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Cue-taking, satisficing, or both? Quasi-experimental evidence for ballot position effects *

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Abstract

Ballot position effects have been documented across a variety of political and electoral systems. In general, knowledge of the underlying mechanisms is limited. There is also little research on such effects in preferential-list PR systems, in which parties typically present ranked lists and thus signaling is important. This study addresses both gaps. Theoretically, we formalize four models of voter decision-making: pure appeal-based utility maximization, implying no position effects; rank-taking, where voters take cues from ballot position per se; satisficing, where choice is a function of appeal, but voters consider the options in the order of their appearance; and a hybrid "satisficing-with-rank-taking" variant. From these, we derive differential observable implications. Empirically, we exploit a quasi-experiment, created by the mixed-member electoral system that is used in the state of Bavaria, Germany. Particular electoral rules induce variation in both the observed rank and the set of competitors, and allow for estimating effects at all ranks. We find clear evidence for substantial position effects, which are strongest near the top, but discernible even for the 15th list position. In addition, a candidate's vote increases when the average appeal of higher-placed (but not that of lower-placed) competitors is lower. Overall, the evidence is most compatible with the hybrid satisficing-with rank-taking model. Ballot position thus affects both judgment and choice of candidates.

Keywords: ballot position effect; open-list PR; satisficing; bounded rationality; electoral systems

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1 Introduction

Candidates near the top of a ballot paper and particularly in the first position receive more votes than candidates listed further down. This observation has been made across a variety of political and electoral systems (see Miller and Krosnick 1998; Blom-Hansen et al. 2016 for reviews). Existing work has also consistently found that ballot position effects are stronger when voters have little knowledge about the candidates (Miller and Krosnick 1998; Brockington 2003; Pasek et al. 2014; Geys and Heyndels 2003; Kim et al. 2015). There are, however, at least two gaps in understanding ballot position effects, which we address in this paper.

First, ballot position effects are compatible with various decision-making processes. Existing work does not always keep these apart in theoretical terms and, to our knowledge, has not tried to disentangle them in a real-world setting that allows for causal identification. On the one hand, voters may favor candidates listed near the top due to cognitive bias (e.g. Krosnick and Alwin 1987) or on the basis of heuristics, i.e. by interpreting ballot position as signal, be it unintended (Meier and Robinson 2004; Kim et al. 2015) or intended (e.g. Katz and Bardi 1980: 108-112). On the other hand, a benefit from a high position is also in line with a classic satisficing model (Simon 1955; Caplin et al. 2011; Rubinstein and Salant 2006): ballot position determines the order in which candidates are evaluated, and the first option deemed "good enough" is chosen.[1] However, different considerations may enter the judgment of acceptability in a satisficing explanation. We innovate by distinguishing overall four models of voter decision-making: appeal-based utility maximization (no ballot position effects) as null model; "rank-taking" (ballot position effects, albeit in varying size, for all candidates), pure satisficing (with

[1] To avoid confusion, note that throughout the text we use the words "high" and "low" in the context of list ranks in a positional way, i.e. higher means further up.

order affecting only the sequence of consideration, but not the verdict of sufficiency), and a hybrid model (with ballot position influencing both the order of consideration and the assessment of sufficiency). We differentiate the models in a formal way and derive observable implications discriminating among them.

Second, empirical work on ballot position effects focuses on what Shugart (2005) refers to as nominal electoral systems, which allocate seats to candidates rather than parties. When there is just one candidate per party, as typically the case in single member districts (SMDs), ballot position effects refer to a fused choice of candidate and party. In contrast, preferential-list PR (PLPR) systems, as, e.g. , frequently found in Europe, allow voters both to choose a party and to express preferences for individual candidates on its list.[2] Given the importance and strength of party labels in (most) parliamentary systems of Europe, here the more interesting type of order effect concerns the intra-party competition among candidates. What makes studying ballot placement effects particularly interesting in this institutional setup is that parties typically present ranked lists to voters, so ballot position may be interpreted as a substantive signal.[3] At the same time, this makes the identification of ballot position effects especially difficult. Because parties likely try to signal to voters, anticipate position effects, or simply agree with citizens on what constitutes a "good" candidate, list placement will be strongly correlated with candidate quality.[4]

Under PLPR, a natural experiment based on random rotation of candidates across

[2] In 2011, from 30 European countries covered by Pilet et al. (2016), 60% used a PLPR system (ten with flexible lists, six with open lists and two with open list panachage systems).

[3] With semi-open/flexible lists, pre-electoral list ranks can matter for intra-party seat allocation after the votes are cast, since candidates need to reach a certain number of preference votes to qualify for being moved to the top of the post-electoral ranking (Marsh 1985; Renwick and Pilet 2016). However, even in fully open list PR (OLPR) systems, where this is not the case, lists may be intentionally ordered. In elections to the European Parliament, five out of ten OLPR cases (Estonia, Italy, Latvia, Lithuania and Poland) use ranked lists although the pre-electoral ranks have a direct influence on post-electoral vote–seat translation only in the case of a tie (Däubler and Hix 2017: 3).

[4] Throughout the paper, we use the terms list position, list rank and ballot position interchangeably.

electoral subunits, now akin to a methodological gold standard in nominal systems (e.g. Miller and Krosnick 1998; Chen et al. 2014; Pasek et al. 2014; Ho and Imai 2008), is typically not available. In addition to observational work (e.g. Villodres 2003; Marcinkiewicz 2014; Lutz 2010; van Erkel and Thijssen 2016), two studies draw on quasi-random variation created by multi-column ballot designs (Geys and Heyndels 2003; Blom-Hansen et al. 2016). However, this approach does not allow for inferences about position effects at the top of the list, which are theoretically and substantively the most interesting ones. We study causal effects of ballot placement for all ranks, while distinguishing between competing explanations for them. To do so, we use the unique institutional setting of the OLPR tier in elections to the state parliament of Bavaria, Germany.

Bavaria employs an unusual form of a mixed-member proportional system with region-specific open lists in addition to a single member district (SMD) tier (Massicotte 2011). As the key feature that we harness, all SMD-candidates need to run on regional lists (covering several SMDs), but each SMD candidate is removed from this list in her local SMD. Hence, the open list varies over SMDs by one candidate. Thus, we observe the same candidates in different list positions, in the form of upward shifts by one slot, for all realized ranks. Faas and Schoen (2006) were the first to use the quasi-experimental setting in this case. They provide descriptive results for the (supposedly) exogenous rank effect, showing that the median and average vote shares of candidates improve by moving up the list, especially when shifting from second to first rank. While theirs is a valuable contribution, it just studies whether ballot placement effects are present, for just one election, and does not consider the uncertainty in what is an estimated effect. Importantly, we argue that the Bavarian setting can actually also teach us about the mechanisms underlying ballot position effects and voter-decision-making under OLPR more generally. The shift in ballot placement through dropping a candidate actually goes hand in hand with variation in the

choice set; this is what helps us to learn about the mechanisms driving placement effects. Our study appears to be the first that compares the evidence for the different theoretical approaches in one and the same real-world setting.

We find clear evidence for considerable ballot position effects in Bavaria's OLPR tier. These are higher near the top of the list, and they vary with changes in the choice set, depending on the appeal of competitors ranked above a candidate. In quantitative terms, moving from rank two to rank one implies a more than four-fold increase in vote share. Even on rank 15, the average vote share of a candidate still increases by 14% (albeit from a small baseline) when moving up one rank . Additionally, we find that a candidate's votes are not consistently affected when the set of competitors listed below that candidate changes. Taken together, these results are most compatible with the hybrid "satisficing-with-rank-taking" model, where voters consider the candidates in the order of their appearance, while ballot position as such also informs the decision whether a candidate is "good enough". Overall, we provide evidence that bounded rationality approaches are helpful in understanding placement effects in general and candidate choice under PLPR in particular. In the conclusion, we discuss the wider implications of our findings for voter decision-making.

2 Four ideal-types of voters' decision-making

Building on and extending the main arguments from the literature, we discuss four models (appeal-based decision-making, rank-taking, pure satisficing, and satisficing with rank-taking) of how voters can decide among different candidates from the same party, and how ballot position influences choice in each model. By doing so, we distinguish two types of effects: Primarily, we want to assess the effect of a candidate's own list position on her performance in the four theoretical models. Empirically, we will later identify the effects

of marginally improving one's list position. In addition, we consider the effect of the competitive environment on a candidate's performance, i.e. of the characteristics of other candidates in the voters' choice set. This helps us to further distinguish the four models. Empirically, we will later assess the effects of marginal changes to the set of competitors (i.e. removing a candidate from this set). Our arguments refer to a situation in which voters choose from candidates within a party, from a pre-ranked list.[5] Considerable parts of the reasoning, however, also translate to nominal systems in which a considerable number of voters have weak preferences over alternatives, as further discussed in the conclusion.

We start with what corresponds to the null model regarding ballot position effects. Appeal-based deciders assess the full set of candidates based on one or several candidate features. We understand appeal broadly as any candidate feature other than the list position per se. It therefore subsumes an array of candidate characteristics which have been shown to affect the candidate vote under OLPR, such as gender (e.g. Wauters et al. 2010; Górecki and Kukołowicz 2014), local roots (e.g. Tavits 2010; Jankowski 2016), incumbency (e.g. Dahlgaard 2016), or ideology (Blumenau et al. 2017). Thus, our definition of appeal includes, in the terminology of Brockington (2003: 3-4), both primary information the voter collected prior to entering the polling booth and secondary information inferred from the ballot (e.g., gender, residence, or occupation). This broad definition of appeal results from our focus on isolating effects of ballot position from all other factors, even though secondary information may closely resemble third-level ballot position in acting as a cue. In the case of appeal-based decision-making, the electoral result of a candidate

[5] As discussed by Rudolph and Däubler (2016: 748-749) in more detail, this is a plausible approach in the context of OLPR. Many citizens can be expected to choose first a party and then a specific candidate, and even if they do not proceed that way, differences between the most preferred candidates within lists are unlikely to affect the choice between lists (as long as party-level factors weigh strong in voters' utility function, which is typically the case in the parliamentary systems of Europe).

is a consequence of a candidate's appeal relative to the aggregated appeal of the whole set of candidates to choose from. List position per se would not influence candidates' vote shares.[6] Similarly, with changes in the set of competitors, only their appeal, but not their list placement would matter.

In what we refer to as the rank-taking model, voters make their decision exclusively on the basis of the candidate's observed list position. Different mechanisms may underlie such effects. First, particularly in an institutional setting with ranked candidate lists, voters may turn to (list) position as a signal of candidate quality (Katz and Bardi 1980; Lutz 2010; Marcinkiewicz 2014; van Erkel and Thijssen 2016; Blom-Hansen et al. 2016)– even in the extreme case of not knowing anything about the candidates. A position near the top of the ballot constitutes a form of endorsement by the party, which voters may use as orientation in their choice, even if these candidates are actually not more appealing.[7] Second, citizens may resort to a heuristic linked to generic spatial associations of the form "up is good" (Meier and Robinson 2004; Tourangeau et al. 2013; Kim et al. 2015). Resorting to such shortcuts is particularly likely when deciders have little information about the options or little is at stake (Miller and Krosnick 1998; Brockington 2003). Finally, rather than based on the use of heuristics, voters may, in cognitive terms, engage more closely with options they encounter first, which subsequently leads to a greater probability of choosing them (Krosnick and Alwin 1987). If cognitive bias is the driving mechanism of a pure ballot position effect, speaking of cue-taking would be misleading.

[6] Empirically, list position and the candidate vote would be correlated, of course–this, however, is only a consequence of unobserved confounders (e.g. media attention after the ranking is made (van Erkel and Thijssen 2016)) that may add to the appeal of a candidate.

[7] Through a related mechanism, list rank may signal the viability of a candidate, which would matter to voters who choose strategically and may also anticipate that many other voters take list position as a cue. We do not elaborate on this, since strategic voting should not be relevant to the ballot position effect that we identify in our context and with our research design. In addition to our doubts that many voters cast their OLPR vote instrumentally, very sophisticated voters understanding the consequences of the complex Bavarian electoral system would also grasp that viability in the OLPR tier must be assessed on a region-wide basis, rather than using the list observed in the given SMD.

For the purpose of this paper, we therefore speak of rank-taking. Hence, with rank-taking observed list position determines votes, and the composition of the competitor set would be irrelevant to a candidate's performance.

A third way of explaining ballot position effects centers around satisficing (e.g. Simon 1955; Miller and Krosnick 1998; Ho and Imai 2008; Caplin et al. 2011; Meredith and Salant 2013). According to this model, voters do not engage in a comparison and choose the best alternative from the entire choice set but evaluate candidates sequentially. Starting at the top of the ballot, voters choose the first candidate who is satisfactory, since continuing to evaluate the alternatives is tiring or not worth the effort. This is in line with findings from PLPR-focused observational studies pointing to a vote bonus of the best-ranked candidate with an (on average) appealing characteristic such as being female (Blom-Hansen et al. 2016) or living in the same area (Jankowski 2016).[8]

In what we refer to as a pure satisficing world, ballot position affects the order of candidate assessment, but not the actual verdict of acceptability. The probability to choose any of the lower-ranked candidates depends on the chance that such a candidate is "good enough", provided that she reaches the consideration stage, i.e. none of the higher-ranked candidates already satisfied the voter. We would therefore not only expect position effects in this world (as higher-ranked candidates are evaluated earlier), but also composition effects if the appeal of competitors *above* a candidate changes.

The three ideal types introduced thus far are mutually exclusive. We can, however, also think about a hybrid form, in which a voter considers the candidates in order of their appearance on the ballot, while also taking into account the list position for judging the acceptability. According to this logic, the order on the ballot affects choice through

[8] Meredith and Salant (2013), on the other hand, report that simple satisficing cannot explain the patterns found in multi-winner races in California city council and school board elections, employing random rotation.

two mechanisms: it determines the sequence of consideration like in the plain satisficing model, but the ballot position (here in addition to appeal-based factors) influences the assessment whether a given candidate under evaluation is assessed as being "good enough", comparable to the rank-taking case. We call this form of choosing satisficing with rank-taking.

