

The Importance of Personal Vote Intentions for the Responsiveness of Legislators: A Field Experiment

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Abstract

Individual legislators can be important agents of political representation. However, this is contingent upon their responsiveness to constituency requests. To study this topic, an increasing number of studies use field experiments in which the researcher sends a standardized email to legislators on behalf of a constituent. In this paper, we report the results of an original field experiment of this genre with the members of the German *Bundestag*. Supplementing previous research, we explore whether constituency requests in which voters mention a personal vote intention (rather than a partisan vote intention) increase legislators' responsiveness, and how this treatment relates to electoral system's incentives. We find that legislators treated with a personal vote intention were more likely to respond (67%) and respond faster than those treated with a partisan vote intention (59%). However, we also show that the treatment effect is moderated by electoral system incentives: it is larger for nominally-elected legislators than for those elected via a party list. Our results suggest that electoral system's incentives matter for legislators' responsiveness only when constituents explicitly signals an intention to cast a personal vote.

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Introduction

To what degree are individual legislators responsive to their constituents, and why? Our study addresses this question by focusing on a European party democracy, where parties traditionally function as key mechanisms to connect citizens and the state, and thus help secure responsive governments. However, in European party democracies, electoral dealignment (Dalton 2016; Dalton and Wattenberg 2000) and party cartelization (Katz and Mair 1995; Mair 2013) resulted in decreasing capacities of political parties to fulfil this function. Fewer citizens identify with established parties and use them to channel their concerns, while established parties show diminished interests in mobilizing and involving citizens in meaningful ways. Hence, responsive governments may increasingly require individual legislators to supplement parties and reinforce the interactions between citizens and the state that is essential to democratic governance (Ohberg and Naurin 2016).

This study advances from a growing body of literature that uses field experiments to study the responsiveness of legislators to constituency requests (for a review see Grose 2014; for a meta-analysis see Costa 2017). In these experiments, the researcher sends a standardized email to legislators on behalf of one of their constituents and records the answers (or the absence thereof), as well as their content. These responses are considered as accurate measures of responsiveness because they are based on real behavior. The first studies of this genre were conducted in the US (e.g.,; Broockman 2013; Broockman and Butler 2011; Butler and Nickerson 2011; Dropp and Peskowitz 2012), but were later reproduced in Europe (Alizade et al 2018; De Vries et al 2016; Giger et al 2019; Habel and Birch 2019; Hess et al 2018), Canada (Loewen and McKenzie 2019), as well as in non-Western countries (Chen et al 2016; Gaikwak and Nellis 2016; McClendon 2016). In this paper, we report the results of a field experiment conducted with the members of the German *Bundestag*.

Our contribution to the literature on legislators' responsiveness is twofold. Firstly, we add new insights to the conditions of responsiveness by using a new experimental paradigm. Most of the literature so far focused on legislators' responsiveness to social groups and whether responses are affected by taste-based or statistical discrimination (e.g. Broockman 2013; Butler and Broockman 2011; Gell-Redman et al 2018; Habel and Birch 2019; Kalla et al 2018). Additional studies aimed to explore whether legislators' responsiveness depends on the content of constituency requests, e.g., service or policy requests (Butler et al 2012). In our experiment, we propose a new treatment that focuses on voters' intention, in particular whether they signal an intention to cast a personal or partisan vote. To explore this issue, we randomly divide our population in two groups. Half of the legislators received a demand in which the constituent signals attention to the personal records and qualifications of the legislator, and also the constituent's intention to cast a personal vote for them (*personal vote intention*).² The other half received a request in which the constituent signals attention to the record of the legislators' party and also the constituent's intention to cast a party vote (*partisan vote intention*). We find that those legislators treated with a personal vote intention are more likely to respond (67%) than those treated with a partisan vote intention (59%).

