The Contraction Effect: How Proportional Representation Affects Mobilization and Turnout

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A substantial body of research examines whether increasing the proportionality of an electoral system increases turnout, mostly based on cross-national comparisons. In this study, we offer two main contributions to the previous literature. First, we show that moving from a single-member district system to proportional representation in multimember districts should, according to recent theories of elite mobilization, produce a contraction in the distribution of mobilizational effort across districts and, hence, a contraction in the distribution of turnout rates. Second, we exploit a within-country panel data set based on stable subnational geographic units before and after Norway's historic 1919 electoral reform in order to test various implications stemming from the contraction hypothesis. We find significant support for the predictions of the elite mobilization models.

A substantial body of research examines whether increasing the proportionality of seat allocation rules in an electoral system increases voter turnout (e.g., Blais 2006; Blais and Carty 1990; Eggers 2015; Jackman 1987; Ladner and Milner 1999; Powell 1980, 1986). The verdict has been characterized in widely different ways, with some (e.g., Selb 2009, 527) insisting that "evidence that turnout is higher under proportional representation (PR) than in majoritarian elections is overwhelming," and others (e.g., Herrera, Morelli, and Palfrey 2014, 4) opining that the empirical results are "rather mixed." In a meta-analysis of 14 studies, Geys (2006) reports that 70% of the estimated correlations between proportionality and turnout are significantly positive.

Most of the studies surveyed by Geys conduct cross-sectional analyses of aggregate turnout levels in a relatively small sample of industrialized democracies; two focus on subnational units, and a few include larger samples of countries. In this study, we offer two main departures from the previous literature.

First, we tie our predictions about the turnout effects of electoral systems to recent theoretical work on elite mobilization (Cox 1999; Herrera et al. 2014). This work predicts that a transition from single-member districts (SMDs) to multimember districts with PR will produce two off-setting effects. In geographic units where the prereform SMDs were hotly contested, the introduction of PR will result in a decrease in competitiveness and turnout; in the less competitive prereform units, both will increase. Thus, the distribution of turnout will *contract* toward an intermediate level. Depending on how many prereform SMDs are hotly contested, mean turnout may increase or decrease.

Second, we exploit what is essentially a panel data set—a series of observations on stable subnational geographic units before and after the 1919 Norwegian electoral system reform from a two-round runoff SMD system to a multimember district PR system. Our within-country analysis allows us to avoid relying on cross-sectional comparisons that may be plagued by multiple confounds. Moreover, with our data, we

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can measure the competitiveness of each district before and after the switch to PR, which allows us to test our predictions about the contraction effect of electoral reform on turnout.

We find substantial empirical support for the contraction effect predicted by the elite mobilization models: following the Norwegian reform, both competitiveness and turnout declined in the most competitive SMDs, falling toward the PR level, but increased in the noncompetitive SMDs, rising toward the PR level. Aggregating across districts, mean turnout increased (because most of the prereform SMDs were noncompetitive), while cross-district variance declined. Aside from a few observations made long ago by Gosnell (1930, 183) and Tingsten (1937, 224–25), our study is the first to provide systematic empirical evidence of the contraction effect produced by reforming electoral systems from SMD to PR.

PROPORTIONALITY AND TURNOUT

Multiple studies using cross-national comparisons of advanced industrialized democracies have found that mean turnout is higher under PR electoral systems than under SMD systems (e.g., Blais and Carty 1990; Blais and Dobrzynska 1998; Franklin 1996; Powell 1980). Within the set of industrialized democracies, the most widely acknowledged exceptions to the rule that turnout is higher under PR are Switzerland (relatively low turnout, despite PR) and prereform New Zealand (relatively high turnout, despite plurality rule). These exceptions suggest the importance of other variables or institutional arrangements—for example, the disaggregation of the electoral calendar (high in Switzerland, low in prereform New Zealand)—many of which are country specific.[1]

A few notable studies look at before-and-after evidence from within countries that experienced electoral reform. For example, Gosnell (1930) and Tingsten (1937) observe that aggregate turnout increased in Germany and Norway, respectively, following the adoption of PR. In a more recent study, Karp and Banducci (1999) find that turnout in New Zealand increased temporarily following the switch from SMD to a mixed-member proportional (MMP) system in 1993.

A handful of other studies look at subnational variation within countries. For example, Ladner and Milner (1999) find that turnout is higher in Swiss communes that use PR. Similarly, Bowler, Brockington, and Donovan (2001) find higher turnout in US municipalities that use cumulative voting rather than plurality rule. Eggers (2015) uses a regression discontinuity design applied to municipal elections in France—where towns with populations above 3,500 must use PR rather than a type of plurality system. He finds a slight (1-percentage-point) increase in mean turnout under PR, and a lower level of variance in turnout across PR municipalities than across plurality-rule municipalities.

Three basic, and partially related, explanations have been advanced in the existing literature to explain higher turnout under PR (Blais and Carty 1990). The first explanation is that, especially at higher levels of district magnitude, the translation of votes into seats is less distorted, thereby increasing voter feelings of efficacy. In a survey of voters before and after New Zealand's electoral reform, for example, Banducci, Donovan, and Karp (1999) find an increase in voters' perceptions of the efficacy of their votes under the MMP system, especially among supporters of smaller parties.

A second explanation is that PR is more permissive to the entry of smaller parties (Cox 1997; Duverger 1954), so voters have less reason to abstain for lack of options matching their preferences (especially since they need be less concerned that their votes will be "wasted" on losing parties) (e.g., Cox 1997; Jackman 1987; Ladner and Milner 1999; Powell 1986). However, although many studies have found a relationship between PR and the number of parties entering competition (e.g., Cox 1997; Eggers 2015), the relationship between the number of parties and turnout has little to no empirical support (e.g., Blais and Aarts 2006; Brockington 2004; Grofman and Selb 2011). This may be because more parties can lead to coalition governments and less clarity in voter choice (Jackman 1987).

The third basic explanation is that PR elections tend to be more competitive (Jackman 1987). Early rational choice work argued that close elections increase the chance that a single voter might become "pivotal" in determining the outcome and thus increase voter turnout (Downs 1957; Riker and Ordeshook 1968; Tullock 1968). However, after the realization that these pivotal voter theories predict vanishingly small turnout rates in large electorates (Palfrey and Rosenthal 1985), several scholars—beginning with Morton (1987, 1991) and Uhlaner (1989)—attempted to resolve the "paradox of voting" by focusing instead on the mobilizational efforts of politicians and interest groups (e.g., Aldrich 1993; Cox and Munger 1989; Shachar and Nalebuff 1999). The basic argument is that elite actors in close races might rationally invest in mobilizing voters, while those voters might rationally respond to such mobilization by turning out to vote.

Subsequent mobilization models, such as Cox (1999) and Herrera et al. (2014), have explored how elite incentives to mobilize are conditioned by electoral rules. In the next section, we make our own contribution to this literature by

1. The relationship between proportionality and turnout is also less consistent in new and developing democracies (e.g., Blais and Aarts 2006; Fornos, Power, and Garand 2004; Gallego, Rico, and Anduiza 2012; Kostadinova 2003; Pérez-Liñán 2001).

exploring the potentially heterogeneous effects of a PR electoral reform on turnout.

ELITE MOBILIZATION AND THE CONTRACTION EFFECT

We use an elite mobilizational model based on Herrera et al. (2014) to consider the effect of a hypothetical reform from plurality rule in SMDs to "perfect" PR. The basic logic of the model is as follows: imagine a situation where two parties, A and B, compete for office, and voters come in two types, with a proportion q supporting party A and a proportion $(1 - q)$ supporting party B. A party will increase its mobilizational effort as long as the marginal benefit to doing so (an expected increment in seats) outweighs the marginal cost (resources expended). The cost of mobilization is assumed to be some convex function of mobilizational effort. The benefits of increased effort depend on (i) how effort translates into votes and (ii) how votes translate into seats—that is, the electoral rule (Cox 2015).

