

# Holding Individual Representatives Accountable: The Role of Electoral Systems

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Voters are reluctant to sanction representatives for individual misconduct if they have to balance candidate-level and party-level factors in their choice, but this trade-off is affected by the electoral system. Our general theoretical model explains why individual accountability can empirically occur in single-member district (SMD) systems but is expected under less restrictive conditions using open-list proportional representation (OLPR). The latter not only decouples party and candidate choice but also makes seat allocation more vote elastic. For a thorough empirical test of our argument, we draw on real-world evidence from state-level elections in Bavaria, Germany, which are held under an unusual mixed-member system. Exploiting a recent public scandal involving one-third of representatives, we examine how electoral punishment of the same candidates by the same voters differs across electoral rules. Drawing on difference-in-differences as well as matching/regression estimators, we show that electoral punishment is substantially larger under OLPR than under SMD systems.

Since the quality of democracy depends on effective mechanisms for selecting representatives and holding them accountable, it is a troubling fact that politicians who have shown individual misconduct are frequently re-elected.<sup>1</sup> One influential account to explain this puzzling observation centers on citizens' lack of sufficient and reliable information about misbehavior (e.g., Chang, Golden, and Hill 2010; Ferraz and Finan 2008). A second key explanation builds on the insight that candidate integrity is only one of several factors that voters take into account when making their choice (Peters and Welch 1980; Rundquist, Strom, and Peters 1977). This conclusion also follows from the theoretical literature on the role of valence in electoral competition (Adams 1999; Adams, Merrill, and Grofman 2005; Ashworth and Bueno de Mesquita 2009; Enelow and Hinich 1982; Groseclose 2001).

Both these mechanisms are affected by the electoral system. Generally, it is well known that electoral systems influence the degree to which representation is collective or individual (Carey 2009; Colomer 2011; Grofman 2005). Yet theoretical and empirical ambiguity remains about the extent to which different electoral systems are conducive to individual accountability. On the one hand, most of the electoral systems literature considers intraparty competition among candidates as a crucial condition for a personal vote (Carey and Shugart 1995; Shugart 2013; but see Norris 2004).<sup>2</sup> On the other hand, for single-member district (SMD) systems, empirical studies also typically find that individuals having been involved in scandals are indeed punished at the electoral stage (e.g., Basinger 2013; Dimock and Jacobson 1995; Eggers 2014; Pattie and Johnston 2012; Vivyan, Wagner, and Tarlov 2012).<sup>3</sup>

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1. On individual accountability see, e.g., Crisp et al. (2014), Fearon (1999), and Strøm, Müller, and Bergman (2006). Accountability failures are discussed by Basinger (2013), de Sousa and Moriconi (2013), Dimock and Jacobson (1995), Eggers (2014), Pattie and Johnston (2012), and Vivyan et al. (2012).

2. A large literature considers personal vote-seeking incentives as independent variables in explaining legislative behavior, constituency work, and campaign styles (for a recent review, see André, Depauw, and Shugart [2014]).

3. The same holds for results regarding the electoral impact of legislative behavior (e.g., Bowler 2010; Cain, Ferejohn, and Fiorina 1987; Canes-Wrone, Brady, and Cogan 2002). Note that prior to the electoral stage, candidate selection (primaries in the US context) may serve as an additional mechanism increasing accountability (Basinger 2013; Dimock and Jacobson 1995).

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What is lacking is a well-developed theoretical argument about the conditions under which different electoral systems allow citizens to hold individual representatives accountable.

Starting from a micro-level model of voter decision making in a multiparty setting, we develop a general theoretical framework for assessing differences in sanctioning of individual misbehavior under SMD and open-list proportional representation (OLPR). We formally show that the decoupling of party and candidate choice by OLPR allows individual accountability under less restrictive conditions, notably also in party-centered settings. This effect is reinforced as intraparty seat allocation under OLPR is typically vote elastic, that is, it is responsive even to small changes of the intraparty vote share.

Whether institutions work in practice as expected by theory is an empirical question. First, institutions do not determine but only constrain behavior (North 1990; Tsebelis 1990), and so we should examine if the postulated constraints in fact apply. Second, the accountability potential of electoral systems with intraparty choice may be undermined by a counterbalancing effect linked to the “informational” mechanism. When each party runs multiple candidates, the electorate is likely to know less about each individual politician. Voters then refer to information shortcuts such as ballot position (Brockington 2003; Faas and Schoen 2006; Marcinkiewicz and Stegmaier 2015) or candidates’ local ties (Shugart, Valdini, and Suominen 2005). If too many voters merely rely on cues, however, we cannot expect that intraparty choice will bring about individual accountability. Third, only a few empirical studies examine the link between representatives’ behavior and the personal vote under electoral systems with intraparty choice (Ames 1995; Chang et al. 2010; Crisp et al. 2013). While they find the expected effects, trying to assess electoral system differences from disparate single-system studies is problematic. Ideally, we should draw our inferences from comparing the electoral performance of the same set of representatives (who differ in behavior/integrity) among the same set of voters while varying the electoral rules.

The second contribution of this article is therefore empirical. Using real-world evidence from an unusual mixed-member electoral system, we isolate the institutional difference in electoral punishment under SMD and OLPR. To do so, we exploit a 2013 public scandal concerning members of parliament (MPs) of the state-level parliament in Bavaria, Germany. In spring of that year—less than five months before the scheduled elections—it became public that about one-third of the Bavarian MPs employed close relatives as staff members (von Arnim 2013). Like the 1992 US “House Banking Scandal” (see, e.g., Banducci and Karp 1994; Dimock and Jacobson 1995) and the 2009 UK “Parliamentary Expenses

Scandal” (see, e.g., Eggers 2014; Larcinese and Sircar 2014; Pattie and Johnston 2012; Vivyan et al. 2012), what became known as the “relatives affair” therefore has got features of a quasi-experiment: the scandal disclosed a similar type of misconduct at the same point in time for some incumbent MPs but not for others.<sup>4</sup> We are not only able to draw on the exogenous shock to candidate valence to infer the degree of electoral punishment, we can also make use of the special type of mixed-member compensatory system used in Bavaria (Faas and Schoen 2006; Massicotte 2011).

The combination of an SMD tier and a regional OLPR tier in Bavaria provides several good opportunities for assessing the impact of valence effects on the electoral performance of party and candidates under different electoral rules. We conduct three types of analyses, with the following main findings. First, we analyze the impact of the scandal on the SMD vote at the local district level with a difference-in-differences design. When a district was represented by an implicated MP, we find moderate losses for the respective party/candidate vote. Second, we propose a differencing estimator drawing on the gap between the first tier and the second tier party vote at the district level. This isolates the pure candidate integrity effect under SMD; again, we find moderate negative effects. Finally, a regression/matching approach analyzes the aggregate decision making of voters who have opted for a party list including scandal candidates. This approach reveals strong punishment for the intraparty vote shares of affected candidates. Comparing the magnitude of sanctioning under OLPR and SMD, we conclude that, under pure OLPR, implicated incumbents would have been voted out of office with much higher probability. Our results therefore confirm the argument that using OLPR improves the accountability of individual representatives.

## THEORETICAL FRAMEWORK

Our interest lies in explaining how electoral rules affect the extent to which voters sanction individual representatives for misconduct. To do so, we first formulate a model of vote choice at the micro level, drawing on the literature analyzing electoral competition with valence components (Adams et al. 2005; see also Adams 1999; Ashworth and Bueno de Mesquita 2009; Groseclose 2001; Mauerer, Thurner, and Debus 2015).<sup>5</sup>

4. Independent of us, Kauder and Potrafke (2015) also examine the electoral consequences of this scandal. Their analysis, however, does not deal with electoral system differences. It concentrates on estimating the scandal’s impact on the combined CSU vote share (SMD plus OLPR vote).

5. The model resembles the one in Eggers (2014), although ours refers to the multiparty case, uses a more fine-grained distinction among utility components, and is probabilistic rather than deterministic.

