} is close to .5. Some of the 2010-2014 changes in the shape of the distribution, however, appear to improve the geographic efficiency of the left-vote.²² As the thickness of the confidence interval for the extrapolated 2014 vote distribution shows, some of this change was quite conceivable within the parameters revealed by electoral history by 2010. But the wide violet line of the facts of 2014 slips out of this confidence interval for most of the value range. That is, the 2014 observed outcome is not one of the many outcomes that could have occurred given what was already there by 2010. Instead, it is something that was quite a bit out of the realm of possibilities without something actively changing the fabric of election results after 2010.

This impression is reinforced by Figure 8, which displays the expected seat share of the left estimated, on the one hand, by a uniform-swing method that merely adds particular constants to the 2014 district-level vote shares to estimate which party may have won each seat at selected benchmark values of the nationwide vote distribution, and the variable-swing method discussed above. Indeed, the uniform swing estimates are narrowly but consistently outside of the confidence interval of the variable-swing estimates at 38% (the left's actual share of the two-party vote in the election) and just above, where the left apparently improved its position compared to expectations. However, reflecting the relative increase in how safe Fidesz's safest were, the left's share of the seats is lower than expected by variable-swing when the left's vote is around 59% percent of the nationwide two-party vote. Overall, the seats-to-votes curve is less steep, i.e. the responsiveness of seat shares to vote shares is lower, in the actual event of the 2014 election than we may have expected on the basis of the data from before 2011 plugged in the variable swing method. This is in line with the flatter density curve – i.e. lower kurtosis of the distribution of the district-level vote shares – shown in Figure

²² Part of this happens on the left-side of the distribution: the expected probability of very safe seats for Fidesz apparently went up, which results in the left “wasting” less votes in hardly-winnable districts. One might speculate though that this was not so much a conscious adaptation by the left but rather a further drop – at least relative to what it gets elsewhere – in areas where Fidesz incumbents overwhelmingly dominate all elected offices, including local ones. On the right hand side, however, we may see some genuine improvement in the left's position, which very slightly but, it would seem, statistically significantly increased the probability of districts where it could get just over half the two-party vote in the district with its 2014 nationwide share of the vote.

A key argument of this paper is that the partisan bias of the districting plan in an election can be more accurately assessed based on the election results extrapolated from the past than on the basis observation about the election in question. The results shown in Figures 3-7 have no bearing on whether this is correct or not – that has to be decided on the basis of theoretical arguments. Figure 8 merely shows the empirical relevance of the distinction in the case at hand by comparing the bias estimates from the variable-swing model to those shown in Figure 2 based on the uniform swing method.²³

Figure 8 about here

he third component will be 61 constants for jurisdiction-wide average swing that can take the cross-district average of the left share of the two-party vote to .20, .21, .2279, .80, respectively. Two separate series of constants will need to be used to hit the benchmark values first the unweighted and then a weighted average of V_{i5k} . The weighted average will correspond to the estimated jurisdiction-wide left share of the total two-party vote. The weight will be the combined number of votes for the two main parties in district i in hypothetical election k as a fraction of the total size of the electorate in the entire jurisdiction. The weights are obtained for each hypothetical election k as V_{i1k} times V_{i2k} times $(1-V_{i3k})$ times $(1-V_{i4k})$.

5. Discussion

An important yield of developing viable methods for quantifying partisan bias in districting plans under multiparty systems is that it can promote comparative cross-national research into the

²³ Note that the comparison is not entirely fair: the consequences of the different methods are mixed up here with the consequences of the two different methods, and uniform swing is known to produce somewhat more extreme estimates than variable swing (Gelman and King 1994a; Monroe 1998). However, this comparison is as far as I can get with the data at hand.

determinants of bias. Such comparisons across different party systems would undoubtedly be useful – after all, much of the knowledge about the impact of US-style institutions and processes on partisan bias in districting plans comes from broad-based comparisons across US states and time (see e.g. Brady and Grofman 1991a, 1991b; Campagna 1991; Campagna and Grofman 1990; Cox and Katz 2002; Gelman and King 1994b; King and Gelman 1991; McGann et al. 2016; Niemi and Jackman 1991; Niemi and Winsky 1992). Single country studies have been undoubtedly useful in advancing our knowledge about other methods of redistricting, notably those used by non-partisan bodies like the Boundary Commissions of the United Kingdom and a few US states (Grofman and Handley 2008; Rossiter et al. 1999; King 1989). However, cross-national correlational studies ought to be feasible and informative - even if not compelling on their own - regarding whether the bias in single-member districting plans is likely enhanced or reduced by various institutional design elements like, e.g., the frequency of mandatory redistricting, the threshold of legal tolerance for malapportionment, the methods and actors (partisan or non-partisan) involved in redistricting, the possibility of judicial review, and so forth. This is a worthy research agenda that can be pursued without insistence on a litigation-ready validity of the estimates regarding individual districting plans. A key obstacle to such studies is the dearth of methodologically credible and at the same time informative methods for quantifying partisan bias in districting plans under multiparty systems, which is what most countries have. This paper aimed at reducing this shortfall.