Secondly, this paper shows how electoral systems condition the effect of a signaled vote intention on legislators' responsiveness. We thus also contribute to the literature that studies the direct effects of electoral systems on the responsiveness of legislators, where candidate-centered systems are assumed to facilitate responsiveness (André et al 2014; André et al 2015; Bowler and Farrell 2011; Cain et al 1987; Carey and Shugart 1995; Heitshusen et al 2005; Rudolph and Däubler 2016). Given that the German *Bundestag* is elected under a mixed-member system, we are able to take advantage of the mandate-divide argument, which assumes nominally-elected legislators to react differently to our treatment than party-list legislators (Manow 2015). To our knowledge, we are the first to test whether constituents' vote intention affects legislators' responsiveness, and how this effect

² For our field experiment, we sent email ourselves on behalf of a fictitious (male) constituent. We discuss the ethical implications of this design in more detail in Appendix A.

is conditioned by candidate-centered electoral rules. In this vein, we find that a positive treatment effect of receiving a personal vote intention (compared to a partisan vote intention) only for nominally-elected legislators. This shows that the effect of signaled vote intention is moderated by the type of electoral rule. In other words, we find that electoral system's incentives matter for legislators' responsiveness only when constituents explicitly mentions an intention to cast a personal vote.

Conditions of legislators' responsiveness

Despite all the differences across countries, legislators in European party democracies share a common characteristic: they face manifold demands and command scarce resources. When do they invest resources to directly respond to citizen-initiated requests? A common answer to this question stresses their vote-seeking concerns and resulting efforts to respond to individuals and groups that most affect their re-election prospects. Along this line, the electoral context is a key factor explaining legislators' responsiveness, particularly the marginality of their seat and the 'candidate-centeredness' of the electoral rule.

Theories about the roles of marginality argue that tight electoral competitions incentivize legislators to make extra efforts to respond to voters (Griffin 2006; Kuklinski 1977). The reason is that those facing strong competition are likely to need, more than others, every single vote to secure their re-election, and that responding to any constituent request can be a good strategy to achieve this goal. The field experiments with legislators presented above usually aim at estimating whether personal characteristics of the sender, such as their ethnicity, have an effect on the propensity of the legislators to respond to a standardized email from a constituent (e.g., Broockman and Butler 2011; Habel and Birch 2019). Yet these studies also confirm the importance of marginality: the more uncertain legislators are about their re-election, the more responsive they are to this email

(Broockman and Butler 2011; Butler and Nickerson 2011; De Vries et al 2016; Dropp and Peskowitz 2012; Giger et al 2019).

Theoretical accounts on the electoral sources of legislators' responsiveness furthermore highlight the role of electoral systems, and how they moderate the effects of electoral uncertainty. From this perspective, nominal modes of election in single-member districts render legislators responsive to geographic constituents. The reason is that, under plurality rule, these constituents act as second principals to legislators (i.e., the agents) in addition to parties. In other words, they directly affect their re-election prospects and hold them accountable (Carey 2007). Any positive or negative policy outcome can be directly related to the activities of their district representatives, positively and negatively. In contrast, legislators elected via party lists function as members of teams vis-à-vis national coalitions of voters. They share collective responsibilities which renders their re-election prospects contingent upon voters' evaluation of their parties (Bowler and Farrell 2011; Cain et al 1987; Mayhew 1974). This provides fewer incentives to respond individually to constituency requests since parties remain the key principal to legislators.

Independent of conventional accounts about the roles of marginality and electoral rules determining legislators' responsiveness, some scholars have questioned the direct effects of these factors. Morgenstern and Swindel (2005) for instance show in their comparative analysis that electoral systems' features hardly matter for the local vote and rather stress the importance of voters. This is where the experimental literature about the responsiveness of legislators can make a contribution. In this literature, the researcher can evaluate the effect of constituents' characteristics such their identity, and how this resonates with those of the legislator, as well as the broader electoral context. Our study advances from this and explores the role of constituents' vote intentions, i.e. whether they aim to vote for candidates or parties.

