



Do more flexible lists increase the take-up of preference voting?

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ABSTRACT

In many preferential-list proportional representation systems, voting for a candidate is voluntary. Previous studies suggest a positive relationship between the impact of preference votes on seat allocation and how often they are used. However, existing work uses indirect measures on the right-hand side or suffers from the reverse causality problem. Focusing on electoral reform, this study argues that a change in rules affects the extent of personal voting if it alters the beliefs about which types of seat allocation are possible. These types are captured using a new trichotomous measure of list flexibility. The anticipated changes to seat allocation induced by a reform in Sweden allow us to carry out an over-time analysis with appropriate counterfactuals. The results show that moderate increases in list flexibility lead to more preference voting, whereas strong increases cause a drop. Additional analyses suggest that the unexpected decrease results from local parties adjusting their candidate selection strategies.

As documented by [Renwick and Pilet \(2016\)](#), European electoral systems have experienced a clear trend toward more “personalization” through electoral reform since 1945. In many countries, changes in electoral rules have granted citizens more opportunities to express preferences for individual candidates, and the impact of preference votes upon intra-party seat allocation, i.e. the assignment of seats within parties, has risen. This often happened by making the lists in proportional representation (PR) systems more flexible; in other words, it became easier for voters to alter the pre-electoral ranking proposed by the party selectorate. In their case studies, [Renwick and Pilet \(2016\)](#) also find that politicians often introduce such reforms to combat citizens’ dissatisfaction with politics. Given that citizens are receiving more choice and influence, one would expect them to welcome the new opportunities and make ample use of them.

In many list PR systems that feature a candidate vote (i.e. preferential-list PR systems), expressing a preference for a candidate is voluntary.¹ This provides a window into understanding whether electoral rules and their changes affect the extent to which candidates seek and voters cast a personal vote. Existing data clearly show that the take-up of preference voting, if optional, is far from universal. Indeed, there is considerable variation across space and time, which also holds true with regard to the development following electoral reforms ([Renwick and Pilet, 2016:235–240](#)). This study therefore examines whether making lists more flexible increases the take-up of preference

voting. Beyond its interest to scholars of candidate and voting behavior, this question is worth answering to both help us assess the consequences of past changes to electoral rules as well as predict the effects of future reforms and gauge the scope for electoral engineering more generally.

It may seem obvious that more flexible lists lead to more preference voting. After all, it is intuitive that the optional candidate vote is more frequently taken up if the preference expressed is expected to have a larger electoral impact ([Lakeman, 1974](#); [Marsh, 1985](#); [André et al., 2012](#); [André and Depauw, 2017](#); [Thijssen et al., 2018](#)).² There are two complications, however. First, theoretically, it is not straightforward to operationalize the actual flexibility of a list or the associated impact of preference votes on seat allocation. Second, empirically, the reverse causality problem looms large. When many voters cast candidate votes, it is more likely that politicians can be elected on their personal vote, but this link also runs the other way around. These challenges make it attractive to focus on how electoral reforms change preference vote use. While [Renwick and Pilet \(2016:235–240\)](#) do so, their analysis is limited to simple before/after comparisons at the level of the entire electorate. As research on inter-party seat allocation (i.e. seat assignment between parties) has pointed out ([Fiva and Folke, 2016](#); [Ponattu, 2018](#); [Ward, 2019](#)), it is important to establish the appropriate counterfactual outcomes *if the reform had not occurred*.

This article presents a new approach to addressing these two

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¹ In contrast to only four cases with obligatory preference voting, [Bol et al. \(2018:23\)](#) list 16 European countries with preferential-list PR systems that allow voters to cast a party vote.

² Other explanations of preference vote usage focus on district-level characteristics such as population density (e.g., [André et al., 2012](#)) and voter-level variables ([Oscarsson and Holmberg, 2013](#); [Berg and Oscarsson, 2015](#)). A different strand of research studies the voting results of individual candidates, including [von Schoultz and Papageorgiou \(2019\)](#) and [Folke and Rickne \(2020\)](#).

challenges. As a theoretical contribution, it argues that reforms affect preference vote use if they change the nature of seat allocation that actors believe to be possible. In this context, it introduces the concept of the post-electoral allocation type (PAT), a trichotomous classification of list flexibility. On the empirical side, after demonstrating the endogeneity issues that hamper existing research, this study provides new evidence based on anticipated changes in list flexibility from the 2010 reform of the Swedish flexible list system. Using an over-time analysis (difference-in-differences and lagged dependent variable models) with an appropriate control group, the results show that the reform indeed affected lists with varying potential PATs in a different manner. Lists anticipated to be no longer closed experienced a moderate increase in preference voting, although the uncertainty of the estimate varies across models. By contrast, for lists that saw the largest increase in flexibility, preference vote take-up actually declined. Additional analyses suggest that this happened since local parties counteracted the anticipated reform effects by selecting less popular challengers (and perhaps by restricting their personal vote-seeking efforts).

1. List flexibility and the take-up of preference votes

Within the list PR family, we can differentiate three *ballot structures* based on the formal rules for casting votes and allocating seats to candidates (Marsh, 1985; Katz, 1986; Karvonen, 2004; Shugart, 2005; Renwick and Pilet, 2016; Däubler and Hix, 2018).³ This article further differentiates the *type* of intra-party seat allocation, which is linked but not equivalent to the formal ballot structure.

With closed lists, voters cannot express a preference for any individual candidate, and seat assignment within parties – after the votes have been cast – is based entirely on the list rank, which is decided by the parties before the election. Under (fully) open list systems, preference votes exist and intra-party seat allocation exclusively follows the candidate votes that citizens cast.⁴ Flexible (or semi-open) lists are a hybrid form, since (1) parties present pre-ranked lists, (2) voters can express a preference for individual candidates, and (3) additional rules specify under which conditions a candidate can “jump the queue” given by the pre-electoral list ranking. This means that the actual post-electoral seat allocation within parties can be based on the list rank only like in a closed list system (if nobody meets the condition), on the candidate votes only like in a fully open list system (if a sufficient number of candidates fulfil the criterion), or on the list rank for some candidates but on personal votes for others. This variation in allocation type can occur not only across flexible list systems, but also within a given system across lists.

A variety of rules determine whether and how to take into account preference votes for seat allocation in flexible lists (Renwick and Pilet, 2016:26–28). The most common variant in Europe is *threshold-type* flexible lists: the post-electoral ranking starts from the pre-electoral list order, but any candidates who have garnered the number of preference votes implied by the pre-specified threshold are moved to the top. Typically, the threshold is based on a certain share of the total vote for the party list. Candidates with as many preference votes as $x\%$ of the total vote for the list are pushed to the top of the ranking.