I suggested above that bias estimates for multiparty elections cannot be meaningfully construed as a scalar as in analyses of balanced two-party competition. Instead, they need to be built and analyzed as a multidimensional array. This array has to gauge, for instance, how much a districting plan may have favored party A over party B at different jurisdiction-wide breakdowns of the two-party vote between them while the geographic distribution of the vote for all parties remained unchanged. One dimension of the array is thus defined by relevant party pairs for the given election, while another dimension is provided by increments of the shift in the breakdown of the total two-party vote between A and B. Further dimensions of the array can be provided by incremental shifts in the jurisdiction-wide vote shares of other parties, scenarios of expected change in the geographic distribution of the vote, variations in model parametrization and estimation methods, and so forth. There is no technical obstacle in adding these further

dimensions to the array of bias estimates using the toolkit discussed in this paper, and would certainly lead to a richer characterization of the probability of various future outcomes.

At least two aspects of the toolkit proposed here could greatly benefit from further work. First, establishing a good standard for assessing model performance remains a challenge. Second, while all variable-swing models to date, including those of Gelman and King (1990, 1994a) assume that district deviations from the cross-district average of a quantity do not change as the size of the jurisdiction-wide swing in that rises or falls. Mathematical necessity suggests that this cannot be fully true, even though it appears to be a good approximation of facts across the rather limited ranges of swing that can, in most contexts, be observed in real-world elections.²⁴ Thus, it would be desirable to estimate how much the district-level variability between elections depends on jurisdiction-wide swing.

²⁴ Prior works arguing the approximate plausibility of the uniform swing model in various contexts are relevant in this respect. See e.g. Butler (1951), Brookes (1953, 1959), Campagna and Grofman (1990), Blau (2001).

