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Mechanical and Psychological Effects of Electoral Reform*

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Abstract

To understand how electoral reform affects political outcomes, one needs to assess its total effect, incorporating how the reform affects the outcomes given the political status quo (the mechanical effects) and the additional reactions of political agents (the psychological effects). We propose a framework that allows us to ascertain the relative magnitude of mechanical and various psychological effects. The empirical approach is based on pairwise comparisons of actual and counterfactual seat allocation outcomes. We use the design to analyze a nationwide municipal electoral reform in Norway, which changed the seat allocation method from D'Hondt to Modified Sainte-Laguë. We document clear psychological effects.

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1 Introduction

Electoral institutions shape democracies by governing how vote counts are translated into political power. Changes in electoral systems may, therefore, give rise to strategic responses of voters and politicians in anticipation of the electoral systems' mechanical constraints.¹ For example, a shift towards more proportional representation could cause voters to shift their support to smaller political parties. Small political parties may, similarly, be more likely to participate in elections when the threshold for representation is low. Duverger famously labeled these behavioral responses “psychological effects”, as opposed to the “mechanical effect”, which is the simple translation of votes into seats.²

Since the 1980s many established democracies, for example, France, Italy, New Zealand and Japan, have experienced major electoral reforms.³ In other countries, for example, the United Kingdom, changes of the electoral system have been high on the agenda, although the status quo has prevailed. To evaluate the consequences of electoral reforms, proposed or implemented, we must understand both the mechanical and psychological effects. However, this has proved a challenge. In an early review of the literature, Taagepera and Shugart conclude that “the Duverger psychological effect is the one major relation within the electoral system that remains unquantified”.⁴ There are two main reasons why the psychological effect has proved “elusive”.⁵ First, it is challenging to ascertain the relative magnitudes of mechanical and psychological effects. Second, electoral reforms often arise in times of political change making inference more complicated.⁶

Early attempts at capturing Duverger's mechanical and psychological effects relied on cross-country data and produced mixed results.⁷ Cross-country analyses of electoral systems are problematic since countries differ along many dimensions, making identifica-

¹Cox, 1997.

²Duverger, 1954, p. 224-226.

³Renwick, 2010.

⁴Taagepera and Shugart, 1989, p. 208.

⁵Shively, 1970; Blais and Carty, 1991.

⁶The strategic behavior of political elites played, for example, a key role in the adoption of proportional representation in European countries at the turn of the century (Rokkan, 1970; Boix, 1999).

⁷Taagepera and Shugart, 1989; Lijphart, 1990; Blais and Carty, 1991.

tion of causal effects difficult. To mitigate this omitted variable problem scholars have increasingly exploited within-country variation in electoral laws. These studies suggest that strategic adjustments to electoral reform could be substantial.⁸ We add to this literature by combining a strong identification strategy with a novel method for disentangling psychological and mechanical effects.

To date, the best attempt to separate the psychological and mechanical effects is provided by Blais et al.⁹ They use data from simultaneous lower and upper house elections in Switzerland and simultaneous elections for the Japanese Lower House. They find that psychological effects pertaining both to voters and parties are empirically relevant. Our empirical approach builds on the method proposed in their paper. Like Blais et al., our basic idea is to use the electoral system's formulaic structure to generate a large set of counterfactual election outcomes.

In comparison to Blais et al. our paper makes two key contributions. First, we extend their framework for comparing electoral systems to looking specifically at reforms. This has important implications for how the two types of effects should be considered. Specifically, we demonstrate that the psychological effect consists of two components when applied to electoral reforms: first, how the parties and voters adjust in response to the new system and, second, how these strategic responses change the mechanical effect.

Our second contribution is to apply the methodological framework to an empirical setting where we can estimate the causal psychological effects under plausible assumptions. A specific concern with the approach of Blais et al. is that simultaneous elections can be expected to have an independent effect on both voting and party behavior.¹⁰ Our empirical approach, utilizing an arguably exogenous change in the electoral system, rests on a weaker identifying assumption. For example, it is not problematic for our research design if there are omitted factors impacting political outcomes as long as these factors remain constant over time.

⁸See, for example, Cox, 1997, Cox, Rosenbluth, and Thies, 1999; Cox, Rosenbluth, and Thies, 2000, Benoit, 2001, the collection of papers in Grofman, Blais, and Bowler, 2009, and Fujiwara, 2011.

⁹Blais et al., 2011.

¹⁰Bafumi, Erikson, and Wlezien, 2010; Kern and Hainmueller, 2006; Lago and Montero, 2009.

While most existing analyses of electoral reforms are based on national data, often comparing majoritarian to proportional electoral systems, we study a Norwegian municipal electoral reform within the class of proportional election systems. Specifically, we analyze a switch from a D'Hondt (DH) to a Modified Sainte-Laguë (MSL) seat allocation formula, which took effect with the 2003 municipal elections. The reform was uniformly imposed by the central government on the municipalities. This makes it plausible that the reform can be treated as exogenous with respect to local political outcomes. Another attractive feature of the reform is that it only affected municipal elections, while the simultaneously held county elections were not affected. This institutional feature allows us to isolate the electoral reform's effect from other general time trends.