In a plurality-rule SMD system, when the electorate is not evenly split ($q \neq 1/2$) the ex ante leading party is the ex post leading party in equilibrium.[2] In such contests, the probability that an additional vote will be pivotal in determining the election result will decline rapidly in large populations of voters. This will discourage any mobilizational effort by elites to get out the vote. However, if the electorate is evenly split ($q = 1/2$), pivot probabilities decline more slowly with the expected number of voters, and the marginal benefit of mobilizational effort to party A (or B) is higher.

In the perfect PR case, the marginal benefit of raising the level of mobilization depends less on q. Regardless of the breakdown of partisan preferences in a given district, each additional vote for party A yields a positive finite increase in the expected seats for party A. Putting these results together, Herrera et al. (2014) show that an SMD system induces higher turnout if and only if the election is expected to be close; PR induces higher turnout than SMD systems in less competitive races.

Following this reasoning, we can explain the contraction effect produced by electoral reform from SMD to PR with some additional notation. Let A and B compete in prereform SMDs of equal size, indexed by $j = 1, \ldots, J$. Denote the expected margin of victory in a prereform district j by $M_{j,\text{pre}}$; the expected level of mobilizational effort by each party by $E_{j,\text{pre}}$; and the expected turnout of voters in district j by $T_{j,\text{pre}}$. After the transition to PR, let $M_{j,\text{post}}$ denote the margin of victory in the PR district which contains prereform district j. Let the expected level of mobilizational effort in the area corresponding to prereform district j be $E_{j,\text{post}}$ and the expected turnout be $T_{j,\text{post}}$.

Initially, we can think of the PR system as collapsing all J prereform SMDs into a single J-seat nationwide district, with the allocation of seats based on some method of PR.[3] We shall also imagine that the party system remains fixed (just two parties), and that voters' preferences for the two parties also remain fixed. Finally, we imagine that year-specific influences on turnout, such as rainfall affecting the cost of voting, are comparable before and after reform.

With these assumptions, the Herrera-Morelli-Palfrey model predicts that the distribution of expected margins of victory across the SMD areas $j = 1, \ldots, J$ will contract following reform. For example, if there is zero underdog compensation, then $M_{j,\text{pre}} = |2q_j - 1|$. Meanwhile, $M_{j,\text{post}} = M$ for all j under nationwide PR. We shall denote this postreform margin by M_{PR}, and define it as the smaller of the following two positive numbers—(a) how many votes party A would need to gain, in order to gain another seat (in the nationwide PR district); and (b) how many votes party B would need to gain, in order to gain another seat—expressed as a share of all votes cast. With some mild assumptions about the prereform SMDs—namely, $\min\{q_j\} < 1/2 < \max\{q_j\}$ and $q_j = 1/2$ for some j—it follows that $0 = \min\{M_{j,\text{pre}}\} < M_{PR} < \max\{M_{j,\text{pre}}\}$. In other words, margins in the SMDs can vary widely, from razor thin in the swing districts to wide in the safe districts; but there is only one margin after the transition to nationwide PR, and it will be intermediate between the prereform margins experienced in the swing and safe districts. The model thus predicts a contraction of competitiveness when a system reforms from SMD to PR:

- (C1) Competitiveness in the most competitive prereform SMDs (for which $M_{j,\text{pre}} < M_{PR}$) will decrease toward the postreform level, M_{PR}.
- (C2) Competitiveness in intermediate prereform SMDs (for which $M_{j,\text{pre}} = M_{PR}$) will remain at the postreform level, M_{PR}.
- (C3) Competitiveness in noncompetitive SMDs (for which $M_{j,\text{pre}} > M_{PR}$) will increase toward the postreform level, M_{PR}.

2. Herrera et al. (2014) also consider an "underdog compensation effect," namely, that supporters of the party that is expected to lose will exhibit higher turnout rates than supporters of the party that is expected to win. If the underdog compensation effect is only "partial," then it will not fully compensate for the ex ante advantage of the leading party.

3. For the purposes of the model, the seat allocation formula under the PR system (e.g., D'Hondt, Sainte-Laguë, etc.) is not important.

Note that the further a prereform district's margin is from the postreform level, the bigger its adjustment in expected competitiveness will be.

When pre- and postreform years are otherwise comparable, we can characterize changes in mobilization and turnout as follows. First, in sufficiently competitive prereform SMDs—those for which $M_{j,\text{pre}} < M_{PR}$—expected prereform mobilization and turnout will be higher than in the same area postreform. That is, $E_{j,\text{pre}} > E_{j,\text{post}}$ and $T_{j,\text{pre}} > T_{j,\text{post}}$. The intuition is that mobilization is driven by how close the contest is (or is expected to be). In the prereform era, district j may be a "swing" seat closely contested by the two parties and thus heavily mobilized. After the reform, the parties' incentives to mobilize in the same area will hinge on how close the contest is for the last-allocated seat in the nationwide district (Selb 2009). While that last-allocated seat will typically be closely contested, given nationwide PR, expected margins in swing SMDs will be even smaller (C1). Second, in prereform SMDs such that $M_{j,\text{pre}} = M_{PR}$, the expected mobilization and turnout levels will be the same before and after reform. That is, $E_{j,pre} = E_{j,\text{post}}$ and $T_{j,\text{pre}} = T_{j,\text{post}}$. Third, in all other prereform SMDs—those for which $M_{j,\text{pre}} > M_{PR}$—expected prereform mobilization and turnout will be lower than in the same area postreform. That is, $E_{j,\text{pre}} < E_{j,\text{post}}$ and $T_{j,\text{pre}} < T_{j,\text{post}}$.[4]

Putting these three predictions together, the elite mobilization model also predicts a contraction effect on turnout in the prereform SMDs toward the postreform level:

- (T1) Turnout in competitive prereform SMDs (for which $M_{j,\text{pre}} < M_{PR}$) will decrease toward the postreform level, T_{PR}.
- (T2) Turnout in intermediate prereform SMDs (for which $M_{j,\text{pre}} = M_{PR}$) will remain at the postreform level, T_{PR}.
- (T3) Turnout in noncompetitive SMDs (for which $M_{j,\text{pre}} > M_{PR}$) will increase toward the postreform level, T_{PR}.

Note that the further a prereform district's margin is from the postreform level, the larger its adjustment in expected turnout will be.

We should also note that the turnout contraction effect can be obscured when pre- and postreform years differ systematically. For example, suppose the introduction of PR coincides with a large uniform reduction in the cost of voting. In this case, a hotly contested prereform district should experience a decline in expected margin and, hence, in mobilizational effort. Yet, this decline in mobilization would be offset by the concomitant decline in voting costs. A large enough decline in costs would mean that even the most closely contested prereform districts might exhibit a turnout increase. Whether shifts in the cost of voting (or other year-specific effects) swamped the turnout contraction effect in Norway is an empirical issue on which our results below will shed some light. The theoretical point is just that, when year effects are unchanged, we should observe a contraction of competitiveness and turnout toward the PR level.

The model also yields two predictions about aggregate turnout effects:

- (A1) Nationwide mean turnout will decrease if the fraction of competitive prereform SMDs, κ, exceeds a threshold K; will remain the same if $\kappa = K$; and otherwise will increase.
- (A2) The cross-SMD variance in turnout will decrease, as long as $\kappa \in (0, 1)$.