In addition to the list form, electoral rules specify whether preference voting (where it exists) is obligatory. Some systems allow a party vote, for example, by marking a separate circle at the top of the ballot card rather than next to a candidate. When lists are flexible, it is intuitive that the incentive to vote for candidates rises with the potential impact of the preference votes. At the same time, however, this creates a reciprocal relationship. Since the threshold is usually defined relative to the total party list vote, candidates are more likely to pass the threshold when more voters of the party use the preference vote option. Suppose a list wins 1000 votes and the threshold is 5%. A candidate should find it easier to reach 50 votes if 500 voters are ready to support the candidates (which amounts to 10% of all the *preference* votes) compared with when, say, only 250 are (which requires 20% of all the preference votes). If politicians and voters predict the impact based on past experience, this leads to an endogeneity problem, making it hard to disentangle whether preference voting for a list is high because it has been high before or because actors expect the personal vote to matter more.

Some recent empirical work considering within-country variation has focused on Belgium, where candidate voting is voluntary and a slightly different flexible-list system of the transfer type is in place (Renwick and Pilet, 2016:28). André et al. (2012) suggest that in this system preference votes matter more if lists win more seats, since intra-party seat allocation may proceed to a stage at which they decide.⁵ Thijssen et al. (2018) replicate this association between party magnitude and preference vote use in further Belgian elections, but their more fine-grained analyses suggest that these preference votes frequently go to the list leaders rather than the marginal candidates. In addition to the critique by Thijssen et al. (2018), party magnitude may be correlated with unobserved list-level factors that also influence preference voting.

In the most comprehensive investigation of preference vote use, André and Depauw (2017) rely on survey data covering seven elections from six countries. Their study uses “the proportion of incumbents per district that is elected on the basis of their preference votes and not their position on the list” (André and Depauw, 2017:605), from the previous election, as the variable capturing the effectiveness of preference voting.⁶ There are two issues with their analysis, however. First, it is not clear why the district level rather than the list level (i.e. party–district combinations) should be the relevant unit for measuring the impact of preference votes. Arguably, under preferential-list PR, candidate votes largely result from intra-party competition, at least in parliamentary systems with strong parties (Rudolph and Däubler, 2016). Indeed, as discussed in more detail below, party size should have a clear influence on list flexibility. Second, their analysis suffers from the reverse causality problem. When more voters use the preference vote option at t_0 , this increases effectiveness at t_0 . A simple regression of preference vote use at t_1 on effectiveness at t_0 can easily fail to identify a causal effect. This happens if the time-invariant characteristics of the district have the same impact on preference vote use in both time periods or if preference vote use at t_0 directly affects preference vote use at t_1 . Appendix A.1 discusses this further and shows empirically that the positive association between the lagged effectiveness of preference votes and subsequent use disappears when including the lagged dependent variable or party–district fixed effects.

³ The usage of the terms “open” and “flexible” list often varies by study.

⁴ Two remarks are in place. First, openness as understood here does not refer to the possibility of panachage (choosing candidates from more than one party), which should be considered as a separate dimension that regards voters’ choice but not intra-party seat allocation. Second, even with (fully) open lists, pre-electoral ranking by party selectors may be in place. However, while top ranks per se are helpful in attracting preference votes at the electoral stage (for causal evidence see Däubler and Rudolph, 2020; Faas and Schoen, 2006; Geys and Heyndels, 2003;), this pre-electoral ranking affects the *post-electoral* seat assignment in a direct manner at most in the case of a tie (Däubler and Hix, 2018:1800).

⁵ These authors refer to the last of the following stages: First, candidates who have reached the (Droop) quota on the basis of their personal votes are elected; second, if seats remain vacant, a limited number of the votes cast for the list as a whole is apportioned to elevate candidates to the quota, starting at the top of the pre-electoral list; and third, if seats are still to be assigned after the list votes available for distribution are exhausted, the sum of the original preference votes and distributed list votes decides.

⁶ Based on the descriptive statistics André and Depauw (2017) provide, in threshold-type systems, the criterion is whether a candidate has reached the threshold (rather than, say, having been placed at a list rank insufficient for election under a closed list system).

The endogeneity problem makes examining the consequences of electoral rule changes particularly worthwhile. Renwick and Pilet (2016:235–240) compare preference vote use (at the level of the entire electorate) before and after electoral reforms and conclude the following: “Overall, it appears that personalizing electoral reforms and rates of preference voting are linked causally. That link can go both ways.” While appropriately cautious, this ambiguous conclusion shows that a more systematic analysis establishing the relevant counterfactuals (see Fiva and Folke, 2016; Ponattu, 2018; Ward, 2019 for work on the inter-party dimension) is required to pin down the consequences of reforms changing the impact of preference votes.

2. Electoral reform, (possible) allocation types, and preference vote use

An electoral reform that makes it easier for candidates to be elected on the basis of their personal votes should increase the impact of these votes, and hence their take-up. This section develops this notion into a more nuanced argument. The main proposition is that an electoral reform affects the expected impact and preference voting by changing the beliefs about which type of intra-party seat allocation is possible in future elections. One implication of this argument is that the reform effect should not be uniform. Politicians and voters adapt only if they believe that the relative importance of the list rank and preference votes for determining who will win a seat is altered by the new rules.

Similarly as to the link between election closeness and turnout (Blais, 2007), the impact of preference votes may affect their use in two ways. First, citizens may more or less *directly* assess impact (e.g., based on their personal experience or media reports). Although the extent to which that happens is likely to depend on the context, many voters may lack the skills and information to do so.⁷ Second, election-seeking politicians should know whether their individual vote will matter (Marsh, 1985; Cox et al., 2016). If this results in intensified campaigning for personal votes, top-down mobilization should clearly increase the extent of candidate voting.⁸ In summary, although theoretical reasons suggest that the second mechanism is stronger, the overall reasoning in this section does not depend on this expectation.⁹

To develop a refined argument about the impact of candidates' votes in a threshold-type flexible list system, it is useful to introduce the concept of the PAT. As mentioned above, such systems specify a threshold *relative* to the total list vote as a criterion for re-ordering the ranking of candidates (Renwick and Pilet, 2016:26–28). In other words, a candidate requires $x\%$ of all the votes cast for a party in a district (including the ballots cast for the list as a whole, if candidate voting is optional) to be moved to the top of the post-electoral ranking. As a consequence, we can observe the following three PATs:¹⁰

PAT-LR (list rank-based): all the seats the party wins are allocated in the order of the pre-electoral list ranking, like under a closed list system.