At the most general level, suppose the utility  $U$  that voter  $i$  gains from supporting party  $p$  and candidate  $c$  amounts to

$$U_{ipc} = \beta_{n_p} n_{ip} + \beta_{v_p} v_{ip} + \beta_{n_c} n_{ic} + \beta_{v_c} v_{ic} + \epsilon_{ip} + \epsilon_{ic}, \quad (1)$$

where  $n_{ip}$  ( $n_{ic}$ ) reflects the “closeness” of voter  $i$  to party  $p$  (candidate  $c$ ) and  $v_{ip}$  ( $v_{ic}$ ) represents the degree of integrity voter  $i$  ascribes to party  $p$  (candidate  $c$ ). Each of these factors has a specific nonnegative weight  $\beta$  attached to it, assumed constant across voters. In addition, there are unobserved factors at the voter-party ( $\epsilon_{ip}$ ) and voter-candidate level ( $\epsilon_{ic}$ ) that also affect the choice.<sup>6</sup>

Before proceeding, two general remarks are indicated. First, we assume that voters choose sincerely, because in our empirical application, strategic voting considerations (Myerson 1993) are likely unimportant.<sup>7</sup> More generally, we leave open the nature of the utility voters derive from their choices. Voters may be driven by an instrumental motivation to alter election prospects of a party or candidate, but, particularly in the context of individual misconduct, voters may refrain from supporting candidates for expressive reasons (Brennan and Hamlin 1998). Second, our setup differs from the more common approach of distinguishing spatial proximity and valence, where valence refers to “anything that is not proximity” (Grosseclose 2001). In contrast, we conceive of closeness in broader terms, that is, closeness can be a consequence of, for example, spatial proximity, ascribed economic/issue competence, party identification in the behavioral tradition, or homophily in candidate preferences.

In the following, we consider the consequences of a drop in the average value of (perceived) candidate integrity  $v_{ic}$  in an SMD system. Party and candidate vote are fused, so we denote the candidates as  $c[q]$  and  $c[r]$ .<sup>8</sup> Suppose party  $r$  runs a candidate with lower valence than party  $q$ . A voter will still choose party  $r$  rather than party  $q$  if  $U_{iqc[q]} < U_{irc[r]}$ , that is, if

$$\begin{aligned} \beta_{v_c}(v_{ic[q]} - v_{ic[r]}) &< \beta_{n_p}(n_{ir} - n_{iq}) + \beta_{v_p}(v_{ir} - v_{iq}) \\ &+ \beta_{n_c}(n_{ic[r]} - n_{ic[q]}) + (\epsilon_{ir} - \epsilon_{iq}) + (\epsilon_{ic[r]} - \epsilon_{ic[q]}). \end{aligned} \quad (2)$$

6. In the very widely used additive setup, the effect of a change in one component is independent of the value of other components. Enelow and Hinich (1982) suggest a model where valence and spatial proximity interact, motivated by the argument that valence also reflects the probability that a proposed policy will be implemented. With our narrow understanding of  $v_{ic}$  as perceived candidate integrity (and a broad definition of the “closeness” terms  $n_{ip}$  and  $n_{ic}$ , which include ascribed competence), we do not see compelling theoretical reasons for giving up the much more tractable additive specification. Importantly, as argued below, our core insights do not depend on this linear specification.

7. Importantly, the Bavarian electoral system does not provide any incentives for strategic voting in the Duvergerian sense.

8. We refer to the electoral stage only, not considering candidate selection within parties.

Put differently, party  $r$  is still preferred if the weighted valence advantage of  $q$ 's candidate is trumped by the weighted sum of party-level factors (closeness, perceived party integrity, other factors), of closeness to the candidate, and of differences in unobserved candidate characteristics. In party-centered political systems, we expect that  $\beta_{n_p} > \beta_{v_p} > \beta_{n_c}, \beta_{v_c}$ , that is, voters care much more about closeness to the party and party integrity as compared to candidate integrity. Voters strongly drawn towards party  $r$  eventually bite the bullet and support a candidate of questionable honesty, since party-level considerations are more important to them. In addition, at the candidate level, other characteristics (say competence) may outweigh integrity considerations. Therefore, few voters' choices will be strongly influenced by the candidate integrity differential. Misconduct by  $c[r]$  will sway only voters who are nearly indifferent between the two parties on the ground of all other factors.

We can generalize this notion to assess the impact of a change in perceived candidate integrity on the aggregate party vote share in a multiparty contest. To do so, we make the common choice of a type I extreme value distribution for the sum of the unobserved quantities in the utility function (Adams 1999; Adams et al. 2005; Thurner 1998). The expected probability  $W_{ip}$  of voter  $i$  to choose party  $p$  is then equal to  $W_{ip} = e^{U_{ip}} / \sum_p e^{U_{ip}}$ , and it follows that the expected vote share of the party is the mean of this probability across all voters:  $E(V_p) = (1/I) \sum_i W_{ip}$ . The marginal effect of candidate integrity on the expected party vote share  $E(V_p)$  of party  $p$  is then given by

$$\frac{\partial E(V_p)}{\partial v_{ic[p]}} = \frac{1}{I} \sum_i \frac{\partial E(V_p)}{\partial U_{ip}} \frac{\partial U_{ip}}{\partial v_{ic[p]}} = \frac{1}{I} \sum_i W_{ip} (1 - W_{ip}) \beta_{v_c}. \quad (3)$$

So the aggregate electoral impact of a shock to perceived candidate integrity depends on two factors: unsurprisingly, any change in candidate integrity has a stronger impact if its weight in the utility function is larger (higher  $\beta_{v_c}$ ). More interesting, for any specific voter, her perception of a candidate's integrity will matter more strongly for her vote choice if the voter is closer to being indifferent between that party-candidate combination and the field of all other party-candidate bundles.<sup>9</sup> The aggregate impact of candidate valence therefore also depends on the district-level average degree of indifference between the candidate's party and the other parties. If party-level factors and candidate features

9. Since this important insight follows from  $\partial E(V_p) / \partial U_{ip}$ , it does not depend on the choice of a specific utility function. The other central argument below—that OLPR makes available substitute goods in terms of candidates that are identical on party grounds—likewise is not bound to the specific functional form in eq. (1).

other than integrity weigh heavily in voters' calculations and the parties/candidates differ with regard to those other factors, the impact of individual misconduct on the vote is limited.

We now turn to our institutional comparison with OLPR. By OLPR, we refer to a system with one or more multi-member districts, where parties run multiple candidates, citizens vote for one candidate from one list, candidate votes pool to the party level for interparty seat allocation, and seats within parties are assigned solely on the basis of candidate votes. The key difference between the OLPR case and the SMD case lies in the opportunity of choosing from multiple candidates of the same party. Put in economics language, substitute goods are available within each party.

To start with, suppose citizens choose a party and candidate simultaneously; that is, they compare options both across and within lists. When comparing a candidate  $e$  who is inferior in perceived integrity to a candidate  $d$ ,  $e$  is nevertheless preferred if

$$\beta_{v_c}(v_{id} - v_{ie}) < \beta_{n_p}(n_{ip[e]} - n_{ip[d]}) + \beta_{v_p}(v_{ip[e]} - v_{ip[d]}) + \beta_{n_c}(n_{ie} - n_{id}) + (\epsilon_{ip[e]} - \epsilon_{ip[d]}) + (\epsilon_{ie} - \epsilon_{id}). \quad (4)$$

Under OLPR, the two specific candidates under consideration may vary in terms of their party affiliation or not. If they do ( $p[d] \neq p[e]$ ), the choice among the pair is characterized by the same logic as in the SMD case, and many voters still pick candidate  $e$ . When  $p[d] = p[e]$ , however, candidate  $e$  with the less favorable reputation is only supported if

$$\beta_{v_c}(v_{id} - v_{ie}) < \beta_{n_c}(n_{ie} - n_{id}) + (\epsilon_{ie} - \epsilon_{id}). \quad (5)$$

Since all party-level factors are identical between the candidates, candidate  $e$  could only compensate the disadvantage by means of other candidate characteristics subsumed under the "closeness" term  $n_{ie}$  (e.g., policy competence) or other unobserved candidate-level features  $\epsilon_{ie}$ . Since there are typically several intraparty competitors available on a list, it is quite likely that the citizen will find a copartisan colleague who matches the lower integrity candidate in terms of other candidate characteristics. Since the open list offers comparable alternatives, perceived candidate integrity can have a considerable impact on candidate choice, even if its weight in the utility function  $\beta_{v_c}$  is actually low.

The decoupling of party and candidate choice under OLPR holds regardless of the temporal order of the two choices. For choosing among the candidates within a party, party-level factors will obviously not matter since they are shared by all candidates. So, within a list, only the most

preferred candidate is considered.<sup>10</sup> These "favorites" from each party should typically not differ much in perceived honesty among themselves, and therefore candidate integrity is unlikely to affect the choice between parties. (And, as we learned from the SMD case above, even pronounced differences in candidate integrity could affect party choice only under restrictive conditions.) Finally, it is plausible that in practice many citizens choose sequentially, that is, they decide first between parties solely on the basis of party-level factors and only then pick a candidate from among the selected list.