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Figure 1

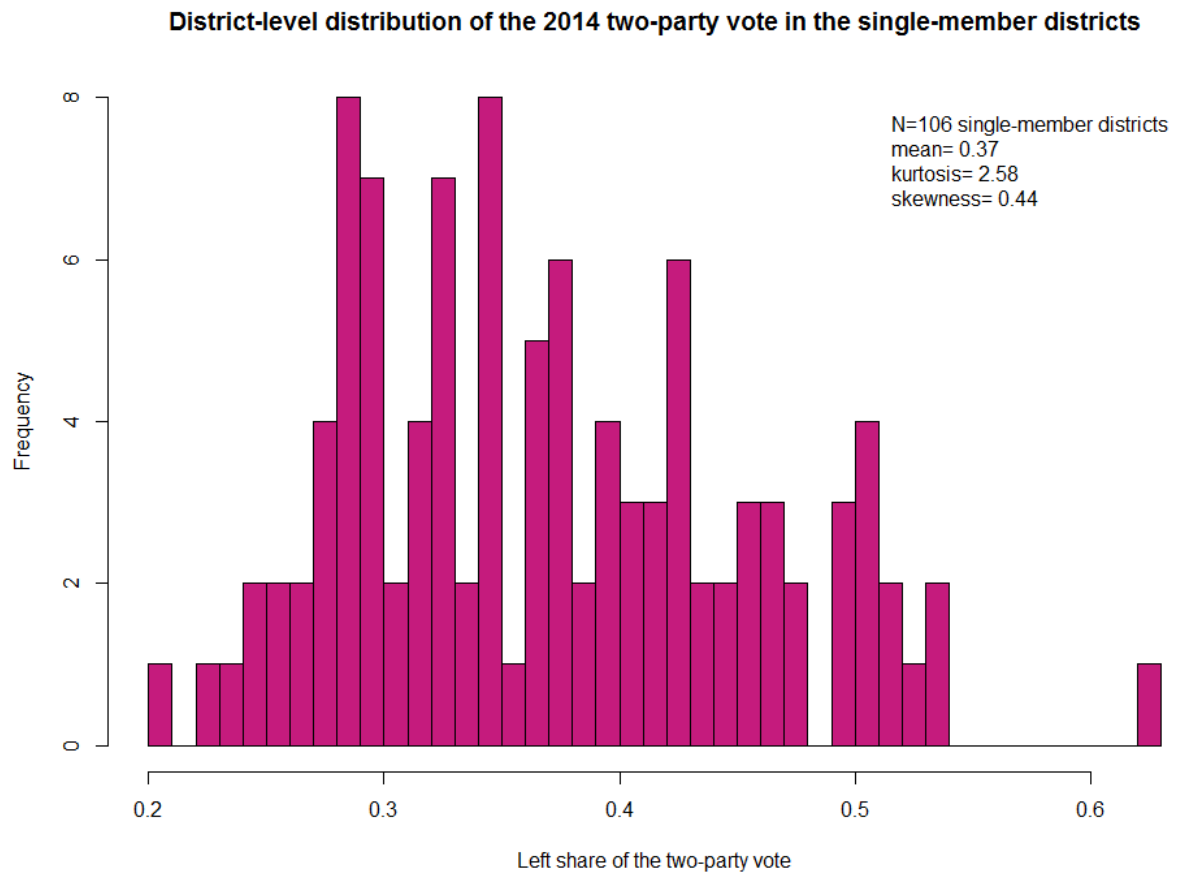


Figure 2.a

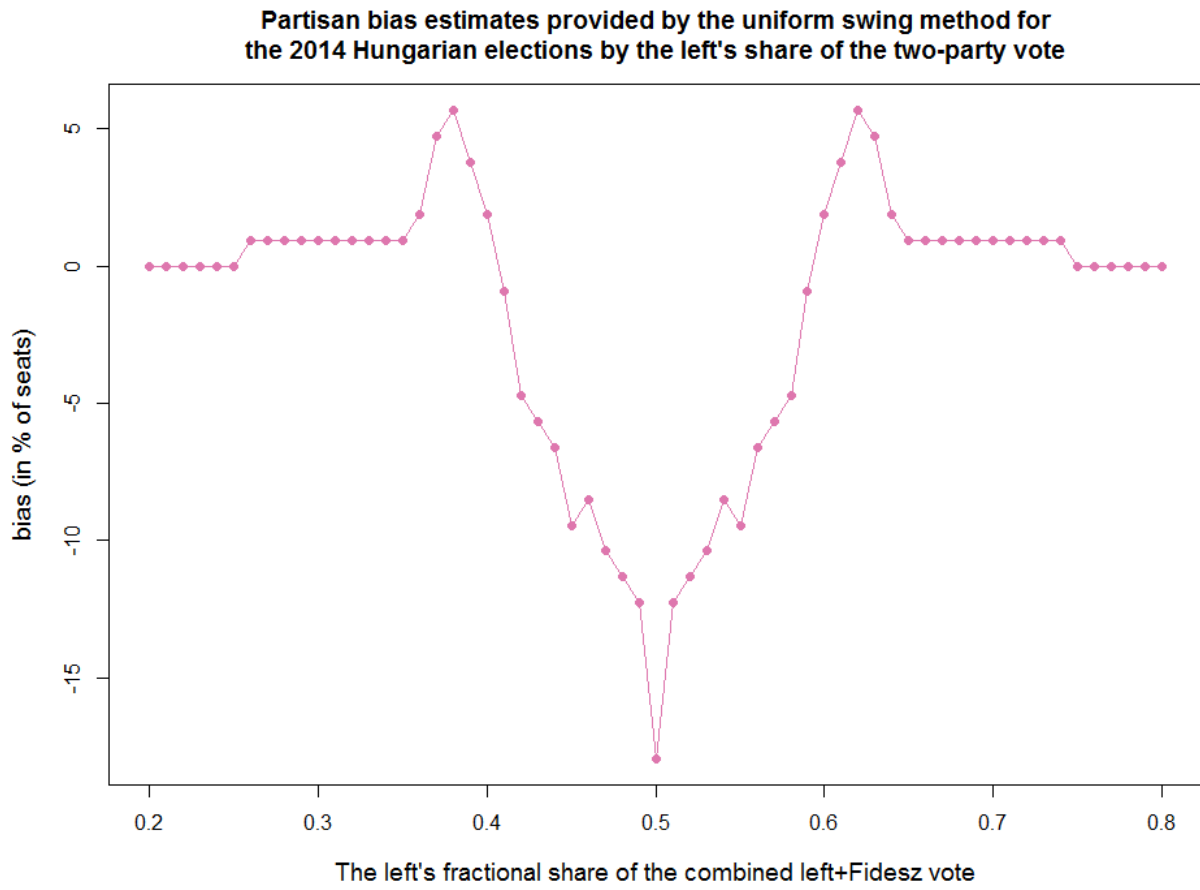