This applies when at most the list leader has reached the threshold.¹¹

PAT-PV (personal-vote-based): all the seats the party wins are allocated based on preference votes, like in a (fully) open list system. For a list winning S seats, this applies if $S + 1$ candidates reach the threshold.¹²

PAT-MX (mixed type): some seats the party wins are assigned on the basis of preference votes, while the rest is awarded on the basis of pre-electoral list ranks. For a list winning S seats, this applies if a candidate other than the least leader, but fewer than $S + 1$ candidates reach the threshold.

How hard is it for individual candidates to reach the threshold? While the rule specifies the relative amount, $x\%$ of all the votes cast for a party in a district (including the ballots cast for the list as a whole), that the candidate requires to be moved to the top of the post-electoral ranking, this can imply a large range of *absolute* votes. The absolute number depends on both district-level and party-level factors, particularly district electorate and party size. It is plausible to assume that a threshold lower in absolute terms is easier to reach, as most hopeful candidates may have a similar-sized “baseline” personal support base that is easy to mobilize—because of friends-and-neighbors voting (e.g., Górecki and Marsh, 2012) and local ties (e.g., Fiva and Smith, 2017).

Since the relative threshold can imply different absolute vote numbers, two or three of the PATs may coexist at the same time within a system, as well as within the district across different lists. In essence, the results of a given election are not characterized by one PAT, but rather a distribution of PATs, with lists as the relevant units. A second feature complicates threshold-type flexible list systems. Unlike in closed and (fully) open systems, the eventual PAT for a specific list is not known, at least not with certainty, ahead of the election. Actors need to form an *expectation* (Cox, 1997) about how seats will be assigned within a given list. In the sense of a model, we can think of actors as assigning a probability that each of the three PATs occurs. An actor may be fairly sure that seats will be allocated one way or another (e.g., $p = \{9/10, 1/20, 1/20\}$), but she may also assign two or even all three PATs similar chances of materializing (in the extreme case $p = \{1/3, 1/3, 1/3\}$).

We can now link the expectations about the PAT to the expected impact of casting a preference vote. If actors feel practically certain that all the seats will be awarded on the basis of the list rank (i.e. PAT-LR), there can be no instrumental motivation (in the narrow sense) for seeking or casting a personal vote. If no candidate has the chance to defy the list order by passing the threshold, the votes are useless for that purpose.¹³ By contrast, if an allocation based exclusively on personal votes is expected (i.e. PAT-PV), individual electoral performance is crucial and the instrumental motivation should be high. The third case (i.e. PAT-MX) should fall in between in this regard: it is feasible for candidates not topping the list to make the threshold, although a high pre-electoral rank (relative to the number of expected seats) still suffices to be elected. In the case of PAT-MX, preference votes can re-order the list, but the electoral safety of those on the highest list ranks remains unaffected by this possibility.

⁷ See Lyytikäinen and Tukiainen (2019), however, who provide evidence for a causal effect of intra-party vote pivotality on turnout from local elections under open-list PR in Finland, which they ascribe to voters rather than elites.

⁸ There is evidence that candidates' campaign effort is larger under open than under closed lists (Hangartner et al., 2019), and the same holds for turnout (Sanz, 2017).

⁹ For the Swedish case studied below, a basic search in the newspaper archive tidningar.kb.se does not suggest huge media interest in the new rules. For the three months before the 2014 election, it returns 36 articles for “personval AND (spärr OR spärren)” [personal election AND (a threshold OR the threshold)], compared with 455 for “valkrets OR valkretsen” [a district OR the district].

¹⁰ Compared with Däubler and Hix (2018), the PAT has a clearer rationale and is more fine-grained than their distinction between weakly flexible lists (PAT-LR) and strongly flexible lists (comprising PAT-MX and PAT-PV). In a formal theory model with only two intra-party candidates, Buisseret and Prato (2019) use a parameter reflecting list flexibility, but do not provide a general operationalization.

¹¹ If the list leader passes the threshold, the post-electoral ranking remains identical to the pre-electoral one and a considerable share of the list leader's votes may have been caused by ballot position. Hence, that happening does not show that it is possible to defy the list order.

¹² This operationalization takes the perspective of the politician, which is likely to be more relevant if top-down mobilization is the dominant mechanism. For a voter, PAT-PV pertains when all the seats will be awarded on the basis of preference votes. From the politician's view, there is a slight difference. If a list wins S seats, and she herself and $S - 1$ other candidates reach the threshold, she can still hope to win the last seat at the next election based on her pre-electoral list position.

¹³ Independent of the allocation, preference votes may contribute to a politician's reputation, and they have been shown to improve intra-party promotion and list position in later elections (André et al., 2017; Crisp et al., 2013; Folke et al., 2016).

Taken together, the key hypothesis is then:

H1. Incentives to seek or cast preference votes follow the order PAT-LR < PAT-MX < PAT-PV.

The PAT can have different points of reference. For instance, actors observe the PAT for a list in past elections, they expect a certain PAT for a list in an upcoming election, and they can form beliefs about the PAT across a broader set of possible future electoral contests. The key theoretical argument is that an electoral reform *can* change the beliefs about which PATs are *possible* after the change of rules. Actors, likely politicians more than voters, update their beliefs about the probability that a certain PAT occurs in the future based on applying the new rule to previous election results. If the new rule had switched the PAT in the past election to a type not previously experienced by the list, the new PAT has a good chance of rematerializing in later elections. On the contrary, if the new rule did not change the PAT in the past election, it is unlikely to affect the beliefs about the allocation in future contests. Consequently, if (and only if) the reform changes the beliefs about the potential impact of preference votes in any future election, actors should be more likely to seek or cast them.¹⁴

The notion of preference vote impact underlying the PAT is related to the concepts of intra-party election closeness. Such approaches consider the marginality of candidates (André et al., 2015; Selb and Lutz, 2015) or simulate the probability that a single vote is decisive (Lyttikäinen and Tukiainen, 2019). The PAT captures the nature of candidate competition more broadly, answering two questions: Is the pre-electoral list ranking changeable? If no, the PAT is LR. If yes, will a pre-electoral list rank $\leq S$ be sufficient for being elected in future elections if the party wins S seats? If yes, the PAT is MX, otherwise it is PV. If we think about the reform as affecting the beliefs about future PATs, the key point is whether the reform makes actors believe that a different *kind* of competition is now *possible*. As Appendix A.4 shows empirically, the PATs differ in terms of the probability that preference votes change the order of the list (only possible in MX and PV) and in terms of the probability that preference votes decide the set of elected candidates (which, in practice, distinguishes LR and MX from PV).¹⁵

3. Flexible-list PR in Sweden and its 2010 reform

For elections to the national parliament, Sweden is divided into 29 electoral districts (Hermansson, 2016). In total, 310 seats are assigned at the district-level, among those parties polling more than 4%

nation-wide, using the Modified Sainte-Laguë method. To reduce disproportionality, a second tier allocates a further 39 “leveling” seats, which are also assigned to districts. Overall, these return between 2 and 38 (in 2010) and 39 (in 2014) seats, with a median of 11.