The first prediction (A1) is a straightforward consequence, although one cannot predict the precise value of K. The intuition of (A2) is that, under PR, the areas corresponding to the previous SMDs are all equally competitive postreform (M_{PR}). Their competitiveness is determined by the contest for the last-allocated seat in the nationwide district in which they all reside. Other time-invariant turnout-relevant features of these areas are held constant. Thus, we expect a reduction in postreform variance.

The variance reduction hypothesis (A2) has been previously articulated by Cox (1999) and empirically investigated by Selb (2009) and Eggers (2015). We hope to contribute to this growing line of investigation. Note, however, that our contraction hypothesis is sharper than the variance reduction hypothesis which it entails. First, a reduction in variance does not imply a contraction. It would be possible, for example, for the tails of the distribution of turnout to pull in, leaving the middle of the distribution unaltered. This would produce a reduction in variance but would not be consistent with the contraction hypothesis. Second, if the contraction hypothesis is valid, then one should be able to identify a "contraction point" (a specific turnout rate) and show that the further away from this turnout rate a district was prior to reform, the more it contracts toward that level postreform.[5] For safe districts ($M_{j,\text{pre}} > M_{PR}$), the larger $M_{j,\text{pre}}$ is, the bigger the increase in competitiveness and hence in expected turn-

4. We do not directly measure mobilization. However, in a recent study, Rainey (2015) makes a similar argument that mobilization will be higher in competitive SMDs than under PR, and finds some cross-national survey evidence (measured as candidate contact) to support the argument.

5. This does require unchanging year effects from before to after reform.

Table 1. Model versus Empirics

	Model Prereform	Model Postreform	Empirics Prereform	Empirics Postreform
Number of parties	2	2	About 3	About 5
Number of districts	J	1	126	29
District magnitude	1	J	1	3 to 8
Seat allocation method	Plurality	"Perfect PR"	Runoff	D'Hondt

out. For hotly contested districts ($M_{j,\mathrm{pre}} < M_{PR}$), the smaller $M_{j,\mathrm{pre}}$ is, the bigger the decrease in competitiveness and hence in expected turnout. This implies that, if we regress the change in turnout in the area corresponding to a prereform district on that area's prereform margin, we should find a positive coefficient—a prediction we test in our empirical analyses that follow.

OUR EMPIRICAL CASE: NORWAY, 1909–27

From 1906 to 1918, members of the Norwegian Storting (parliament) were elected in SMDs with a two-round runoff system. If a candidate secured a majority of votes in the first round, he or she would be elected. Otherwise, a second round was held in which the candidate with a plurality of votes would get the seat. The runoff system was unusual in that there were no restrictions on candidate entry in the second round—even a candidate who did not compete in the first round could do so in the runoff. The average number of candidates competing in the first and second round was 3.4 and 2.8, respectively (Fiva and Smith 2016).

Male suffrage (for those 25 years and above) was implemented in 1898.[6] Female suffrage was gradually extended during the first decade of the twentieth century, and universal suffrage was implemented in 1913. With the expansion of suffrage, support for the socialist Labor Party (Det Norske Arbeiderparti) increased, but the SMD system resulted in the party's consistent underrepresentation. In part as a strategy of socialist "containment" similar to the pattern in many other European democracies in the early twentieth century (Blais, Dobrzynska, and Indridason 2005; Boix 1999; Rokkan 1970),[7] the nonsocialist parties in the Norwegian parliament conceded in 1919 to change the electoral system to a multimember PR system using the D'Hondt seat allocation formula.[8] Our empirical analysis is based on four parliamentary elections preceding this reform (1909, 1912, 1915, and 1918) and three elections following the reform (1921, 1924, and 1927).[9] Our primary aim is to quantify how the electoral reform affected voter turnout in the short run. The 1918 and 1921 elections are therefore of particular interest, but we will also explore alternative specifications.

Table 1 summarizes the key differences between the electoral reform assumed in our theoretical model and the electoral reform experienced in Norway. We can think about extending the predictions (C1)–(C3) and (T1)–(T3) for plurality rule to two-round runoff with the aid of two assumptions. Assumption 1 is that second-round contests are at least as closely contested as a counterfactual plurality contest in the same district would have been. This assumption seems reasonable because second-round contests occur only if there is enough competition to force a second round. Thus, second-round elections should be particularly likely to be "competitive" for purposes of prediction.[10] Assumption 2 is that first-round contests that someone wins are no more closely contested than a counterfactual plurality contest in the same district would have been. This seems reasonable since, if someone wins the first round, that same person would likely win the

6. The voting age was lowered from 25 to 23 in 1920.

7. See also Cusack, Iversen. and Soskice (2007) for an alternative explanation. In addition to Norway, Austria (1907–19), France, Germany, Italy, the Netherlands (apart from urban districts), and Switzerland (three rounds until 1900) also switched from two-round systems to PR. Belgium, Luxembourg, and the urban districts of the Netherlands switched from multimember runoff systems to PR. Denmark, Iceland, pre-independence Ireland, Spain, and Sweden switched from single-round plurality to some form of PR (Boix 1999).

8. See Aardal (2002) for a detailed overview. In the 1930–45 period, parties could team up into joint electoral cartels (*listeforbund*). Voters would cast their votes for the individual party lists, but the allocation of seats was based on the total sum of votes cast for the participating parties in the cartel. In 1953, the D'Hondt method was replaced by a Modified Sainte-Laguë seat allocation formula, which mechanically produces a more proportional seat allocation outcome (Fiva and Folke 2016). Adjustment seats were introduced in 1989, further increasing the proportionality of the system.

9. We exclude the 1906 election from our analysis due to the lower quality of data for that first election. We end our panel data set in 1927 to avoid complicating the analysis with the introduction of electoral cartels in 1930.

10. The literature has not provided a full analysis of mobilizational incentives in two-round SMD elections. However, consistent with assumption 1, several studies find that closer competition in the first round tends to result in increased turnout in the second round (De Paola and Scoppa 2014; Fauvelle-Aymar and François 2006; Garmann 2014; Indridason 2008), including in the case of Norway (Fiva and Smith 2016).

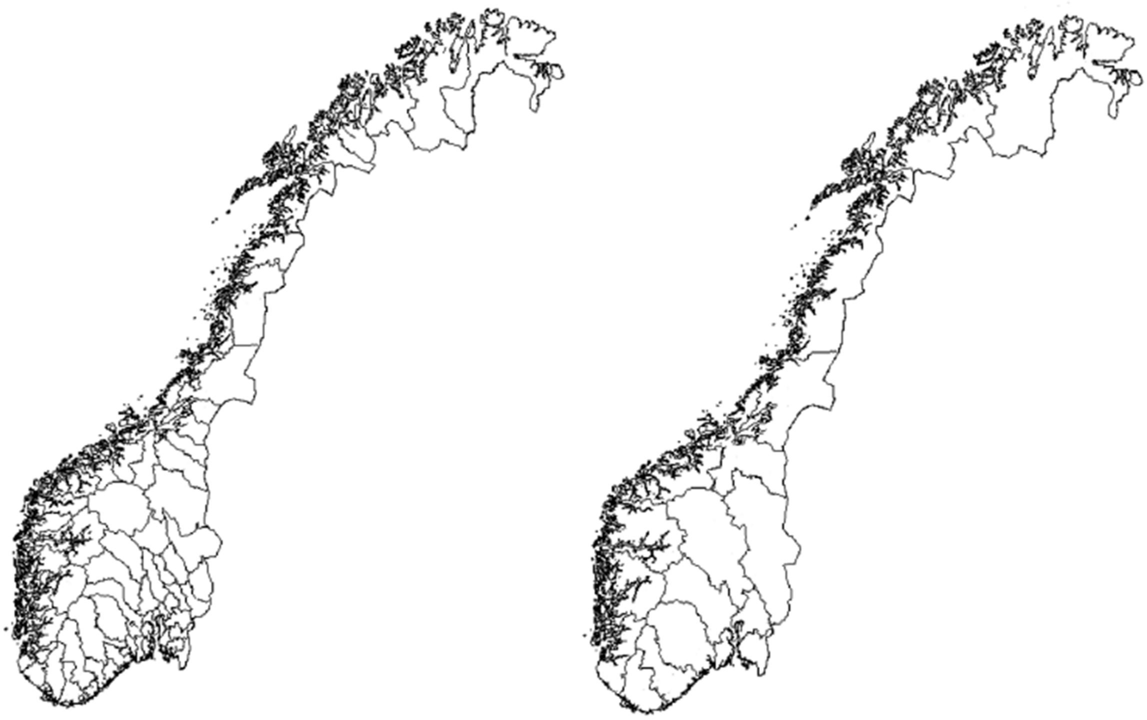

Figure 1. Prereform SMD and postreform PR district boundaries. The map on the left shows the prereform SMD boundaries in 1918; the map on the right shows the postreform PR district boundaries in 1921. Note that some urban districts are too small to be visible in the maps.