We consider personal vote intention signals in a constituency request to increase the perceived importance for legislators to respond and thus also changes how they allocate their scarce time, in times of electoral dealignment. The electoral map is increasingly complicated. The times of structured electoral markets populated by homogeneous social groups attached to particular parties are gone (Mair et al 2004, Dalton 2016). While legislators running for office still seek to maximize their votes, they now face a different breed of voter with different vote intentions. Scholars emphasized that due to electoral dealignment increasing portions of voters may rest their vote choices on candidates' valence and/or more generally on their personal attributes and records (Bol et al 2016; Eggers 2014; Kam 2009). This also suggests that weaker partisanship among voters might incentivize candidates and legislators to seek personal votes. Advancing from these considerations, we aim to test the following hypothesis.

H₁: Legislators receiving a constituency request that includes a personal vote intention are more responsive than those receiving one that includes a partisan vote intention.

Moreover, in line with the literature on electoral sources of legislators' responsiveness, we expect that legislators are also affected by the incentives that originate from electoral rules. Yet, we also consider that these incentives can be conditioned by citizens' signaled vote intentions (for a similar approach, see Zittel et al 2019). Hence, we envision the possibility that electoral incentives need to get triggered before being translated into a responsive behavior of legislators. For our experiment, we selected the German case because of the country's mixed-member system and the variance it provides with regard to different modes of election. It allows voters to simultaneously elect 299 legislators on the basis of a nominal vote in single-member districts, and an approximately equal number of legislators on the basis of a party list vote in 16 multi-member districts. Proponents of the mandate-divide hypothesis argue that both electoral tiers function like pure electoral systems. Consequently, nominal legislators are assumed to function in a pure plurality mode. They face

particular incentives to seek personal votes compared to their colleagues elected on the basis of a party vote and assumed to function in a pure proportional mode (Klingemann and Wessels 2001; Lancaster and Patterson 1990; Sieberer 2010; Stratmann and Baur 2002; Zittel and Gschwend 2008). Consequently, our second hypothesis is the following:

H₂: The treatment effect of receiving a constituency request that includes a personal vote intention (compared to a partisan vote intention) is greater among nominal legislators than among party-list legislators.

It must be said that the mandate-divide hypothesis is not without critics. The large number of dual candidates in the German system (e.g., about 85% all incumbents in the *Bundestag* competed in both tiers at the 2013 federal election) provides reasons to question the assumed independence of the two tiers of election, and to consider the German mixed-member system as a unique type of system that results in distinct incentives. Specifically, district losers that nevertheless earned a mandate via their party list, continue to keep a close eye on the geographic district they ran in, hoping to win it next time (Ferrara et al 2005; Manow 2015). Following up on this argument, Zittel and Gschwend (2008) show that it is particularly narrow district losers that adopt strategies that are similar to district winners.³ In the final step of the empirical analysis, we consider this possibility. Yet, it is important to note that the existence of a contamination between the two electoral tiers means that our test of the effect of the electoral system on the responsiveness of legislators (H₂) is a hard one. Due to the mutual interdependence of the two tiers, we would expect a broad-based proclivity to be responsive to constituents regardless of the legislator's mode of election.

Research design and experimental setup

³ It is important to note that our analysis only focuses on legislators that ran as dual candidates.

To empirically test our hypotheses, we conducted a field experiment with members of the German *Bundestag* on the eve of the 2013 election campaign (between May and July 2013). Our design involved submitting a randomized stimulus to German legislators on behalf of one of their constituents, and observing their (absence of a) response, as well as how long it took them to respond.⁴

The choice of a field experiment provides several advantages compared to traditional modes of research on political representation (Grose 2014). First, it allows factual observations in light of counterfactuals, and thus to increase the robustness of the causal inference made. Second, it permits focusing on actual behavior instead of survey-based self-reported behavior that can suffer from misperceptions and strategic efforts to misrepresent past efforts and success stories (Bailey and Brady 1998; Gerber and Lewis 2004; Wahlke et al 1962).