Each party has its own ballot papers. Party ballots can either list the candidates or show no candidate names. Blank ballots are also provided at polling stations. Parties can run several lists in one district.¹⁶ Different in that regard are the Sweden Democrats, which in 2010 and 2014 used only a national list and are not included in the following analysis. As shown in Table 1, the mean and median number of candidates above the threshold increase from the pre-reform to the post-reform election. Otherwise, there is little change, notably for preference vote use, which refutes the idea that the reform had a uniform effect on lists.

Candidates can – and the most prominent party figures frequently do – run in more than one district. There are rules for resolving cases in which candidates are elected in more than one district. From parties other than the Sweden Democrats, eight (of 329) candidates were originally elected in two districts in 2010. Similarly, in 2014, eight candidates were elected in two districts, the leader of the Left Party in three and the leader of the Christian Democrats in seven.

Sweden abandoned closed lists and initially moved to a flexible list system in the national elections of 1998 (Berg and Oscarsson, 2015; Davidsson, 2006), using one optional candidate vote and a preference vote threshold of 8% (relative to all the ballots cast for the list, including those with a party vote).¹⁷ After a review of the first reform (SOU, 2007), this hurdle was lowered to 5%. As was formally required, the Swedish parliament passed the constitutional change in two parliamentary votes with a general election held in between. The votes took place in June and November 2010, which meant that all the actors were aware of the new rule for subsequent elections from the beginning of the 2010/14 legislative period.

Table 1
Descriptive statistics of party–district lists.

	Min	25%	Median	Mean	75%	Max
N candidates						
2010	10	22	25	27.38	30	59
2014	10	22	26	28.33	32	67
N seats						
2010	1	1	1	2.20	3	15
2014	1	1	1	2.12	3	13
Preference vote use						
2010	0.15	0.20	0.23	0.25	0.28	0.54
2014	0.14	0.19	0.23	0.23	0.28	0.49
Share of preference votes to list leader						
2010	0.08	0.21	0.29	0.33	0.40	0.90
2014	0.11	0.21	0.28	0.33	0.41	0.90
Gini of preference votes						
2010	0.43	0.60	0.67	0.67	0.75	0.95
2014	0.43	0.61	0.70	0.69	0.77	0.96
N candidates above threshold						
2010	0	0	0	0.45	1	2
2014	0	0	1	0.92	1	3

Note: Included are party–district combinations that used one district-level list and won at least one seat (N = 149 in 2010, N = 141 in 2014).

¹⁴ The argument shows parallels to the analysis in Fiva and Folke (2016), who use counterfactual applications of post-reform rules to pre-reform votes (and vice versa) to *separate* the mechanical and psychological effects of (inter-party) seat allocation formulas. By contrast, the interest of this study lies in how the counterfactual mechanical effect in the last pre-reform election *shapes* the (psychological) tendency to campaign for or use the personal vote in the first post-reform election.

¹⁵ Considering reform-induced changes in *expected closeness* is not a promising analytical strategy. It is questionable whether the counterfactual application of the new rule to the pre-reform election provides an accurate estimate of the expected closeness before the post-reform election since the specific set of candidates competing can change in the meantime (for results and further discussion see Table A.2 in Appendix A.4). Incorporating changes to the candidate set empirically would be problematic, especially because candidate nomination for the post-reform election itself occurs after the treatment and can be endogenous to the anticipated changes induced by the reform.

¹⁶ In both the 2010 and the 2014 elections, there was one party-in-a-district running two lists. Some parties employ a national list in addition to the district-level list. The median share of preference votes going to national-level candidates (relative to all the preference votes for a party-in-a-district) was zero and it never exceeded 3.4 percentage points. Where data availability allows (2006–2014), preference votes to national-level candidates are not counted for the dependent variable in the analyses below.

¹⁷ While preference voting was already possible, it had “in practice, absolutely no effect on election results” (Renwick and Pilet, 2016:220), and 1998 is typically seen as the year in which flexible lists were introduced.

3.1. Research design

The timing of reform passage creates an opportunity to study whether the changes in the potential impact of preference voting due to reform anticipations affected actual preference vote use in the 2014 election. Consider the example of intra-party seat allocation for a list of the Left Party from 2010, as shown in Table 2. The list won one seat, and the preference vote threshold based on the original rule was 733 votes ($\geq 8\%$ of the 9154 ballots). As no candidate reached this threshold, the post-electoral ranking remained the same as the pre-electoral list order, and Mr. Persson from position one was elected (the PAT is LR). To predict whether the reform will change the beliefs about the future PAT, it is plausible that actors start from the actual 2010 PAT, but apply the new 5% threshold. In this example, had the new rule been in place already, the threshold would have been only 458 votes ($\geq 5\%$ of the 9154 ballots), and Mr. Henriksson from position six would have received the seat. The PAT would then have switched to MX. While this logic can be applied more generally, it should be kept in mind that the 2010 election is just one electoral outcome for a given party–district list under the 8%-rule. Of most interest are lists for which applying the new rule implies a PAT they hitherto (1998–2010) had not experienced. These are cases for which the reform should change the beliefs about *possible* PATs for future elections under the new rule more generally.

Fig. 1 summarizes the distribution of the observed 2010 PAT and indicates the lists that change by applying the new 5% rule. The columns show the parties in left-to-right order and the rows display the districts sorted by the share of lists with more open PATs. The circles represent the 2010 PAT (hollow = LR, gray = MX, black = PV). The lists for which the PAT changes due to the counterfactual application of the new threshold are indicated by squares (change to MX) or triangles (change to PV). Fig. 1 shows that anticipation of the reform only made a difference for intra-party seat allocation for some lists. As a general policy implication, this means that we would not expect the reform to have a uniform effect nationally or even within districts. From a research design perspective, this provides analytical leverage. Lists with unaltered incentives, which also had the same PAT as in 2010 in earlier elections, form the basis for constructing a counterfactual trajectory of preference vote use for the “treated” lists had the reform not happened (see Appendix A.2 for a further discussion and causal graph).