plurality contest; and the other candidates' incentives to coordinate are thus relatively weak regardless of the electoral rules.

If these assumptions hold, then a contraction effect is weakly more likely when a country transitions from a two-round runoff system to PR than when it transitions from a single-round, plurality-rule system to PR.[11] If both assumptions are reversed, then contraction is more likely to be observed in plurality-to-PR reforms than in runoff-to-PR reforms. Finally, if exactly one assumption is false, then we can no longer say which kind of reform is more likely to generate a contraction (but we can still say that a contraction is theoretically possible under both).

The maps in figure 1 show the prereform SMD and postreform PR district boundaries. In the 1909, 1912, and 1915 elections, there were 123 SMDs. In 1918, three additional districts were established. After the electoral reform, the total number of seats increased from 126 to 150. At the same time, the number of districts was reduced from 126 to 29. Our analysis is based on municipality-level election data provided by Statistics Norway. In the period we study, about 700 municipalities existed. Since municipalities map into SMDs, and SMDs map into PR districts, these data allow us to construct a panel data set covering the 1909–27 period based on the prereform district structure, shown in the left panel of figure 1.

Most of the SMDs covered multiple municipalities. However, the most populous municipalities contained multiple SMDs. The capital, Oslo (Kristiania), for example, contained five SMDs. Since we only have postreform election outcomes measured at the municipality level, we exclude 19 SMDs that did not encompass the entire municipality. In addition, we exclude all districts that did not contain the same set of municipalities over the entire prereform period (12 SMDs), and districts that contained municipalities that ended up in separate multimember districts after reform (three SMDs). Our final data sample is a balanced panel of 92 units covering seven elections. The 92 units map into 22 postreform PR districts.[12]

An editorial in the conservative *Aftenposten* newspaper on the day of the 1921 election illustrates that elites were well aware of the new mobilizational incentives under PR, including the need for secondary mobilization: "In today's election, all votes count equally no matter where in the city [Oslo/Kristiania] they are cast. The city is now one electoral district and not five, as previously. It is necessary that all people who share our opinion, east and west, head to the polls. Moreover, it is not enough that every one of you vote, you should also encourage all the people you know to do

11. This assumes that we use final-round turnout as, in fact, we do.

12. In our sample, the average electorate population increases from 9,168 to 48,765 between 1918 and 1921. The average number of eligible voters per Storting representative remains, however, quite stable (8,540 in 1921).

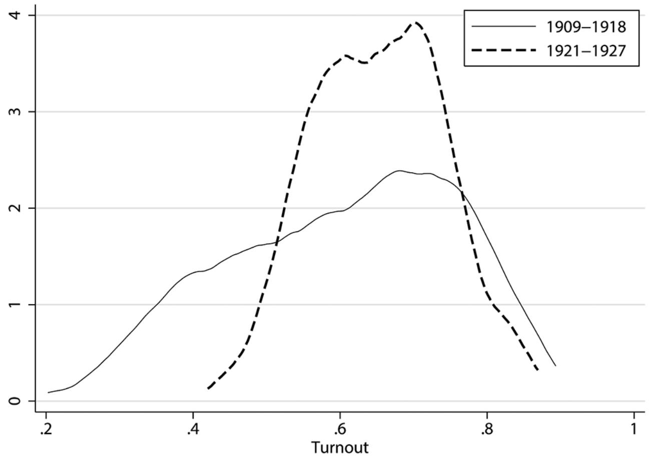

Figure 2. Kernel density plot of voter turnout, pre- and postreform. The figure shows separate kernel density plots (Epanechnikov kernel with optimal bandwidth) of voter turnout in the pre- and postreform period. Two-round elections were used from 1909 to 1918, proportional representation from 1921 to 1927. The data set is based on the prereform district structure.

the same." Additionally, the newspaper reminded readers to "vote today, and only today—there is no runoff!"

Based on the elite mobilization models and the contraction effect discussed above, we expect to observe heterogeneous effects on turnout at the district level depending on the competitiveness of prereform districts: very competitive prereform districts should experience a decrease in competitiveness (C1) and turnout (T1) following reform, while less competitive districts should either experience no effect (C2, T2), or an increase in competitiveness (C3) and turnout (T3). At the aggregate level, we expect mean turnout to increase as long as safe districts are sufficiently common in the prereform period (A1) and variance to decrease (A2). We begin our empirical analysis with these aggregate-level predictions, as they are the most straightforward.

AGGREGATE EFFECTS

We measure voter turnout by the ratio of approved votes to eligible voters in the final round. In other words, for the prereform period, we use the second-round turnout if two rounds were held; otherwise we use first-round turnout. In our sample, 45% of elections were decided in the first round. For the postreform period, there is only one round of voting.

Figure 2 shows kernel density plots of voter turnout before (solid line) and after (dashed line) the electoral reform.[13] Mean voter turnout was 60% in the prereform period and 65% in the postreform period. This indicates that the fraction of competitive prereform SMDs κ was below the theoretical threshold K at which the introduction of PR would actually result in a decrease in aggregate turnout.

The box-and-whisker plots in figure 3 illustrate the distribution of voter turnout over time in the seven elections in our sample. Together, figure 2 and figure 3 give clear support for the predictions that PR increases mean turnout (A1) and decreases cross-district variance (A2). Focusing on the two elections immediately before and after the reform, we find that mean turnout increased from 58% in 1918 to 65% in 1921, and the standard deviation of turnout fell from 15 percentage points to 9 percentage points.

THE CONTRACTION EFFECT

The graphical analyses support the aggregate-level predictions and provide visual evidence that the distribution of turnout contracted. In this section, we explore the contraction effect(s) in more detail.

Contraction of competitiveness

To quantify competitiveness in the prereform period, we use the difference in vote shares of the front runner and runner-up in the first round ($\text{Margin}_{j,\text{pre}}$ in the following). $\text{Margin}_{j,\text{pre}}$ is the empirical counterpart to $M_{j,\text{pre}}$ from the theoretical model. In our sample, some districts were very competitive, others much less so. For example, 27 SMDs had an average $\text{Margin}_{j,\text{pre}}$ below 10 percentage points across the four prereform elections,

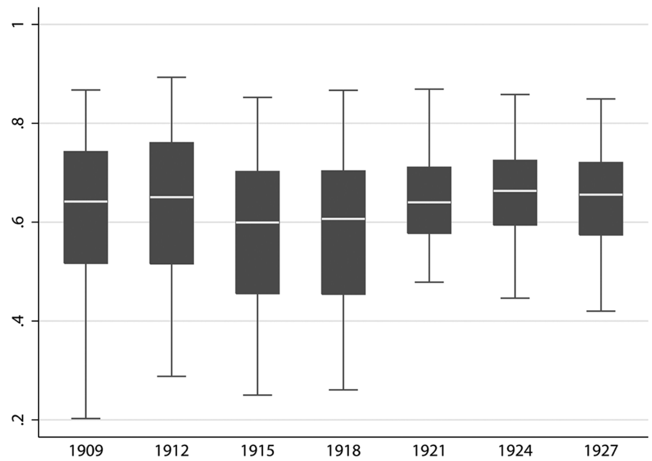

Figure 3. Voter turnout 1909–27. Box-and-whisker plots based on yearly district-level (final round) turnout. Two-round elections were used from 1909 to 1918, proportional representation from 1921 to 1927. The data set is based on the prereform district structure.