3 Bavarian state elections as a quasi-experimental setting

The four worlds of decision-making outlined above suggest that ballot position, candidate appeal, or a combination thereof influence the personal vote of a candidate. Ideally, we would therefore like to observe candidates in an environment in which list position (or a candidate feature) is randomly assigned, or in a setting in which we observe the same candidate in different ballot positions (or with just one varying feature).

In some contests under nominal electoral systems, ballot position assignment is random and possibly combined with permutation across geographical subunits. Under preferential-list PR rules, however, parties frequently present ranked lists. On the one hand, this is what makes rank-taking particularly interesting to study. On the other hand, a party-chosen ranking is clearly at odds with an appeal-independent ordering of candidates. Similarly, it is rare to find that vote-attracting candidate features are randomly assigned (with the exception of incumbency in close races, see, e.g. Dahlgaard (2016) for PLPR systems).

The Bavarian case we study has specific features that lead to quasi-experimental variation in both candidate rank and the set of competitors. In Bavaria, 90[9] local single-

[9] As of 2013; 2008: 91; 2003: 92

member districts (SMDs, in Bavaria referred to as *Stimmkreise*) are clustered in seven regional districts (*Wahlkreise*). Citizens have two votes: one for a single-member plurality race, another for an OLPR contest, with which they choose one specific candidate from one of the open party lists within the seven regional districts. Preference voting is induced by the ballot layout, as there is no specific option to not choose an individual candidate presented to voters.[10] In addition to the candidate's name, the ballot provides information about occupation, academic title, and place of residence.

Candidates can compete only in one regional district. The regional lists are prepared by parties, with democratic intra-party procedures deciding upon list positions. Parties present one list per regional district, which assigns each candidate a fixed pre-election ranking (which we refer to as a candidates' 'baseline list position', r_{c_i}). Among the candidates presented on the list, some also compete as SMD candidate. The origin of the quasi-experiment lies in the fact that the Bavarian electoral rules allocate seats within parties based on the *sum* of a candidates' first/SMD and second tier/OLPR votes (passing over the already elected SMD winners). To rule out an advantage of unsuccessful SMD candidates in comparison to list-only co-partisans when summing individual votes across tiers, SMD candidates do not run on the party list in their SMD. They are omitted from the OLPR list.

Hence, all other OLPR candidates with ranks $r_c < r_{c[SMD]}$ move up one observed rank in this SMD. Therefore, across local districts (within each of the seven regions), the appearance of the regional OLPR list varies: one of the candidates from the full list (which is never realized as an actual choice set) is removed, and any candidates placed below the

[10] Although not doing so, i.e. indicating a party preference by markings on the list, has been deemed lawful by the constitutional court. Marcinkiewicz and Stegmaier (2015) suggested that obligatory preference voting is associated with stronger placement effects compared to systems where the candidate vote is optional and less informed voters may vote for the list "as a whole". However, whether this argument extends to placement effects beyond the first list position is theoretically unclear.

removed candidate move up one spot. Consequently, we observe as many realizations of a list with $C-1$ candidates as there are SMDs in that region. This aspect of the Bavarian electoral system was first used by Faas and Schoen (2006) to describe pure ballot position effects . As we will discuss next, there is, however, much more to learn from the Bavarian setup about such effects and voters' decision-making.

3.1 Deriving observable implications

To obtain specific expectations, it is helpful to introduce some formal notation. Candidates c appear on the ballot on the observed list rank r_c and have appeal a_c. The utility of voter i to vote for candidate c is:

$$U_{ic} = \alpha\, a_c + \beta_r r_c + \epsilon_{ic}$$

Since we do not expect the utility of list position to be a linear function of that position (e.g. since the strength of the signal the party sends is non-monotonically decreasing), β_r represents the discrete value of rank r.

Under standard utility maximization with type I extreme value errors (Thurner 1998; Adams 1999), the probability of i to vote for c is:

$$P_{ic} = \frac{e^{\alpha\, a_c + \beta_r r_c}}{\sum_c e^{\alpha\, a_c + \beta_r r_c}}$$

The expected vote share of a candidate equals the mean of this probability across voters:

$$\frac{1}{I}\sum_i P_{ic} = \frac{1}{I}\sum_i \frac{e^{\alpha\, a_c + \beta_r r_c}}{\sum_c e^{\alpha\, a_c + \beta_r r_c}}$$

The summation and averaging notation can be ignored, since the systematic part of utility

does not vary across voters. In addition, we can take the logarithm to obtain an additive representation:

$$log(P_c) = \alpha\, a_c + \beta_r r_c - log(\sum_c e^{\alpha\, a_c + \beta_r r_c})$$

Under appeal-based decision-making, $\beta_r = 0$, and the expected vote share as represented by the "mean" voter equals:

$$log(P_c) = \alpha\, a_c - log(\sum_c e^{\alpha\, a_c})$$

Therefore, under appeal-based voting, the personal vote result of a candidate will be better if that candidate is more attractive relative to the aggregated appeal of all competitors (cf. Expectation 2) in Table 1). Applied to within-candidate variation of electoral performance in the Bavarian setting, this will be the case in districts in which a candidate is more attractive than in others and in which a candidate of higher appeal is removed in the respective SMD.

Let us consider the satisficing model next. Recall that satisficers evaluate candidates on the basis of appeal, strictly in the order they appear on the ballot, and select the first option they deem "good enough". If citizens choose according to this ideal-typical scenario, the higher-ranked competitors of a candidate affect only whether the latter will be evaluated at all, but not *how*, and any lower-ranked candidates are completely irrelevant for the decision on that candidate.[11]

In formal terms, to be chosen, a candidate's appeal needs to reach the reservation

[11] Thus, this model also rules out any anchoring (Ariely et al. 2003), contrast (Simonson and Tversky 1992), or other effects between the alternatives. We do by no means claim that they are irrelevant in the context of candidate choice but leave an incorporation of such effects for further research.

value ν (Caplin et al. 2011: 2906):[12]

$$\alpha a_1 + \epsilon_{i1} \geq \nu$$

If $\epsilon_i \sim Logistic(0, 1)$, the probability to vote for the candidate ranked first is given by the familiar logit function

$$\begin{aligned} P_{i1} &= P(\epsilon_{i1} \leq \alpha a_1 - \nu) \\ &= \frac{1}{1 + e^{\nu - \alpha a_1}} \end{aligned}$$

The probability to choose any of the lower-ranked candidates depends on the chance that such a candidate is above the reservation value, given that she reaches the consideration stage, i.e. none of the higher-ranked candidates already satisfied the voter. Assuming independence (as posited by the ideal-typical model), this results in the product of the respective probabilities:

$$P_{ic[r]} = \frac{1}{1 + e^{\nu - \alpha\, a_c}} \prod_{1}^{r-1} (1 - P_{ic[r]})$$

As above, there is no indivividual-level variation in the systematic part of utility, and we take the logarithm:

$$log(P_{c[r]}) = -log(1 + e^{\nu - \alpha\, a_c}) + \sum_{1}^{r-1} log(1 - P_{c[r]})$$

In addition to the own appeal, what impacts upon the candidate's election result under this model is the appeal of any candidates ranked higher (cf. Expectation 3a) in Table 1). If the latter are less appealing, it is more likely that the voter will reach the ballot

[12] As pointed out there, an equivalent way of interpreting the model is that candidate utility is fixed, but voters are uncertain about their reservation value.

position of a candidate during her choice process, without already having encountered a satisfactory alternative. This implication is similar to the one from the appeal world, but there is an important difference. The choice of a satisficing voter whether to embrace a candidate in a given position is independent of the appeal of lower-ranked candidates (cf. Expectation 3b) in Table 1).

There is another interesting implication here, which is best illustrated with an example. Suppose we compare the electoral gain for the candidates on baseline list rank two and three, when the candidate with baseline rank one is dropped. In the pure satisficing world, the electoral result of each candidate should change by the same amount on the log scale (or the same factor on the natural scale), since the log of the probability (the probability) that the originally first candidate is "good enough" drops from the sum (the product) in the last part of the equation in both cases.

In our framework, there are two ways to think about a pure rank-taking model. According to the first one, voters consider the full set of candidates and choose the best one, but they only consider list rank and not any factors linked to appeal (i.e. $\alpha = 0$). The candidate's vote share would be given by:

$$log(P_c) = \beta_r r_c - log(\sum_c e^{\beta_r r_c})$$

Another way to derive a rank-taking model would be as a sequential judgement of candidates (as under satisficing), while only considering the list position as criterion for acceptability. This results in the following expected vote share:

$$log(P_{c[r]}) = -log(1 + e^{\nu - \beta_r^* r_c}) + \sum_1^{r-1} log(1 - P_{c[r]})$$

Regardless of which variant one prefers, there are two implications here. The first one

is that candidates will do better if their list rank is of higher value, i.e. when they are listed in a higher position on the list (cf. Expectation 1) in Table 1). This is the standard ballot position hypothesis. In addition, since appeal is irrelevant and only position matters, it will not matter who is removed from the list. In the first formulation, the second part of the equation is the same for any realization of the list across SMDs.[13] Based on the second formulation, again only the value of the list ranks of higher-ranked competitors affects the vote share. We refrain from making further assumptions about β_r or ν, and consequently do not aim to differentiate the two variants empirically.

We can, however, gain additional analytical leverage from a fourth model, in which a voter considers the candidates in order of their appearance on the ballot, while taking into account both appeal and the list position for judging the acceptability. We call this form of choosing satisficing with rank-taking. Under this model:

$$log(P_{c[r]}) = -log(1 + e^{\nu - \alpha\, a_c - \beta_r r_c}) + \sum_{1}^{r-1} log(1 - P_{c[r]})$$

The sequential decision-making process is represented by the functional form, and the decision whether a given candidate is acceptable is affected both by appeal a_c and her rank r_c. As before, this utility r_c, associated with the rank, may have different origins (it could result from following a party signal, from cognitive bias, or from employing a general heuristic).

To contrast the hybrid approach with the simple satisficing model, let us go back to the example. If satisficing also incorporates rank-taking, moving from second to first rank should be more valuable than moving from third to second rank: both candidates gain equally from removing the originally first-ranked candidate, but we expect the signalling

[13] In case the value of a certain list position to a voter does not vary across parties or districts, it would actually be the same for any list of the same length.

value of the first position (β_1) to be larger than that of the second one (β_2). Hence, the candidate moving from second to first position will benefit more than the one moving from third to second rank. Expressed more generally, the effect of advancing by one slot on the list will depend on the baseline rank (cf. Expectation 1a) in Table 1). While this coincides with the prediction of the simple rank-taking model, other implications of the satisficing with rank-taking model differ from the former: own appeal and the appeal of a higher-ranked politician who is dropped matter only in the hybrid world, but not under plain rank-taking. Another dissimilarity between both models concerns the variation in own appeal. Under plain satisficing, the judgment of a candidate's acceptability is based on appeal but is independent of list rank. However, if list rank also affects whether a candidate is deemed good enough, the marginal benefit from being more appealing will be weaker for higher-ranked politicians (cf. Expectation 2a) in Table 1).[14]

Table 1 summarizes our expectations. In the next section, we outline the research design that allows us to consider the evidence for each set of expectations.

[14] This is because such candidates are more readily accepted due to the value of their list position alone, and the assumed acceptance function has a sigmoid rather than linear shape (so additional gains decrease for baseline acceptance probabilities above .5).

Table 1: Key theoretical expectations for a candidate's intra-party vote share across the four ideal worlds

	Appeal	**Rank-taking**	**Satis-ficing**	**Satisf. + rank-taking**
1) Moving up in rank (controlling for appeal)	0	+	+	+
1a) Effect of moving up depends on baseline rank (contr. for appeal of dropped)		yes	no	yes
2) Own appeal	+	0	+	+
2a) Effect of own appeal depends on rank	no		no	yes
3) Appeal of dropped candidate				
3a) if higher-ranked c. removed	+	0	+	+
3b) if lower-ranked c. removed	+	0	0	0

4 Data and research design

4.1 Data

We draw on data for the 2003, 2008 and 2013 Bavarian state elections. We observe data for candidate performance in 90 (2003:92; 2008:91) local districts, nested in 7 regional districts. We draw on candidates from the five parties (CSU, SPD, Greens, FW, FDP) that were represented in at least one of the state parliaments following these three elections. Altogether we observe 2554 candidates (2003: 858; 2008: 863; 2013: 833) and 39,918 candidate–district–year observations. Among these, we focus on the observations near the top of lists (ranks 1-15) in most of our estimations. With 3 (years) * 7 (regions) * 5

(parties), the data covers 105 lists, i.e. it includes 105 candidates per baseline list rank. Our data for 2008 and 2013 stems from the Bavarian Election Administrator.[15] For 2003 we use data kindly provided by Faas and Schoen (2006).

Our main dependent variable is the vote share of candidates c in districts j and within regional lists k, i.e. $Y_{cjk} / \sum_{c=1}^{C} Y_{cjk}$.[16] We use the natural log of the vote share received within the respective regional list.[17] This transformation is warranted by our theoretical approach (for more detail see Appendix Section A.1).[18] Appendix Figure A.1 shows the distribution of our (logged) dependent variable by treated and untreated candidates.

4.2 Research design

To address the endogeneity problem pervasive in the study of ballot position effects under PLPR, we exploit two features of the Bavarian electoral system. Recall that parties field one identical PLPR list with c candidates per regional districts. These regional district lists are never fully realized, though, as within each SMD district j the parties' SMD candidate is removed from the PLPR list, leading to changes in list positions of all lower-placed candidates. Hence, we observe candidates c at differing list ranks r over districts j. Additionally, we observe the same candidates c with different sets of competitors s. Our target is an estimation of the list position effect (holding everything else constant, including the set of competitors), and an estimation of the competition effect (holding everything else constant, including observed candidate list position).

Empirically, we can compare instances in which candidates move up in rank as a

[15] See `http://landtagswahl2008.bayern.de/` for 2008 and `http://landtagswahl2013.bayern.de/` for 2013. Additionally, we use information on individual candidates from *Haus der Bayerischen Geschichte* (`http://www.hdbg.de/parlament/content/index.html`).

[16] List length C varies between 13 and 60 candidates.