The fact that the 2010 election is just one realized electoral outcome out of many potential ones has another implication. Reconsider the example in Table 2. Mr. Persson or Ms. Tsouplaki could have polled a few more preference votes if the election had played out slightly differently. If at least one of them had reached 458 votes (more than 5% of the ballots), applying the new rule would switch the PAT to PV rather than just to MX. This possibility is likely to enter actors’ calculations

Table 2

Example of intra-party seat allocation (2010, Left Party, Västmanland County district)

Ballots cast for party: 9154 Seats won: 1 Sum of preference votes: 2530.

Name	Pre-electoral list rank	Personal votes		Vote rank	Post-electoral rank (8% rule)	Post-electoral rank (5% rule)
		N	% of ballots			
K. Persson	1	431	4.7	2	1	2
V. Tsouplaki	2	429	4.7	3	2	3
L. Johansson-Arnqvist	3	131	1.4	5	3	4
A. Trygg	4	211	2.3	4	4	5
A.-C. Andersson	5	45	0.5	12	5	6
S. Henriksson	6	624	6.8	1	6	1
G. Söderström	7	21	0.2	20	7	7
...	[8–26]	[1; 79]	[0.0; 0.9]	[6; 26]	[8–26]	[8–26]



Fig. 1. PAT in 2010 and anticipated changes across lists. N = 149 lists (those using one district-level list in 2010 and winning at least one seat). Changes are indicated if applying the new 5% rule to votes as observed in the 2010 elections results in a PAT not experienced in 1998–2010.

when forming their beliefs about PATs under the new rule. To take this into account, a bootstrap elections approach (Kotakorpi et al., 2017) is used to simulate random deviations from the observed personal vote patterns (see Appendix A.3). The bootstrap results allow us to build two refined specifications for the analysis: a corrected categorical treatment approach (with two lists switching to MX recoded as switching to PV, including the one from Table 2, and one list excluded from the analysis since it had a high probability of having PAT-PV already under the 5% rule); and a continuous treatment approach, with the simulated probabilities of having PAT-MX or PAT-PV as the explanatory variables. This results in an analysis set of 103 lists, which operated practically like closed lists before the reform, with some changing (possible) PATs (N = 10 to MX and N = 9 to PV).¹⁸

3.2. Selection into the treatment and estimation

The context of the Swedish reform makes it possible to distinguish two treatment groups (consisting of the lists for which the possible PATs

¹⁸ These lists had an observed PAT-LR in the elections during 1998–2010 (as far as publicly available data allow us to judge), won at least one seat in 2010, and did not use more than one district-level list in 2010 and 2014. The models with the continuous treatment variable need not exclude the one list with the unusual bootstrap profile (N = 104).

change) and a control group (without such changes). Since the changes in the potential PATs are not randomly assigned, the treated and control lists may differ in their trajectories of preference voting in the absence of the treatment due to reform.

Difference-in-differences estimation using fixed effects is consistent and unbiased as long as this trajectory is parallel and selection into the treatment is not due to the past outcome (Morgan and Winship, 2015:373). If the past outcome affects selection into the treatment while the trajectory is parallel, conditioning on the lagged outcome is required (Morgan and Winship, 2015:388). Since data on the development of preference voting before the reform are available, selection into the treatment can be examined. Fig. 2 summarizes the link between treatment and two explanatory variables, namely a one-period lag of preference vote use and the mean of the one-period to four-period lags. The former captures selection based on the past outcome, whereas the latter serves as a proxy for a list fixed effect (Morgan and Winship, 2015:383–385). The four panels in Fig. 2 show the predictors in the columns (scales on the x-axes) and the types of PAT changes (from LR to MX and from LR to PV) in the rows. Each subpanel plots the treated cases at the top and the control group observations at the bottom (with random jitter on the y-axis). The curves represent the fitted values from the bivariate logistic regressions.

The conclusions are clear. Changes from PAT-LR to PAT-MX are driven by preference vote use as in the previous election (the fitted line is steeper in the top-left panel than in the top-right one). The switch from PAT-LR to PAT-PV is due to the fixed party–district characteristics (the curve is steeper and estimated with less uncertainty in the bottom-right panel than in the bottom-left one).¹⁹ This means that selection effects must be taken into account, but differently so for the two types of treatments. The effects of a change to PAT-MX are inferred from those models with a lagged dependent variable, whereas those of a change to PAT-PV come from the models with list fixed effects. This way of proceeding is backed up by the results of a specification test with pre-treatment outcomes, as reported in Table A.4 in Appendix A.6. When including 2010 preference vote use (fixed effects), the PAT-MX (PAT-PV) treatment does not show any association with 2006 preference vote use.

Let y_{it} be preference vote use by voters of list i at time $t = \{1, 2\}$, corresponding to 2010 and 2014. With indicator variables toMX and toPV for the categorical treatments, the statistical models are

$$y_{i2} = \beta_0 + \beta_1 \text{toMX}_i + \beta_2 \text{toPV}_i + \beta_3 y_{i1} + \gamma \mathbf{P}_i + \varepsilon_{i2} \quad (\text{LDV}),$$

$$y_{it} = \beta_1 \text{toMX}_i * (t - 1) + \beta_2 \text{toPV}_i * (t - 1) + \beta_3 (t - 1) + \gamma \mathbf{P}_i * (t - 1) + \delta_i + \varepsilon_{it} \quad (\text{fixed effects}),$$

where \mathbf{P}_i is a vector of the party indicator variables. The estimation is based on ordinary least squares (OLS).²⁰ Standard errors are clustered by the 29 electoral districts, using the small-sample methods (CR2 estimation and Satterthwaite correction) recommended by Pustejovsky and Tipton (2018).²¹

¹⁹ As shown in Table A.3 in Appendix A.5, a multinomial logit model that includes both predictors returns a coefficient of the one-period lag that is statistically significant at 10% for the change to PAT-MX, while that of the mean across four lags is significant at 5% for the change to PAT-PV.

²⁰ While the dependent variable is a proportion, OLS is appropriate, since the aim is to infer the average causal effect rather than to make predictions (Angrist and Pischke, 2008). In addition, Table 1 shows that no observed values of the dependent variable are near the boundaries. Estimating non-linear panel data models for fractional responses is complex (Papke and Wooldridge, 2008). Lastly, the models with the categorical treatment variables are essentially non-parametric comparisons of the mean rates.

²¹ Appendix A.7 shows that using alternative specifications for the standard errors does not change the conclusions.