For the OLPR case, it therefore makes sense to examine the electoral performance of candidates among the group of voters who opted for the respective party:

$$U_{ic|p=p} = \beta_{n_c}n_{ic} + \beta_{v_c}v_{ic} + \epsilon_{ic}. \quad (6)$$

In analogy to equation (3), the marginal impact of a change in candidate integrity on a candidate's share of the intraparty vote can be derived as

$$\frac{1}{I_p} \sum_{i_p} W_{ic|p=p}(1 - W_{ic|p=p})\beta_{v_c}. \quad (7)$$

So the absolute size of the integrity effect on the intraparty vote share depends (a) on the importance of candidate integrity to voters and (b) the average degree of indifference between the respective candidate and the field of copartisans in the specific party. And here another implication following from the availability of substitute candidates becomes important. Under OLPR, with larger district magnitude, parties tend to win several seats, and intraparty seat allocation becomes vote elastic: small differences in candidate vote shares decide who will obtain the last seats within a party.<sup>11</sup> In the case of SMD, in contrast, there is always just one seat to be allocated (to a party and a candidate at the same time). So in practice there are many districts where party support is so

10. The pooling of candidate votes to the party level may create incentives for strategic voting. Outcome-oriented voters may fear that their vote for a high-integrity candidate will be irrelevant for intraparty seat allocation but pivotal in gaining an additional seat that could be allocated to a low-integrity candidate. Voters would then need to compare the utility increase from an additional party MP to the decrease that this MP might be of questionable character. Importantly, this latter term must be weighted twice: the allocation of the last seat to a low-integrity candidate is probabilistic, and typically the share of such candidates on the list will be low. Additionally, a vote for a high-integrity candidate may on the margin change the ballot order and at best even oust a low-integrity candidate. If some voters indeed engage in this calculation, we suppose that few of them derive a positive expected utility from the strategic choice.

11. Bergman, Shugart, and Watt (2013) show empirically that under OLPR the intraparty vote of the candidate winning the last seat decreases with district magnitude. They ascribe this effect to the increasing number of available candidates.

skewed that large vote swings would be required to unseat the incumbent candidate/party.<sup>12</sup>

In addition to OLPR decoupling candidate-level and party-level factors at the election stage, it is therefore also more responsive at the seat-allocation stage. Due to both effects, an open-list system facilitates individual-level accountability. Our key expectation is thus that *for a given shock to candidate integrity, electoral punishment is more consequential under OLPR than under SMD*.<sup>13</sup> Prospects for direct individual accountability are on average worse under SMD than they are under OLPR. Note, however, that the effect of having the latter electoral system rather than the former will also depend on the local context. It is another straightforward implication of equation (3) that the increased sanctioning potential brought by a counterfactual change to OLPR will be more pronounced if the prospects for holding individual representatives accountable under SMD are poor to begin with, for example, due to a polarized party landscape.

#### THE “EMPLOYED RELATIVES SCANDAL” IN BAVARIA

Electoral competition in Bavaria has been dominated by the Christian Social Union (CSU), sister party of the Christian Democratic Union (CDU), for decades.<sup>14</sup> A recent peak was the 2003 election, where the CSU reached a two-thirds majority in parliament under their long-term party leader

Edmund Stoiber. His successor Günther Beckstein in comparison suffered a defeat in the 2008 election with 43% of the vote. For the first time since 1962, the CSU was forced into a coalition government. An explicit CSU campaign goal for 2013 under the new party leader Horst Seehofer was to regain its absolute majority.

The CSU was ultimately successful in doing so, reaching 48% of the total vote and 101 out of 180 seats in parliament in the September 2013 state-level election. The “Employed Relatives Scandal” we discuss in the following had threatened this goal, however. The scandal centered on MPs of the Bavarian state parliament having employed relatives as staff members, paying their contracts out of public funds. The affair was set off in April 2013, when retired law professor Hans-Herbert von Arnim (2013) launched a popular science-style book. It heavily criticized current MP and party compensation schemes in the Bavarian parliament in general and the on-going employment of relatives in particular. This publication took the political establishment by complete surprise and quickly changed the dynamics of the ongoing campaign for the September 2013 state elections. Especially the governing CSU came under immense pressure, and the party leadership swiftly reacted.

Bavarian MPs receive a lump sum for running their parliamentary office. Until regulations were changed in the aftermath of the scandal, they enjoyed broad discretion in using these funds with minimal bureaucratic oversight. Regarding staff allowances, MPs, for example, only had to state that these were used in accordance with legal provisions. The book by von Arnim (2013) directed public attention to two issues. First, a large number of MPs continued to employ relatives of first degree, based on older contracts that were considered formally legal due to a transitional rule. In addition, employment of second- or third-degree relatives by MPs was—unlike in national parliament or public service in general—fully permitted, and it was widespread.

Most contracts were not considered formally illegal, but parliament was heavily criticized by von Arnim in how it had regulated itself. The revelations created strong attention and public debate. The most prominent individual case concerned Georg Schmid, parliamentary party group leader of the CSU, who had paid his wife a monthly salary of up to €5,500 for office duties over several years. He soon resigned as party group leader and announced that he would not stand again in the upcoming elections. On May 2, 2013, Bavarian Broadcasting published a comprehensive list of MPs who had employed relatives in the recent past (von Arnim 2013, appendix 11). The document was based on official information (concerning first-degree cases) published in late April and early May 2013 by the Bavarian parliament itself (press releases

12. In OLPR systems that use ranked lists like in Bavaria (whose ordering will matter only at the electoral stage, but not for seat allocation as such, as by our definition), having obtained a very good pre-electoral list position may have similar effects to running in a SMD seat with a favorable partisan makeup, since ballot position effects serve as information shortcuts (Brockington 2003). In the theoretical model, any ballot position effects form part of the candidate closeness term  $n_{ic}$ . Note that therefore ballot position effects may either increase or decrease the absolute effect of candidate integrity, depending on their contribution to  $W_{ic|p=p}$  in eq. (7). Also, while ballot position effects can rescue the reelection of high-ranked low-integrity candidates, they also facilitate sanctioning other candidates by making the allocation of a party's last seats more vote elastic.

13. We develop our argument here specifically for OLPR systems. The general framework can, of course, also be used to derive expectations about other electoral systems with intraparty choice, but they are likely different. For example, in flexible list systems, intraparty seat allocation may also be based on pre-electoral list rank if not enough candidates win a sufficient number of preference votes. Under the nonlist systems STV and SNTV, several aspects may differ from the OLPR case: first, candidate-level characteristics may generally weigh more heavily in the utility function; second, there may typically be fewer candidates per party, since parties run the risk of overnomination; and third, seat allocation may be less vote elastic.

14. The CSU is very strong in the whole state—the major opposition party SPD was able to win only one SMD in 2008 (and none in 2003). We see this CSU dominance as an important feature of our research design: the quality of local opposition, which is difficult to control for, hardly affects electoral outcomes.

85/13 and 94/13), as well as own inquiries by Bavarian Broadcasting. The list included 19 current MPs of the CSU (and one from the opposition) who had—at some point during 2013—employed relatives of first degree under the transitional rule. An additional five sitting CSU MPs (and seven from the opposition) had hired relatives of second or third degree. Based on this list, in total 26% of CSU incumbent MPs were implicated in the scandal.

The Bavarian Broadcasting list is both comprehensive and authoritative, and we therefore use it as the basis for a broad definition of our treatment group, aiming at a conservative estimate of treatment effects (see table A1 in the appendix, available online, for details on the implicated MPs). Although this list was picked up by other media and broadly discussed in the public, not all politicians included can be expected to have suffered the same drop in perceived integrity. Therefore, we also develop a continuous treatment indicator as a robustness check. Since the literature on political accountability stresses the importance of information for electoral sanctioning of misconduct, we base this measure on media reports (see Eggers 2014; Larcinese and Sircar 2014). More specifically, we searched for articles mentioning the affair and an MP's name in all regional and local media available through the press databases Factiva and the German-language WISO Press. The indicator reflects the average number of scandal-related articles across 14 Bavarian newspapers.<sup>15</sup>

The subsequent analysis focuses on the estimation of treatment effects for CSU MPs only. The number of cases from the other parties is very small, and while almost all affected CSU MPs represented an SMD when the scandal broke, none of the affected opposition candidates held an SMD mandate. From the 24 CSU MPs implicated in the scandal, 14 were rerunning for the 2013 state parliament election. Of the remaining 10 (in what follows, *stepped-down* candidates) eight had announced their withdrawal

from state politics already in 2012, before their family employment was publicly debated. Two of the involved MPs resigned after public criticism following their implication in the scandal. It is important to note that party lists had been fixed by and large already in 2012. Thus, party internal decisions concerning rerunning candidates and party list positions are exogenous to the scandal, with the exception of those two strategic resignations.