A change from the DH to MSL method mechanically increases the proportionality of the seat allocation – mostly because of a reduction in the effective electoral threshold.¹¹ This will, holding the behavior of political agents constant, increase the number of parties winning representation.

All agents that care about the election's outcome may react strategically to the mechanics of the electoral system. The psychological effects therefore encompasses strategic behavior pertaining to both citizens and political elites.¹² Changing the electoral system from DH to MSL is expected to give rise to three types of psychological effects.

First, we expect the reform to lead to less strategic desertions of small parties. The reason is that votes for small parties that were previously viewed as wasted are now more likely to be seen as going to a party that has a chance of winning representation. Second, we predict similar adjustments at the political system's supply side: Since entry is costly, both in terms of effort and resources, parties will enter the election only if the benefits from running outweigh the costs. We therefore expect more parties to run in a given

¹¹There is no formal electoral threshold for when a party will receive its first seat for either of the seat allocation methods. Following Lijphart (1994), Boix (1999), and others, we define the *effective electoral threshold* as the proportion of votes that secures representation to any party with a probability of at least 50 percent. In our sample, a municipality with a median sized council (25 members) would have its effective electoral threshold reduced from 3.85 percent to 2.86 percent as a consequence of the reform. In Appendix A, we explain the mechanics of the seat allocation methods in detail.

¹²Blais and Carty, 1991; Cox, 1997.

district after the reform.

Finally, in our empirical setting, a municipality’s discretion to set the size of its council may be used to offset the effect of the reform. Reducing the council size will mechanically increase the effective electoral threshold and increase the advantage for large parties. Thus, we would expect to see a reduction in the council sizes at the time of the reform. Such “defensive behavior” is expected to dampen the reform’s effect.

Our results confirm our prior expectations and show that both voters, parties and incumbents responded to the reform. We find that the number of parties that won council seats increased by, on average, about 0.2. About half of this effect is due to mechanics of the seat allocation formula. The other half is due to a shift in the voting distribution towards smaller parties. In the absence of council size reductions, the total effect of the number of parties winning representation would have been slightly larger.

The psychological effect on the lists winning council seats seem to be driven by dynamic adjustment on the political system’s supply side. In line with the conjecture of Cox, our results indicate that strategic responses in the elite strata are relatively more important than strategic responses in the mass electorate.¹³

2 Application

In October 1997, Norway’s national government appointed an electoral reform commission with the mandate to simplify and revise the electoral system. In January 2001, this commission presented a report with proposed electoral reforms. One of the proposed reforms was to change the allocation formula used for translating votes into seats at the municipal level from a D’Hondt to a Modified Sainte-Laguë formula. The reform commission argued that this change would be advantageous since it would harmonize electoral rules across all governmental tiers.¹⁴ This proposal was incorporated in the

¹³Cox, 1997, p. 98.

¹⁴The seat allocation formula in use at the municipal level in Norway before the electoral reform consisted of two steps, which were a mix between a largest remainder method and a highest average method. It can be shown that the first step is superfluous and that the seat allocation method is

electoral law in June 2002.¹⁵

Descriptive Analysis

Our empirical analysis is based on data from 387 municipalities for the election preceding the reform (1999) and the election following the reform (2003).¹⁶ Table 1 offers descriptive statistics by election year on the main outcome variables we use in the empirical analysis. These are the number of parties winning representation (NoP), the effective number of parties (ENoP), an index developed by Laakso and Taagepera, and the index of disproportionality proposed by Gallagher.¹⁷ In addition, we provide descriptive statistics for some underlying factors that may also be affected by electoral reform. These are the number of parties running, the effective number of parties based on votes cast (ENoP^{Votes}), the number of joint lists and the council size (for further details, see Appendix B).

We document an increase in NoP and ENoP, and a fall in the disproportionality index that coincides with the electoral reform. We also find that the average number of parties running is higher after the electoral reform. The average jumps from about 6.35 to 6.73. We also note that there are fewer joint lists after the electoral reform, but the difference is not statistically significant.

When calculating the effective number of parties based on votes cast, rather than on the allocation of seats, we find a shift in the vote distribution towards small parties. The difference is, however, not statistically significant. In Appendix B, we investigate

equivalent to a DH method (Hylland, 2010). The MSL formula had been in use at the national level since 1953 and at the county level since the first county election in 1975.

¹⁵The electoral reform commission's other proposals were mostly relevant for the national level of government. However, the commission's report resulted in three additional small changes in the electoral law that were relevant for the municipal level. In Appendix C, we describe and analyze the potential impact of these other changes in the electoral law in detail. The conclusion from this analysis is that these changes are unlikely to explain the effects of the reform that we find.

¹⁶In 2003, the total number of municipalities is 434. We drop 41 municipalities where, for any election, the distribution of votes is inconsistent with the distribution of seats in the data that we have available. In most of these cases the inconsistency is minor, and our results are basically unaltered if we include these observations in our empirical analysis. In addition, we exclude municipalities that have parliamentary systems (two municipalities), have a majoritarian electoral system (one municipality), municipalities that were involved in mergers during this time period (two municipalities) and that have missing data (one municipality).