[17] We include zeros with half the observed value, i.e. we count zeros as half a vote.

[18] Put differently, our theoretical framework provides a theoretical rationale for the log-transformation, which is often motivated from a data perspective (Blom-Hansen et al. 2016; van Erkel and Thijssen 2016; Marcinkiewicz 2014).

higher-placed competitor is dropped from the full list. Hence, we can causally identify the effect of simultaneously moving up one rank ($r_c = x - 1$) and competing in a setting in which the group of higher-ranked competitors is reduced by one, so the competition set changes to s^*. This, first, gives the average difference in candidate performance due to moving up, $E(d_c)$, as

$$E(d_c) = E(Y_{cj'}|r = x - 1, s^*) - E(Y_{cj}|r = x, s) \ [Eq\ A1].$$

Second, within the subgroup of cases where a rise in rank has taken place (so $r = x-1$ throughout), we can further differentiate sets of competitors s'_1 and s'_2 that result from removing different (types of) candidates. This gives us the average effect of changes in the set of higher-placed competitors, $E(f_c)$, as

$$E(f_c) = E(Y_{cj'}|r = x - 1, s'_1) - E(Y_{cj''}|r = x - 1, s'_2) \ [Eq\ B1].$$

Finally, for any change in ballot composition s^* that originates from a candidate being dropped at rank $r_s < x$, we can estimate the average difference in candidate performance due to changes in the set of lower-placed competitors, $E(g_c)$, as

$$E(g_c) = E(Y_{cj'''}|r = x, s^*) - E(Y_{cj}|r = x, s) \ [Eq\ C1].$$

Two assumptions are necessary to interpret these effects as causal evidence of a list position (for Eq A1) and ballot composition (Eq B1 and Eq C1) effect: First, we contrast a candidate's performance over districts. Candidates move up where higher-placed candidates are SMD-candidates. Hence, we compare a candidate's performance in general with her performance in districts of higher placed candidates. In expectation over all cases, av-

erage candidate performance and upward movement in list position therefore needs to be uncorrelated. Consequently, average voter characteristics must be similar. Overall, this assumes quasi-random determination of the districts in which candidates are removed.

Of particular concern in this context could be consequences of strategic party and candidate behavior. Inter-party seat allocation is based on the sum of all SMD and OLPR votes a party receives within a region. From the perspective of maximizing a party's seat share, parties hence may have incentives to place strong candidates in local districts in which they gain most SMD votes. Intra-party seat allocation is based on the sum of all candidate-level SMD and OLPR votes (SMD votes for most candidates surpass OLPR votes by far). To maximize their total personal vote, candidates benefit from running in party stronghold SMDs and/or SMDs with no strong SMD-level competitors from other parties. In which SMDs parties field candidates and hence where they are dropped from the OLPR list should therefore be affected by considerations regarding the parties' prospects in the SMD; these are not obviously related to district-level OLPR performance of the candidate. One may suspect that a removed candidate would do better on the OLPR list in the SMD in which she competes as SMD candidate (which is not observed). This does not imply, though, that candidates that move up in rank perform better there as well. Hence, from a theoretical perspective, there is no strong argument for confoundedness.[19] In addition, from an empirical perspective, we provide balance tests on observable characteristics[20] that are reassuring: Appendix Table A.6 reports that the

[19] Even if there is correlation in candidate performance across tiers, there is no obvious bias, since the counterfactual focuses on another candidate: if we expect that some SMD-candidate A would do better in the OLPR tier of her SMD, this leads to a comparison of some other candidate B's performance in SMDs in which A competes (and has not got the advantage) with B's performance in the SMD of A (in which A would do better but does not appear on the list).

[20] Binary variables: district with 2013 CSU relatives affair (Rudolph and Däubler 2016; Kauder and Potrafke 2015), urban district (SMD district predominantly is a city or town); continuous variables, binarized at the 50th percentile: CSU first vote, employment share, immigrant share, population density, population influx, number of constructions, number of farmers, taxes per capita.

distribution of pre-election list ranks does not differ by these observables, which supports our argument that districts in which candidates from lower list ranks are removed are comparable to those in which candidates from higher ones are removed.[21]

Second, in the case of Eq. A1 we observe a compound change (Hernán and VanderWeele 2011): We estimate the simultaneous effect of changing list position by one rank and facing a set of competitors s' that differs over districts by one compared to the full list. Keeping the appeal model in mind, the set of competitors might matter for a candidates' electoral outcome. We can still interpret the compound effect as rank effect in two instances: first, if the change in competition is negligible in any case; second, if the average quality of the set of competitors does not change through this procedure and voters take the full set into account, the composition effect is zero by definition. Otherwise, if anything, it is plausible that the effect works in the direction $E(Y|s) \leq E(Y|s')$, as the dropped candidate is higher-placed by definition (hence, given a correlation between rank and quality, in expectation a "better" candidate). Interpreting the ATE of Eq. A1 as pure rank effect would therefore potentially overestimate it. We deal with this in two ways: First, we explicitly assess traits of the removed candidates and whether the general pattern of results holds in cases in which high and low quality candidates are removed.[22] When we interpret the substantive size of the rank effect[23], we draw on rank effect estimates from changes in which non-incumbents and non-prominent party members are dropped (assuming that the average changes in the quality of the set of competitors is close to zero then). Second, we assess whether our results hold in longer and shorter lists. From

[21] Additionally, Appendix Figures A.2 and A.3 provide a graphical overview of the places of residence of candidates on general list position 1 to 3 and SMDs with a candidate on first, second or third OLPR list rank. As can be seen, there is some clustering in larger cities (especially Munich and Nuremberg; note, though, that these as well have 8 and respectively 4 local SMD districts). Overall, however, spatial patterns do not seem to point to systematic clustering of candidates with higher OLPR list rank in specific types of districts.

[22] Which they do, cf. Tables 2, 3 and A.5

[23] Comp. Appendix Section A.2

the perspective of the appeal model, the composition effect would decrease with list size (and the amount of competitors).[24]

Turning to our empirical models, we estimate treatment effects with a candidate fixed effects model (Brüderl and Ludwig 2015; Angrist and Pischke 2008):

$$Y_{cj} = \psi_c + \mathbf{r_{cj}}\delta + \mathbf{X_{cj}}\theta + \epsilon_{cj} \; [EqA2]$$

This approach is motivated by close links to the theoretical models (as outlined in Appendix A.1) and has the great advantage of implicitly controlling for all observable and unobservable candidate characteristics that are constant over districts, as represented by ψ_c.

Additionally, we explicitly include covariates $\mathbf{X_{cj}}$ for some district-varying observable candidate attributes, particularly whether the candidate resides in a given district, or whether she resides in a neighboring district (compare Górecki and Marsh 2012; Jankowski 2016). Given our assumption of quasi-random variation in list rank, we would not expect these variables to affect δ, however (which they indeed do not, as shown below). The average treatment effect is estimated by including the observed rank variable r_{cj}, which varies for candidates over districts. As this rank variable likely shows non-linear effects, we estimate separate treatment effects for moving up one position from pre-election ranks 2, 3, 4-6, 7-10, and 11-15 in our preferred specifications. Relating Eq. A2 to Table 1, we test whether voters choose according to an appeal world (hence $\delta = 0$) or rather are rank-takers and/or satisficers ($\delta > 0$). Additionally, we interpret the coefficients for district-varying candidate covariates as informative about the appeal model.

Second, we explore effects of differing choice set quality for higher-ranked candidates for the vote share of candidate c (Eq. B1). For this purpose, we interact candidate rank

[24] When estimating our models for long lists, our results show similar patterns (see Table A.7).

with a binary indicator for the average choice set quality. We differentiate two types of removed candidates: those with high appeal ($a_d = 1$) and those with low appeal ($a_d = 0$), as this affects the average quality of the choice set s.[25] As our appeal indicators are fixed characteristics of dropped candidates c_d, the analysis conforms to a subgroup analysis of rank effects. We can compare two types of rank effects now, whose difference $\delta_1 - \delta_2$ can be interpreted as effect of variation in choice set above:

$$Y_{cj} = \psi_c + \mathbf{r_{cj}^{a_d=1}}\delta_1 + \mathbf{r_{cj}^{a_d=0}}\delta_2 + \mathbf{X_{cj}}\theta + \epsilon_{cj} \; [EqB2]$$

Relating Eq. B2 to Table 1, this tests whether voters are rank takers or take appeal into account.

Finally, we estimate treatment effects for Eq C1 (effect of difference in the set of lower-placed candidates) with a similar candidate fixed effects design, including binary indicators for the appeal a of removed candidates further down the list:

$$Y_{cj} = \psi_c + \gamma a_j + \mathbf{X_{cj}}\theta + \epsilon_{cj} \; [EqC2]$$

Relating Eq. C2 to Table 1, this again tests whether voters choose according to an appeal world or rather are rank takers and/or satisficers.

In all our models, standard errors are clustered at the level of the candidate to caution against correlation of error terms.[26]

[25] As such covariates, we use direct indicators of candidate appeal (incumbent MP, prominent politician) or indirect ones, such as the rank of the dropped candidate.

[26] Clustering at a higher level (we considered clustering at the district–year–party level) did not change our substantive conclusions; the changes to the standard errors were marginal. As clustering at a higher level is not the per se more conservative approach (standard errors are smaller for some coefficients), we cluster at the candidate level given the bias–variance trade-off inherent in choosing larger clusters (Cameron and Miller 2015).

5 Results

We begin by examining to which extent the evidence from the Bavarian case supports any kind of model implying a ballot position effect as compared to voting driven exclusively by candidate appeal. In line with [Eq B1], we choose the candidate fixed effects specification for estimating our baseline treatment effects of observed list rank on logged vote shares (Table 2). In Model 2-1, we focus on candidates on the first fifteen baseline ranks, and specify the rank effect via dummies. For simplicity, treatment variables referring to shifts further down the list are pooled.[27]

Three key messages emerge from Table 2. First, there is clear evidence that ballot position matters for individual electoral performance, as the treatment effects are statistically and substantively significant. The estimated effects are considerable. Based on Model 2-1, moving from rank two to rank one implies a more than four-fold increase ($e^{1.46} \approx 4.3$) in intra-party vote share. Candidates progressing from third to second rank receive a vote boost of approximately 40%. The other coefficients imply that being elevated matters starting from all baseline positions we observe. For positions four to six, seven to ten, and 11 to 15, moving up one rank increases the vote share by 25%, 15% and 14%, respectively (drawing on Model 2-1). We can directly interpret these effects as the compound treatment effect of moving up one rank (together with the average change in the quality of the list by dropping one higher-placed candidate). For now, our interpretation of these effects is that rank makes a difference when holding a candidate's quality constant. Note that the estimated coefficients of the rank effects are significantly different from each other.[28] This implies that we observe considerable heterogeneity in treatment

[27] Appendix Table A.2 shows that our results hold for the full sample, and also with one single treatment dummy providing an average effect over all candidates (see discussion in Appendix Section A.4).

[28] With the exception of the coefficient for moving up from 7th to 10th as compared to 11th to 15th, although this difference is significant with controls in Model 2-2.

Table 2: Baseline treatment effects of moving up one list position

	(1)	(2)	(3)
DV: OLPR candidate vote (%, logged) in district	Candidate fixed effects		List fixed effects
$2 \rightarrow 1$	1.46**	1.49**	1.42**
	(0.082)	(0.074)	(0.084)
$3 \rightarrow 2$	0.35**	0.37**	0.33**
	(0.066)	(0.057)	(0.075)
$4 \rightarrow 3$ / $5 \rightarrow 4$ / $6 \rightarrow 5$	0.22**	0.20**	0.23**
	(0.038)	(0.032)	(0.045)
$7 \rightarrow 6$ / (...) / $10 \rightarrow 9$	0.14**	0.12**	0.15**
	(0.034)	(0.026)	(0.046)
$11 \rightarrow 10$ / (...) / $15 \rightarrow 14$	0.13**	0.072**	0.23**
	(0.033)	(0.026)	(0.049)
Neighboring		0.82**	
		(0.020)	
Residence		3.04**	
		(0.059)	
Constant	3.46**	3.26**	3.62**
	(7.4e-12)	(0.0049)	(0.24)
Pre-election rank	Yes	Yes	Yes
List fixed effects	No	No	Yes
Candidate fixed effects	Yes	Yes	No
N	19425	19425	19425
Adjusted R^2	0.63	0.80	0.48

Candidate fixed effects estimation with clustered standard errors. Models considers only candidates with baseline list rank less than or equal to 15.
* (+,**) indicates $p < 0.05$ (0.1, 0.01)

effects even on the log scale between baseline list positions 2 and 15.[29] We will further discuss this result below.

Second, taking into account appeal does not change the estimated treatment effects that much. As can be seen from Model 2-2, the ballot position effects are robust to the inclusion of district-varying candidate appeal (localness), in line with our assumption of quasi-random treatment assignment. Concerning these controls, clearly, candidates get more votes where (20.9 times) or near where (2.3 times) they live. These numbers appear large, but the mean vote share in the untreated condition on baseline ranks 1-15 amounts to merely $\approx$ .69 on the log-scale, which corresponds to $\approx$ 1.99% of the intra-party vote (compare also Appendix A.1).[30] We also point out that, while the rank-related coefficients represent well-identified causal effects on the basis of the assumptions outlined in Section 4.2, the geographical variables are mere correlations.

Third, ballot position effects alone do not provide the full story. Model 2-3 contrasts the candidate fixed effect models discussed so far with a model including list–party–year fixed effects.[31] As we no longer control for candidate appeal by means of candidate fixed effects, the explained variance (in form of adjusted r^2) decreases considerably (from .63 to .48). Perhaps more surprisingly, the effects from moving up one rank actually do not change much whether or not controlling for appeal that goes beyond baseline rank. The changes in coefficient size (e.g. 1.42 in Model 2-3 compared to 1.46 in Model 2-1 for 2→1) are small. A possible reason is that the candidate fixed effects in Model 2-1 add "only" a control for differences in individual appeal among the candidates in the sample (those

[29] Models A.2-1 and A.3-2 in the Appendix show that the argument of a diminishing return to list rank is mirrored in the (correlational) pattern that higher pre-election list rank is associated with increasingly higher vote shares.