## BAVARIAN ELECTORAL SYSTEM

In estimating effects of the scandal, we exploit peculiarities of the Bavarian electoral system that allow us to compare the electoral consequences of misconduct across SMD and OLPR settings. The electoral system used in Bavaria is an example of a mixed-member proportional system, but it is unusual as it uses open lists in addition to the SMD tier (Massicotte 2011). Ninety local SMD districts (*Stimmkreise*) are clustered in seven regional districts (*Wahlkreise*).

Figure 1 illustrates some basic features of the electoral system. Citizens cast two votes: with their first vote, they choose the district candidate of one party in an SMD election; with their second vote, they choose one specific candidate from one of the open party lists within the seven regional districts. Note that every SMD candidate also competes in the OLPR tier, while there can be candidates in the OLPR tier who do not stand in any SMD. Importantly, SMD candidates do not appear on the open list in the local district where they run as a SMD candidate. Using a fictitious example for one party, figure 1 exemplifies the district-level differences in list appearance. Shown are four local SMDs: two SMDs of regional District A and two of regional District B. At the regional level, there is a list with a pre-electoral ranking decided upon by a regional party conference. The list comprises all candidates who stand for the party in the SMDs of that region plus all list-only candidates from this region (here, e.g., *List-only candidate a1* on list position 4 in Region A). The appearance of the party list now differs in one regard across constituencies within a region: SMD-candidates are dropped from the list in those constituencies where they compete for the SMD seat. For example, *SMD candidate A1* is generally on the OLPR list in Region A, but he does not appear on the regional list for all voters in his local SMD A1.

Interparty seat allocation takes place at the regional level (with a state-wide 5% threshold). Importantly, seats are distributed between parties on the basis of the sum of first and second votes. Therefore, neither SMD nor OLPR votes for candidates with little chances of winning will be wasted with regard to interparty seat allocation. Seats are first assigned to successful local district candidates. Any remain-

15. We collected data for measuring the continuous indicator for MPs with a binary treatment indicator of one. The continuous indicator is zero by definition for the other cases. We looked for articles published between April 1 and election day. Since the number of articles may be partly driven by a resignation as such, the search algorithm excluded hits mentioning MP name and "resignation" close by. Before taking the mean to construct the final measure, the counts were divided by the newspaper-specific standard deviation to obtain a comparable scale. We eventually excluded counts from three newspapers, since their paper-specific scores correlated at less than  $r = .75$  with the initial sum across all papers. The 14 newspapers come from different parts of Bavaria and include *Bayerische Rundschau*, *Coburger Tageblatt*, *Die Kitzinger*, *Frankenpost*, *Fränkischer Tag*, *Main-Post*, *Münchner Abendzeitung*, *Neue Presse (Coburg)*, *Nürnberger Nachrichten*, *Nürnberger Zeitung*, *Passauer Neue Presse*, *Saale-Zeitung*, *Süddeutsche Zeitung*, and *Süddeutsche Zeitung Online*.

**Voters Have Two Votes in Two Tiers**

*Interparty seat allocation: 180 seats using PR based on the sum of both votes, by region*

		<b>Vote 1: SMD tier</b> <i>allocates SMD seats by plurality</i>	<b>Vote 2: OLPR tier (region)</b> <i>allocates seats within parties based on sum of candidates' SMD and OLPR votes (skipping successful SMD candidates)</i>
<b>Two candidate types:</b>	<b>SMD</b>	SMD candidates compete in one district	SMD candidates compete in all districts of their region except for their local SMD
	<b>List-only</b>	List-only candidates do not compete	List-only candidates compete in all districts of their region

**Regional OLPR Ballots Differ at District Level (Local SMD Candidate Is Left Out)**

<b>Region</b>	<b>Regional ballot of party P</b> (with pre-electoral ballot position) <i>(lists of other parties are competing)</i>	<b>Local SMD</b>	<b>SMD candidate of party P</b>  <i>(candidates of other parties are competing)</i>	<b>Appearance of party P's ballot in SMD</b>
A	1. SMD candidate A1 2. SMD candidate A2 3. SMD candidate A3 4. List-only candidate a1 5. ...	AI	SMD candidate A1	1. SMD candidate A2 2. SMD candidate A3 3. List-only candidate a1 4. ...
		All	SMD candidate A2	1. SMD candidate A1 2. SMD candidate A3 3. List-only candidate a1 4. ...
		...	...	...
B	1. List-only candidate b1 2. SMD candidate B1 3. SMD candidate B2 4. SMD candidate B3 5. ...	BI	SMD candidate B1	1. List-only candidate b1 2. SMD candidate B2 3. SMD candidate B3 4. ...
		BII	SMD candidate B2	1. List-only candidate b1 2. SMD candidate B1 3. SMD candidate B3 4. ...
		...	...	...

Figure 1. Key features of Bavarian electoral system

ing seats are given to candidates with the largest sum of first and second votes within regions. Note that the pre-electoral list position is irrelevant at the seat allocation stage; being ranked near the top is only helpful for attracting OLPR votes at the electoral stage.

To estimate our effects of interest, we draw on three measures of electoral performance as dependent variables: (a) the first vote at the level of the local districts, which reflects the combined choice of party and district candidate; (b) the difference between first and second votes for the party, also at the district level, which allows us to compare

how the party fares among the same voters with a single candidate as opposed to a slate of individuals not including the respective SMD candidate; and (c) a candidate's personal share of the party's second votes in the region, which represents individual performance among citizens who selected the candidate's party with the second vote.<sup>16</sup> The

16. Since SMD candidates do not appear on the OLPR list in their local district, we adjust the denominator of the share variable for SMD candidates by subtracting the number of CSU second votes in their respective district. Put differently, we calculate a candidate's share of those second votes she could possibly have won.

following section explains how we identify and estimate party and candidate integrity effects from these data.<sup>17</sup>

**RESEARCH DESIGN**

Our baseline specification uses a binary treatment indicator that marks implicated candidates (as described above), respectively, districts represented by such an MP in 2013 ( $S = 1$ ). To estimate the effect of scandal implication at the level of local districts  $d$ ,  $d \in \{1, \dots, n\}$ , for the legislative election in year  $t$ , we use a difference-in-differences design with CSU first vote shares as the dependent variable. This approach rests on the core assumption that the counterfactual change in vote shares of scandal districts is on aggregate identical to that in nonscandal districts, conditional on covariates  $X$ . We assess the plausibility of this assumption with placebo estimates for the pretreatment electoral period (Lechner 2010). As reported in table A2 in the appendix, parallel trends are overall plausible for the case at hand: placebo effects for the pretreatment period (i.e., the change in vote shares between 2003 and 2008) are insignificant and substantively small, especially when allowing for a separate regional trend in northern Bavaria.<sup>18</sup> Additionally, as table A3 in the appendix indicates, “scandal” and “non-scandal” districts are balanced with respect to most, though not all, observable covariates in the 2013 cross section. As potential time-varying confounders could therefore bias our estimates, we additionally control for observable changes in both district and candidate quality.

Following the Rubin Causal Model (Rubin 1974), our estimate of the average treatment effect on the treated (ATT) thus equals  $\beta = E(Y_{d,t}^1 - Y_{d,t-1}^0 | S = 1, X_{d,t}) - E(Y_{d,t}^0 - Y_{d,t-1}^0 | S = 0, X_{d,t})$ . We estimate this ATT with fixed effects regressions of the form

$$Y_{dt} = \alpha_t + \beta S_{d,t} + \gamma_d + X_{d,t}\delta + \varepsilon_{d,t}, \tag{8}$$

where  $\alpha_t$  constitutes a time fixed effect,  $S_{d,t}$  the treatment

17. We gather information on party and candidate votes from the office of the Bavarian Elections Administrator (BEA), which also provides covariates for constituencies and candidates. Additional data on MPs and candidates come from the Bavarian parliament and “Haus der Bayerischen Geschichte.” When analyzing over time, we use vote shares for  $t - 1$  national on the district boundaries in  $t$ , published by the BEA for each election  $t$ .