¹⁷Laakso and Taagepera, 1979; Gallagher, 1991.

the shifts in the distributions of votes in more detail. We document a noticeable shift in votes towards small parties after the reform. For example, the share of votes for parties that receive less than 5 percent of the total votes increases by about one-third.

The number of council members is chosen by the previous local council, but the local discretion is subject to restrictions imposed by the Local Government Act of 1992. Table 1 shows that from the 1999–2003 to the 2003–2007 election period the average council size fell by 2.5 members, corresponding to an average reduction of about 10 percent. For all other election years in the period 1975–2007 the average council size change is much smaller (see Appendix B).

Table 1: Descriptive Statistics, Pre- and Post Electoral Reform

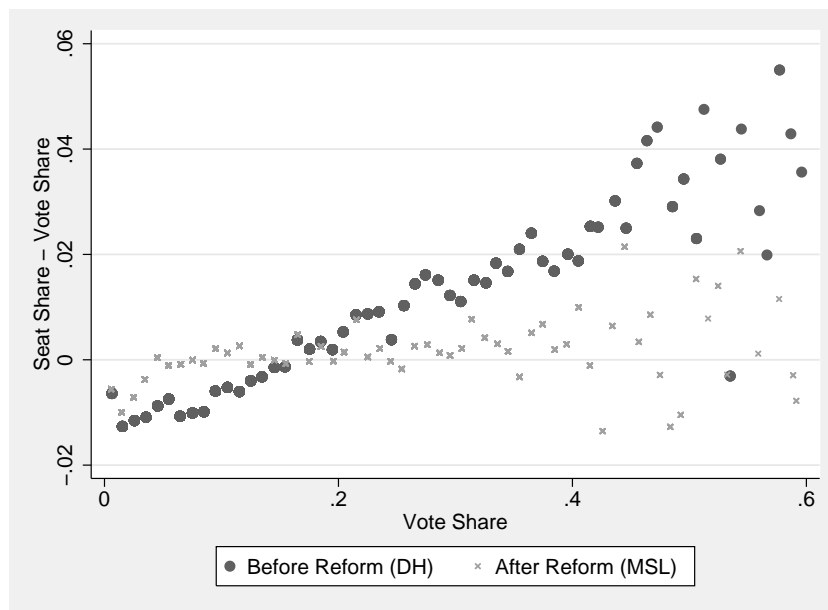
	(1)		(2)		(3)	
	Pre-reform		Post-reform		Difference	
	Mean	SD	Mean	SD	Estimate	SE
Main Outcomes						
NoP	6.000	(1.601)	6.199	(1.601)	0.199***	(0.048)
ENoP	4.112	(1.016)	4.372	(1.126)	0.261***	(0.035)
Index	2.913	(1.108)	2.406	(0.853)	-0.506***	(0.062)
Underlying Factors						
Parties Running	6.346	(1.892)	6.726	(2.021)	0.380***	(0.053)
ENoP ^{Votes}	4.397	(1.085)	4.443	(1.140)	0.046	(0.035)
Number of Joint Lists	0.085	(0.280)	0.067	(0.271)	-0.018	(0.014)
Council Size	27.966	(11.089)	25.491	(9.927)	-2.475***	(0.202)
N	387		387		774	

Note: The main outcome variables are the number of party lists represented in the council (NoP), the effective number of parties (ENoP), and the Gallagher index measuring the disproportionality of the electoral system (Index). Descriptives based on municipal elections in 1999 and 2003. Data from Fiva, Halse and Natvik.¹⁸ Column (3) provides differences in mean estimates with standard errors clustered at the municipal level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Seat Share-Vote Share Curvature

To show how the change in the seat allocation formula changed the relationship between votes and seats we show the seat share-vote share curvature before and after the reform

Figure 1: Seat Share-Vote Share Curvature, Before and After Reform



Note: The figure is constructed by grouping (binning) parties together based on their vote share, using a bandwidth of 1 percentage point. The data are from municipal elections in 1999 and 2003.

in Figure 1.¹⁹

As expected, the advantage given to large parties is greater when using DH than when using MSL. A party that received 40 percent of the votes before the reform would, on average, receive a “seat share bonus” of about 2 percentage points, while it received a bonus of about half a percentage point after the reform.