13. As noted before, turnout is measured at the SMD level both before and after reform. Appendix figure A.1 shows cross-sectional distributions for turnout by election year.

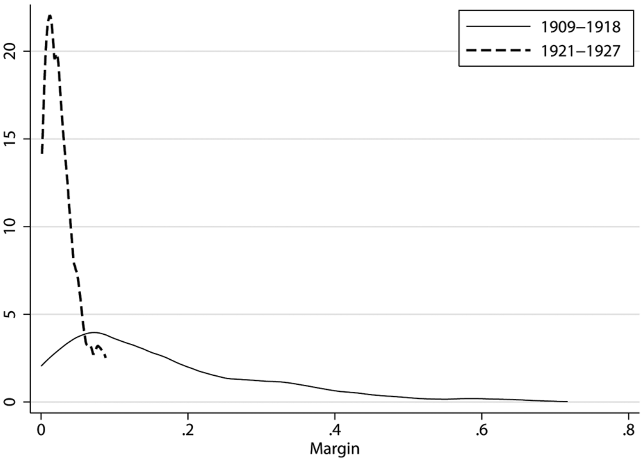

Figure 4. Kernel density plot of margin, pre- and postreform. The figure shows separate kernel density plots (Epanechnikov kernel with optimal bandwidth) of Margin in the pre- and postreform periods. In the prereform period we measure margin by the percentage-point difference in vote shares obtained by the front runner and runner-up in the first round. In the postreform period, we measure margin as the minimal number of additional votes one party would have to win to gain another seat, scaled by the number of votes cast. The data set is based on the prereform district structure.

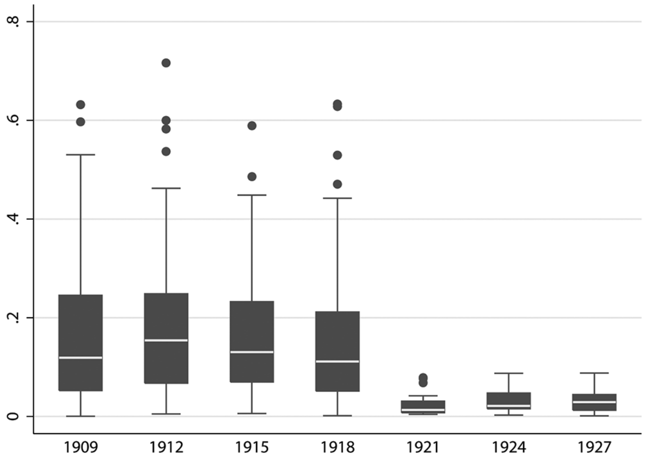

Figure 5. Margin 1909–27. Box-and-whisker plots based on yearly district-level margin. In the prereform period we measure margin by the percentage-point difference in vote shares obtained by the front runner and runner-up in the first round. In the postreform period, we measure margin as the minimal number of additional votes one party would have to win to gain another seat, scaled by the number of votes cast. The data set is based on the prereform district structure.

while eight had an average $\text{Margin}_{j,\text{pre}}$ above 30 percentage points.[14]

Measuring competitiveness in the postreform PR districts is more complicated (Blais and Lago 2009; Grofman and Selb 2009). Let $g[j]$ denote the postreform PR district into which prereform district j maps. We quantify $\text{Margin}_{g[j]}$ as the minimal number of additional votes one party would have to win to gain another seat in district g, divided by the total number of votes cast. $\text{Margin}_{g[j]}$ is the same for all j units in g. This measure is similar to that proposed by Blais and Lago (2009), except that they focus on raw votes, whereas our measure is standardized as the change in the share of votes that would be needed to change the seat allocation outcome in the district. This standardization is reasonable if we believe that parties employ economies of scale in mobilization rather than simply individual door-to-door contact strategies. The *Aftenposten* newspaper editorial presented earlier suggests that this belief is sensible in the Norwegian case.

Figure 4 plots the kernel density distribution of $\text{Margin}_{j,\text{pre}}$ (solid line) and $\text{Margin}_{g[j]}$ (dashed line). There is a clear contraction of the distribution in competitiveness with the switch to PR. The right tail of the competitiveness distribution pulls in substantially, while the left tail pulls in slightly.[15] The mean margin of victory for the last allocated seat under PR is 3 percentage points, compared to 17 percentage points in the prereform SMDs. Figure 5 presents box-and-whisker plots to illustrate the distribution of competitiveness ($\text{Margin}_{j,\text{pre}}$ and $\text{Margin}_{g[j]}$) over time. Both figures indicate a contraction of competitiveness following reform, in support of predictions (C1)–(C3).

An alternative measure proposed by Grofman and Selb (2009) generalizes competitiveness in both SMD and PR districts as a weighted average (by party vote share) of each party's worst-case-scenario incentives to mobilize in a district in order to gain (or not lose) a seat, based on vote share differences and normalized by the threshold of exclusion. Their generalized "index of competition" can range from 0 to 1. We prefer our measure for ease of interpretation but present the pre- and postreform competitiveness using the Grofman-Selb measure in appendix figure A.3 (appendix available online); this figure also shows a clear contraction in competitiveness.[16]

Contraction of turnout

We now turn our attention to the contraction effect on turnout. Such an effect is already quite evident in figure 2. Figure 6 shows how mean turnout developed over time for districts with different levels of $\text{Margin}_{j,\text{pre}}$. In the most competitive

14. Appendix figure A.2 shows the frequency of observations by $\text{Margin}_{j,\text{pre}}$. Measured in vote counts, the average first-round difference between the front runner and the runner-up is 616. The median difference is 466.

15. Specifically, if we compare the prereform average margin to the postreform average margin, 2 SMDs became less competitive, while 90 became more competitive. Comparing the 1918 margin to the 1921 margin, 11 SMDs became less competitive, while 81 became more competitive.

16. Specifically, if we compare the prereform and postreform averages of the Grofman-Selb index of competition, 33 SMDs became less competitive, while 59 became more competitive. We thank Peter Selb for sharing the STATA code for calculating the measure.