4. Results

Fig. 3 illustrates the trend in preference vote use across lists over time. The group of cases switching to PAT-MX is displayed with (red) squares, that switching to PAT-PV with (brown) triangles, and the control group with (gray) points. Fig. 3 reinforces the findings from the selection into the treatment analysis above. For the lists changing to PAT-PV, the trajectory parallels that of the control group, suggesting a link to time-invariant features. By contrast, the cases switching to PAT-MX show a stronger increase in preference vote use in the 2010 election, implying a positive selection on the pre-reform outcome. More importantly, while the control group shows a slight decrease in preference vote use between 2010 and 2014, the trend remains flat for the lists switching to PAT-MX. Moreover, the lists changing to PV experience a considerable drop in preference vote use, which is unexpected. However, while the graph summarizes the broader trends, a more systematic test requires statistical modeling.

Table 3 displays the findings from the analysis with the categorical treatment variables.²² The regressions model preference vote use in the 2014 election as a function of the treatment in specifications using either a 2014 cross-section with the lagged dependent variable (M1, M2) or a 2010–2014 panel with party–district fixed effects (M3, M4). M2 and M4 include additional dummy variables to allow for nation-wide differences in 2014 levels (M2) or 2010–2014 trends (M4) in preference vote use across party electorates. Based on the discussion of selection patterns above, interpretation focuses on the LDV models for the switch to PAT-MX. The point estimate is positive, suggesting that the party lists whose PAT changed to the mixed type receive an additional 1.6 percentage points in preference vote use in 2014 (2.2 percentage points in M2, with party indicators included). However, there is considerable uncertainty in these estimates, and the confidence intervals include zero. This finding is intuitive, given that the practically zero slope in Fig. 3 is similar to the trend line for the control group.

The regression results corroborate the surprising pattern for the lists that moved from PAT-LR to PAT-PV. Focusing on the fixed effects specification as suggested by the selection analysis, the lists that become more open in that sense see a drop in preference vote use of 3.5 percentage points [-6.0, -1.0] (M3) and 4.4 percentage points [-7.3, -1.5] (M4, with varying party trends). This is a considerable effect given the control group 2014 mean of about a fifth of voters supporting a candidate (0.215). This finding suggests that actors responded when the reform altered the potential allocation strongly, but in an unexpected way.

There is no across-the-board increase in preference vote use between 2010 and 2014 for the analyzed set of PAT-LR lists. The results of the fixed effects specification in M4 suggest a slight increase for the Center Party (intercept, 1.8 percentage points), but the confidence interval includes zero. All the other parties experience even lower increases or a net reduction in preference vote use over time. Overall, the changes in the PAT resulting from reform anticipation indeed affect preference vote use, but not necessarily as expected.

The models discussed so far used the categorical treatment variables. A downside of this approach is that some lists may experience more gradual changes in the probabilities that actors ascribe to the three allocation types. Table 4 summarizes the models with the bootstrap-based difference in the simulated probability of having a PAT-MX/PV under the 8% and 5% rules as a continuous treatment variable. This is a more fine-grained measure, but its use assumes that the effect is linear; for example, a list making the full transition in the form of a zero to one probability change for PAT-PV would experience twice the change in preference vote use than a list whose probability of having a PAT-PV increases by only 0.5.

Models 5 and 6 in Table 4 provide a larger estimate for a unit switch

²² As shown in Table A.6 in Appendix A.8, the results are similar when using the categorical treatment without bootstrap-motivated recordings.

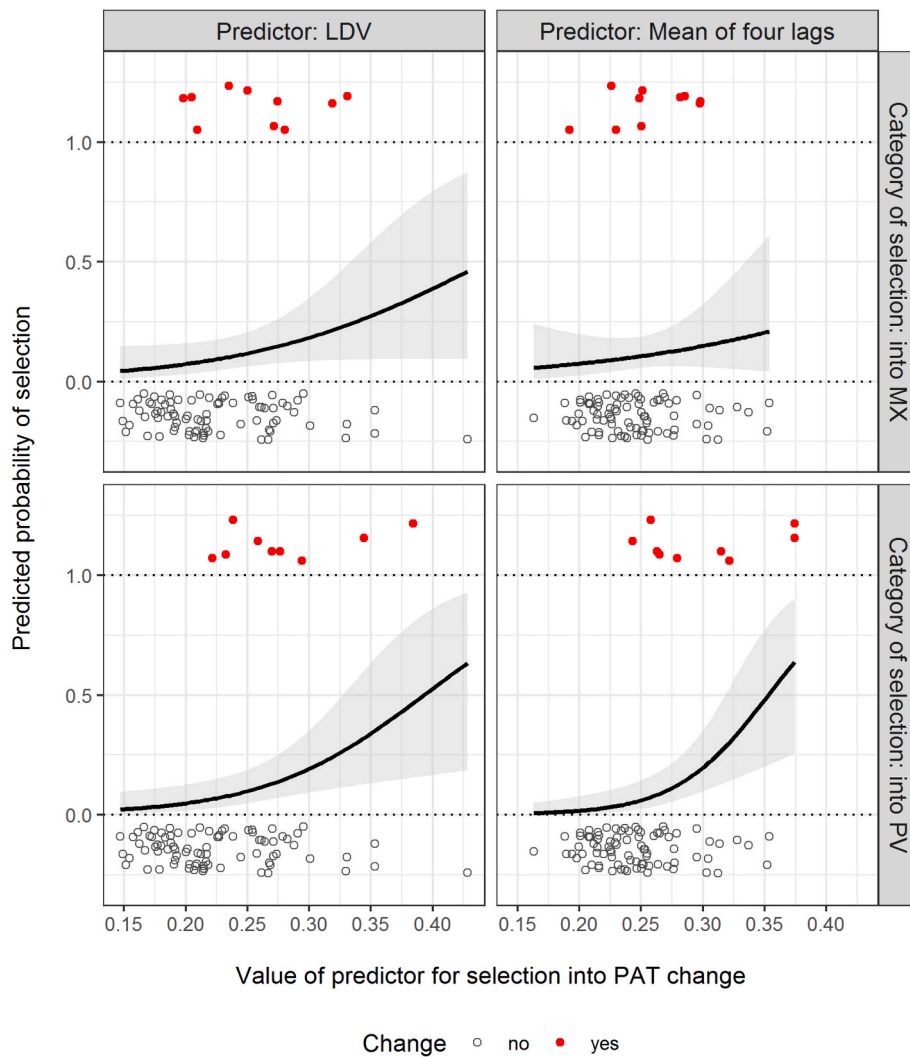


Fig. 2. Selection into changes of PAT. $N = 103$ lists from the main analysis set. Uses the categorical treatment with bootstrap-motivated recodings. Curves are bivariate logit fits. Random jitter on the y-axis avoids overplotting.