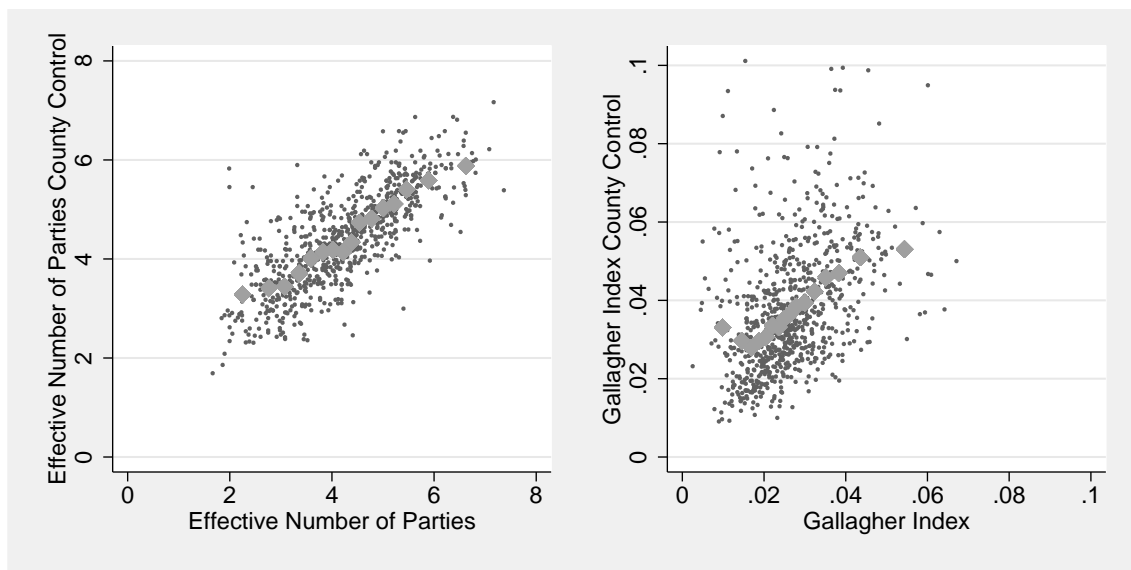
County Elections

An important part of our identification strategy is that we can use the county election returns in each municipality to control for general time trends in party support. In Appendix B, we document that there is a strong degree of overlap between patterns of voting across the two offices. Our outcomes of interest are also similar if we use the county election returns to calculate them: In Figure 2, we show the relationship between the actual municipal outcomes and the counterfactual outcomes where we use the votes

¹⁹More specifically, this is the relationship between a party’s vote share (measured on the x-axis) and the difference between seat and vote shares (measured on the y-axis).

for the county election (measured at the municipality level) to allocate the seats, but keep everything else constant. We show both the scatterplot and the binned averages of the county controls as a function of the actual outcome. For both ENoP and Gallagher’s Index there is a strong and essentially linear relationship. This provides a strong rationale for our identification strategy, which will be laid out in more detail below.

Figure 2: Municipal Outcomes and the County Control



Note: The scatterplot to the left shows the relation between the effective number of parties based on the local council and the effective number of parties of a counterfactual local council based on votes for the county election (measured at the municipal level). The scatterplot to the right shows the relation between the Gallagher index based on municipal vote and seat data and the corresponding variable for the county level voting and (hypothetical) seat data (measured at the municipal level). The data are from municipal and county elections in 1999 and 2003. The larger circles are binned averages with 50 observations in each bin.

3 Methods

The key empirical challenge with separating mechanical and psychological effects of electoral reform comes from the fact that they occur jointly. Our empirical approach addresses this issue through pairwise comparisons of actual and counterfactual seat allocation outcomes. The basic intuition behind this approach is to use counterfactual seat allocations

to answer the ‘what if’ questions that provide the foundation to separating mechanical and psychological effects of the reform. The estimate of the mechanical effect, for example, answers the question ‘What would have happened if we changed the electoral system, but kept everything else constant’. By creating a counterfactual seat allocation in which we keep everything constant we can answer this exact question.

To illustrate our empirical strategy we use Figure 3, which shows both actual (cells A and D) and counterfactual seat allocation outcomes (cells B, C and D_{99}). In each cell we show the mean values for our three main outcome variables. We return to these in Section 4 where we present the results.

The counterfactual seat allocations necessary for separating psychological and mechanical effects are constructed as follows. In cell B we apply the post reform seat allocation method, MSL, to the pre-reform vote distribution. In cell C we instead apply the pre-reform seat allocation method, DH, to the post-reform vote distribution.

In cell D, which is split in two parts, we use the actual post-reform vote distribution and seat allocation method. The difference between the left-most part (which we refer to as D) and the right-most part (which we refer to as D_{99}) is that the former uses the actual council size to distribute seats, while the latter uses the pre-reform council size. A comparison between these two parts of cell D allows us to isolate the impact of the reduction in council size.

Before we explain how we separate the mechanical and psychological effects, we turn to the total effect of the reform. This is pinned down by simply comparing actual pre- and post-reform seat allocation outcomes (A vs. D).

The mechanical effect of the reform is how the electoral outcome would have changed with the introduction of the new seat allocation method, if everything else had remained constant. Cell B captures such a ‘parallel world’. Thus, by comparing counterfactual outcome B to the actual pre-reform outcome A, we can exactly measure the mechanical effect.