18. As the northern part of Bavaria is historically and culturally different, and especially as the 2008 prime minister of Bavaria was the first (and only) from northern Bavaria since the early 1960s, a special electoral bonus for the CSU is expected in northern Bavaria in 2008. This could potentially be a problem, as districts where MPs stepped down prior to the revelation of the scandal are clustering in northern Bavaria (see fig. A1 in the online appendix). We therefore allow for separate regional trends in these two regions.

indicator,  $\gamma_d$  district fixed effects,  $X_{d,t}$  a vector of time varying controls, and  $\varepsilon_{d,t}$  an idiosyncratic error term. The specification draws on elections in  $t = \{2008; 2013\}$ . Here  $X_{d,t}$  includes socioeconomic variables capturing changes in the demographic and economic situation of constituencies,<sup>19</sup> candidate-specific variables capturing the quality of candidates,<sup>20</sup> and potential constituency-level confounders.<sup>21</sup> Table A3 in the appendix reports summary statistics for these controls.

Importantly, we discern districts where the involved MP has stepped down prior to the 2013 elections from those where the involved MP decided to run again. This allows us to differentiate effects of a drop in (perceived) party and candidate integrity (for districts where scandal MPs run) from effects on party integrity alone (for districts where such MPs stepped down).<sup>22</sup>

To substantiate our results, we develop the “difference-in-tiers” approach as a specific strategy for assessing the person-specific impact of the scandal. We assume that party factors affect party choice equally in both tiers. The quality of the district candidate influences only the decision regarding the first vote, however. We can thus estimate the ATT for the candidate integrity effect of the scandal via the difference of CSU first vote share  $Y_{FV,d}$  (SMD tier) and CSU second vote share  $Y_{SV,d}$  (OLPR tier) in districts  $d$  with

$$\Delta Y_d = Y_{FV,d} - Y_{SV,d} = \beta(S_{FV,d} - S_{SV,d}) + (X_{FV,d} - X_{SV,d})\delta + (\varepsilon_{FV,d} - \varepsilon_{SV,d}). \tag{9}$$

If the party integrity effect of the scandal is constant over tiers,  $\beta$  identifies the candidate integrity effect. The covariate vector  $X$  includes observable factors that influence the voting decision differently at both levels. We control especially

19. These are population density, population influx, immigrant share, employed persons subject to social insurance contributions, housing constructions, agricultural businesses, per capita communal tax revenue, and per capita communal debt. All socioeconomic control variables for the 2013 election are 2012 figures. Missing values were replaced with the closest available figures.

20. These are dummies for incumbency status, government membership, leading party functions (president, vice-president, parliamentary party group leader, secretary general), regional party leadership, membership in local interests committee (Committees for Petitions and Complaints; for Nutrition, Agriculture, and Forestry; for Economy, Infrastructure, Mobility, and Technology) and the number of legislative periods.

21. These are dummies capturing effects for the 2013 Danube flood (which occurred between the scandal and the election), for local competition by the leader of an opposition party, and for scandal involvement by candidates of other parties.

22. These party integrity effects have to be understood relative to the potential drop of party integrity in districts without the scandal, as SUTVA might not hold: the scandal potentially affects CSU integrity in all of Bavaria.



for candidate quality and list quality (i.e., regional electoral district).<sup>23</sup> Note that all unobserved factors that affect party choice to a similar extent over both tiers drop out of this equation. This estimation strategy assumes that we have identified all unobservable confounders that affect the difference in party choice. This assumption is supported by placebo estimates in appendix table A2.

Finally, we estimate the effect of the scandal on CSU candidate vote shares in the OLPR tier by analyzing the choice of the subsample of voters opting for the CSU with their second vote. The dependent variable is the intraparty vote share of 164 CSU candidates in seven regional electoral districts. Voters select their preferred candidate  $c$  within the list. A first indication for these effects is given by a simple regression framework where

$$Y_c = \beta S_c + \mathbf{X}_c \delta + \varepsilon_c. \quad (10)$$

Identification requires that average counterfactual outcomes for scandal candidates equal those of nonaffected competitors,  $E(Y_{c,t}^0 | S = 1, \mathbf{X}_c) = E(Y_{c,t}^0 | S = 0, \mathbf{X}_c)$ , controlling for covariates  $\mathbf{X}_c$ . Using controls is necessary, since implicated candidates are of better average quality than their nonaffected colleagues (e.g., in terms of cabinet/committee membership, see table A4 in the appendix). While this should in principle bias our estimates downward, we substantiate our identification strategy with a matching estimator: The ATT will then be computed as a difference in outcomes of treatment and control candidates with weight  $w_c$ , where treatment and control observations are selected from respective groups and control units are weighted with  $h_{c,j}$  (Gangl and DiPrete 2006):  $ATT_{\text{matching}} = \sum_{c|S=1} w_c \times (Y_c^1 - \sum_{j|S=0} h_{c,j} \times Y_j^0)$ . This approach is less dependent on modeling assumptions and guarantees common support of treatment and control units (Sekhon 2010). We apply coarsened exact matching (CEM), developed by Iacus, King, and Porro (2012), since we have ex ante knowledge on relevant categories of covariates (particularly pre-electoral list position).<sup>24</sup> After obtaining weights, we apply the regression model (10).

23. Controls include, next to dummies for the regional list, those of FN 20 and FN 21. Additionally, we include a variable capturing district-level indifference between CSU and other parties. It reflects the district-level average of  $W_{ip}(1 - W_{ip})$  from eq. (3) and is based on a conditional logit model that predicts pretreatment state-level vote intention (in summer 2010) with party identification, left-right distance, and demographics.

24. Variables additionally used for matching are dummies for district incumbency, incumbency since 2000, number of legislative periods, being local district candidate, cabinet and leading party functions, regional party leadership, local interests committee, gender, title, cabinet position, the seven regional party lists, and age as continuous variable (coarsened to 20–29, 30–44, 45–59, 60–81).

## EMPIRICAL RESULTS

### Results for CSU first vote shares (local district level)

Model 1 of table 1 reports the ATT for districts with CSU MPs implicated in the scandal in 2013.<sup>25</sup> CSU first vote shares drop by 2.68 percentage points in these districts (significant at the 5% level). This estimate is robust to measurement with the continuous treatment indicator (model 2). As expected by theory, our estimate for the scandal-related impact on the first vote, which reflects citizens' choice between fixed party-candidate combinations, is negative, though moderate. Note that this estimated vote loss can be the result of two different mechanisms. Obviously, if "affair MPs" run again, this usually implies that the (perceived) personal integrity of the CSU candidate is lower than at the previous election. In addition, if there are spill-over effects from individual to party reputation, the perceived integrity of the party may also have suffered more in those districts than elsewhere as a consequence of the scandal being more visible. As we are primarily interested in direct candidate-level accountability, models 3 and 4 aim at distinguishing party and candidate effects.

Models 3 and 4 differentiate between districts where the implicated MP has stepped down (retired or resigned) and those where the "affair MP" decided to run again. Effects for running "affair MPs" are negative and barely miss the 5% significance level with control variables (model 3). They are highly significant when measured with the continuous indicator (model 4). Results for districts where implicated MPs stepped down are as well negative, but they are smaller, especially when measured with the continuous indicator. So CSU first vote shares drop, although the "affair MP" was replaced by a "clean" candidate. This hints at the existence of specific party integrity effects in these districts. If responsibility were exclusively attributed to individual candidates, we should not observe this pattern. The effects found for running affair candidates should therefore also reflect a mixture of party and candidate sanctioning.<sup>26</sup>

### Results for the difference in SMD and OLPR votes (local district level)

A solution to isolating the electoral punishment due to the candidate integrity shock lies in comparing the CSU vote

25. We report coefficients for all control variables and results of additional specifications in the appendix. Table 1 (2, 3, 4) corresponds to table A5 (A6, A7, A8).

26. Note that any additional party integrity effect that is constant over treatment and control districts cannot be estimated with the fixed effects design at hand.