Figure 2.b

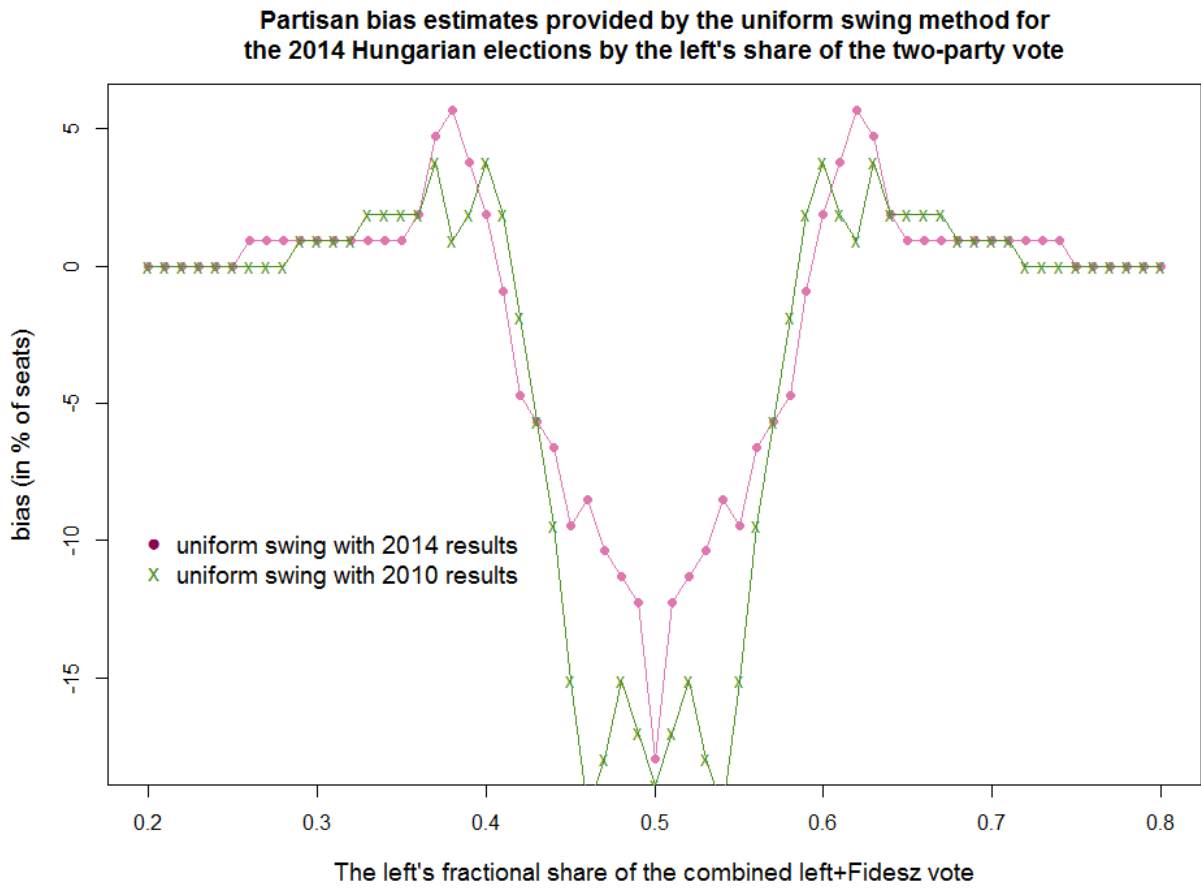


Figure 3

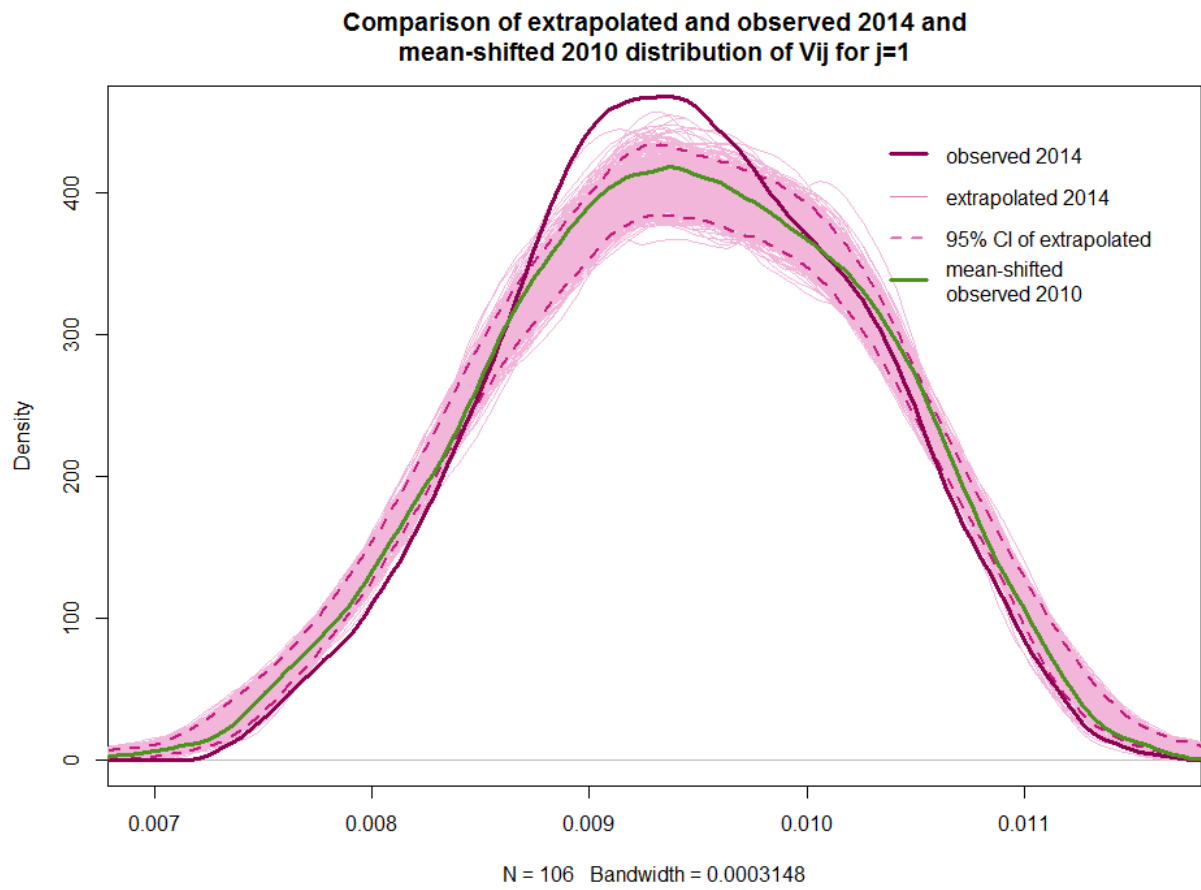


Figure 4