Outcome B does not take into account the strategic reactions of voters and parties, and

Figure 3: Illustration of Empirical Strategy

	Outcome	DH		MSL		
1999	<i>NoP</i>	A	6.00	B	6.12	
	<i>ENoP</i>		4.11		4.37	
	<i>Index</i>		2.91		2.16	
2003	<i>NoP</i>	C	5.99	D	6.20	6.26
	<i>ENoP</i>		4.08		4.37	4.39
	<i>Index</i>		3.34		2.41	2.17

Note: The figure shows the actual pre- and post- political outcomes (A and D) and the counterfactual political outcomes (B, C and D₉₉). Reported are mean values of the number of party lists represented in the council (NoP), the effective number of parties (ENoP), and the Gallagher index measuring the disproportionality of the electoral system (Index).

a comparison of A vs. B, therefore, gives us only a part of the full picture. To understand the consequences of electoral reform we also need to quantify the psychological effects. We do this by keeping the seat allocation method constant, while allowing everything else to change. In our framework, this is done by contrasting the counterfactual outcome B, where we apply the post-reform seat allocation method to the pre-reform voting outcome, to the actual post-reform seat allocation outcome D. Thus, the total effect of reform ($A \rightarrow D$) is the sum of the mechanical effect ($A \rightarrow B$) and the psychological effect ($B \rightarrow D$).

The psychological effect can be partitioned into two subcomponents. First, we have the direct effect of political agents adjusting in response to the new system. For example, a substantial shift in votes towards smaller parties will increase the number of parties winning representation, even if we (counterfactually) use the pre-reform seat allocation method to allocate the seats. This part of the psychological effect is quantified by contrasting the actual pre-reform seat allocation outcome A with the counterfactual seat

allocation outcome C, where we apply the pre-reform seat allocation method, DH, to the post-reform vote distribution.

Strategic responses of voters and parties may also impact political outcomes indirectly by changing the mechanical effect (the impact of a lower effective electoral threshold). To see why, consider the same example as above: A shift in the vote distribution towards smaller parties, either as a consequence of strategic behavior from voters or parties, would lead to a heavier left tail of the vote distribution. The mechanical effect of changing from DH to MSL using the post-reform vote distribution will, therefore, have a larger impact on the number of parties winning representation than if we use the pre-reform vote distribution. The second component of the psychological effects is therefore to reinforce the mechanical effect. To measure this effect we compare the impact of changing the seat allocation method after the political actors responded to the reform ($C \rightarrow D$) to the unmitigated mechanical effect ($A \rightarrow B$), which is calculated under the political status quo.

Finally, we examine the impact of changes in council sizes. Strategic changes in council size may affect the political outcomes we study both directly and indirectly. The mechanical constraints of the electoral system will directly change as a consequence of changes in the council sizes. As we discuss above, this will partly offset the effect of the changes in the electoral formula. This may then, indirectly, affect the behavior of voters and parties. With our framework we cannot isolate the consequences of actions that are indirect responses to the reform. However, we can quantify how much of the total psychological effect was driven by the direct effect of reducing the council size. We do this by comparing the actual post-reform seat allocation outcome D to a counterfactual situation where the council size remained constant at the 1999 level (D_{99}).

Estimation Strategy

The basic principle of the estimation strategy is a pair-wise comparisons of the outcomes in Figure 3, in different pairs of cells, c . The regressions analysis takes the following form:

$$Y_{i,c} = \alpha_i + \beta Reform_c + \gamma Y_{i,c}^{County} + \varepsilon_{i,c}, \quad (1)$$

where $Y_{i,c}$ is an outcome variable based on the outcome (NoP, ENoP, Index) in cell c for municipality i , α_i is a set of municipal fixed effects, and $Reform_c$ is a dummy variable equal to one for the cell that corresponds to a reform cell, and zero for the comparison cell. For example, to estimate the mechanical effect of changing the electoral system from DH to MSL we define $Reform_A = 0$ and $Reform_B = 1$. Here, β is the parameter of interest capturing the effect of the electoral reform on $Y_{i,c}$. We cluster standard errors at the municipality level to allow for arbitrary correlation within each municipality.

To avoid contamination of the estimated reform effects by general changes in party support, we exploit that municipal and county government elections coincide in time and space. More explicitly, we utilize the information we have on voting behavior by the same electorate for a separate office, but where the electoral formula remained constant before and after the municipal electoral reform. Even though the seat distribution at the county level is determined by considering the entire county jointly, we exploit election returns measured at the municipal level to construct a set of control variables $Y_{i,c}^{County}$.²⁰

Thus, β captures the causal effect of the electoral reform on $Y_{i,c}$ as long as $Cov(Reform_c, \varepsilon_{i,c}) = 0$. The identifying assumption is that after conditioning on $Y_{i,c}^{County}$ there are no time varying factors (correlated with reform) that have an independent impact on $Y_{i,c}$.

As mentioned in the introduction, the closely related analysis of Blais et al. utilizes differences in electoral rules across simultaneous elections to identify psychological and mechanical effects. For this strategy to produce unbiased estimates one needs to assume that all factors affecting voter and party behavior, except the electoral rules, are similar across both elections.²¹ This assumption is likely to be violated as the strategic decisions of political agents may give rise to interaction or contamination effects across simultaneous elections. Voters might, for example, engage in electoral balancing across legislatures.