Table 1. Effects of Scandal Involvement on the Trend in CSU First Vote Shares

	Dependent Variable: CSU First Vote Shares			
	Binary Treatment (1)	Continuous Treatment (2)	Binary Treatment (3)	Continuous Treatment (4)
“Affair MP” in district 2013	−2.68* (1.13)	−3.36* (1.00)		
Running “affair MP”			−2.75 (1.39)	−4.34* (1.40)
Stepdown “affair MP”			−2.38* (1.12)	−2.34* (.62)
Year 2013	3.05* (.97)	2.64* (.95)	2.97* (.96)	2.73* (.95)
Constant	45.75* (14.51)	50.02* (13.45)	46.40* (14.55)	55.49* (13.44)
Candidate and district controls, regional trends	Yes	Yes	Yes	Yes

Note. Fixed effects regression of 2008–13 CSU first vote shares with robust standard errors, clustered by district, in parentheses. Number of observations = 180. Control group mean 2013 = 46.82. We observe 23 implicated districts, of which 14 are with running “affair MPs” (in one district an “affair MP” stepped down and was succeeded by an also implicated candidate). Control variables included are population density (in 1,000s), share of employed population (subject to social insurance contributions), immigrant share, in-migration (in 1,000s), building completions (in 1,000s), farms (in 1,000s), per capita communal tax (in euros), per capita communal debt (in euros), CSU candidate member of parliament, number of legislative periods of candidate, candidate member of local interests committee, candidate member of government, candidate regional party leader, candidate leading party functionary, opposition party leader in district, implication of opposition candidate, and major damage of 2013 June flood in district. Regressions allow for separate regional trends in northern Bavaria.

\*  $p < .05$ .

share in 2013 within local constituencies, that is, between the SMD and OLPR tier (see table 2). This analysis assumes that party-related factors remain constant over both tiers and cancel out. Differences in first and second votes are thus driven by the quality of the SMD candidate and the quality of the candidates on the OLPR ballot. All models therefore include respective controls. As incumbency is expected to capture important aspects of unobserved candidate quality, we restrict the analysis to incumbents in models 1 and 3 (preferred specification). Models 2 and 4 draw on the full sample. The estimated ATT for the candidate-integrity effect (model 1) lies at  $-2.73$  percentage points, significant at the 5% level. The effect is robust to measurement with the continuous treatment indicator (model 3) and to estimation with the full sample (although in model 2, the  $p$ -value increases to .108).<sup>27</sup>

27. The binary pure candidate effect from model 1 in table 2 appears larger than the estimates of table 1. This is a consequence of the different counterfactual, as our preferred specification for the difference-in-tiers compares to incumbents only.

### Results for candidates' intraparty vote share (regional district level)

We now turn to the analysis of electoral sanctioning in the OLPR tier. Table 3 reports results of a regression of CSU candidates' share of the intraparty regional ballot votes on a (candidate-level) treatment dummy. In this specification, we control for candidates holding one of the first three pre-electoral list positions (using three dummy variables), as a majority of voters choose among the top three (Faas and Schoen 2006). All models reveal negative effects for scandal candidates, significant at the 5% level. The loss due to their involvement in the affair amounts to approximately 3.61 percentage points as estimated by model 1 of table 3. This effect holds when including additional controls for candidate quality and OLPR district (model 2), where it is estimated to be slightly stronger.<sup>28</sup> The result is robust to measurement with the continuous indicator in otherwise identical specifications (models 3 and 4). Together these estimates indicate

28. OLPR districts differ with regard to ballot length and placement practice. Regional party conventions decide how many candidates to nominate on their respective lists and where to place the SMD candidates (blocked at the top/blocked at the bottom/unblocked).

Table 2. Effects of Scandal Involvement on the 2013 Differences of CSU First Vote and Second Vote Share

	Dependent Variable = Difference-in-Tiers			
	Binary Treatment (1)	Binary Treatment (2)	Continuous Treatment (3)	Continuous Treatment (4)
Running "affair MP"	-2.73*	-1.88	-3.14*	-3.01*
	(1.34)	(1.15)	(.76)	(.81)
Constant	4.17	-3.57	3.68	-3.71
	(6.66)	(5.23)	(6.64)	(5.18)
Controls for candidate quality and indifference	Yes	Yes	Yes	Yes
Controls for region	Yes	Yes	Yes	Yes
Number of observations	59	90	59	90

Note. Regression of the 2013 difference of first vote and second vote share of the CSU at district level (difference-in-tiers), with robust standard errors in parentheses. For models 1 and 3, the sample draws on 2013 incumbents only. We observe 14 districts with running "affair MPs." Control variables for candidate quality include member of local interests committee, cabinet member, regional party leader, leading party functionary, opposition party leader in district, implication of opposition candidate, major damage of 2013 June flood in district, age (in years), dummies for female and academic title, and a measure for district-level aggregate indifference. Regressions also include dummies for the OLPR districts (regions) in Bavaria.

\*  $p < .05$ .

that CSU scandal candidates were clearly punished by the electorate in the OLPR tier.

To caution against extrapolation from an unbalanced sample, we report as well results based on coarsened exact matching (CEM) in table 4 (Iacus et al. 2012). Importantly, in the process of obtaining maximum balance, both some control as well as some treatment observations are discarded in models 1–3. Consequently, the estimate changes to a local ATT valid only for the sample retained. Table 4 reports results of CEM specifications that represent different choices regarding the trade-off between imbalance reduction (largest in model 1) and the number of observations retained (largest in model 4).<sup>29</sup> The local ATTs reveal negative effects for second vote shares of CSU candidates across all models. Effects are significant at the 5% level for three of the models and almost reach the 5% level in model 3 ( $p$ -value = .051). Note that the size of the treatment effects varies with the sample retained. Models 1–3 are not able to match observations at the upper list positions, which explains both the smaller size of the treatment effect and the lower mean outcome in the control group (bottom row of table 4).

### Comparison of effect sizes

In the following, we assess the relative amount of punishment and the likely consequences of this punishment under

pure systems. A first way of assessing the relative amount of punishment in the OLPR and the SMD tiers lies in calculating the share of the effect on control group outcomes: The relative effect size under OLPR amounts to roughly 40% of 2013 control outcomes (bottom row, table 4, models 2–4), the effect size under SMD only to approximately 5% (see note to table 1, model 1). This already indicates a stronger punishment under OLPR.

For evaluating the substantive impact of the candidate-integrity effect on re-election prospects under OLPR, the most conservative approach consists in comparing it to the threshold of exclusion, that is, the vote share that suffices to win a seat even under the most unfortunate conditions (Lijphart 1994, 25). The CSU won between 9 and 33 seats in the seven regional districts. In a pure OLPR system, the intraparty vote that would guarantee a candidate election ranges between 2.9% ( $100\%/(33 + 1)$ , with 33 seats to be allocated) and just above 10% (with 9 seats to be allocated). In practice, many seats can be won with much lower shares. If we compare the differences in intraparty vote share between the hypothetical (due to the mixed system) last winners and first losers from the seven regional CSU lists, they fall within a range of 1.15 (Niederbayern, 11 seats) to 0.005 (Oberbayern, 33 seats) percentage points. So even the more conservative estimate of a scandal-related loss of slightly more than 1 percentage point (as suggested by the matching-based results) would strongly affect re-election prospects of the individual MP. The electoral sanctioning we observe under OLPR would be consequential in a pure

29. Table A1 in the appendix indicates which scandal candidates are retained in the matched sample.

Table 3. Impact of Affair on Vote Shares of Candidates within CSU Party Lists: Regression Solution

	Dependent Variable: Intraparty Vote Share			
	Binary Treatment (1)	Binary Treatment (2)	Continuous Treatment (3)	Continuous Treatment (4)
Running "affair candidate"	-3.61* (1.77)	-4.69* (2.13)	-4.13* (1.94)	-5.47* (2.61)
First ballot position	42.43* (5.65)	31.76* (4.56)	43.02* (5.61)	31.32* (4.58)
Second ballot position	12.64* (4.27)	8.23* (3.19)	13.28* (4.29)	8.42* (3.05)
Third ballot position	2.12* (.61)	.15 (1.17)	2.79* (.96)	.63 (1.16)
Constant	2.23* (.21)	6.00 (5.01)	2.13* (.20)	5.72 (4.82)
Controls for ballot and candidate quality and OLPR district	No	Yes	No	Yes

Note. Regression with robust standard errors on 2013 second vote shares of CSU candidates within their respective party list. Number of observations = 164. We observe 14 running "affair candidates." Controls (used where indicated) include dummies for first, second, and third list position, absolute list position, length of list, dummies for candidates being member of local interests committee, cabinet member, regional party leader, leading party functionary, CSU frontrunner, district incumbent, being district candidate, having academic titles, being female, being incumbent since 2000, the number of legislative periods, and age in years, as well as dummies for the seven OLPR ballots (electoral districts) of Bavaria.