Treatment groups

The stimulus that we randomly assigned to legislators consisted of an email sent by a fictitious (male) constituent who explicitly claimed to (1) live in the largest city of the legislator's district, (2) have not voted for them in previous elections, but (3) consider voting for them at the upcoming election. In the 'personal vote intention' group, the sender stressed the legislators' personal track record as a reason why the constituent considers voting for the legislator. He thus voiced an explicit personal vote intention, as he promised a nominal vote in return to the specific political activities and records of an individual incumbent (Cain et al 1987). In the 'partisan vote intention' group, the constituent stressed the record of the legislator's party as a reason for his upcoming vote choice and indicated to cast a party-list vote rather than a nominal vote. Figure 1 documents the literal translation of the entire text of both emails in English.⁵

Figure 1. Emails sent by treatment group

⁴ We stopped recording responses 64 days after sending our email.

⁵ The original German version of the emails can be found in the Appendix B.

<u>Personal vote intention group</u>	<u>Partisan vote intention group</u>
<p>From: [alias]@gmx.de Object: Citizens request: Your work as member of parliament</p> <p>Dear [name of legislator],</p> <p>my name is [alias] and I am from [city in district]. I am sending you this email as a citizen concerned about his future. I normally vote for a different party. However, I recently became aware of you and your work as a member of parliament. I like your competent commitment to the concerns of your voters. I may consider casting my vote for you in the next election.</p> <p>In order to make an informed choice, I would like to ask what you want to do advance in the near future. Can you tell me what is the most important political project that you aim to pursue and according to which you want to be measured in the next election?</p> <p>Many thanks in advance for your answer.</p> <p>Best regards, [alias].</p>	<p>From: [alias]@gmx.de Object: Citizens request: Work of [party] in the parliament</p> <p>Dear [name of legislator],</p> <p>my name is [alias] and I am from [city in district]. I am sending you this email as a citizen concerned about his future. I normally vote for a different party. However, I recently became aware of the [party]’s work in the parliament. I like your party’s competent commitment to the concern of its voters. I may consider casting my vote for the [party] in the next election.</p> <p>In order to make an informed choice, I would like to ask what you want to do advance in the near future. Can you tell me what is the most important political project that you aim to pursue and according to which you want to be measured in the next election?</p> <p>Many thanks in advance for your answer.</p> <p>Best regards, [alias].</p>

Note: Randomized parts are in bold.

Randomization procedure

A serious challenge to the internal validity of this kind of field experiment is the possibility of being exposed by the experimental subjects (i.e., the members of the German *Bundestag*). This might result in selective refusals to answer, in suspicious and defensive responses, and perhaps most importantly, leads to a contamination between randomized groups (SUTVA assumption). To minimize the chances of exposure, we used four different names for the fictitious constituent. We employ four standard German male names (Alexander Müller, Markus Becker, Michael Weber, and Thomas Schmidt) to minimize the impact of racial and gender-related biases that has been found to be recurrent in this type of email correspondence (Butler & Broockman, 2011).

Emails addresses were created according to the aforementioned names in two widely used German free email services (web.de and gmx.net). The emails were also sent in four waves (on May 6, 7, 8, and 9, at 2pm). The time span between these waves was held at a minimum, in order to minimize the effect of unobserved contextual factors. Moreover, we avoided the summer holiday period to minimize the risk of legislators to be out of office. Thus, in fact we implemented a 2 (personal vs. partisan vote intention emails) x 4 (names) x 4 (waves) experimental design. A subsequent analysis (reported in Appendix C) shows that neither the alias- nor the wave-factor have a systematic impact on the probability of legislators' responses. Moreover, we show in Appendix D that our randomization procedure was successful because both the probability to receive one email or the other is similar across pre-treatment covariates. These tests show that the key treatment is balanced across groups of legislators, and across other treatments.