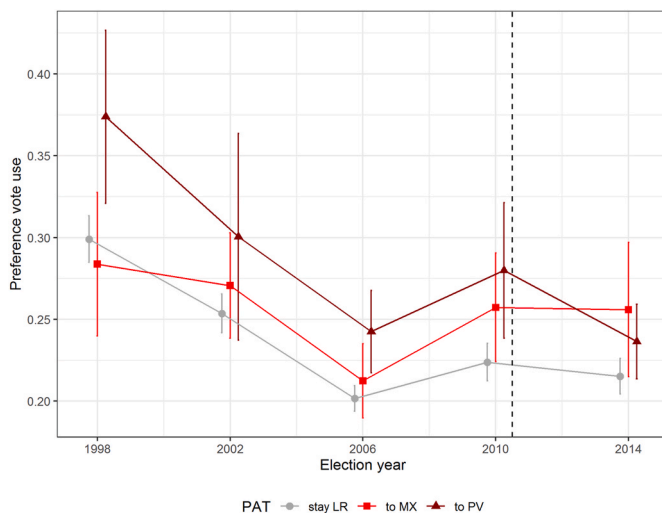


Fig. 3. Trends in preference vote use over time, by the change in PAT. $N = 103$ lists in the main analysis set. Confidence intervals are based on the t -distribution.

from PAT-LR to PAT-MX than the corresponding Models 1 and 2. Preference voting in such lists increases by an estimated 2.9 percentage points $[-0.1, 5.9]$ (M5) and 3.7 percentage points $[0.3, 7.1]$ (M6). Given the control group 2014 mean share of 0.215, this estimate is non-negligible. Hence, these results suggest that when it is possible to reorder the list, the main characteristic of PAT-MX, use of the preference vote option indeed increases. The estimated effects are also more pronounced for switches to PV. M7 and M8 show the sizeable reduction in candidate voting for those lists, equaling -4.1 percentage points $[-7.2, -1.1]$ and -6.2 percentage points $[-9.4, -2.9]$. Finding larger effects with the continuous treatment concurs with the broader theoretical argument. To make actors believe that a PAT other than LR is *possible* in 2014, it is not necessary that the new rule had actually switched the 2010 PAT; it is sufficient that it was only possible.

Overall, the findings propose a nuanced picture of how the potential impact affects preference vote use. First, lists whose ordering became changeable due to the reform saw a subsequent increase in candidate voting. Although this is expected, the size of the effect and its uncertainty vary by specification. The second result is more clear-cut. Lists that switched from an allocation based entirely on the list rank to a potential allocation where a prominent list rank may no longer be sufficient for election underwent, contrary to expectations, substantial drops in preference vote usage.

Why did preference voting become less frequent in these cases? We

Table 3
Regression models with the categorical reform treatment.

	(M1) LDV	(M2) LDV + party	(M3) Fixed effects	(M4) Fixed effects + party
Change to PAT-MX	0.016 [-0.016, 0.048]	0.022 [-0.014, 0.058]	0.007 [-0.028, 0.042]	0.014 [-0.022, 0.05]
Change to PAT-PV	-0.02 [-0.039, -0.002]	-0.032 [-0.057, -0.008]	-0.035 [-0.06, -0.01]	-0.044 [-0.073, -0.015]
Lagged preference vote use t = 2014	0.736 [0.639, 0.833]	0.732 [0.626, 0.838]	-0.009 [-0.015, -0.002]	0.018 [-0.006, 0.043]
Party indicators (ref.: Center)				
FP		-0.022 [-0.046, 0.002]		-0.015 [-0.038, 0.008]
KD		-0.011 [-0.043, 0.021]		-0.015 [-0.05, 0.021]
M		-0.041 [-0.063, -0.019]		-0.033 [-0.055, -0.01]
MP		-0.019 [-0.06, 0.022]		-0.011 [-0.056, 0.035]
S		-0.043 [-0.065, -0.021]		-0.048 [-0.076, -0.019]
V		-0.021 [-0.048, 0.007]		-0.028 [-0.06, 0.004]
Constant	0.051 [0.027, 0.075]	0.079 [0.046, 0.113]		
N	103	103	206	206

Note: Dependent variable is the proportion of list voters casting preference votes for candidates on the district list. Shown are the OLS coefficients, with the 95% confidence interval based on clustered standard errors (district level, CR2, with Satterthwaite correction) in brackets. The 2014 outcome for the untreated lists is ≈ 0.215 .

Table 4
Regression models with the continuous reform treatment.

	(M5) LDV	(M6) LDV + party	(M7) Fixed effects	(M8) Fixed effects + party
Change in Pr(PAT-MX)	0.029 [-0.001, 0.059]	0.037 [0.003, 0.071]	0.016 [-0.019, 0.051]	0.025 [-0.011, 0.061]
Change in Pr(PAT-PV)	-0.021 [-0.044, 0.003]	-0.047 [-0.074, -0.019]	-0.041 [-0.072, -0.011]	-0.062 [-0.094, -0.029]
Lagged preference vote use t = 2014	0.736 [0.643, 0.828]	0.732 [0.635, 0.829]	-0.009 [-0.016, -0.003]	0.026 [0.006, 0.047]
Party indicators (ref.: Center)				
FP		-0.028 [-0.051, -0.004]		-0.022 [-0.045, 0]
KD		-0.017 [-0.044, 0.01]		-0.026 [-0.057, 0.006]
M		-0.049 [-0.069, -0.028]		-0.042 [-0.062, -0.022]
MP		-0.029 [-0.069, 0.01]		-0.022 [-0.066, 0.023]
S		-0.052 [-0.072, -0.033]		-0.058 [-0.083, -0.033]
V		-0.027 [-0.052, -0.003]		-0.036 [-0.065, -0.007]
Constant	0.049 [0.027, 0.072]	0.085 [0.056, 0.115]		
N	104	104	208	208

Note: Dependent variable is the proportion of list voters casting preference votes for candidates on the district list. Shown are the OLS coefficients, with the 95% confidence interval based on clustered standard errors (district level, CR2, with Satterthwaite correction) in brackets. The 2014 outcome for the untreated lists is ≈ 0.215 .

may initially suspect that some form of regression-to-the-mean is in place, but the evidence rejects this. First, the selection into the treatment analysis discussed above suggests that time-invariant factors rather than previous preference vote use distinguishes lists with a change from PAT-LR to PAT-PV from those with constant PAT. This makes it less plausible that the units switching to a preference-vote based allocation simply observed exceptionally frequent preference voting in 2010 that reverted to the mean in 2014. To investigate this further, the analysis is extended to the second post-reform election of 2018. [Figure A.6](#) in [Appendix A.9](#) shows that, while preference vote take-up for lists that had earlier changed from PAT-LR to PAT-PV indeed increased again between 2014 and 2018, it did not recover to the mean level of 2010. Moreover, [Table A.7](#) shows that the pattern of effects found in 2014 is mirrored when modeling the change between 2010 and 2018 (while allowing for generic party-specific changes in that period). The coefficients are marginally lower in absolute terms, and there is more uncertainty in the estimates, but the conclusion remains the same: lists switching to PAT-MX experienced a moderate increase in preference vote use, while those switching to PAT-PV saw fewer voters choosing a candidate.