²⁰Andersen, Fiva, and Natvik (2014) study voter motivation using Norwegian data and a similar identification strategy.

²¹Blais et al., 2011.

Simultaneous elections can also affect party behavior. For example, consider the case of the Swiss simultaneous elections to the upper and lower house used by Blais et al. Here, the lower house elections are proportional, while the upper house elections are conducted in single- or two-member districts. Small parties therefore have incentives to put their best candidates in the lower house elections, since they have little chance of winning representation in the upper house election. If this is the case it would bias the estimates.

Our empirical approach, utilizing an arguably exogenous change in the electoral system, rests on a weaker identifying assumption. It is not problematic for our empirical strategy if there are omitted factors impacting the political system as long as these factors remain constant over time. Yet, interaction or contamination effects across simultaneous elections may still bias our results. For example, if electoral reform incentivizes strategic entry at the municipal level, it may increase the number of parties running at the county level as well. Such lack of independence across elections would bias our tests against finding any effect of electoral reform.²² We therefore report results both with and without county controls.

A limitation of our approach is that the total psychological effects may unfold gradually over subsequent elections.²³ Our empirical approach only captures the short-term effects, and may therefore be considered a lower bound on how electoral systems affect the strategic behavior of political agents.

4 Results

In Table 2, we present our main results. In the first row we show the estimates for the total effects of electoral reform ($A \rightarrow D$). This is followed by the estimates of the mechanical effect ($A \rightarrow B$), the total psychological effect with ($B \rightarrow D$) and without council size changes ($B \rightarrow D_{99}$), the two subcomponents of the psychological effect ($(A \rightarrow C)$ and $([C \rightarrow D] - [A \rightarrow B])$) and then finally the effect of the council size reduction ($D_{99} \rightarrow D$).

²²Cox, Rosenbluth, and Thies, 2000.

²³Taagepera, 2007.

Table 2: Decomposition of Mechanical and Psychological Effects

<i>Outcome</i>	(1) NoP	(2) NoP	(3) ENoP	(4) ENoP	(5) Index	(6) Index
Total Effect						
$A \rightarrow D$	0.199*** (0.05)	0.216*** (0.05)	0.261*** (0.03)	0.247*** (0.03)	-0.506*** (0.06)	-0.507*** (0.06)
County Control		0.185*** (0.06)		0.429*** (0.06)		3.192 (4.94)
Mechanical Effect						
$A \rightarrow B$	0.121*** (0.020)		0.251*** (0.012)		-0.751*** (0.044)	
Psychological Effect						
$B \rightarrow D$	0.075 (0.048)	0.089* (0.048)	0.010 (0.035)	0.005 (0.032)	0.245*** (0.045)	0.244*** (0.045)
$B \rightarrow D_{99}$	0.132*** (0.047)	0.144*** (0.047)	0.024 (0.035)	0.019 (0.032)	0.005 (0.041)	0.007 (0.041)
Components Psychological Effect						
$A \rightarrow C$	-0.013 (0.051)	0.007 (0.051)	-0.030 (0.035)	-0.044 (0.032)	0.432*** (0.080)	0.430*** (0.079)
$[C \rightarrow D] - [A \rightarrow B]$	0.088*** (0.028)	0.088*** (0.028)	0.039** (0.016)	0.039** (0.016)	-0.188*** (0.064)	-0.188*** (0.064)
Effect of Council Size Change						
$D_{99} \rightarrow D$	-0.057*** (0.013)		-0.014* (0.007)		0.240*** (0.030)	
<i>N</i>	774	774	774	774	774	774
County Control	No	Yes	No	Yes	No	Yes

*Note: Outcomes refer to notation presented in Figure 3. The dependent variables are the number of party lists represented in the council (NoP), the effective number of parties (ENoP) and the Gallagher index measuring the disproportionality of the electoral system (Index). The county control is computed by using municipal-level voting data for the county elections. The county elections coincide in time and space with the municipal elections, but the allocation formula in use did not change. Outcome subscripts denote the council size used to allocate the seats. Municipality fixed effects included in all specifications. Standard errors clustered at the municipality level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.*

For the mechanical effect, the county control is irrelevant since we evaluate the reform's effect for a given vote distribution. This is also the case when we evaluate the effect of the council size change. All other regressions are estimated both with and without the county control, but we only report the estimates for the county control for the total effects.

Number of Parties

The results for the number of parties (NoP) represented in the council are shown in columns 1 and 2 of Table 2. In line with the descriptive analysis, we find that the reform increased the number of party lists in the council by 0.20. This is a nontrivial effect, which is statistically significant at the 1 percent level. If no municipality increases the number of party lists by more than one, the point estimate indicates that an additional party list will be present in one out of five municipalities. The county control is also statistically significant with the expected positive sign, but including it leaves the estimate of the reform effect basically unaltered.