\*  $p < .05$ .

system.<sup>30</sup> Contrast this with the amount of punishment for individual misbehavior we found for the SMD case. The effect sizes of around 3 percentage points do not appear tiny, but in our case they do not change electoral fortunes at all. Given the dominance of the CSU as a party in 2013, an effect of this size would not have overturned a single seat. The average CSU lead in control districts is at about 25 percentage points. And overall only 4 out of the 90 SMD seats had a margin of victory below 5 percentage points in 2013 (in 2008: 5 seats). We can, of course, think about political contexts where party competition is not as lopsided and smaller effects would be more consequential under SMD. Holding individual representatives accountable under OLPR, however, does not depend on such favorable background conditions.

### Robustness checks

In addition to the results presented above, we performed a series of robustness tests, explicated in detail in the appendix (10ff.). We show results of tables 1–4 with inclusion

30. It is correct that the election of top-ranked candidates under OLPR would hardly be at risk after a vote loss of a few percentage points. Note, however, that these figures are based on an estimate of the average treatment effect. From eqs. (6) and (7), it follows that top-ranked candidates are sanctioned to a different, presumably larger, degree.

and exclusion of control variables (table A5–A8). We also perform a "leave-one-out-analysis" for the district and the OLPR level to check whether effects depend on single observations (see tables A9 and A14). This is not the case for the SMD results; for OLPR, one candidate seems to be influential in some specifications, but this person is not included in the matching models 1–3. As unobserved candidate quality might bias our results of the difference-in-tiers and OLPR effect, we apply an additional difference-in-differences approach and get similar results (tables A10 and A13). For the OLPR effect, we replicate our analysis with a logistic regression following the approach of Papke and Wooldridge (1996) and with candidate ranking as alternative dependent variable (tables A11 and A12). Overall, this leads us to conclude that the 2013 "Employed Relatives Scandal" in Bavaria indeed resulted in a clear electoral response: we observe moderate punishment in the SMD tier and substantial punishment in the OLPR tier.

### Examining microfoundations using survey data

The aggregate data analyzed so far are particularly suitable for applying causal inference techniques, but they provide only indirect evidence about voters' considerations. To substantiate the microfoundations spelled out in the theoretical framework, we make use of a survey that was conducted specifically for the 2013 Bavarian state-level election

Table 4. Impact of Affair on Vote Shares of Candidates within CSU Party Lists: CEM Matching Solution

	Dependent Variable: Intraparty Vote Share			
	Binary Treatment (1)	Binary Treatment (2)	Binary Treatment (3)	Binary Treatment (4)
Running “affair candidate”	-1.29*	-1.26*	-1.10	-4.03*
	(.41)	(.48)	(.54)	(1.88)
Constant	2.87*	2.03	5.86*	4.09*
	(1.53)	(1.97)	(1.59)	(1.42)
Matching variables included as controls	Yes	Yes	Yes	Yes
Number of observations	16	34	45	136
Matched treated	4	7	9	14
Matched controls	12	27	36	122
Reduction in pre- to post-matching				
L1 imbalance	.79	.26	.25	.28
Control group mean (with CEM weights)	1.68	2.83	2.98	9.99

Note. Average treatment effect on the treated for second vote shares within CSU regional lists for running “affair candidates” with weights obtained by coarsened exact matching (standard errors in parentheses). Control variables adjust for remaining imbalance (L1 statistic) in the sample and improve efficiency. For model 1, candidates are matched by ballot position (coarsened to 1, 2, 3, 4–6, 7–10, else), dummies for government function, being frontrunner, regional party leader, party functionary, academic title, gender, being SMD candidate, dummies for the seven regional ballots and age (coarsened to 20–29, 30–44, 45–59, 60–80). For model 2, candidates are matched by ballot position (coarsened to 1, 2, 3–10, else), dummies for government function, title, gender, regional ballot dummies and age (coarsened to 20–29, 30–44, 45–59, 60–80). Model 3 differs from model 2 by coarsening ballot position broader, only by 1, 2, else. Model 4 only matches by ballot position (coarsened to 1, 2, 3, 4–6, 7–10, else).

\*  $p < .05$ .

(within the Making Electoral Democracy Work project). The aim of the survey data analysis lies in corroborating three arguments: that voters trade off “party closeness” (broadly understood) and integrity considerations, that the “Employed Relatives Scandal” was particularly important to indifferent voters in local districts with implicated candidates, and that CSU voters considered scandal involvement for their candidate choice in the OLPR tier.

To do so, we start with estimating a conditional logit model for party choice using party closeness factors only.<sup>31</sup> The model is used to predict a citizen’s probability to vote for the CSU, which yields a measure of “general” proximity to the party. As a binary dependent variable, we consider a respondent’s self-reported assessment that the “relatives affair in the state-level parliament” was considered “very important” for the “voting decisions” (in plural form). We look at this indicator separately for voters in districts with and without rerunning involved candidates. Based on the

31. These are party identification, left-right distance, ascribed problem-solving competence, evaluation of government economic policy, and some demographics interacted with party indicators. As dependent variable, we use second vote party choice, but results are virtually the same when using the first vote.

theoretical model, we expect that the scandal has particularly mattered to voters who are offered an implicated SMD candidate and at the same time are undecided between the CSU and other parties based on party closeness alone.<sup>32</sup>

Figure 2 examines this relationship for two samples, in the left panel for all survey respondents (who report having voted) and in the right panel for the subset having a strong political interest (scoring higher than 7 on a 0–10 scale). Nonparametric smoothing lines (based on lowess) are shown in black for respondents from districts with a running “affair MP” and in gray (and dashed) for respondents from all districts whose outgoing MP was not involved. To start with, there is a general trend that, with increasing closeness to the CSU, respondents are less inclined to state the affair was very important to their choice. This makes sense, because respondents are unlikely to answer the survey question directly on the basis of counterfactual reasoning about their vote in absence of the scandal. Put differently, many people rationalize that the affair mattered to their choices, even if they had a small probability of choosing CSU regardless of the scandal.

32. Note that in this context we do not aim at separating candidate integrity from any party integrity effects due to spillage.