Instead, the unexpected finding may suggest less of a need to use personal votes, since parties promoted candidates popular in 2010 to better list positions in 2014. Alternatively, if district-level parties anticipated that the list rank may lose its relevance for determining seat assignment, did local party branches restrict personal campaigns or were they more careful in selecting candidates? It turns out that all nine lists switching from PAT-LR to PAT-PV won just one seat in the 2010 election (eight of which did so again in 2014, whereas one list lost their seat),

which is not surprising given the definitional requirement of having at most the list leader reaching the threshold under the 8% rule and more than 5 candidates above it based on the 5% rule. To avoid an apples-and-oranges problem, comparing the nine lists with those 36 non-switching lists that also won one seat in 2010 seems the best option.

Contrasting these two groups reveals some interesting patterns. When looking at whether candidates from 2010 rerun in 2014, the outcomes for the 2010 list leaders appear similar (keeping in mind the small number of cases). In the switching-to-PV group, 60% (3/5) of the list leaders that also topped the preference vote ranking and 50% (2/4) of the other list leaders stood again, with corresponding rates of 50% (16/32) and 75% (3/4) in the comparison group. By contrast, the 2014 rerunning rates for candidates that were popular with voters in 2010 but failed to be elected, defined as those on the first or second preference vote rank in 2010, differ.²³ In the switching-to-PV group, only 33% (3/9) of these appeared on the 2014 ballot, while 58% (21/36) did so in the comparison group.²⁴ Hence, there is no support for the notion that 2010 list leaders resigned when competition became more personal vote based. Instead, the strong but unsuccessful 2010 candidates did not feature again in 2014. While this remains speculative, it seems plausible that 2010–2014 incumbents fended off popular competitors when the

²³ This does not include candidates elected in more than one district.

²⁴ The difference in the form of the odds ratio becomes even more pronounced in a Bayesian logit regression adjusting for covariates (list rank, preference vote rank, sex, age and age squared, seniority, party) than in a bivariate model.

reform made them more of a threat to their re-election. Further support for this argument comes from comparing the ratio of the best to the second-best individual preference vote tally. In 2010, competition for personal votes was much tighter in lists switching to PV (1.40, t-based CI [1.19, 1.61]) than in the comparison group (2.50 [1.72, 3.29]). In 2014, the most popular candidate outperformed the runner-up in both groups to a similar degree (mean factors of 2.68 [1.14, 4.23] and 3.09 [1.95, 4.29]). Hence, either the one-seat lists strongly affected by the reform chose weaker candidates, or they found ways to restrict personal vote campaigns.²⁵

5. Conclusion

This study examined whether more flexible lists increase the take-up of preference voting. It argued that a reform lowering the preference vote threshold should do so only if it changes actors' beliefs about possible seat allocation types. The empirical analysis focused on the 2010 Swedish preference vote reform. Leveraging on the idea that anticipating reform effects should have altered those beliefs only for some lists allowed us to establish systematic counterfactual trajectories of preference voting, had the reform not occurred.

The results show that lists for which the reform introduced the possibility of changes to the pre-electoral ranking saw an increase in preference vote use. While this finding is somewhat uncertain, it provides more persuasive evidence for the theoretically intuitive link between preference vote impact and take-up than that proposed in earlier work. However, lists for which the reform undermined the value of pre-electoral rank for securing an election consequently experienced a drop in candidate voting. Additional analyses support the interpretation that local parties reacted to the institutional change by selecting less popular challengers or limiting personal campaigning (unless candidates practiced self-restraint). There may have been pressure or consensus that voters should not be encouraged to disturb the ranking decision by party selectors. This interpretation would concur with studies suggesting that parties restrict personal campaigning (Bøggild and Pedersen, 2018), even in preferential-list PR systems (Cheibub and Sin, 2020).

Since the analysis studied just one instance of reform, it is justified to ask whether the results can be generalized. As Renwick and Pilet (2016:236–237) point out, the Swedish reform of 2010 did not actually increase preference voting at the level of the entire electorate, which makes it stand out from similar cases in four other long-standing democracies (Austria, Belgium, Netherlands, Norway). While the empirical analysis presented in this paper is not designed to infer any *overall* effect of reform, the theoretical arguments suggest that no uniform effect across all the lists should be expected.²⁶ The more or less stable level of candidate voting at the aggregate level may result from the fact that many lists in Sweden did not change possible PATs (cf. Fig. 1) and that those lists switching to PAT-PV experienced a drop in preference voting. Positive aggregate changes in other countries may simply be due to more lists moving to the mixed type of allocation, or the counteracting of switches to preference vote-based assignment may not have happened there.²⁷ This represents a possible future research direction.

²⁵ An alternative explanation might be that lists switching to PAT-PV attract new voters who are less likely to vote for candidates, perhaps because more intense personal campaigning makes it harder for voters to remember or choose among candidates. This does not seem to be the most plausible account, especially because Oscarsson and Holmberg (2018:8) estimate that the proportion of voters switching parties between 2010 and 2014 (among citizens turning out in both elections) was only 35.4%.

²⁶ It is conceivable, however, that a high number of changes at the list level triggers aggregate responses by national party organizations, which could give rise to additional party-level effects.

²⁷ Reactions to the switches to PAT-PV in Sweden may also have played out this way because the affected lists had all won just one seat in 2010.

The PAT concept and argument that reforms affect behavior by changing the beliefs about possible PATs provide a new approach for analyzing electoral reforms at the intra-party dimension. They can be used to examine past reforms as well as predict the effects of reform proposals. The Swedish reform of 2010 is especially suitable for examining the consequences of the anticipated reform effects since its passage long before the first post-reform election does not require any assumptions about the timing of personal vote-seeking efforts. Reform adoption closer to upcoming elections does not make the approach unfeasible, but does alter what can be inferred. Changes in the beliefs about possible PATs could also be used to study “learning” about rule changes *after* the first post-reform election. However, this would not equal a study of an institutional effect in the narrow sense. As the Swedish case shows, the anticipation of mechanical effects can already lead to psychological reactions in the first election under new rules.

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Appendix A. Supplementary data

Supplementary data to this article can be found online at <https://doi.org/10.1016/j.electstud.2020.102232>.

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