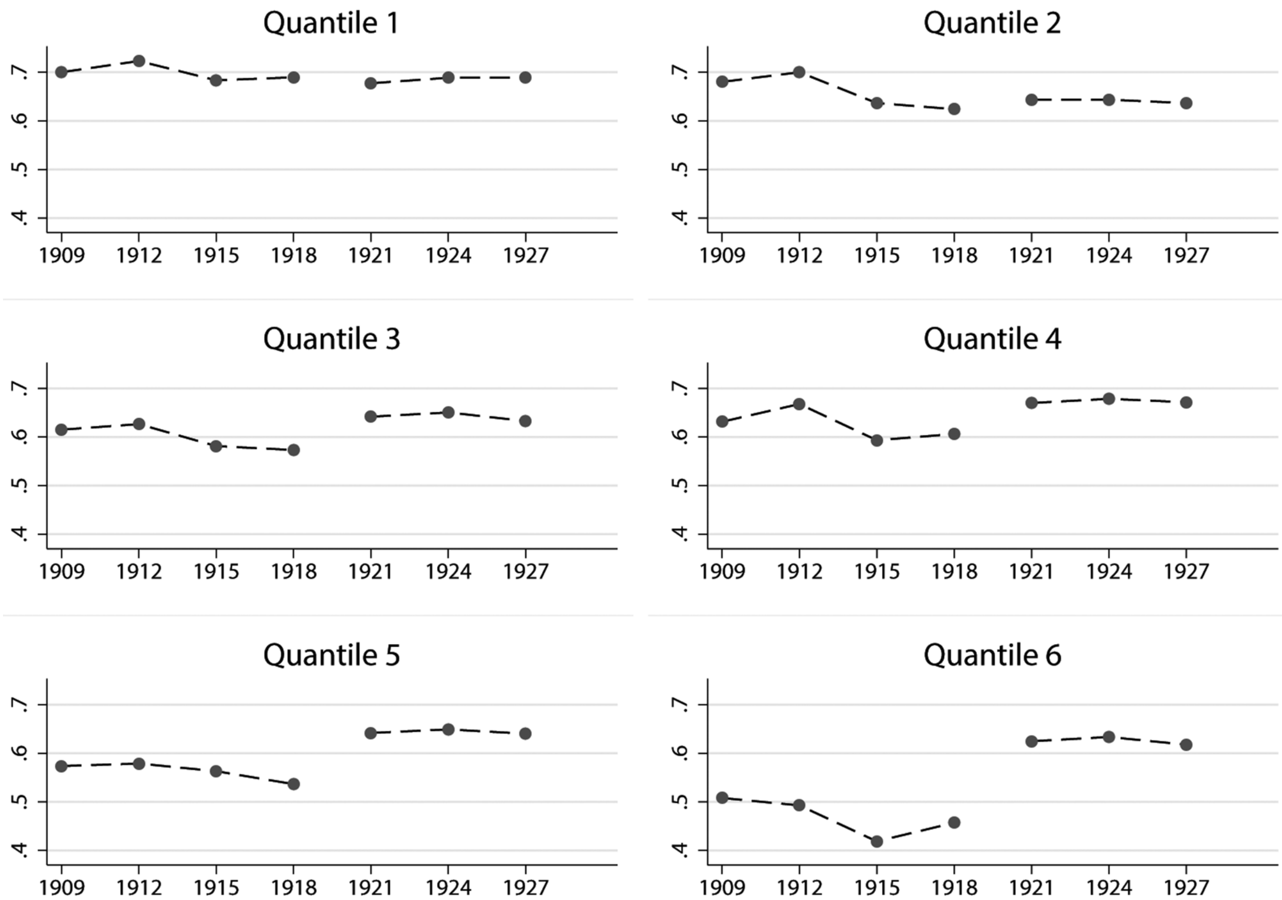

Figure 6. Mean voter turnout 1909–27, split by mean 1909–18 margin. The figure shows the average district-level turnout rate by election year, split by electoral closeness in the 1909–18 period. Electoral closeness is measured as the average difference in vote shares between the first-round front runner (sometimes winner) and the runner-up in the 1909–18 period. The top left panel is based on districts belonging to the first quantile of the closeness distribution (the most competitive districts), the bottom right panel is based on the sixth quantile of the closeness distribution (the least competitive districts). The other panels show the intermediate categories. The data set is based on the prereform district structure.

sextile of prereform districts (top left panel), turnout decreased from 1918 to 1921 (from 69.0% to 67.7%). However, turnout increased in all remaining sextiles, with successively larger increments observed in successively less competitive districts.

To analyze the district-level contraction effect more formally, we use a regression framework. Exploiting data from the two elections immediately before and after the electoral reform, 1918 and 1921, we estimate variants of the following equation:

$$\Delta T_j = f(\text{Margin}_{j,\text{pre}}, \text{Margin}_{g[j],\text{post}}) + u_j, \qquad (1)$$

where j is a prereform district under the SMD system and its geographic counterpart under the PR system, and $g[j]$ denotes the postreform district to which j belongs. The dependent variable ΔT_j measures change in voter turnout for j from 1918 to 1921. We relate this to the average first-round difference between the front runner and runner-up in the prereform period, $\text{Margin}_{j,\text{pre}}$, and the average minimum distance to a seat threshold in the postreform period $\text{Margin}_{g[j],\text{post}}$. This allows us to test the contraction hypothesis explicitly and also allows us to investigate the threshold of $\text{Margin}_{\text{pre}}$ ($M_{j,\text{pre}}$ from the theoretical model) for which the predicted ΔT turns negative.

Since prereform districts (j) are nested within postreform districts ($g[j]$), we allow for arbitrary correlation in the error term, u_j, within postreform districts by clustering at this level. Since the number of clusters are relatively few, we also present regular heteroscedasticity-robust standard errors.

Table 2 provides the main results. Specification (1) is a simple linear regression relating ΔT_j to $\text{Margin}_{j,\text{pre}}$. This model fits the data remarkably well: 43% of the variation in ΔT_j is explained by $\text{Margin}_{j,\text{pre}}$. Adding a second-order term to the model—see specification (2)—does not further increase the R^2. The point estimate of 0.65 suggests that a 10-percentage-point increase in $\text{Margin}_{\text{pre}}$ (roughly corresponding to a standard deviation increase) increases ΔT by 6.5 percentage

Table 2. Prereform Margin and Change in Turnout

	(1)	(2)	(3)	(4)
$\text{Margin}_{\text{pre}}$	.649	.684	.651	.523
	(.083)	(.262)	(.083)	(.066)
	[.095]	[.340]	[.097]	[.085]
$\text{Margin}^2_{\text{pre}}$		−.083		
		(.646)		
		[.789]		
$\text{Margin}_{\text{post}}$			−.498	
			(.570)	
			[.775]	
Constant	−.040	−.043		
	(.013)	(.020)		
	[.014]	[.023]		
PR district fixed effects	No	No	No	Yes
N	92	92	92	92
R^2	.428	.428	.433	.847

Note. The dependent variable is the change in voter turnout from 1918 to 1921. Heteroscedasticity-robust standard errors in parentheses, cluster-robust standard errors in brackets.

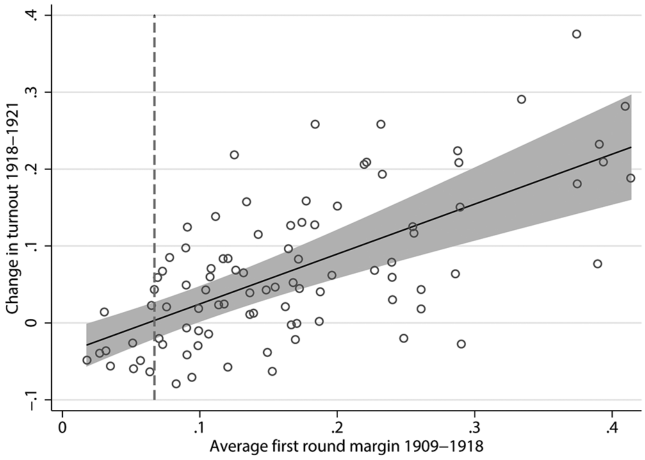

Figure 7. Relationship between prereform margin and change in turnout. This figure shows the relationship between the prereform margin and the change in turnout (ΔT) based on a simple linear regression model. The fitted line shows the predicted values for ΔT and a corresponding 95% confidence interval using cluster-robust standard errors, in addition to the 92 scatter points. The dashed vertical line indicates the point at which the fitted line crosses the *x*-axis.

points. The effect is highly statistically significant using both regular and cluster-robust standard errors, with *t*-values of about 7.[17]