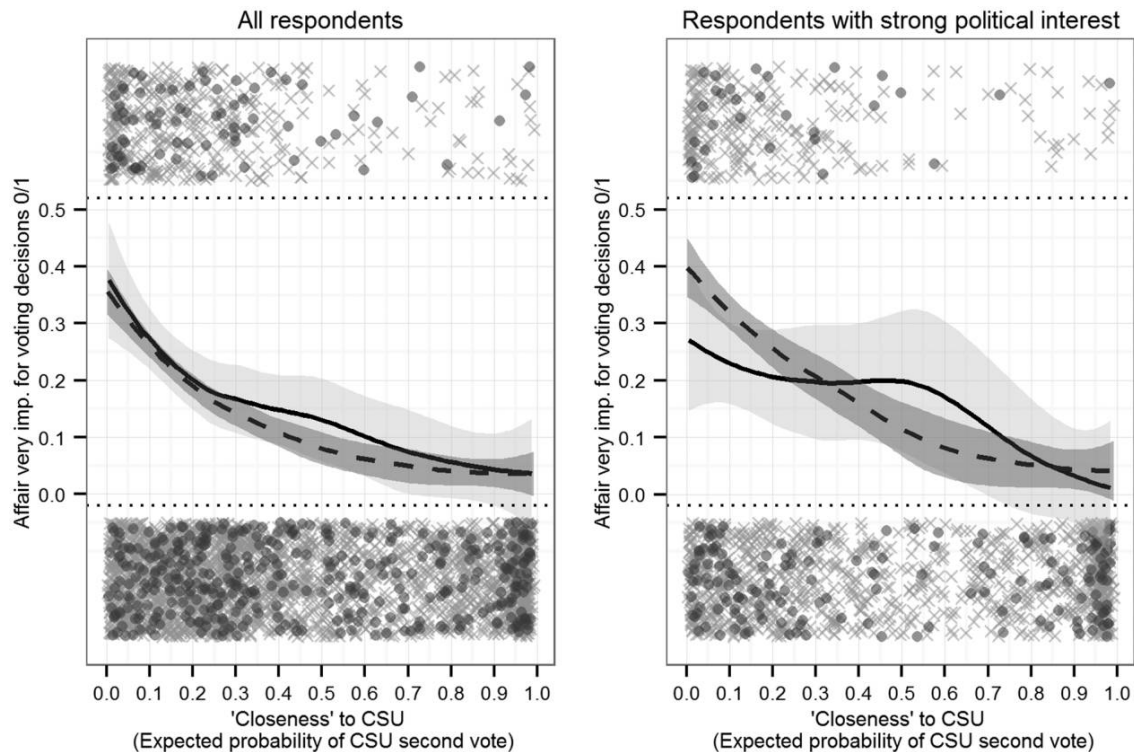


Figure 2. Party closeness and reported importance of scandal for voting decisions. Left panel is based on survey respondents who voted; right panel is for subset with strong political interest (scoring higher than 7 on a 0–10 scale). Lowess-smooths (bandwidth of  $N = 100$ ) in black (solid line) for respondents from districts with a running “affair MP” and in gray (dashed line) for those from districts whose outgoing MP was not implicated. Bands show 95% confidence intervals. Strips indicate the distribution of the dependent variable, with zeros at bottom and ones at top. Crosses represent untreated ( $N = 2,592/N = 1,262$ ), circles treated cases ( $N = 453/N = 194$ ).

The key insight, however, is that we can additionally detect the expected curvilinear effect as visible in the difference between the lines for citizens from “treated” and “untreated” districts. Since those respondents who are indifferent on the basis of party closeness (party identification, ideology, etc.) indeed more frequently report that the scandal mattered to their choices, the two lines start to diverge as they approach the midpoint of the  $x$ -axis and then converge again toward the end of the scale. In addition, the right panel points out the important role of information about the scandal. The curvilinear effect is more pronounced when considering only those people who have a strong political interest, who are more likely to have received detailed information about the candidates. These results have to be interpreted cautiously, as there is considerable uncertainty in the estimates for the treatment group (due to a rather small number of cases, particularly in the right panel). Still, they provide additional evidence—based on citizens’ self-reported vote choice motivations—on the trade-off between party closeness and integrity considerations for voters’ utility.

Using a similar approach, analysis of the survey data also provides further support to the argument that CSU voters decided against implicated candidates in the OLPR tier because of their involvement. For this purpose, we consider those respondents supporting the CSU in both tiers who voted for any of the 45 candidates retained in the matching sample from model 3 in table 4.<sup>33</sup> Unsurprisingly, none of the 10 cases opting for an “affair candidate” reports that the scandal was “important” or “very important” for their voting decisions. However, 16 out of 86 respondents (19%) supporting nonimplicated candidates do so. This is remarkable when keeping in mind that at the party level they chose both times CSU, which should heavily decrease the probability to report strong relevance of the scandal in the first place. The number of observations is, of course, small, but the results lend additional credibility to the argument that the matching approach used above creates a sample of similar

33. Information on candidate choice is available for 84% of respondents who chose CSU with their second vote (from the six regional districts with any implicated candidates on the list).

candidates who mainly differ in terms of their scandal involvement and that CSU voters sanctioned candidates exactly for such involvement.

## DISCUSSION

Despite its relevance for the quality of representation on the extent to which voters take candidate integrity into account in different electoral contexts has not been analyzed systematically. We introduced a bottom-up theoretical framework for predicting differences in electoral sanctioning of individual misbehavior under single-member district systems and open-list PR. If the candidate/party vote is fused, as under SMD, we argue that theoretically candidate integrity can only be decisive for voters close to indifference on party terms, which strongly dampens the prospects for aggregate punishment of individual misbehavior. Only for unusual settings, where party-level considerations are of little relevance for voters or where all parties are alike, are prospects for candidate accountability similar in both electoral systems. As soon as party considerations matter, partisanship and the weights of party versus candidate integrity in the utility function determine the degree to which we expect OLPR to outperform SMD electoral systems in generating candidate accountability. This is in line with Eggers (2014), who finds that the degree of punishment in the British SMD system depends on the partisan stakes in a district race. It also explains why the links between individual behavior and electoral performance appear to be smaller in the more party-centered system of the United Kingdom compared to the United States (Basinger 2013; Dimock and Jacobson 1995; Eggers 2014; Pattie and Johnston 2012; Vivyan et al. 2012). In our case, Bavaria, with a polarized party system, party-level factors weigh heavily, so prospects for individual accountability under SMD are low.

Thus, our main theoretical insight is that shocks to candidate integrity lead to more consequential punishment under OLPR than under SMD. The 2013 Bavarian parliamentary employment scandal serves as such a shock, implicating a large number of MPs at the same time, five months before elections, in a public debate about their use of tax payers' money. Due to the peculiarities of the Bavarian electoral system, a mixed-member system with SMD and OLPR, we can identify three main findings. First, and most important and in line with our predictions, we find electoral punishment effects in both tiers, but those from the OLPR tier are of a size that is much more consequential. Second, as expected by theory, micro-level analysis indicates that the observed drop in first votes stems from voters close to indifference between parties. Finally, we can identify larger scandal effects for more strongly implicated MPs. Taken together, we can therefore

conclude that, given a certain type of misbehavior and sufficient information, voters more easily hold individual representatives accountable under OLPR than SMD.

In the introduction, we discussed that the increase in the number of district-level candidates under OLPR compared to SMD may imply a reduction of information about these candidates. We cannot say how relevant (in absolute terms) this potentially accountability-threatening mechanism was in the case we studied, as the overall information level is fixed in our research design. At least this mechanism was not strong enough to wash out the effect from decoupling party and candidate vote. Future work should investigate the implications of variation in information levels across electoral systems, for instance, by extending the theoretical model to allow for uncertainty over candidate integrity or by incorporating variables capturing variation in candidate information into the empirical model.

Additionally, we suggest that further research should more closely track the individual-level mechanisms that bring about accountability. Our analysis proposes that sanctioning results from different subsets of the population in each of the two electoral systems. As mentioned above, it is indifferent or swing voters who punish in the SMD tier by voting for another party. Under OLPR, on the other hand, sanctioning is due to those who do vote for the party, and particularly by those not following any ballot placement cue in selecting the specific candidate. This means accountability would be induced by well-informed core party supporters under OLPR.

As we used a mixed-member system to compare MPs' electoral performance across SMD and OLPR, to what extent can we generalize our findings to pure systems? Our analysis constitutes an innovative application of a research design that previously has been used to explain differences in the number of parties and in MP behavior under SMD and closed list PR (e.g., Moser and Scheiner 2012; Stratmann and Baur 2002). This literature has also asked whether contamination between tiers biases results away from what one would expect under pure systems (Ferrara, Herron, and Nishikawa 2005; Stoffel 2014). Contamination could affect our findings, too, because almost all implicated MPs are SMD-based candidates and most are from fairly safe districts. One could argue that voters use their OLPR vote differently if they know that some candidates on the list are almost certainly elected in the SMD tier. We have three responses to concerns about contamination. First, the homogeneity of candidates' background is an asset for our research design, since we do not end up having two quite different types of incumbents (SMD and list based). In addition, in Bavaria, all SMD candidates must also be list candidates, so we do not face the problem of having strong variation in

candidacy types (SMD only, list only, dual). Second, it is by no means clear that an average voter would take for granted that “affair MPs” would not lose their SMD seats in the 2013 election. Third, irrespective of immediate electoral relevance, individual performance in the OLPR-tier affects intraparty standing and the chances of receiving political rewards (Crisp et al. 2013; Folke, Persson, and Rickne 2013). In this sense, a vote for a specific candidate in the OLPR tier is never wasted.

The findings of this article have wider implications for our understanding of electoral systems and accountability. Studies on the link between electoral systems and corruption vary in their assessment of OLPR systems. Some authors see intraparty competition as the root of corruption since personal campaigns require financial resources that are not always obtained by legal means (Chang 2005; Chang and Golden 2007). Others are more optimistic, recognizing that intraparty choice encourages responsible behavior (Kselman 2011; Persson, Tabellini, and Trebbi 2003). Our results are in line with the latter view, but they cannot directly say anything about the former. There are several different mechanisms along the long causal chain between the electoral system and corruption outcomes, and a certain electoral system may have counterbalancing effects (Persson et al. 2003). We have established that OLPR allows for easier sanctioning, given a certain type of misconduct and sufficient information about it. OLPR may have undesirable side effects, but these likely also differ across contexts.

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