[30] Also note that the candidate fixed effects capture the denominator of the expected vote share, so the model does not predict values out of range, see Appendix A.1. In addition, Table A.4 provides empirical evidence that the marginal benefit of appeal is higher in lower list positions, which anticipates the evidence for the satisficing-with-rank-taking model further discussed below.

[31] See Appendix Table A.3 for results displaying pre-election rank coefficients and including controls.

on baseline ranks one to fifteen). The list fixed effects in Model 2-3 already capture differences between the average in-sample and the average out-of-sample candidate at the list level.

The results discussed so far provide evidence for positional effects, assuming that the change in list composition induced by changing competitor sets is negligible. We have also seen that observed and unobserved features of a candidate affect her performance in intra-party competition. Next, we examine the heterogeneity in the treatment effects by the appeal of the removed candidate [Eq C1]. To do so, we run two analyses. The first one of these takes into account the baseline list position of the dropped candidate, which should constitute a proxy for the overall appeal of that candidate. Put differently, we use the correlation that typically annoys students of ballot position effects to our advantage.

Candidates in baseline list position three or lower on the list experience improvements in their ranking under two conditions: either when a baseline list leader is dropped or or when any other higher-ranked candidate is dropped. Hence, we can compare the performance of the very same candidates on a list in which they are placed in the same list position – but either with an on average higher-appeal or a (relatively) lower-appeal competitor listed higher. Figure 1 displays results of models that investigate whether the treatment effect varies between these two scenarios. In a pure rank-taking world, the effect of moving up should be the same, since it is a move of the same magnitude (one rank) in a list of the same length. If there is a difference between the scenarios, this speaks in favor of voters taking appeal into account in *some* way. The findings shown in Figure 1 clearly point in the latter direction. Regardless of whether we include control variables (comp. Table A.5 for full results), we observe substantial heterogeneity in treatment effects depending on who is removed from the list. Wherever the baseline list leader is removed (solid blue points), subsequent vote gains for a candidate moving up are

more substantial compared to the vote gains when a subsequent, but still higher-ranked candidate is taken off the list (hollow green circles). In the 3→2 case, the two treatment coefficients are .52 and .18, which imply vote share boni of 68% and 20%. We see such differences, albeit smaller in size, also for shifts further down the list.[32]

This implies that quality plays a role in the size of the treatment effect. In the rank-taking world, the coefficients for improving one's own rank would not be affected by who is removed. Table 3 provides further support for this line of reasoning. As many other studies (e.g. Faas and Schoen 2006; van Erkel and Thijssen 2016), we use current office of candidates (being an incumbent MP, being a prominent politician) as reflecting observable differences in appeal and test whether these variables moderate the treatment effect.[33] For our purposes it is irrelevant why exactly office holders are more attractive (e.g. is it actual quality or mere visibility?), as long as they are more appealing on average. We interact the treatment effects of moving up one rank with the office of the candidate who is removed (prominent politician in a narrow sense in Model 3-1 and 3-2 and prominent politician or incumbent MP in Model 3-3 and 3-4). Unlike in the previous analysis, this type of interaction can also be examined for the $2 \rightarrow 1$ cases.

The results reinforce our conclusions from the previous table. The gain from being elevated to a higher list rank tends to be larger if the removed candidate is an office

[32] Political attention during campaigns is usually focused on one candidate ("Spitzenkandidat") in each party, who leads the campaign across the whole state. Since these candidates, who lead (no more than) one list, typically also run as SMD candidates, one may wonder to which extent their removal drives the observed differences in treatment effects shown in Figure 1. Models 2 and 3 of Appendix Table A.8 contrast lists with and without *Spitzenkandidat*. While the difference in effects depending on which list position is dropped is much larger where a *Spitzenkandidat* takes baseline rank 1, it is still considerable in the remaining cases (In the 3→2 case the two treatment coefficients imply vote gains of 63% versus 21% even without a *Spitzenkandidat* being removed). Note, however, that *Spitzenkandidaten* tend to cluster in one (and the largest) OLPR district, Upper Bavaria (10 out of 14 observed cases).

[33] The indicator variable for being a prominent politician refers to the time just before the election and equals one for ministers (excluding junior ministers), party leaders, parliamentary party group leaders, party general secretaries, and state-wide campaign frontrunners (if these positions exist in the party).

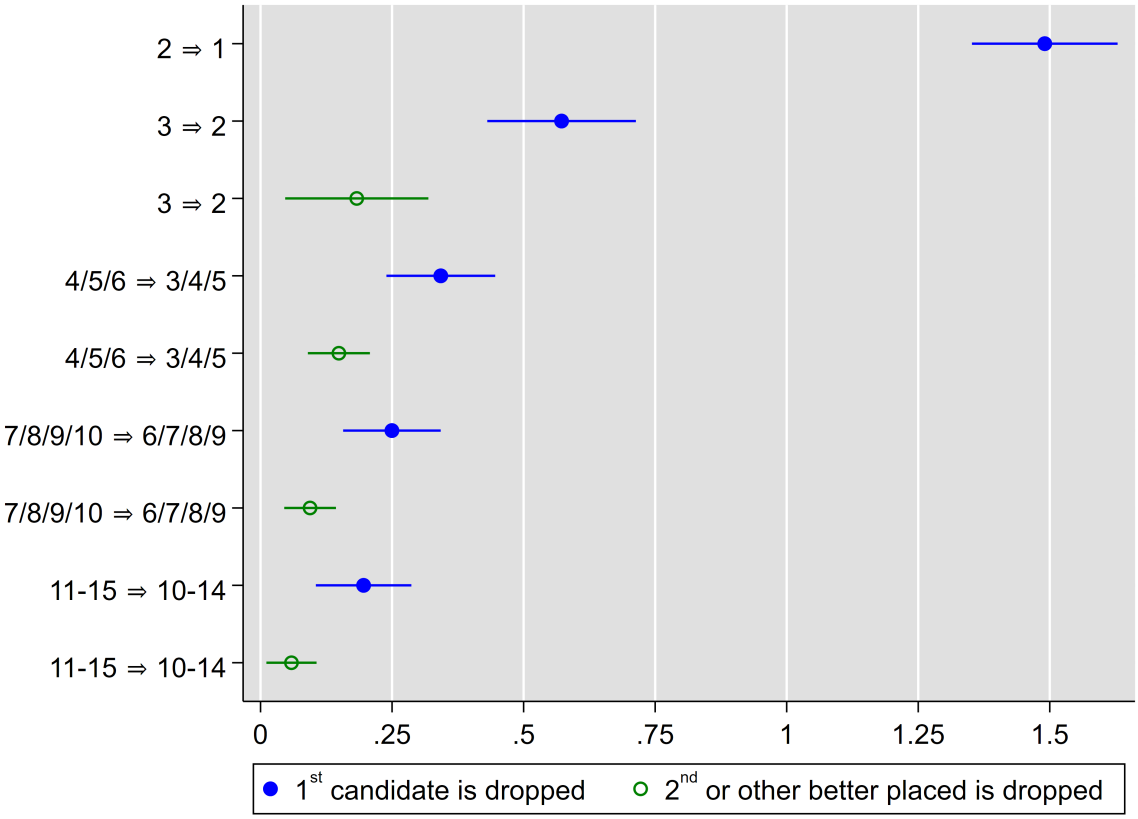

Figure 1: Effects of moving up one rank by rank and by omitted first versus any subsequent baseline list position.
Figure reports coefficients for rank effects from candidate fixed effects estimation with clustered standard errors. Analysis considers only candidates with baseline list rank less than or equal to 15. Control variables included: residence and neighboring district. Bars depict 95% confidence intervals. See Appendix Table A.5 for full results.

holder. The differences across the conditions are stronger when appeal is captured by the prominent politician dummy variable (Models 3-1 and 3-2) rather than the broader measure also including incumbent MPs (Models 3-3 and 3-4). While not all interaction terms are statistically significant across models, all but two are positive. When comparing the specifications including controls for localness (Models 3-2 and 3-4) to the other models, it can be seen that the interaction effects for the $2 \rightarrow 1$ decrease, whereas the other interaction effects become larger.

To learn more about how candidate appeal enters voters' decision-making, we next turn to a model that allows for separating general appeal-based decision-making from satisficing. Recall that, under a satisficing model, removing a lower-ranked candidate (and thus the variation in the attractiveness of such a candidate) should not make any difference to the electoral performance of a given candidate. In the appeal world, on the other hand, it should matter: dropping other highly attractive candidates is supposed to increase a candidate's vote share, regardless of from where in the list the candiate is removed.

We therefore seek to find out whether a candidate with baseline rank r improves her vote share if a high-quality candidate with lower baseline rank $s > r$ is taken off the list. Table 4 reports results of such models, which are run separately for each of the baseline ranks one to three. The treatment variable recovers the baseline treatment effect for moving up one rank if a higher-placed candidate is dropped. More importantly, the other two indicators capture whether an incumbent MP or whether a prominent politician is removed from the list below the candidate of interest. There are two types of specifications: one considers any such removals further down the list (Models 4-1 to 4-3), another one only those in the subsequent list position (Model 4-4 to 4-6). This is warranted since, once again, the list rank of the removed candidate likely correlates with

Table 3: Heterogeneity in treatment effects by features of dropped candidates

	(1)	(2)	(3)	(4)
DV: OLPR candidate vote (%, logged) in district	High appeal = minister or visible party office		High appeal = incumbent MP or minister or visible party office	
2 → 1=1	1.32**	1.39**	1.29**	1.42**
	(0.072)	(0.072)	(0.10)	(0.093)
2 → 1=1 × high appeal cand. dropped=1	0.60**	0.47*	0.25+	0.13
	(0.22)	(0.19)	(0.15)	(0.13)
3 → 2=1	0.31**	0.32**	0.24**	0.24**
	(0.071)	(0.062)	(0.085)	(0.084)
3 → 2=1 × high appeal cand. dropped=1	0.21	0.34*	0.17	0.27**
	(0.16)	(0.15)	(0.12)	(0.10)
4 → 3 / 5 → 4 / 6 → 5 =1	0.18**	0.17**	0.19**	0.17**
	(0.039)	(0.032)	(0.043)	(0.036)
4 → 3 / 5 → 4 / 6 → 5 =1 × high appeal cand. dropped=1	0.25*	0.26**	0.035	0.091+
	(0.12)	(0.10)	(0.072)	(0.054)
7 → 6 / (...) / 10 → 9 =1	0.11**	0.085**	0.13**	0.078**
	(0.034)	(0.026)	(0.037)	(0.028)
7 → 6 / (...) / 10 → 9 =1 × high appeal cand. dropped=1	0.19+	0.29**	0.0044	0.12**
	(0.11)	(0.091)	(0.054)	(0.040)
11 → 10 / (...) / 15 → 14 =1	0.12**	0.049+	0.13**	0.042
	(0.033)	(0.026)	(0.036)	(0.028)
11 → 10 / (...) / 15 → 14 =1 × high appeal cand. dropped=1	0.071	0.22*	-0.019	0.091*
	(0.10)	(0.088)	(0.052)	(0.036)
High appeal cand. dropped=1	-0.0088	-0.068	0.028	-0.041+
	(0.073)	(0.064)	(0.030)	(0.024)
Residence		3.04**		3.05**
		(0.057)		(0.057)
Neighboring district		0.82**		0.82**
		(0.019)		(0.019)
Constant	0.41**	0.074**	0.40**	0.079**
	(0.0088)	(0.0094)	(0.010)	(0.011)
N	19425	19425	19425	19425
Clusters	1572	1572	1572	1572
Within R^2	0.017	0.48	0.016	0.48

Candidate fixed effects estimation with clustered standard errors. Models consider only candidates with baseline list rank less than or equal to 15.
* (+,**) indicates $p < 0.05$ (0.1, 0.01)

unobserved components of her appeal.

Table 4: Changes in vote shares if a prominent candidate/incumbent further down the list is dropped

	(1)	(2)	(3)	(4)	(5)	(6)
DV: OLPR candidate vote	Rank 1	Rank 3	Rank 3	Rank 1	Rank 2	Rank 3
(%, logged) in district	Unrestricted removal			Removal in subsequent two list positions		
Moving up one rank		1.47**	0.35**		1.47**	0.34**
		(0.079)	(0.066)		(0.079)	(0.064)
Prom. c. removed below	0.078	0.070	-0.53*	0.12+	0.081	-0.48
	(0.048)	(0.21)	(0.22)	(0.074)	(0.23)	(0.41)
Incumbent removed below	0.0039	0.00089	0.067	-0.023	0.068	-0.16
	(0.023)	(0.076)	(0.11)	(0.058)	(0.15)	(0.21)
Constant	3.50**	1.71**	0.96**	3.50**	1.70**	0.97**
	(0.0055)	(0.017)	(0.024)	(0.0016)	(0.0065)	(0.011)
N	1273	1270	1274	1273	1270	1274
Clusters	105	105	105	105	105	105
Within R^2	0.0015	0.22	0.026	0.0012	0.22	0.025

Candidate fixed effects estimation with clustered standard errors. Models estimated for candidates with baseline list rank 1 (Models 1, 4), 2 (Models 2, 5), and 3 (Models 3, 6).
* (+,**) indicates $p < 0.05$ (0.1, 0.01)

The results are mixed: the coefficients for the incumbent variable are substantially small and not significant at conventional levels; this would be in line with a satisficing interpretation. The coefficients for the prominent politician measure point in a positive direction for the first and the second baseline list position, but in negative direction for baseline list position three. We interpret this as inconclusive evidence: There might be circumstances in which list composition matters independent of where the candidate is dropped; but there is at least no strong support for a pure appeal-model.[34]

These findings therefore favor the satisficing model over a pure appeal-based explanation. Recall, however, the findings that the treatment effect is smaller for baseline ranks lower on the list (Table 2), a pattern that prevails when holding the identity of

[34] Note that only 20 candidates are removed below baseline list position two and only 12 below rank three. The latter are mainly CSU candidates in 2003. This could lead to statistical anomalies, which is also why we refrain from estimating effects for baseline list positions four or further down the list.

the dropped candidate constant (i.e. when we compare across the instances of removing the first candidate, represented by the solid blue points in Figure 1). This pattern is not consistent with a simple satisficing explanation, in which the ranking merely affects the order of consideration (compare Expectation 1a). In this case, dropping one and the same candidate should affect the performance of lower-ranked candidates equally (on the log scale). We therefore prefer the satisficing-with-rank-taking model over simple satisficing. This view is further corroborated by the findings detailed in Appendix Table A.4, which shows that the association between higher appeal (in form of local residence) and electoral performance is smaller in the highest list positions. As per Expectation 2a), we would expect this pattern also only under the hybrid model.