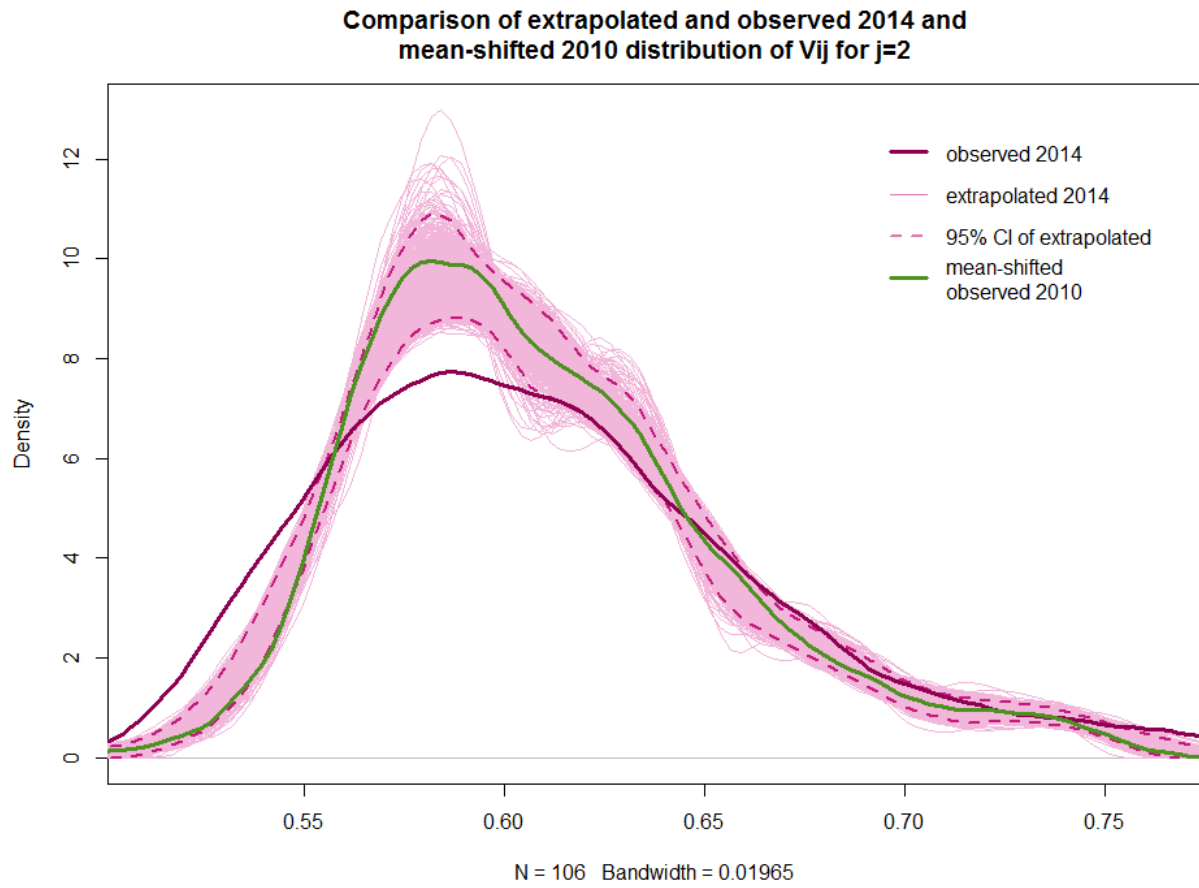


Figure 5

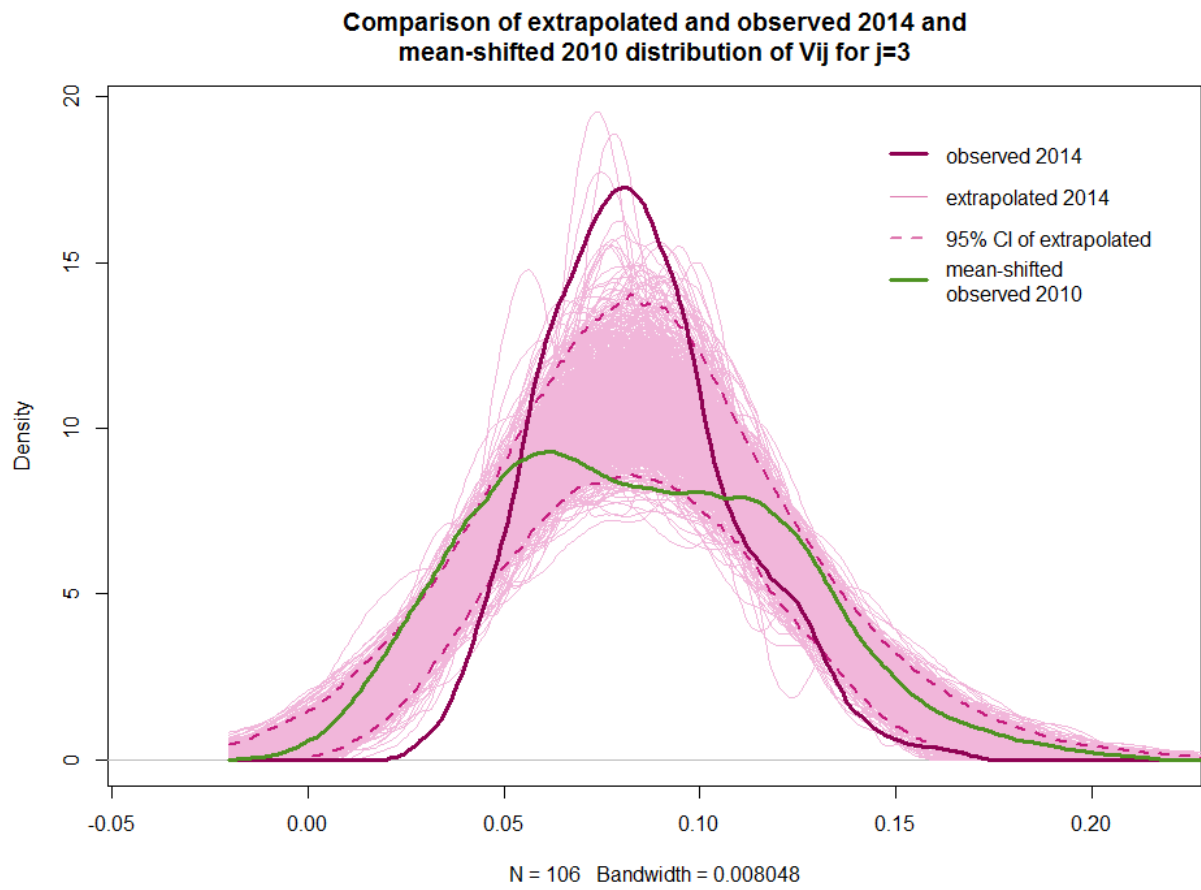


Figure 6

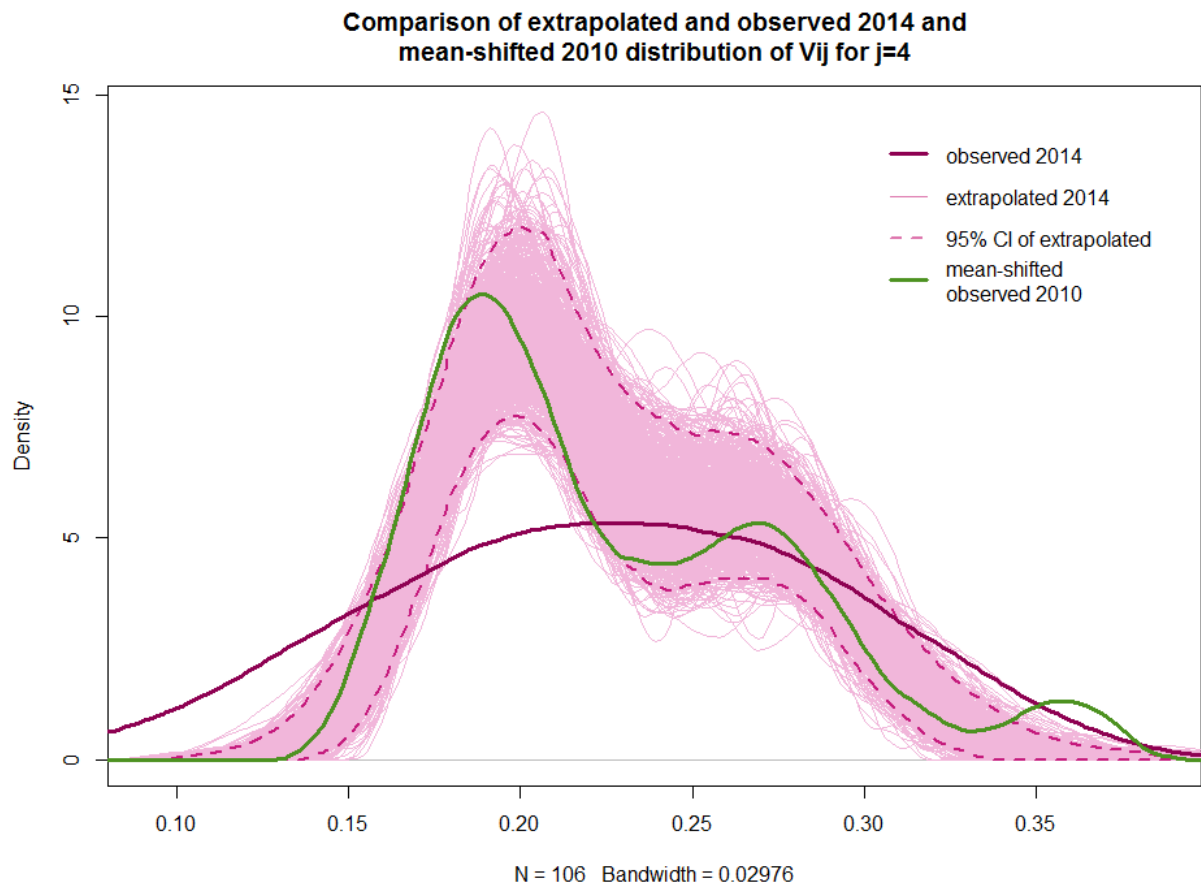


Figure 7