In specification (3), we control for the average postreform margin in the PR district to which *j* belongs. This additional control does not affect the impact of the prereform margin. Moreover, using the Grofman-Selb measure of competitiveness instead of ours also leaves the effect of prereform margin unchanged (results omitted for brevity). As regards the effect of postreform competitiveness on the change in turnout, specification (3) shows that SMDs belonging to less competitive PR districts (i.e., those having higher postreform margins) tend to have a lower ΔT. Although the effect is statistically insignificant, this lack of significance is to be expected. Figure 4 shows that there was very little variation across the PR districts in competitiveness. As the sample variance of any regressor declines, however, the analyst's ability to detect its effects on any dependent variable necessarily declines (i.e., we lack statistical power). Consistent with this observation, we also cannot reject the null hypothesis that postreform margins had just as much effect (in magnitude) as prereform margins.[18]

Finally, in specification (4) we control in a more flexible way for postreform competitiveness by including a set of fixed effects capturing the postreform district structure. Hence, we are comparing changes in turnout for "SMDs" ending up in the same postreform district. The postreform fixed effects improve the model considerably (the R^2 is roughly doubled). The point estimate of interest, however, does not change much. It falls only moderately in comparison to our baseline estimate and is still highly statistically significant (*t*-value above 6).

Figure 7 graphically illustrates the relationship between $\text{Margin}_{\text{pre}}$ and ΔT. The scatter points are the values for the 92 "SMDs" in our sample; the fitted line represents the predicted values for ΔT based on specification (1); the shaded area represents a 95% confidence interval of these predicted values. The dashed vertical line indicates the point at which the fitted line crosses the *x*-axis. In other words, specification (1) suggests that for a prereform SMD where $\text{Margin}_{\text{pre}} < 0.067$, the introduction of PR reduced voter turnout. This finding provides support for the theoretical argument advanced by Herrera et al. (2014) and the corresponding predictions (T1), (T2), and (T3) presented earlier, that the introduction of PR may have heterogeneous effects on turnout, depending on the competitiveness of the prereform SMDs.

17. Cluster-robust standard errors may be biased downward if the number of clusters is "few." Depending on the situation, "few" may range from less than 20 to less than 50 clusters (Cameron and Miller 2015). In our application, we have 22 clusters. As an alternative, we therefore applied the resampling methods of Cameron, Gelbach,. and Miller (2008) when clustering at the postreform district level. With this method, the estimated contraction effect remains highly statistically significant ($p < .001$).

18. That is, we cannot reject the null hypothesis that the coefficient on $\text{Margin}_{\text{pre}}$ equals the (absolute value of) the coefficient on $\text{Margin}_{\text{post}}$.

SENSITIVITY ANALYSES

Our research design is based on within-district changes in voter turnout. This design hinges on a fundamentally untestable parallel trend assumption (namely, that the change in SMD-level turnout from 1918 to 1921 due to "period effects" is independent of the degree of competitiveness). We can, however, shed some light on the plausibility of this assumption by estimating placebo contraction effects based on nonreform election years. Figure 8 graphically presents the results of such a falsification exercise. The bottom left panel reproduces the actual contraction effect presented in figure 7. The five other panels of figure 8 are based on nonreform election years. The top left panel, for example, relates the change in turnout from 1909 to 1912 to Margin$_{pre}$. Reassuringly for our identification strategy, there is no systematic relationship between ΔT and Margin$_{pre}$ in nonreform years. Hence, it seems plausible that in the counterfactual situation where the electoral reform did not happen, turnout would not have contracted.

Another possibility is that our results might be due to a change in the number of electoral parties. With the introduction of PR, the number of parties running for office increased from about three to about five (see appendix fig. A.4). A concern might be that the number of parties (*NoP*) increased more in low competition areas and that this increase is responsible for the observed change in turnout. If so, the mechanism through which PR increases turnout does not go through increased competitiveness, but rather through increased options (parties) for voters. To explore this alternative explanation, we include ΔNoP as a control variable in our regression framework. Specification (1) in table 3 shows that the estimated effect of ΔNoP is close to zero and statistically insignificant. In specification (2), we replace ΔNoP with ΔNoB, the number of political blocs participating in the election (Left, Center, Right, Agrarian, Other), and find a small positive effect, statistically significant at the 5% level. The point estimate of 0.02 indicates that when one additional bloc is participating in the election, turnout increases by two per-

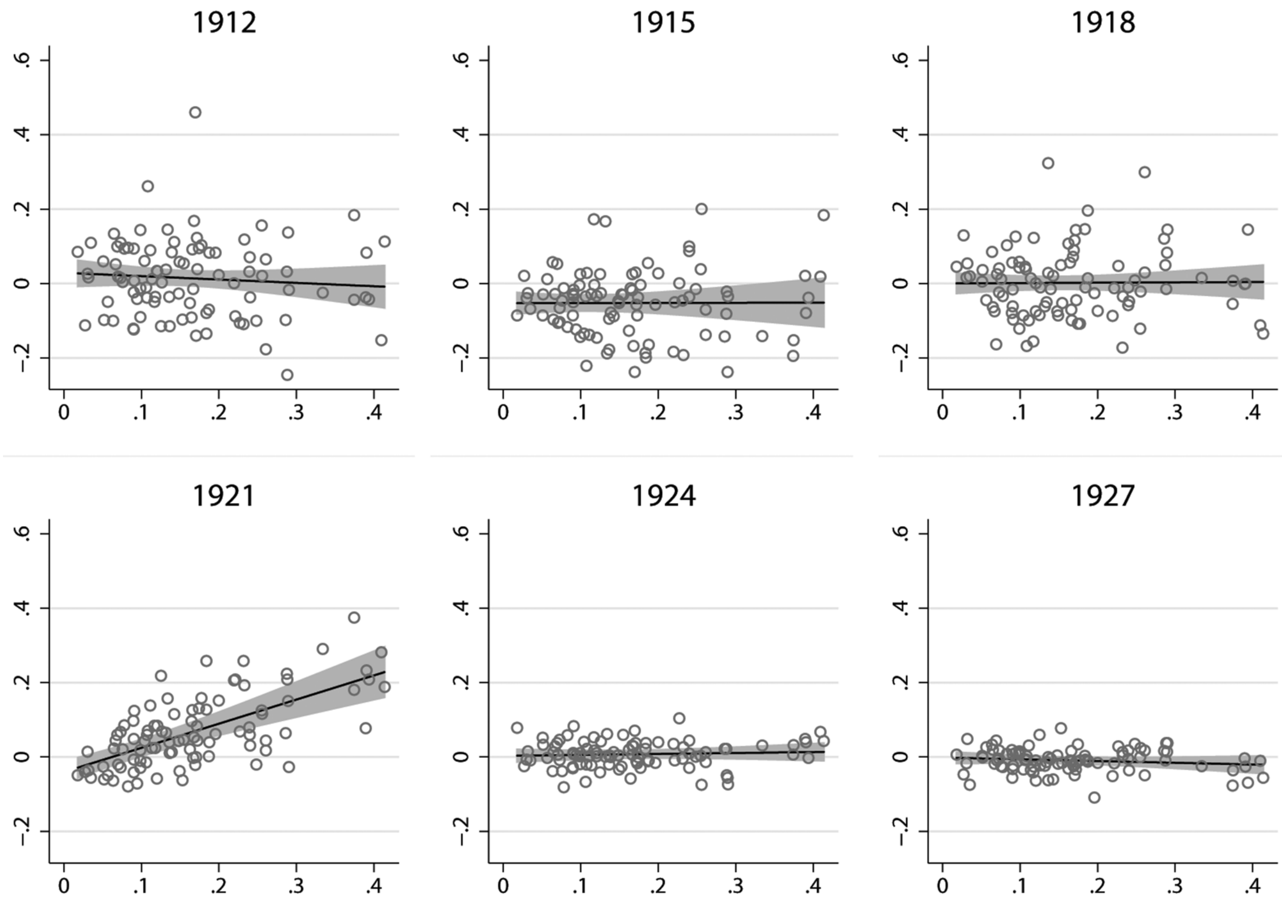

Figure 8. Falsification test: linear regression model. The figure shows the relationship between the change in turnout and prereform margin based on simple linear regression models for each election year in our sample. The fitted lines show the predicted values for ΔT and corresponding 95% confidence intervals based on cluster-robust standard errors.