Taken together, the evidence therefore corresponds best to the satisficing with rank-taking model, in which list rank affects choice through two mechanisms at once. This can also be seen in Table 5, which recapitulates the theoretical expectations and highlights (in grey) which predictions are supported by the empirical evidence. In our setting, we unfortunately cannot infer to which extent rank-taking results from voters following party signals as opposed to cue-taking of the "up-is-good" (Meier and Robinson 2004; Kim et al. 2015) variety or cognitive bias (Krosnick and Alwin 1987). We would expect that the substantive pre-election ordering by the party reflects a clear signal in the given context, but voter-level data would be required to pin down this mechanism.

The results presented in our analysis add to existing evidence that ballot positions matter also under PLPR systems. A further advantage of our quasi-experimental setup is the possibility to estimate the share of voters whose choice is causally influenced by ballot position (defined as those choosing a candidate they would not have supported if that candidate had been ranked one position further down). As explained in more detail in Appendix A.2, the overall results suggest that approximately 3/4 of votes for

the top-ranked candidate, 1/5 of those for the second-ranked candidate, and 15/100 of those for candidates in positions three to five are caused by marginal increases in ballot position. In total, around 3/5 of votes for candidates on ranks 1-14 are altered by list position (60.2%, 95% CI: [54.6, 65.4]). On a typical (median) list, candidates on these ranks receive 86.3% of the OLPR vote, so approximately one of two votes is changed due to placement (51.9%, [47.1%, 56.5%]).

This is certainly a considerable share. When trying to generalize from our findings, we have to take into account that both factors likely increasing and features probably weakening placement effects are present in the Bavarian case. The estimated share of switched votes is probably driven up by the fact that our ATE does not take into account baseline list leaders (see Appendix A.2 for further discussion). Also, the lack of an explicit party list vote option should lead to stronger ballot position effects, as many poorly-informed voters feel compelled to choose a candidate (Marcinkiewicz and Stegmaier 2015; Blom-Hansen et al. 2016). On the other hand, with additional information provided on the ballot (gender, title, occupation/incumbency, place of residence), position is only one of several cues available, probably dampening its effects (Brockington 2003).

Finally, since we analyzed lists across three years, seven electoral districts, and five parties, we can actually use the rather large within-sample variation to empirically explore some boundary conditions of our case. Appendix Tables A.7 and A.8 replicate Tables 2 and A.5, split across several theoretically interesting subgroups. These subgroups are defined by factors that potentially influence the strength of placement effects. We find that our main results hold (a) in settings with centralized or decentralized campaigning (measured by presence of state-wide campaign frontrunners); (b) in urban and rural contexts; (c) both for the CSU (most of whose MPs enter parliament via the SMD tier and the OLPR vote does hardly affect intra-party seat allocation) and for the other parties

(in which OLPR votes are more influential in deciding who will obtain a seat); (d) in regions with above- and below-median average levels of political interest as well as above- and below-median average ratings of the party chosen (based on survey data from Zittlau et al. (2017)); and (e) across short (up to 20 candidates), medium (21-40) and long (41-60) lists. Although the analysis draws on only one case, we therefore conclude that our results likely apply across a broad set of contexts.

Table 5: Key theoretical expectations on what affects a candidate's within-party-list vote share under OLPR and supporting evidence (grey) across the four theoretical models

	Appeal	**Rank-taking**	**Satis-ficing**	**Satisf. + rank-tak.**
1) Moving up in rank (controlling for appeal)	0	+	+	+
	Evidence: Tables 2, A.2 Rank affects vote shares, independent of whether candidate appeal is controlled			
1a) Effect of moving up depends on baseline rank (contr. for appeal of dropped c.		yes	no	yes
	Evidence: Tables 2, A.2, Figure 1 Logged ballot placement effect, decreasing with baseline rank			
2) Own appeal	+	0	+	+
	Evidence: Table 2 District-varying candidate appeal ('localness') is strongly associated with performance			
2a) Effect of own appeal depends on list rank	no		no	yes
	Evidence: Table A.4 District-varying candidate appeal ('localness') shows weaker associations near the top of the list			
3) Appeal of dropped candidate				
3a) if higher-ranked c. dropped	+	0	+	+
	Evidence: Tables A.5, 3 Rank treatment effect increasing with quality of dropped candidate			
3b) if lower-ranked c. dropped	+	0	0	0
	Evidence: Table 4 Variation in list composition further down the list without effect on a candidate's standing			

6 Conclusion

Our paper focused on intra-party candidate choice in an OLPR setting. In theoretical terms, we differentiated four formalized ideal types of voter decision-making, appeal-based utility maximization, rank-taking, satisficing and satisficing with rank-taking. The real-world evidence drawing on quasi-experimental variation from elections to the state parliament of Bavaria yielded two key findings. First, we find causal effects of moving up one slot across a very large range of baseline list positions, and these are stronger near the top of the list. Based on the inferred effects, we estimate that, in a typical list, approximately every second vote is altered due to ballot position. Second, the overall evidence is most compatible with the the satisficing with rank-taking-model. This suggests that candidate choice under PLPR systems is best understood from a bounded-rationality perspective, in which ballot order affects both the sequence of consideration and the assessment of individual candidates. These results have implications for understanding both voters' decision-making more broadly, and candidate choice under PLPR systems in particular.

In general terms, our study resonates with the view that institutions play a key role in explaining political behaviour, since they crucially shape the choice set, i.e. which alternatives are on offer and how they are organized (Sniderman 2000). Institutions may be understood in a broad sense here, including rules of the game such as electoral systems, but also institutional actors such as parties nominating candidates. The results presented in this paper demonstrate that the choice set matters in terms of both composition and order: applying an innovative research design to real-world election results revealed that these two dimensions of the choice set affect the *judgement* of (candidate) alternatives as well as the final *choice* (cp. Einhorn and Hogarth 1981).

The different decision-making processes we outlined in our theoretical framework are

not specific to the PLPR context. Under nominal electoral systems, there is of course no party signal in the form of list order, and without intra-party competition candidate and party choice become one. Nevertheless, appeal-based utility maximization, (non-party-based) rank-taking, satisficing, and the hybrid model may also apply in such an electoral context. In line with the notion that the choice of the decision-making process may be endogenous to context (Redlawsk 2004), future research could assess the scope conditions of our argument, especially whether only pre-ordered lists let voters apply satisficing with rank-taking. As a plus, elections employing random rotation give rise to convenient natural experiments, which allow for studies with strong research designs that can empirically distinguish the different decision-making types. Also, the set of four models that we considered is by no means exhaustive (Rubinstein and Salant 2006). For instance, features of the preceding or following competitor may affect the choice of a candidate.[35]

Regarding preference voting under PLPR systems, our results echo the view that making a choice from a typically large set of candidates, about which many voters may know little to nothing, is a demanding task. Unsurprisingly, in order to reduce information and decision-making costs, voters resort to cues (e.g. Shugart et al. 2005; Lutz 2010; Marcinkiewicz 2014; Blom-Hansen et al. 2016; Jankowski 2016). Ballot position is readily available and, if lists are pre-ranked by parties, carries information regarding the preferences of the candidate selectorate within the parties. Our results challenge the pessimistic interpretation of ballot position effects under PLPR, suggesting that voters follow their party's choice "blindly" and that intra-party choice is redundant. Van Erkel and Thijssen (2016: 253), for example, conclude that "there are often no rational motivations behind

[35] We also assumed there is only one type of "average" voter, whom the empirical results characterize as deciding on the basis of a mixture of satisficing and rank-taking. On the other hand, we may think of the electorate as being composed of different types of voters, perhaps with variation in composition across districts and parties.

preference voting". We instead argue that the key question to be asked is the central one from the literature on heuristics (e.g. Gigerenzer and Gaissmaier 2011): to which extent and under which conditions do candidate choices on the basis of heuristics approximate the decisions fully informed utility maximizers would reach?

It is here where (pure) satisficing and (pure) rank-taking have different implications. With pure rank-taking, parties' and voters' preferences regarding candidates would have to be alike for cue-taking to result in acceptable choices. There is some empirical evidence that this is the case, since parties in flexible list systems promote candidates who proved popular with voters in the past (more so than expected on the basis of list position) to more promising list ranks in subsequent elections (André et al. 2015; Crisp et al. 2013; Folke et al. 2015).[36] Preferences of voters and parties need not coincide in all contexts, though. If citizens choose candidates on the basis of a satisficing logic, preconditions for reaching accurate decisions are less demanding. Voters are more self-reliant, and their criteria for judging candidates may deviate from those of parties. Even if the reservation value of most voters was small in most cases, shocks that affect voter preferences (and hence their reservation value) can more readily translate into representation that is in line with voter preferences under a PLPR system in which voters follow a satisficing with rank-taking logic (compared to closed lists). While the analyses in this paper cannot say to which extent voters rate candidates on different grounds than parties, findings from other studies show that voters do use PLPR systems to punish candidates they associate with misconduct (Rudolph and Däubler 2016) or a low-regarded ruling elite (Stegmaier et al. 2014). Perhaps preference voting in list systems rather has the character of a veto or correction device, with strong manifestations only visible in rather exceptional

[36] Also note that parties may do this only because lists are not closed, and having the original order overturned may imply reputation costs. In this case, a correlation between list rank and candidate vote share should not lead to the wrong conclusion that PLPR is redundant.

circumstances, when parties nominate candidates who are clearly out of step with voter preferences or when they fail to sort out dishonest politicians.[37] To conclude, we suggest that research on candidate voting in PLPR systems would benefit from a stronger focus on the question to which extent list rank (and other cues) serve as effective heuristics to choose the "right" candidates.

[37] Parties may also strategically anticipate that ballot placement affects voters' choices. The Bavarian Green Party, for example, places frontrunner candidates from time to time not at a top rank. This gives them de-facto-control over the entry of at least one other candidate via OLPR list votes. Similarly, the CSU regularly places SMD candidates at the bottom of lists in some regional districts, for other candidates to gain visibility.

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A Online appendix for 'Cue-taking, satisficing, or both? Quasi-experimental evidence for ballot position effects'

A.1 Link between theoretical and empirical models

Rank-taking

We start with this model, in the standard utility maximization variant, since it is the simplest case. The log of the expected vote share as represented by the "mean" voter was derived as:

$$log(P_c) = \beta_r r_c - log(\sum_c e^{\beta_r r_c})$$

The expression $log(\sum_c e^{\beta_r r_c})$ - which is the aggregate utility across candidates on the observed list and only depends on their list ranks - can be represented by a list fixed effect. The logged vote share is a linear function of list rank dummy variables.

The satisficing based on list rank variant gave:

$$log(P_{c[r]}) = -log(1 + e^{\nu - \beta_r^* r_c}) + \sum_1^{r-1} log(1 - P_{c[r]})$$

The first part of the equation reflects the probability that the candidate is above the reservation value, as a function of her own list rank, whereas the second part captures the probability that none of the higher ranked candidates was considered satisfactory. Since the latter part also only depends on list rank, it is constant across (single member) districts and can be represented by a list fixed effect. List rank dummy variables will pick up the first part of the equation in its entirety.

Note that if this model applies, candidate fixed effects would be the same for all candidates within a list and would not provide additional information regarding electoral performance, when compared to list fixed effects.

Appeal-based world

For the expected vote share we obtained:

$$log(P_c) = \alpha\, a_c - log(\sum_c e^{\alpha\, a_c})$$

The expression $log(\sum_c e^{\alpha\, a_c})$ reflects the aggregate utility across candidates on the observed list, which depends on the sum of the individual candidate appeal values. A different way to think about this expression is as the respective sum for *all* candidates on the regional list (which is never actually observed) less the appeal contribution of the dropped candidate: $log(\sum_{c+1} e^{\alpha\, a_c} - e^{\alpha\, a_{c_{drop}}})$

Although this term is not a simple additive function, we can approximate it using a list fixed effect and covariates capturing the appeal of the dropped candidate. To model the candidate's own appeal we use district-varying candidate features and candidate fixed effects. The latter will subsume the list fixed effect.

Satisficing

The expected vote share is given by:

$$log(P_{c[r]}) = -log(1 + e^{\nu - \alpha\, a_c}) + \sum_{1}^{r-1} log(1 - P_{c[r]})$$

The first part of the equation reflects the probability that the candidate is above the reservation value, as a function of her own appeal, whereas the second part captures the probability that none of the higher ranked candidates was considered good enough by the voter, as a function of their appeal.

A candidate fixed effects specification also makes sense as an approximation in this case: the candidate fixed effect captures district-constant aspects of the candidate's own appeal plus the appeal of the maximum $r_c - 1$ candidates ranked above the candidate of interest. Covariates reflecting the appeal of a dropped higher-ranked candidate capture the respective reduction in the second part of the equation.

Satisficing with rank-taking

The equation is the same as in the previous case, only the value of the observed list rank appears in the first part of the equation for the expected vote share:

$$log(P_{c[r]}) = -log(1 + e^{\nu - \alpha\, a_c - \beta_r r_c}) + \sum_{1}^{r-1} log(1 - P_{c[r]})$$

Therefore also the estimation can be approached in a similar fashion, but we should allow effects of moving up to vary with baseline list rank.

A.2 Estimating the share of switched votes

The quasi-experimental setup allows to infer the causal effect of ballot position. Based on these results, we can also estimate the share of voters whose vote is altered by ballot position. We define this quantity as the share of voters who would not have voted for a set of candidates if these candidates had been placed one slot further down the list. In a nutshell, we calculate expected vote shares of candidates after moving up and compare these to their observed vote shares in the baseline position. If candidates receive x% votes on their baseline rank, but (x+z)% votes when moving up, then a share of $z/(x+z)$ voters' decisions was altered by placement.