Validity

As legislators are busy people, it is reasonable to assume that if they take the time to respond, they do so in meaningful ways. To validate this assumption, we conducted a qualitative exploration of the content of the responses. The result of this made us conclude that legislators took the emails seriously and provided meaningful answers. Most of the responses we received appear to be written by the legislators or their staff as to specifically react to the request that they faced; some of their answers are even quite long and very detailed. To illustrate this point, in the following, we provide a verbatim quote of a response written by a legislator who received the personal vote intention email (we removed information that could compromise their anonymity):

“As you may know, it is since [year] that I am representing our electoral district [...] as your MP [...]. Because I come from [district] myself, I know that aircraft noise disturbs the quietness, where traffic puts pressure on residents, and what matters concern the public here. For me as a politician it is the most

important thing to keep in touch with citizens, to be rooted and not to have one's head in the clouds – or as we call it “to stay close to the people”. Being in touch with the citizen and being reliable, is my guiding principle. It is important to me to know what the concerns of the people are. This is why I would like to work with all the energy I have for you and our district for the next four years here and in Berlin.

With our district in mind, I think the most urgent tasks are securing jobs and vocational training and ensuring equal access to education regardless of one's social status and cultural background. I would like to get involved in improving the quality of life in our district, particularly I think about the reduction of air and rail noise, developing public transport, and ensuring affordable housing.

As the responsible rapporteur of the [party] parliamentary group in the [committee], I will dedicate myself to the strengthening of [specific policy concerns that we removed for reasons of anonymity] and the support of volunteer work. Furthermore, as a [party representative] I support the demands for decent wages and pensions, a better reconciliation of work and family life, especially through providing more and better childcare facilities and I support the expansion of renewable energies. These are the topics I would like to stand and fight for, here in our district, and at the Bundestag in Berlin.” (Mail No. 965, own translation)

Similarly, the following example illustrates that legislators that received the partisan vote intention email and that chose to respond also appear to have taken great care to write a fitted response:

“Dear Mr. Weber, I am very pleased that the [party] could capture your interest and that you want to get more detailed information about our goals. For more than 150 years the [party] stands for practiced social justice. During the next few years we would like to rebalance our society in order to guarantee fair and decent wages to everyone living in Germany and to allow everyone to freely pursue a promising future. It is also essential to tame the financial markets, which need to accept financial and social responsibility for their actions, not only simply take the profits. We as [party representatives] stick up for a modern society, where the ‘we’ is counting more than the ‘I’. Attached you will find the short and the long version of our current government program. I would be happy, Mr. Weber, to convince you of our concepts and ideas. Should you have any questions or comments, do not hesitate to get in touch with me any time. With best wishes, yours” (Mail No. 1013, own translation)

As a last epistemological issue, we are not concerned with who actually answered the email. We are well aware that a large portion of it probably has been written by staff. However, the behavior of staff members should reflect the motivations of their superiors, that is the members of the *Bundestag*. Also, it is important to note that, by law, all German legislators, regardless of their mode of election, have the same amount of resources to employ staff. This precludes any biases that might result from unobserved differences in legislators' resource bases.

Dependent variables and further controls

According to our hypotheses we are interested in particular conditions under which legislators are more or less responsive to constituency requests. Responsiveness is usually measured as responding or not to such emails. In this paper, we also use another way to operationalize the concept by measuring the time legislators take to respond. Consequently, our analysis is based upon two dependent variables. Our first dependent variable indicates whether legislators responded to our email (coded as '1') or not. Legislators are hard pressed regarding their time and resource commitments. The very fact of responding to our email thus constitutes a valid behavioral indicator on whether legislators are responsive to constituents.

Our second dependent variable focuses on the timing of responses. We count the number of days that it took legislators to respond. In light of scarce resources, this is a further and more fine-grained indicator to explore the relative importance they assign to the task of being responsive to constituents. It is reasonable to think that the more they care about the demand, the quicker they respond to it. Also, from the perspective of the constituents themselves, we can assume that they prefer receiving a response sooner than later. To our knowledge, we are the first to model the time

to respond in an analysis of a field experiment with legislators.⁶ Yet, we believe such a dependent variable should also be featured in other field experiments with legislators.

As H_1 revolves around the treatment effect, we primarily focus on a difference in means to test it. By virtue of the design, the treatment is indeed exogenous. Yet, we also estimate regression models and add control variables, as to improve the efficiency of the test. Testing H_2 requires us to bring in another variable, namely the mode of election of the legislators. Since the mode of election is an observational rather than experimentally-manipulated variable, we need to add control variables to account for causal heterogeneity.