The mechanical and psychological effects are of a similar magnitude and give about equal contributions to the total effect. The total psychological effect ($B \rightarrow D$) is only statistically significant at the 10 percent level (and only when the county control is included). However, if we counterfactually hold the council size constant, the psychological effect almost doubles and becomes statistically significant (compare rows 4 and 5). When we separate the two components of the psychological effect (rows 6 and 7), we see that most of the effect operates through the (ex-post) mechanical effect.

The psychological effect can be driven by dynamic adjustment on the political system's supply (strategic entry) or demand side (strategic voting), or both. We cannot fully separate the two mechanisms since strategic responses of parties could take two broad forms. The first concerns the decision to run in an election. The second concerns changes in behavior given that a party decides to run (for example, increased campaigning). We can, however, split the sample according to whether the number of lists was constant, or new lists were running to shed some light on this issue. Conducting such an analysis, we

find there is only a positive psychological effect when new lists were running. This is a clear indication that the effect is primarily driven by strategic responses on the supply side.²⁴

Finally the results show that the effect of reducing the council size (row 8) is statistically significant and that it reduced the number of represented parties by 0.057 per municipality. In the aggregate this means that the reduction in council size stopped about 22 party lists from getting into a municipal council (387×0.057).

Effective Number of Parties

As reported in column 3 of Table 2, the effective number of parties increased by 0.26 as a consequence of the electoral reform. This corresponds to about one-fourth of a standard deviation. This effect is statistically significant at the 1 percent level, and basically remains unaltered if we include the county control (column 4). This suggests that electoral balancing across the two elections does not seem to be a source of bias. The positive effect of the county control implies a positive association between the fragmentation of the local council and the fragmentation of a counterfactual local council based on votes for the county election (measured at the municipal level). Since the electoral reform estimate barely changes when we include a highly relevant control variable it is unlikely to change much if we could perfectly control for changes in party support unrelated to electoral reform. This contention is further supported by a set of placebo regressions using information from elections held in 1995 and 2007 (see Appendix C).

As expected from Figure 3 we see that the mechanical effect is driving the total effect almost in its entirety. The shift in the voting distribution towards smaller parties increases ENoP, but the effect is small and statistically insignificant. The reduction in council size does, however, contribute to a slightly lower ENoP level, but relative to the

²⁴Entry effects are primarily driven by two parties, the Socialist Left Party (SV) and the Progress Party (FrP). In our sample, SV and FrP were running in 61 and 59 percent of the municipalities before the reform. After the reform they were running in 75 and 73 percent of the municipalities, respectively. The fraction of municipalities where these parties obtained representation also increased by similar magnitudes.

total effect the contribution is small.

The reason for why the mechanical effect overwhelms the psychological effects is evident in Figure 1. Under MSL, only the small parties around the threshold for the first seat are disadvantaged. Under DH all small parties are disadvantaged. This results (mechanically) in a much more even distribution of seats under MSL than under DH. The comparable shift in the vote distribution is much smaller. Thus, the psychological effect will only impact a small subset of the parties, while the mechanical effect impacts all parties.

Gallagher's Disproportionality Index

The results for the disproportionality index are provided in column 5 and 6 of Table 2. The total effect of about -0.5 percentage points corresponds to almost one-half a standard deviation decrease in disproportionality. The county control is statistically insignificant.

For the disproportionality index the psychological and mechanical effects go in opposite directions. Given that DH causes a systematic divergence between the seat share and the vote share (evident in Figure 1), the mechanical effect contributes to a reduction in disproportionality. The psychological effects go in the opposite direction. This dampening effect is driven by the reduction in council size (compare row 4 and row 5, see also row 8). Given that the average deviation between the seat share and vote share will automatically increase as we reduce the council size, this result is naturally what we would expect. The shift in the voting distribution towards smaller parties does not contribute to a change in the disproportionality index. This is because there is only a weak relationship between the vote share and the difference between the seat share and vote share under the MSL (cf. Figure 1).

Magnitude of Effects: Historical Context

In 1953, a similar electoral reform to the one that we study was implemented at the national level in Norway. Since the national electoral reform was a compromise between

the Labor Party and the opposition parties one should be cautious about giving changes in outcomes over time causal interpretations.²⁵ They are nonetheless useful for putting our results in context. Lijphart found that the national reform of moving from DH to MSL in Norway in the 1950s led to an increase in the effective number of parties of 0.35 and a decrease of the disproportionality index of 4.15 percentage points.²⁶ The results we find for the effective number of parties are thus comparable to the national reform, while the effect on the proportionality of the election system is much smaller. One explanation for this difference is that the pre-reform level of disproportionality was higher at the national than at the municipal level.

Council Size Reductions

As seen above, the reductions in council size did play an important role in reducing the impact of the reform. However, looking at the full sample we get the average across those places where there was a reduction in the council size and where there was not. To assess the impact of the reduction we should turn to the places where there actually was a reduction in their council size. If these reductions were intended to reduce the impact of the reform, we should also expect the reductions to have a clear effect. In Table 3, we will present the most important estimates, including the total effect ($A \rightarrow D$), the mechanical effect ($A \rightarrow B$) and the total psychological effect ($B \rightarrow D_{99}$). Each effect is estimated separately for the municipalities that kept the council size constant (columns 1, 3, and 5) and those that reduced it (columns 2, 4, and 6). In this analysis we keep the council size constant in order to isolate the part of the psychological effects that comes from the reaction of parties and voters.