Table 3. Sensitivity Analyses

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Margin	.651	.625	.651			.417	.637
	(.087)	(.089)	(.084)			(.085)	(.063)
	[.101]	[.103]	[.095]			[.086]	[.065]
Margin_{1918}				.331			
				(.060)			
				[.058]			
$\text{Margin}_{\text{Final}}$					.584		
					(.095)		
					[.114]		
ΔNoP	−.002						
	(.007)						
	[.010]						
ΔNoB		.020					
		(.010)					
		[.010]					
ΔMagnitude			−.003				
			(.005)				
			[.008]				
Constant	−.037	−.057	−.029	.016	−.040	.041	−.058
	(.017)	(.013)	(.026)	(.012)	(.016)	(.014)	(.011)
	[.021]	[.014]	[.036]	[.016]	[.017]	[.012]	[.013]
PR district fixed effects	No	No	No	No	No	No	No
N	92	92	92	92	92	92	92
R^2	.428	.448	.429	.221	.380	.225	.498

Note. The dependent variable in cols. 1–5 is the change in voter turnout from 1918 to 1921 using final-round turnout in the prereform period. The dependent variable in col. 6 is the change in voter turnout from 1918 to 1921 using first-round turnout in the prereform period. The dependent variable in col. 7 is the change in average voter turnout from 1909–18 to 1921–27 using final-round turnout in the prereform period. Heteroscedasticity-robust standard errors in parentheses, cluster-robust standard errors in brackets.

centage points. Importantly, however, the estimated effect of Margin is not significantly altered when ΔNoP or ΔNoB are included in the model.[19]

Another potentially important mechanism relates to district magnitude. The postreform PR districts vary in magnitude from three to eight. It is plausible that turnout may increase more in “SMDs” under PR that are part of districts with larger magnitude, as larger magnitude will increase the proportionality of the seat allocation results and potentially attract greater mobilization effort by party elites. To investigate this possibility, we include ΔMagnitude as a control variable in specification (3). The results in table 3 show that the effect of this variable is close to zero and statistically insignificant.[20]

Finally, we implement analyses with alternative operationalizations of ΔT and Margin. In specification (4), we use Margin measured in 1918, rather than Margin measured as the average in the prereform period. We find results similar to our baseline analysis, but we explain much less of the variation in ΔT. In specification (5), we rely on the average prereform Margin in the final round rather than the average prereform Margin in the first round. The results are almost unaltered from our baseline analysis. In specification (6) we use first-round turnout rather than final-round turnout to measure ΔT. Again, we find a positive and significant relationship between ΔT and Margin. The positive constant

19. Over time, the switch to PR may have also motivated the creation of more centralized national parties with increased mobilizational capacity. Aggregate turnout may have also been affected by the extension of the franchise in 1913, as well as a public referendum banning alcohol consumption in 1919. However, these events—indeed, any events that pushed turnout more or less uniformly up or down—should simply adjust the threshold *M*. This might hinder our ability to detect the contraction effect, as discussed in the main text, but it cannot artificially generate one where none exists.

20. We also tested models where ΔNoP, ΔNoB, and ΔMagnitude, were interacted with Margin. These interaction terms were, however, always statistically insignificant, and results are omitted for brevity.

term suggests, however, that even the most competitive SMDs experienced an increase in turnout from 1918 to 1921 when we compare with the first-round turnout in the prereform period. Last, in specification (7), we use average turnout in the pre- and postreform periods. Again, results are as in our baseline analysis.[21]

CONCLUSION

Our investigation in this study has aimed to shed light on how strategic mobilization of voters differs under different electoral systems. Most existing studies of how electoral rules affect voter turnout have examined cross-sectional data sets and focused on mean turnout measured at the aggregate, national level. In other words, previous scholars have explored whether turnout tends to be higher on average in countries that use PR in multimember districts than in countries that use SMDs.

However, recent theoretical models of mobilization illuminate more than just aggregate mean turnout. Elite mobilization theories of turnout make detailed predictions about how the closeness of competition, and thus turnout, should change at the district level when national electoral reforms are adopted. More specifically, these models predict that mobilizational incentives (hence turnout) will contract following the adoption of PR, falling in highly competitive prereform SMDs, but increasing elsewhere. The contraction hypothesis also predicts that the more a prereform SMD's competitiveness diverges from the postreform level, the more its competitiveness and turnout should contract toward that level postreform. This implies that the change in turnout is a function of (primarily) the prereform margin of victory and (secondarily, due to low variance) the postreform margin of victory.

We have exploited a rich new data set on Norwegian parliamentary elections, before and after the major electoral reform from two-round runoff in SMDs to PR in multimember districts, in order to provide the first systematic empirical assessment of the contraction hypothesis and the various predictions that flow from it. We find that the data fit the theory's predictions quite well.

As is often the case, some areas for future research still remain. Broadly speaking, the incentive for elite actors to mobilize their supporters in legislative elections depends on what is at stake. The stakes always include the legislative seats themselves; however, other political offices—such as cabinet positions, committee chairmanships, and judicial positions—will also be filled differently depending on the electoral outcome. Thus, elite incentives to exert mobilizational effort ultimately depend on "the translations from effort-to-votes, votes-to-seats, and seats-to-portfolios" (Cox 1999, 387). Here, "portfolios" can be interpreted to include all important executive, legislative, and judicial posts at stake—directly or indirectly—in a particular election. A complete assessment of elite mobilizational incentives must take all three mappings, and interactions between them, into account (Cox 1999; Herrera, Morelli, and Nunnari 2016).

We have focused in this paper on a major reform that changed the votes-to-seats mapping from two-round, plurality-rule runoff in single-member districts to proportional allocation in multimember districts. However, the systematic reallocation of elite effort that we have documented in Norway's electoral districts following the introduction of PR seems to have coincided with a shift in the technology of mobilization, from personal contacts to mass-media appeals and secondary mobilization strategies. Whether this change can be fully documented with additional historical data from Norway remains to be seen, but we believe that a promising area for future research concerns how electoral rules, and particularly electoral reforms, influence mobilizational tactics (and hence the effort-to-votes mapping).

Another promising area for future empirical investigation is how the seats-to-portfolios mapping affects turnout across districts within countries or across countries with different institutional arrangements. Although our empirical investigation for Norway focuses on the immediate period after the electoral reform, the Norwegian case may provide some opportunities for future empirical inquiry in this area. Beginning in 1930, parties could join forces in joint electoral cartels (*listeforbund*). Votes were cast for the individual party lists, but seats were allocated first based on the total number of votes earned by participating parties in the cartel. Such an arrangement may increase the seats-to-portfolios mapping of mobilizational effort if it increases allied parties' chances of getting into government. At the district level, individual politicians who anticipate an appointment to cabinet (or some other postelectoral reward) if their party wins power may also exhibit differential rates of mobilizational effort. More research is needed to elucidate how such anticipations of the seats-to-portfolios mapping influence elite mobilization.

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21. Appendix table A.1 replicates these specifications with postreform PR district fixed effects. The results are largely unchanged.

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