Note that changes in rank may go along with changes in the average quality of competitors a candidate faces. Our results are potentially an overestimation of a pure rank effect if upward changes in rank are accompanied by a systematic decrease in average competitor quality. We do, however, not expect such changes if the variation in the choice set results from the removal of candidates who are neither prominent party politicians nor incumbent MPs.

Hence, we choose the coefficients for the situation when non-high-appeal-candidates (on the basis of the measures we use) are dropped from the list, i.e. the "main" effects from Model 4 in Table 3. This amounts to choosing the treatment effect estimate that conforms to the most conservative estimate available to us.

We proceed in five steps:

1. For each candidate in baseline position 2 to 15, take the mean of the observed logged vote share across observations at the baseline rank (i.e. when the candidate did not move up), $\bar{v}_c$, and exponentiate that for an individual baseline vote share ($e^{\bar{v}_c}$). (Column (2) in Table A.1)

2. Calculate the expected vote share after moving up for each candidate, by exponentiating the sum of $\bar{v}_c$ and the respective coefficient (point estimates, from Model 3-4, are as follows: $2 \rightarrow 1$: 1.42, $3 \rightarrow 2$: 0.24, $4/5/6 \rightarrow 3/4/5$: 0.17, $7-10 \rightarrow 6-9$: 0.078, $11-15 \rightarrow 10-14$: 0.042).

3. Calculate the absolute first difference in vote share for each candidate, by taking the result from the previous step and subtracting the baseline vote share ($e^{\bar{v}_c}$).

4. Calculate the mean of the expected vote share (Column (3) in Table A.1) and the

mean of the first differences across candidates (Column (4) in Table A.1) for each (grouped) baseline rank (2, 3, 4-6, 7-10, 11-15).

5. Ratios of the mean first difference and the mean expected vote share after moving up quantify the share of voters who would not have supported the moved up candidates. This quantifies the share of influenced voters, by (grouped) baseline rank (Column (5) in Table A.1). Summing up the mean first differences and likewise the mean expected vote shares across (grouped) baseline ranks and calculating their ratio reflects the share of voters voting for candidates on ranks 1-14 who would not support these candidates if they had stood in positions 2-15 (Last row of Table A.1).

To take into account the uncertainty of the treatment effect estimates, we repeat steps two to five 5000 times, drawing from a multivariate normal distribution with the point estimates and the associated variance-covariance-matrix as parameters.

Table A.1: Quantifying the share of voters influenced by list rank (figures in %)

(1) Baseline rank	**(2) Mean vote baseline**	**(3) Mean vote moved up**	**(4) Mean vote difference**	**(5) Share of influenced**
2	7.2	30.2	23.0	75.8
		[25.1, 36.1]	[17.9, 28.9]	[71.2, 79.9]
3	3.5	4.4	0.9	20.7
		[3.7, 5.2]	[.2, 1.7]	[6.1, 33.2]
4-6	2.4	2.9	.45	15.5
		[2.7, 3.0]	[.26, .66]	[9.6, 21.4]
7-10	1.87	2.03	.15	7.4
		[1.92, 2.14]	[.04, .27]	[2.3, 12.4]
11-15	1.08	1.12	.05	4.1
		[1.06, 1.19]	[-.01, .11]	[-1.3, 9.3]
$\sum$ 2-15	16.1	40.7	24.5	60.2
		[35.4, 46.6]	[19.3, 30.5]	[54.6, 65.4]

Table A.1 shows the results, which suggest substantively strong effects of ballot position. The candidates on baseline rank 2 receive on average only 7.2% of the OLPR vote when observed in the original position, but expected 30.2% when they occupy the first

position (due to a non-prominent politician being dropped). So 23.0%/30.2% or approximately three in four of those citizens who supported these candidates when ranked on the top spot would not have voted for them if they had been listed lower. For baseline rank three, the respective proportion of affected voters is about one in five, and for baseline ranks four to six it is slightly fewer than one in six. The respective figure for baseline ranks seven to ten is approximately 7%, and for eleven to fifteen 4%.[38] Summing across the groups yields that around six out of ten voters support candidates in the first fourteen ranks which they would not have chosen had they been ranked on position two to fifteen. The associated 95% confidence interval is [54.6%, 65.4%]. To further quantify how many voters that concerns, we can use a typical vote share of candidates on positions 1-14 (which is larger than the 40.7% of the 2-15 candidates in the moved-up state). In our data, the observed median (across party-year-district observations) of this share amounts to 85.3% of the intra-party vote. Multiplying this figure with the share of influenced voters (the 60.2%) yields an estimate of 51.3% (with 95% CI of [46.5%, 55.8%]). So this approach suggests that in a typical list approximately one out of two candidate votes are altered by ballot position.

There is one important caveat linked to this assessment of substantive effect sizes presented here. It rests on what is an extrapolation: we only observe candidates moving up, but no candidates moving down. This may result in a problem that especially concerns candidates in the baseline first list position. The baseline list leaders may on average be more appealing than other candidates, according to factors we do not observe (i.e. other than holding a prominent post or being an incumbent).[39] If voters decide on the basis of a satisficing-with-rank- taking-logic (which is favored by the overall evidence of analyses in this study), one implication is that the higher-appeal baseline list leaders probably lose less when appearing in position two compared to what those on baseline rank two gain when being shifted upwards (since the baseline list leaders are closer to being deemed good enough on the basis of unobserved appeal, in comparison to the baseline number

[38] In some draws the coefficient for shifts from baseline rank eleven to fifteen is negative (since the point estimate is not statistically significant). This results in a negative mean vote difference in $\approx 6.6\%$ of the draws. In these cases, the share of influenced persons in that row takes on negative values (strictly speaking, it is a net share of people pushed to choose the candidates). Note that this will hardly affect the overall results in the last line, since the marginal vote share of candidates in baseline ranks 11-15 is very small anyway.

[39] Observable differences in appeal warrant the use of the coefficients from Model 3-4: 37.7% of those on baseline rank one are ministers/hold a visible party office, compared to only 18.7% on rank two. On the other hand, the difference in incumbents on both ranks is rather small (53.4% vs. 47.7%).

two candidates). This would lead to some overestimation of the share of influenced voters on the basis of the approach chosen here.

A.3 Descriptive plots

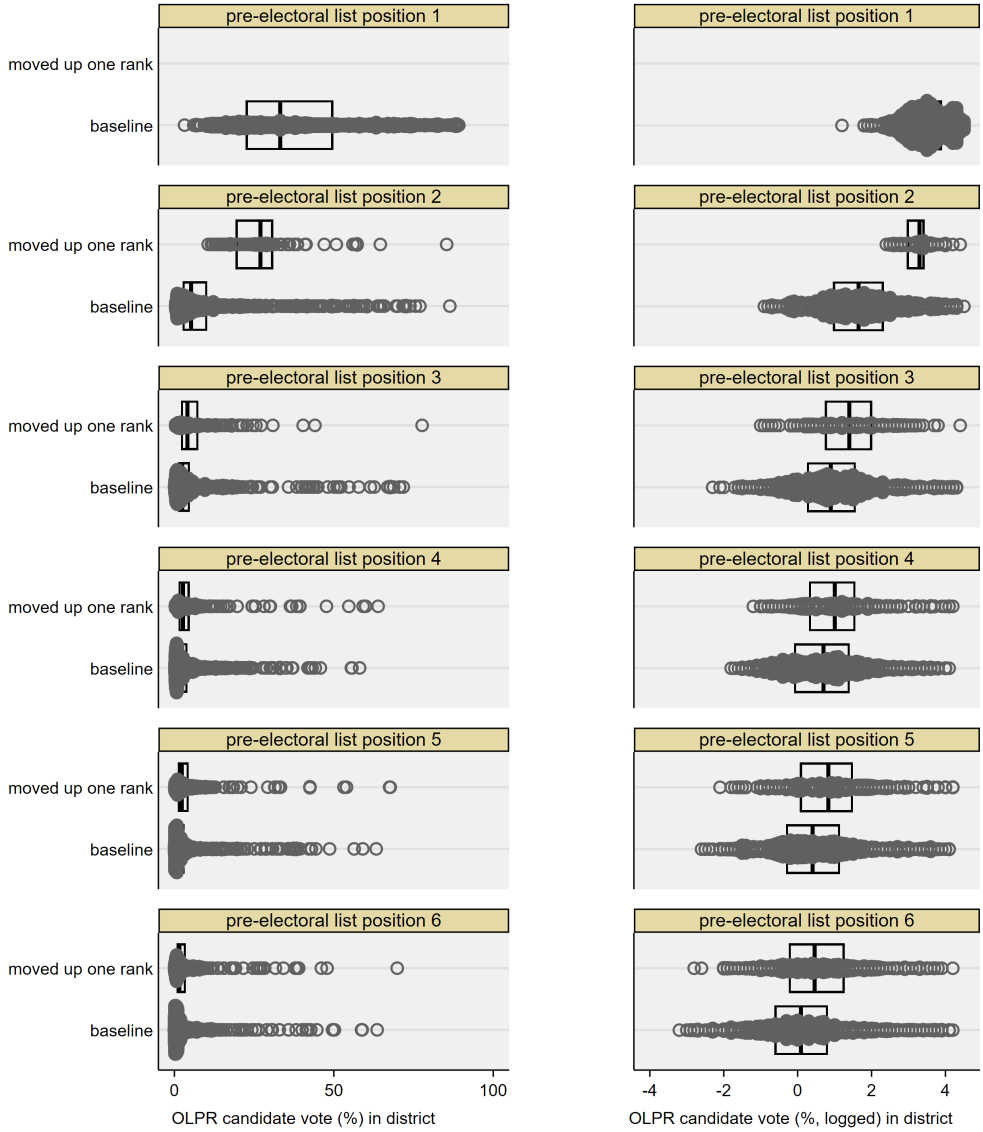

Figure A.1: Distribution of district-level candidate OLPR votes for candidates with baseline pre-electoral list position one to six depending on whether the candidate is observed in his/her baseline position or whether the candidate is observed where his/her position improved by one. Left panel shows distribution with candidate votes in %, right panel with distribution of candidate votes in %, logged. Each point represents a candidate-district-year observation.

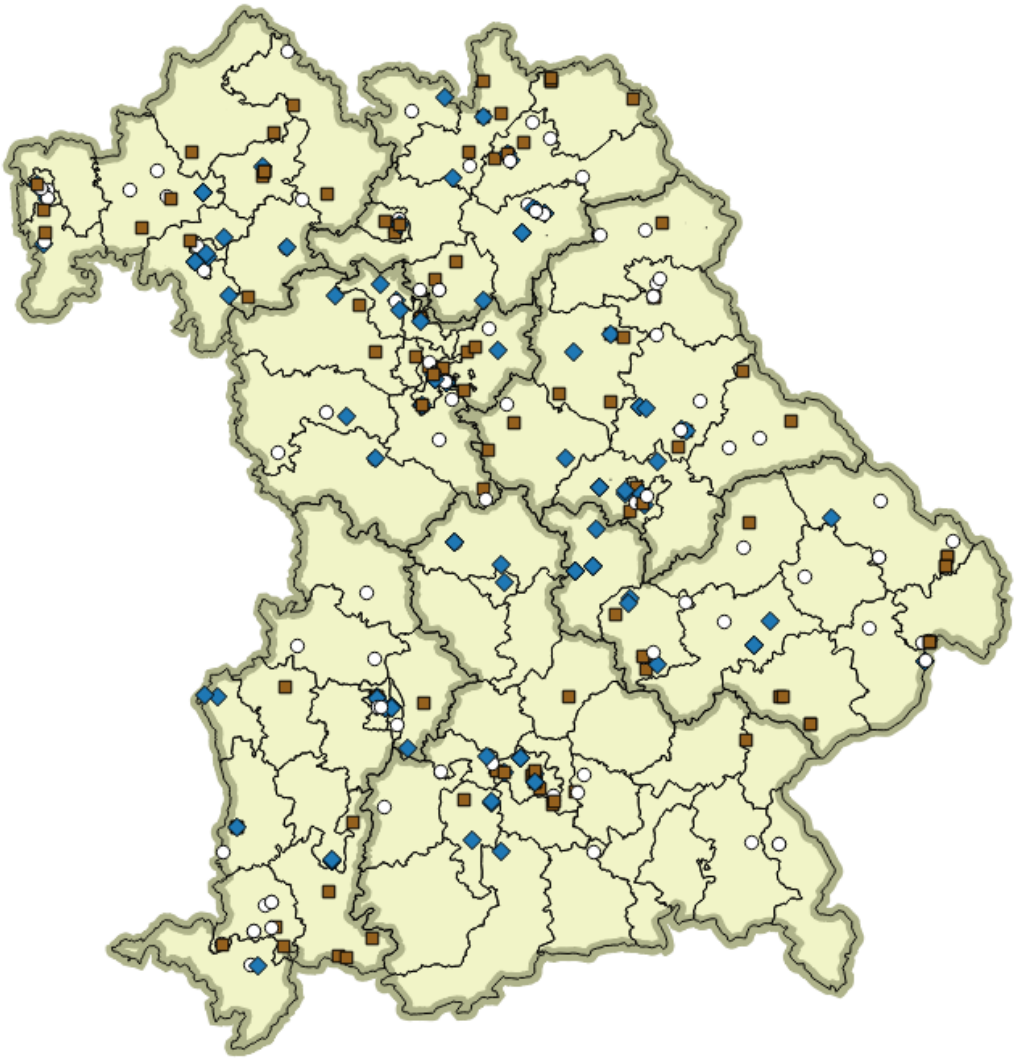

Figure A.2: Geographical distribution of 2013 candidates on pre election listranks r=1 to r=3 by place of residence in Bavaria. Broad boundaries show the seven regional OLPR districts. Nested within are local SMD districts (2013 boundaries). Note: blue diamond: r=1; white circle: r=2; brown rectangle: r=3.