We first turn to the results for the number of represented parties. Where the council size was constant, the total effect was an increase of almost 0.3 parties per municipality,

²⁵While the opposition wanted to change from DH to a largest remainder method, the Labor party wanted to keep the DH method. Labor MPs explicitly argued that an executive bonus for the largest party was necessary in order to provide stable government (Aardal, 2002, p. 191). As a compromise between Labor and the opposition the MSL method was implemented (cf. Rokkan, 1970, p.158-161).

²⁶Enacting the same reform in Sweden led to an increase of 0.05 in the effective number of parties and a 1.15 reduction in the disproportionality index (Lijphart, 1994).

Table 3: Electoral Reform Effects by Council Size Reductions

Outcome	(1) NoP	(2) NoP	(3) ENoP	(4) ENoP	(5) Index	(6) Index
$A \rightarrow D$	0.286*** (0.058)	0.048 (0.081)	0.260*** (0.044)	0.261*** (0.057)	-0.719*** (0.081)	-0.155* (0.090)
$A \rightarrow B$	0.133*** (0.028)	0.103*** (0.025)	0.249*** (0.016)	0.255*** (0.017)	-0.763*** (0.060)	-0.731*** (0.063)
$B \rightarrow D_{99}$	0.149** (0.061)	0.103 (0.072)	0.010 (0.044)	0.047 (0.057)	0.049 (0.058)	-0.068 (0.052)
$D_{99} \rightarrow D$		-0.158*** (0.033)		-0.040** (0.020)		0.644*** (0.066)
N	482	292	482	292	482	292
County Control	No	No	No	No	No	No
Constant Council	Yes	No	Yes	No	Yes	No

Note: For explanatory details, see Table 5.

while there was no increase in municipalities that reduced the council size. Council size reductions prevented a greater number of represented parties after the reform. There was essentially no difference in the mechanical effect between the two groups (second row). The psychological effect is about 50 percent larger where the council size was not changed (third row). Although the difference is not statistically significant, it suggests that voters and parties were more responsive when the implicit electoral threshold was reduced the most. Finally, in the municipalities where there was a reduction, the total impact was to reduce the average number of parties by 0.16.

For the effects on the effective number of parties we do not see much of a difference between the places where there was a reduction and those where there was not. This reinforces the findings from Table 2 that the council size reduction only had a minor impact on this outcome.

For the Gallagher Index the reduction in council size does have an important impact. When we compare the total effects, we see that the council size reductions essentially wipe out the effect of the reform. This is due to the fact that when there are fewer seats

to distribute the average deviation between the seat share and vote share becomes larger. Again, there is no difference in the mechanical effect between the two groups. As we saw in Table 2 the shift in the vote distribution did not have any noticeable impact on the disproportionality, which is also the case when we look at the two groups of municipalities individually ($B \rightarrow D_{99}$). In municipalities where there was a reduction, the total impact was almost of the same size of the mechanical effect, but of the opposite sign.

5 Conclusion

Ever since Duverger (1954) there has been a long standing interest in the mechanical and psychological effects of electoral systems. In this paper, we propose a framework for uncovering the causal effects of electoral reform, which allows us to ascertain the relative magnitudes of these two effects. Using this framework we show how the psychological effect may operate through the mechanical effect of the reform.

Our application is based on a nationwide municipal electoral reform in Norway, which changed the seat allocation method from D'Hondt to Modified Sainte-Laguë. The total effects of the electoral reform are all in line with what we expect: the proportionality of the seat allocation, the number of parties represented and the effective number of parties all increase. For the effective number of parties the mechanical effect drives the total effect. But for the two other outcomes the psychological effects also play an important role.

For the proportionality of the seat allocation the mechanical and psychological effects go in opposite directions. The mechanical effect increases the proportionality of the system, while reductions in the size of the local council, a choice variable for pre-reform incumbents, contribute to reduced proportionality. This psychological effect served to dampen the total effect on the proportionality of the seat allocation.

While the mechanical effects of electoral reforms can be observed instantaneously, the full psychological effects may unfold gradually over subsequent elections. We find that

the reform led to an extra party being represented in about one out of five councils in the first set of elections succeeding the reform. The strategic behavior by political elites contribute to about half of this effect. The other half is due to the mechanics of the seat allocation formula. Voters appear not to have changed their behavior in anticipation of the new electoral systems' mechanical constraints. We cannot, however, rule out that such responses occur in ensuing elections.

The credibility of our empirical strategy is bolstered by the fact that the results remain unaltered when controlling for voting outcomes for county governments elected simultaneously. This suggests that the results are not likely to be driven by changes in voter sentiment between the pre- and post-reform elections.

Our analysis demonstrates that to understand the consequences of electoral reform, one needs to take into account not only mechanical effects but also the strategic responses political agents may make in anticipation of these, and how these forces interact.

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