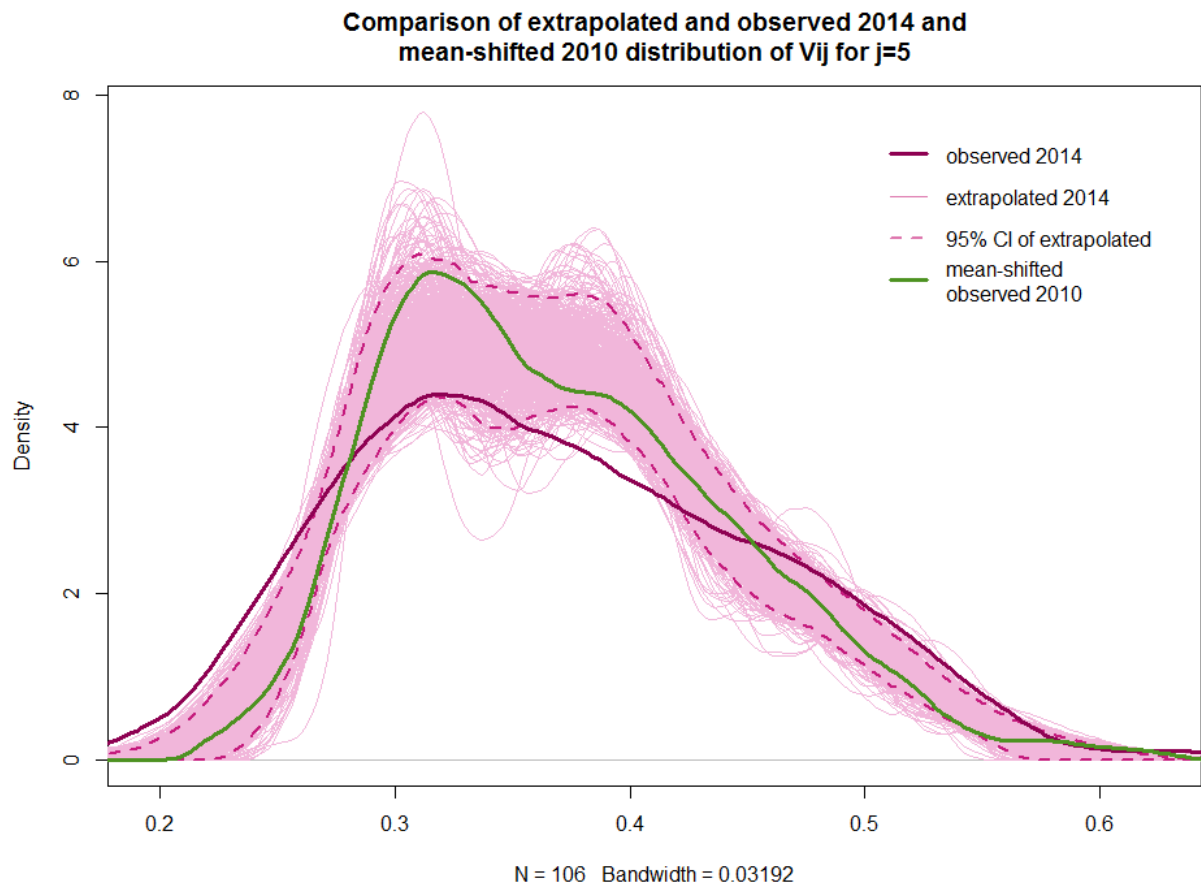


Figure 8

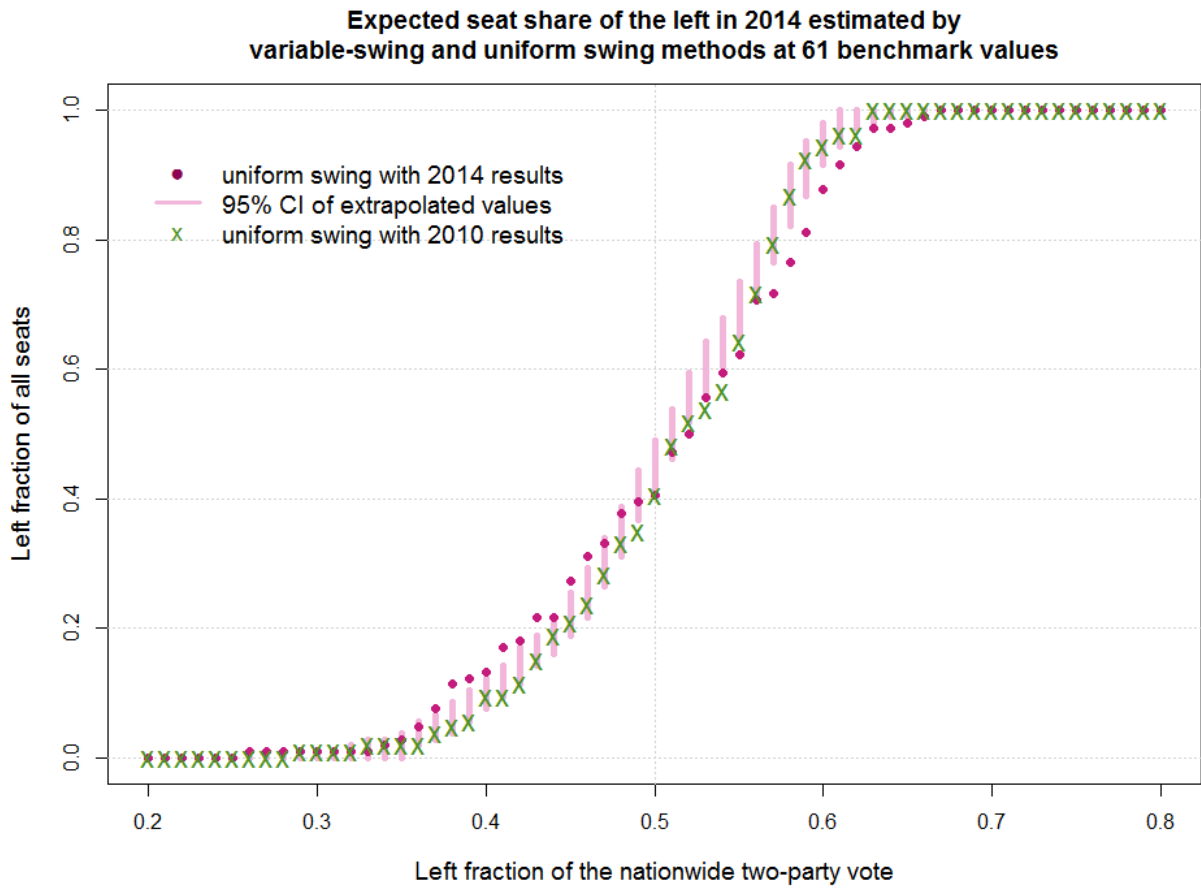


Figure 9

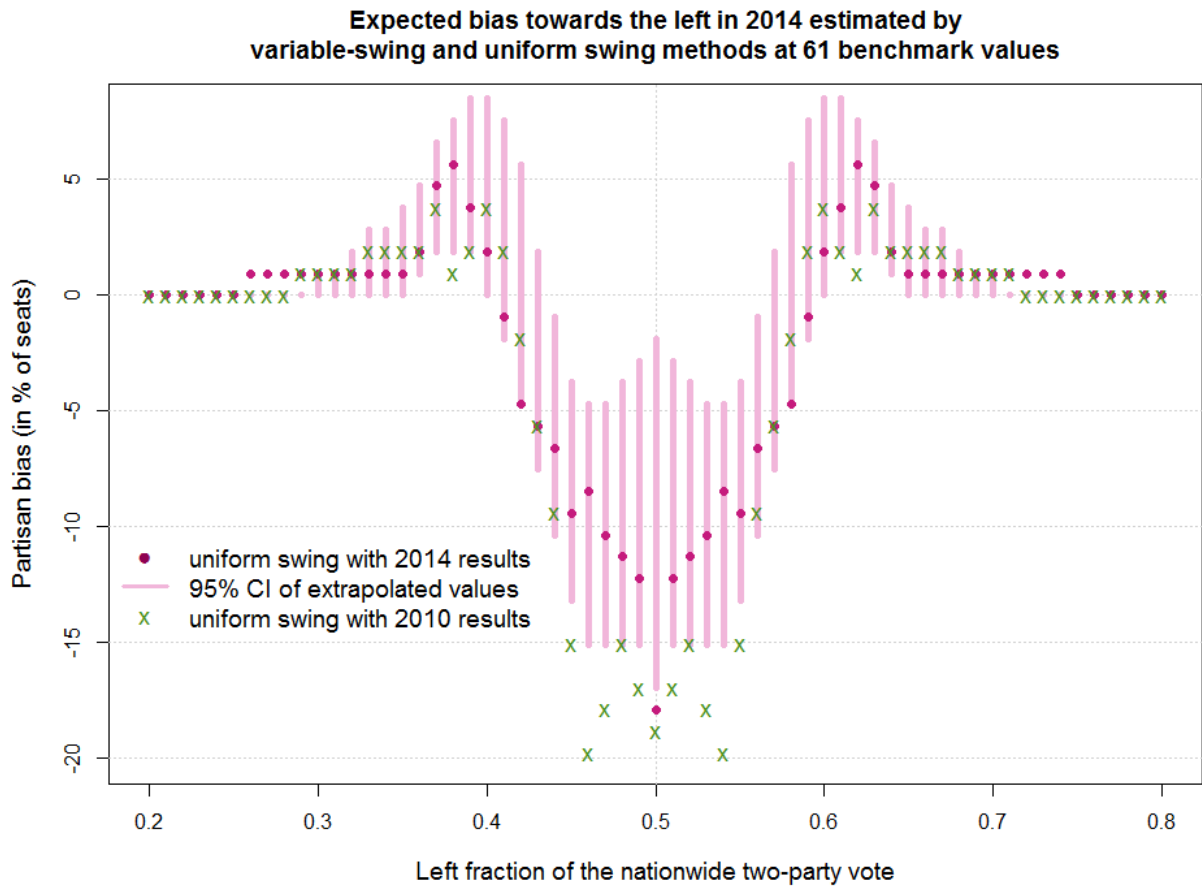


Table 1: Comparing multiple R fit statistics between OLS and KRLS models

Dependent variable D_{ij}	Estimation sample model fit		Fit of extrapolation to 2014 with fact	
	OLS	KRLS	OLS	KRLS
j=1	0.94	0.99	0.85	0.98
j=2	0.97	1.00	0.88	0.98
j=3	0.48	0.73	0.62	0.74
j=4	0.78	0.95	0.73	0.86
j=5	0.93	0.98	0.94	0.97