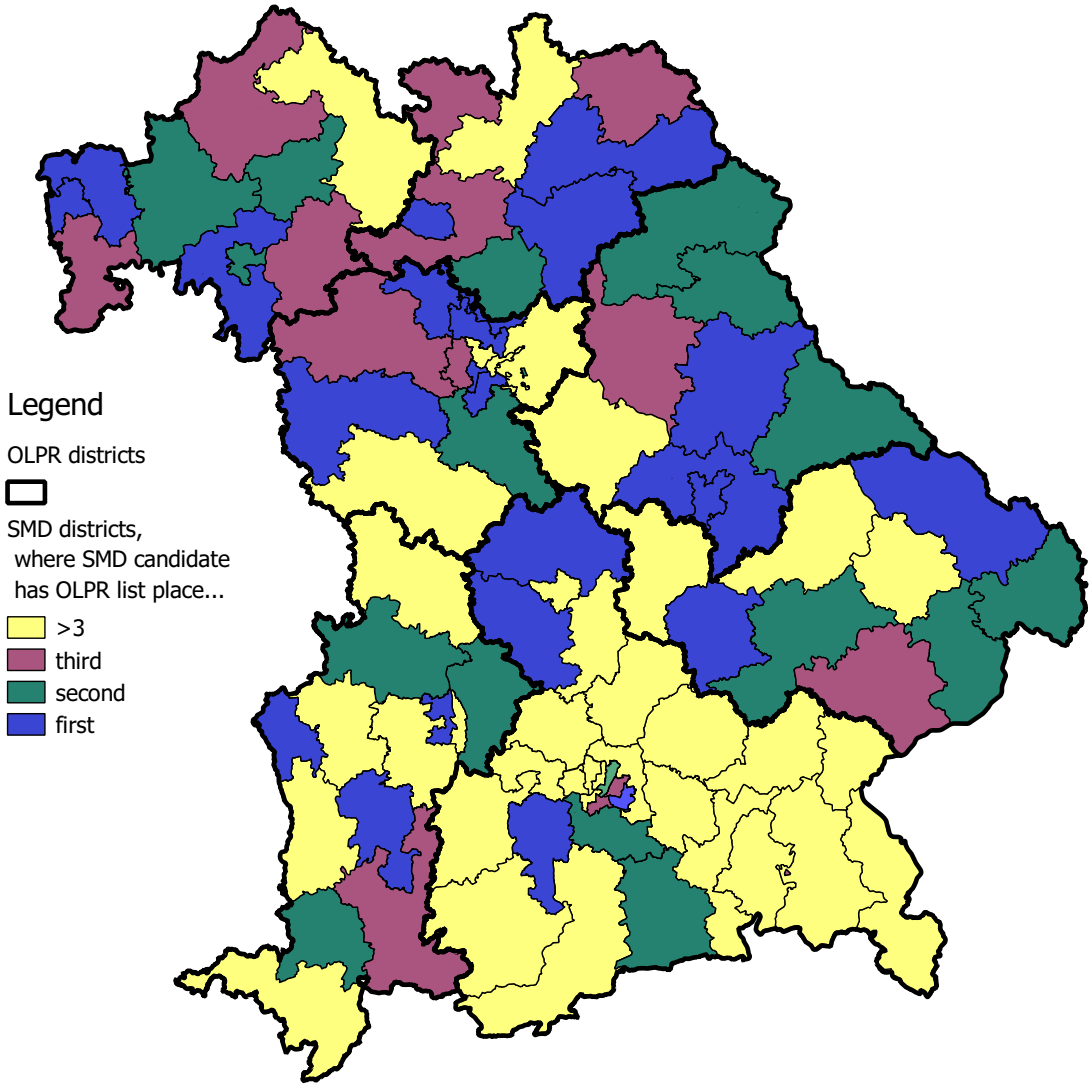

Figure A.3: Geographical distribution of SMDs with 2013 candidates that have pre election listranks r=1 to r=3 in Bavaria. Broad boundaries show the seven regional OLPR districts. Nested within are local SMD districts (2013 boundaries). Note: blue: at least one SMD candidate with r=1; green: with r=2 (if no r=1); purple: with r=3 (if no r=1 or r=2); yellow: else.

A.4 Additional specifications for main models

The following appendix tables show full results and/or additional specifications for the models presented in Tables 2 and underlying Figure 1.

Appendix Table A.2 reports in Model A.2-1 the correlation of observed/realized rank (as presented in a given district to voters) and candidate vote share within this district. We find support for a quadratic relationship: a candidates' vote share is higher further up the list, and even more so the closer to the top he/she is. This interpretation does not take potential endogeneity due to selection in list placement into account. It conforms with the treatment effects reported in Table 2.

Model A.2-2 reports a single Average Treatment Effect of moving up in list position, not taking into account that the treatment effect may vary with baseline pre-electoral list rank. (It is also a compound effect, since moving up occurs in combination with changes in the set of higher-placed competitors.) Here, we use all candidate-district-year observations (N=39918) across the three elections (from the parties we study). This is to show that we find a positive and statistically significant average effect, even when taking into account candidates that are placed below pre-election rank 15.

Model A.2-3 expands the basic result in Model 2-2 for the full sample: Our basic results hold for the full model. For candidates placed lower than pre-election rank 15, the estimated effect is also positive, but statistically insignificant.

Appendix Table A.3 reports in Model A.3-1 results including the coefficients for candidate pre-electoral list positions (omitted due to space constraints in Model 2-3). Comparing the size of these coefficients with the correlational results in Model A.2-1, it becomes again apparent that, even with this non-parametric specification, we observe a negative non-linear association between pre-electoral list position and expected vote share, which is monotonic over most of the list position range. Additionally, Model A.3-2 shows that these results are robust to the inclusion of controls for candidate geography.

Subsequently, Appendix Table A.4 reports the baseline results of Model A.2-2, together with an additional interaction of treatment effect and the geographical controls (both neighboring district and residence in district). First of all, when estimated separately by listrank, the coefficients for residence and neighboring district increase the further down a candidate is on the list. This (correlational) pattern is in line with the argument that lower-ranked candidates tend to be of lower appeal and thus can benefit more from the bonus provided by local residence. More importantly, the interaction of our treatment,

moving up in rank, and residence/neighboring district is significantly negative. This provides causal empirical evidence that the marginal benefit of appeal is higher on worse list positions. This latter finding adds to the evidence for the satisficing-with-rank-taking model, as further discussed in the main text.

Table A.2: Baseline treatment effects - additional specifications for Models 1 and 2 in Table 2

	Logged second vote shares		
	(1)	(2)	(3)
Candidate pre-electoral list position	-0.17**		
	(0.0060)		
Candidate pre-electoral list position × candidate pre-electoral list position	0.0020**		
	(0.00011)		
Moving up one list position		0.18**	
		(0.015)	
2 → 1			1.49**
			(0.070)
3 → 2			0.37**
			(0.054)
4 → 3 / 5 → 4 / 6 → 5			0.20**
			(0.030)
7 → 6 / (...) / 10 → 9			0.12**
			(0.025)
11 → 10 / (...) / 15 → 14			0.077**
			(0.025)
16 → 15 / (...) / last → second-to-last			0.016
			(0.017)
Neighboring district	0.75**	0.80**	0.80**
	(0.026)	(0.020)	(0.020)
Residence	2.69**	2.73**	2.73**
	(0.079)	(0.068)	(0.068)
Constant	1.44**	-0.77**	-0.76**
	(0.066)	(0.0078)	(0.011)
N	39918	39918	39918
Clusters	2554	2554	2554
Within R^2		0.42	0.43
Adj. R^2	0.54		

Model 1: OLS-regression with clustered standard errors. Model 2 and 3: Candidate fixed effects estimation with clustered standard errors. Models considers candidates on all ranks.
* (+,**) indicates $p < 0.05$ (0.1, 0.01)

Table A.3: Effects of moving up one rank assuming list fixed effects - full results for baseline list positions 1-15 (comp. Model 2-3)

	(1)	(2)
	OLPR candidate vote (%, logged) in district	
2 → 1	1.42**	1.47**
	(0.084)	(0.082)
3 → 2	0.33**	0.37**
	(0.075)	(0.070)
4 → 3 / 5 → 4 / 6 → 5	0.23**	0.24**
	(0.045)	(0.040)
7 → 6 / (...) / 10 → 9	0.15**	0.14**
	(0.046)	(0.044)
11 → 10 / (...) / 15 → 14	0.23**	0.11*
	(0.049)	(0.047)
Candidate pre-electoral list position=2	-1.79**	-1.78**
	(0.13)	(0.13)
Candidate pre-electoral list position=3	-2.53**	-2.54**
	(0.13)	(0.12)
Candidate pre-electoral list position=4	-2.75**	-2.76**
	(0.12)	(0.11)
Candidate pre-electoral list position=5	-2.98**	-2.97**
	(0.12)	(0.11)
Candidate pre-electoral list position=6	-3.27**	-3.29**
	(0.14)	(0.13)
Candidate pre-electoral list position=7	-3.38**	-3.38**
	(0.12)	(0.12)
Candidate pre-electoral list position=8	-3.42**	-3.42**
	(0.14)	(0.13)
Candidate pre-electoral list position=9	-3.53**	-3.56**
	(0.14)	(0.13)
Candidate pre-electoral list position=10	-3.54**	-3.58**
	(0.14)	(0.13)
Candidate pre-electoral list position=11	-3.67**	-3.65**
	(0.13)	(0.12)
Candidate pre-electoral list position=12	-3.87**	-3.85**
	(0.14)	(0.13)
Candidate pre-electoral list position=13	-3.80**	-3.79**
	(0.15)	(0.13)
Candidate pre-electoral list position=14	-4.02**	-3.99**
	(0.13)	(0.12)
Candidate pre-electoral list position=15	-4.10**	-4.09**
	(0.13)	(0.12)
Neighboring district		0.75**
		(0.019)
Residence		3.06**
		(0.048)
Constant	3.62**	3.47**
	(0.24)	(0.23)
List fixed effects	Yes	Yes
Candidate fixed effects	No	No
N	19425	19425
Adjusted R^2	0.48	0.64

Results for fixed effects dummy-variable estimator by including year-list dummies (Model 1) or candidate dummies (Model 2) to the model. Standard errors are clustered at the candidate level. Models 3, 4, 7, and 8 consider only candidates with baseline baseline list rank less than or equal to 15.
* (+,**) indicates $p < 0.05$ (0.1, 0.01)

Table A.4: Treatment effects of moving up in rank and interaction of treatment and residence/neighboring district

	logged shares (1) OLPR vote (%, logged) in SMD
2 → 1	1.54**
	(0.071)
3 → 2	0.43**
	(0.055)
4 → 3 / 5 → 4 / 6 → 5	0.26**
	(0.028)
7 → 6 / (...) / 10 → 9	0.12**
	(0.025)
11 → 10 / (...) / 15 → 14	0.067**
	(0.025)
Residence=1 × treatment=1	-0.32+
	(0.17)
Neighboring district=1 × treatment=1	-0.16**
	(0.057)
Residence=1 × listrank=1	0.51**
	(0.12)
Residence=1 × listrank=2	1.93**
	(0.21)
Residence=1 × listrank=3	2.03**
	(0.24)
Residence=1 × listrank=4-6	2.54**
	(0.18)
Residence=1 × listrank=7-10	2.78**
	(0.14)
Residence=1 × listrank=11-15	2.84**
	(0.13)
Neighboring district=1 × listrank=1	0.23**
	(0.029)
Neighboring district=1 × listrank=2	0.56**
	(0.061)
Neighboring district=1 × listrank=3	0.59**
	(0.071)
Neighboring district=1 × listrank=4-6	0.73**
	(0.054)
Neighboring district=1 × listrank=7-10	0.60**
	(0.045)
Neighboring district=1 × listrank=11-15	0.65**
	(0.042)
Constant	0.063**
	(0.0087)
N	19425
Clusters	1572
Within R^2	0.50

Candidate fixed effects estimation with clustered standard errors. Analysis considers only candidates with baseline list rank less than or equal to 15.
* (+,**) indicates $p < 0.05$ (0.1, 0.01)

Table A.5: Effects of moving up one rank by rank and by omitted first versus any subsequent baseline list position (as reported in Figure 1)

	(1)	(2)
	OLPR candidate vote (%, logged) in district	
2 → 1	1.46**	1.49**
	(0.078)	(0.071)
3 → 2 if 1 is dropped	0.52**	0.57**
	(0.086)	(0.072)
3 → 2 if 2 is dropped	0.18*	0.18**
	(0.078)	(0.069)
4/5/6 → 3/4/5 if 1 is dropped	0.38**	0.34**
	(0.067)	(0.053)
4/5/6 → 3/4/5 if other higher-placed is dropped	0.16**	0.15**
	(0.037)	(0.030)
7/8/9/10 → 6/7/8/9 if 1 is dropped	0.19**	0.25**
	(0.062)	(0.047)
7/8/9/10 → 6/7/8/9 if other higher-placed is dropped	0.13**	0.094**
	(0.033)	(0.025)
11-15 → 10-14 if 1 is dropped	0.20**	0.20**
	(0.064)	(0.046)
11-15 → 10-14 if other higher-placed is dropped	0.12**	0.059*
	(0.031)	(0.024)
Neighboring district		0.82**
		(0.019)
Residence		3.05**
		(0.057)
Constant	0.41**	0.068**
	(0.0087)	(0.0094)
N	19425	19425
Clusters	1572	1572
Within R^2	0.017	0.48

Candidate fixed effects estimation with clustered standard errors. Analysis considers only candidates with baseline list rank less or equal than 15.
* (+,**) indicates $p < 0.05$ (0.1, 0.01)

A.5 Additional balance and robustness tests

In the following Appendix Tables A.6 to A.8, core balance and robustness tests are displayed.

A.5.1 Balance in covariates

Appendix Table A.6 tests whether the distribution of pre-election list ranks differs by some observable characteristics. To do so, we provide two-sample Kolmogorov-Smirnov tests for the equality of the distribution of pre-election list ranks by binary indicators (or indicators dichotomized at the median). As indicated in the table header of Appendix Table A.6, we provide this test for the following observable indicators: taxes per capita, number of farmers, number of constructions, population influx, population density, immigrant share, employment share, CSU first vote, district implicated in 2013 CSU relatives affair (Rudolph and Däubler 2016; Kauder and Potrafke 2015), and urbanity. Results show no evidence that the distribution function of the list ranks of candidates in a district differs by these observable characteristics. This implies that removals from lower list ranks seem to occur in districts comparable to those with removals from higher list ranks.

Table A.6: Comparability of Listrank Distributions by Observable District Covariates

Statistic	Tax p.c.	Farmers	Constructions	Pop. influx	Pop. density	Immigrant share	Employment share	CSU first vote	Affair district	City
Combined K-S	0.077	0.035	0.076	0.074	0.053	0.090	0.048	0.088	0.068	0.088
P-value	0.270	0.987	0.303	0.317	0.740	0.136	0.844	0.151	0.823	0.264
P-value (corrected)	0.239	0.983	0.271	0.284	0.708	0.116	0.820	0.130	0.787	0.229
N	673	673	673	673	673	673	673	673	673	673

Two-sample Kolmogorov-Smirnov test for equality of distribution functions. Test reports whether distribution of list rank differs in characteristics of candidate-district observations where list rank is less than or equal to 15. Characteristics are expressed as binary variables (yes/no or above/below median). D-value of K-S-test as well as p-value and corrected p-value for a test on equality of distributions is reported. Data only available for observations from 2008 and 2013. Binary variables: district with 2013 CSU relatives affair (Rudolph and Däubler 2016; Kauder and Potrafke 2015), urban district (SMD district is predominantly a city or town); continuous variables, separated at 50th percentile: CSU first vote, employment share, immigrant share, population density, population influx (1000s), constructions (1000s), farmers (1000s), taxes per capita (1000s).

A.5.2 Robustness of effects for various subgroups

Finally, since we analyzed lists across three years, seven electoral districts and five parties, we can actually use the rather large within-sample variation to empirically explore some boundary conditions of our case. Appendix Tables A.7 and A.8 replicate Tables 2 and A.5, split across several theoretically interesting subgroups, defined by factors that potentially influence the strength of placement effects. We find that our main results hold (a) in settings with centralized or decentralized campaigning (measured by presence of state-wide campaign frontrunners); (b) in urban and rural contexts; (c) both for the CSU (most of whose MPs enter parliament via the SMD tier and the OLPR vote does hardly affect intra-party seat allocation) and for the other parties (in which OLPR votes are more influential in deciding who will obtain a seat); (d) in regions with above and below-median average levels of political interest, and above and below-median average ratings of the party chosen (based on survey data from Zittlau et al. (2017)); and (e) across short (up to 20 candidates), medium (21-40) and long (41-60) lists. Note, however, that this differentiation by subgroups is done for one variable at a time, so it typically reflects a compound differentiation. For example, lists including a state-wide frontrunner candidate also tend to be long. Although we only draw on one case, we therefore conclude that our results likely apply across a broad set of contexts.

Table A.7: Effects in Table 2 for subgroups

	Main effect	Frontrunner on ballot (no/yes)		Urban setting (no/yes)		Dominant party (CSU) (no/yes)		Above average political interest (no/yes)		Above average rating (no/yes)		Short/medium/long list			One media market (no/yes)	
		no	yes	no	yes	no	yes	no	yes	no	yes	short	medium	long	no	yes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
2 → 1	1.49**	1.44**	1.87**	1.41**	1.81**	1.47**	1.67**	1.45**	1.54**	1.37**	1.58**	1.47**	1.48**	1.55**	1.52**	1.33**
	(0.071)	(0.073)	(0.20)	(0.071)	(0.26)	(0.068)	(0.29)	(0.087)	(0.12)	(0.11)	(0.092)	(0.093)	(0.12)	(0.25)	(0.079)	(0.14)
3 → 2	0.37**	0.34**	0.54**	0.35**	0.82**	0.37**	0.37**	0.38**	0.39**	0.42**	0.35**	0.32**	0.40**	0.49**	0.35**	0.57**
	(0.055)	(0.059)	(0.14)	(0.058)	(0.13)	(0.060)	(0.14)	(0.069)	(0.090)	(0.085)	(0.072)	(0.073)	(0.11)	(0.12)	(0.060)	(0.13)
4 → 3 / 5 → 4 / 6 → 5	0.20**	0.18**	0.29**	0.19**	0.31**	0.19**	0.24**	0.17**	0.24**	0.21**	0.19**	0.16**	0.22**	0.24**	0.20**	0.17**
	(0.030)	(0.034)	(0.068)	(0.033)	(0.048)	(0.031)	(0.091)	(0.038)	(0.049)	(0.043)	(0.041)	(0.044)	(0.051)	(0.068)	(0.034)	(0.062)
7 → 6 / (...) / 10 → 9	0.12**	0.11**	0.14**	0.10**	0.17**	0.12**	0.14*	0.087**	0.17**	0.16**	0.099**	0.070	0.17**	0.11**	0.10**	0.17**
	(0.025)	(0.031)	(0.043)	(0.029)	(0.036)	(0.027)	(0.066)	(0.031)	(0.043)	(0.045)	(0.030)	(0.047)	(0.046)	(0.035)	(0.027)	(0.062)
11 → 10 / (...) / 15 → 14	0.072**	0.071*	0.078*	0.043	0.10**	0.056*	0.12*	0.073**	0.070	0.16**	0.050+	0.039	0.13*	0.058*	0.058*	0.19**
	(0.025)	(0.034)	(0.034)	(0.029)	(0.029)	(0.027)	(0.050)	(0.027)	(0.055)	(0.061)	(0.026)	(0.064)	(0.053)	(0.028)	(0.026)	(0.068)
Neighboring district	0.82**	0.83**	0.77**	0.87**	0.32**	0.76**	1.04**	0.87**	0.74**	0.88**	0.78**	0.84**	0.80**	0.83**	0.79**	0.96**
	(0.019)	(0.019)	(0.052)	(0.018)	(0.030)	(0.020)	(0.046)	(0.027)	(0.025)	(0.026)	(0.025)	(0.022)	(0.030)	(0.049)	(0.021)	(0.046)
Residence	3.04**	3.16**	2.29**	3.33**	0.61**	2.92**	3.45**	3.12**	2.93**	3.31**	2.83**	3.34**	2.81**	2.18**	3.02**	3.15**
	(0.057)	(0.047)	(0.24)	(0.042)	(0.12)	(0.065)	(0.11)	(0.087)	(0.068)	(0.060)	(0.086)	(0.050)	(0.092)	(0.24)	(0.063)	(0.12)
Constant	0.068**	0.12**	-0.080**	0.016	0.41**	0.20**	-0.43**	0.036**	0.13**	0.019	0.083**	0.080**	0.099**	0.023*	0.074**	0.027
	(0.0094)	(0.012)	(0.014)	(0.010)	(0.010)	(0.011)	(0.018)	(0.011)	(0.020)	(0.022)	(0.010)	(0.023)	(0.018)	(0.010)	(0.010)	(0.026)
N	19425	14542	4883	16206	3219	15485	3940	12870	6555	6484	12941	7821	5619	5985	16672	2753
Clusters	1572	1362	210	1572	943	1257	315	897	675	674	898	912	450	210	1348	224
Within R^2	0.48	0.51	0.35	0.53	0.21	0.47	0.52	0.47	0.50	0.57	0.42	0.56	0.45	0.32	0.47	0.54

Candidate fixed effects estimation with clustered standard errors. Models considers only candidates with baseline list rank less than or equal to 15. Model restricted to subgroups as specified in heading.
* (+,**) indicates $p < 0.05$ (0.1, 0.01)

Table A.8: Effects in Table A.5 for subgroups

	Main effect	Frontrunner on ballot (no/yes)		Urban setting (no/yes)		Dominant party (CSU) (no/yes)		Above average political interest (no/yes)		Above average rating (no/yes)		Short/medium/long list			One media market (no/yes)	
		no	yes	no	yes	no	yes	no	yes	no	yes	short	medium	long	no	yes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
2 → 1	1.49**	1.44**	1.87**	1.41**	1.81**	1.47**	1.67**	1.45**	1.54**	1.37**	1.58**	1.47**	1.48**	1.55**	1.52**	1.33**
	(0.071)	(0.073)	(0.20)	(0.071)	(0.26)	(0.068)	(0.29)	(0.087)	(0.12)	(0.11)	(0.092)	(0.093)	(0.12)	(0.25)	(0.079)	(0.14)
3 → 2 if 1 is dropped	0.57**	0.49**	1.07**	0.48**	1.02**	0.54**	0.77**	0.57**	0.59**	0.63**	0.54**	0.48**	0.62**	0.83**	0.53**	0.86**
	(0.072)	(0.068)	(0.26)	(0.077)	(0.16)	(0.074)	(0.21)	(0.10)	(0.10)	(0.087)	(0.11)	(0.086)	(0.12)	(0.31)	(0.080)	(0.13)
3 → 2 if 2 is dropped	0.18**	0.19*	0.091	0.23**	0.52**	0.21**	-0.029	0.19*	0.20	0.21+	0.17*	0.17+	0.17	0.25**	0.17*	0.31+
	(0.069)	(0.081)	(0.096)	(0.071)	(0.096)	(0.076)	(0.15)	(0.080)	(0.12)	(0.12)	(0.082)	(0.098)	(0.15)	(0.093)	(0.076)	(0.17)
4/5/6 → 3/4/5 if 1 is dropped	0.34**	0.26**	0.85**	0.27**	0.64**	0.30**	0.65**	0.34**	0.35**	0.34**	0.34**	0.22**	0.40**	0.74**	0.32**	0.45**
	(0.053)	(0.056)	(0.13)	(0.057)	(0.11)	(0.053)	(0.17)	(0.075)	(0.072)	(0.077)	(0.071)	(0.068)	(0.089)	(0.17)	(0.057)	(0.14)
4/5/6 → 3/4/5 if other higher-placed is dropped	0.15**	0.15**	0.12+	0.16**	0.20**	0.16**	0.088	0.11**	0.21**	0.16**	0.14**	0.14**	0.16**	0.11*	0.16**	0.084
	(0.030)	(0.034)	(0.064)	(0.034)	(0.046)	(0.032)	(0.087)	(0.036)	(0.052)	(0.044)	(0.040)	(0.046)	(0.053)	(0.054)	(0.034)	(0.060)
7/8/9/10 → 6/7/8/9 if 1 is dropped	0.25**	0.18**	0.66**	0.17**	0.62**	0.22**	0.47**	0.21**	0.31**	0.31**	0.21**	0.14*	0.40**	0.34*	0.21**	0.48**
	(0.047)	(0.052)	(0.11)	(0.051)	(0.090)	(0.050)	(0.14)	(0.065)	(0.068)	(0.071)	(0.064)	(0.065)	(0.090)	(0.14)	(0.050)	(0.13)
7/8/9/10 → 6/7/8/9 if other higher-placed is dropped	0.094**	0.095**	0.073+	0.092**	0.10**	0.099**	0.064	0.067*	0.15**	0.12**	0.081**	0.055	0.13**	0.086*	0.086**	0.12*
	(0.025)	(0.031)	(0.041)	(0.029)	(0.035)	(0.027)	(0.069)	(0.030)	(0.044)	(0.045)	(0.030)	(0.047)	(0.046)	(0.033)	(0.027)	(0.061)
11-15 → 10-14 if 1 is dropped	0.20**	0.14**	0.54**	0.13*	0.47**	0.13**	0.54**	0.18**	0.21**	0.29**	0.16**	0.16*	0.22**	0.24	0.20**	0.21*
	(0.046)	(0.051)	(0.14)	(0.051)	(0.083)	(0.046)	(0.16)	(0.061)	(0.076)	(0.079)	(0.062)	(0.078)	(0.083)	(0.16)	(0.051)	(0.098)
11-15 → 10-14 if other higher-placed is dropped	0.059*	0.061+	0.039	0.035	0.061*	0.049+	0.057	0.062*	0.050	0.14*	0.039	0.018	0.12*	0.045+	0.043+	0.19**
	(0.024)	(0.034)	(0.033)	(0.028)	(0.029)	(0.027)	(0.050)	(0.027)	(0.055)	(0.061)	(0.025)	(0.064)	(0.053)	(0.027)	(0.026)	(0.070)
Neighboring district	0.82**	0.83**	0.78**	0.87**	0.32**	0.76**	1.05**	0.87**	0.74**	0.88**	0.78**	0.84**	0.80**	0.84**	0.79**	0.96**
	(0.019)	(0.019)	(0.052)	(0.018)	(0.029)	(0.020)	(0.046)	(0.027)	(0.025)	(0.026)	(0.025)	(0.022)	(0.030)	(0.049)	(0.021)	(0.046)
Residence	3.05**	3.16**	2.30**	3.34**	0.63**	2.92**	3.45**	3.12**	2.94**	3.31**	2.84**	3.34**	2.82**	2.19**	3.02**	3.16**
	(0.057)	(0.047)	(0.24)	(0.042)	(0.12)	(0.065)	(0.10)	(0.087)	(0.068)	(0.060)	(0.086)	(0.050)	(0.092)	(0.24)	(0.063)	(0.12)
Constant	0.068**	0.12**	-0.082**	0.015	0.40**	0.20**	-0.43**	0.036**	0.13**	0.020	0.082**	0.082**	0.100**	0.022*	0.074**	0.027
	(0.0094)	(0.012)	(0.014)	(0.010)	(0.010)	(0.011)	(0.018)	(0.011)	(0.020)	(0.022)	(0.010)	(0.023)	(0.018)	(0.010)	(0.010)	(0.026)
N	19425	14542	4883	16206	3219	15485	3940	12870	6555	6484	12941	7821	5619	5985	16672	2753
Clusters	1572	1362	210	1572	943	1257	315	897	675	674	898	912	450	210	1348	224
Within R^2	0.48	0.51	0.37	0.53	0.26	0.47	0.53	0.48	0.50	0.58	0.42	0.57	0.46	0.33	0.47	0.55

Candidate fixed effects estimation with clustered standard errors. Models considers only candidates with baseline list rank less than or equal to 15. Model restricted to subgroups as specified in heading.
* (+,**) indicates $p < 0.05$ (0.1, 0.01)