The control variables are the following. First, previous studies suggest paying attention to the consequences of party organizational factors. Some parties are more cohesive and more collectivist than others. This might result in fewer individual efforts to be responsive to constituents. We thus include party fixed effects. These fixed effects also capture national trends regarding the popularity of parties. In 2013, for example, the legislators from the Free Democratic Party (FDP) were in a very different situation than those of the Christian Democratic Union (CDU). The FDP was losing support in the polls, while the CDU maintained a more than solid support base of 40 per cent of respondents. This specific electoral context is likely to have dampened the personal vote seeking concerns of legislators representing the CDU since their party label promised to be an effective vote-getting device in 2013. However, for ethical reasons that we outline in Appendix A, we do not disclose these effects in the paper.

Second, we include the age of the legislators, as age might be associated with possessing less skills and motivations to use new media and to answer emails. We thirdly control for the gender of the legislator as to avoid same-sex bias in their responses.

⁶ It is interesting to note that Putnam (1994) uses a similar indicator in his seminal study of civic culture in Italy.

Results

In our experiment, we received 311 responses (63%) to our emails from legislators that were up for re-election in single-member districts in the 2013 election.⁷ This response rate is in line with response rates from other field experiment of the genre conducted with elected officials in European party democracies: 62% in Switzerland (Giger et al 2019) and 79% in Germany (Hess et al 2018).⁸ Legislators' probability to answer in this last study is unsurprisingly a bit larger because the email entailed a service rather than a policy requests (Butler et al 2012).

More legislators responded to the personal vote intention email (67%) than to the partisan vote intention one (59%). The difference is thus + 8% points ($p < .1$, t-test difference). Among the emails we received, 5% arrived the very same day of the sending, while 48% arrived within a week. The last response we recorded reached our mailbox 64 days after sending our email. Based on this descriptive evidence, we can conclude that in the aggregate German legislators pay attention to constituency demands, especially when these demands include a personal vote intention.

In the empirical analysis, we first test the two hypotheses with the two dependent variables presented above: responding vs. not responding, and the timing of the response. Then, we explore the possibility of a contamination effect between tiers of election that is often considered as central to understand the functioning of the German mixed-member system (Ferrara et al., 2005; Manow, 2015).

⁷ The 17th German Bundestag consisted at the time we conducted the experiment of 620 members. We did not contact the independent legislator, and the 20 legislators that solely ran on a party list previously, as well as the 6 legislators that were not competing in a nominal district in the election in 2013. This left us with 593 members of the *German Bundestag*. Among these, 99 had announced retirement and thus did not compete in the 2013 election. We omitted this group from our analysis because it violates crucial assumptions on the motivational basis of the legislators' responsiveness. Thus, we end up with 494 legislators in our analysis.

⁸ Note however that the response rate found by De Vries et al (2016) in their study of members of the European Parliament is much lower (28%). This is probably due to the rather particular context of the European Union where the parliament does not have the same power than a national parliament.

Responding vs. not responding

To test whether legislators treated with a personal vote intention were more likely to respond than to those treated with a partisan vote intention (H_1), we specify the following logit model⁹:

$$\Pr(\text{RESPONSE} = 1) = \text{logit}^{-1}(\beta_0 + \beta_1 \text{PERSONAL VOTE INTENTION} + \beta_i \text{CONTROLS})$$

We expect β_1 to be positive. Then, to test H_2 , we need to specify a model that includes the treatment group, the mode of election (whether the legislator is elected in a single-member district or via a party list), and an interaction between the two. We expect a positive interaction effect because legislators elected in a single-member district that received a personal vote intention email should be particularly likely to respond. We specify the following logit model:

$$\Pr(\text{RESPONSE} = 1) = \text{logit}^{-1}(\beta_0 + \beta_1 \text{PERSONAL VOTE INTENTION} + \beta_2 \text{MODE OF ELECTION (NOMINAL} \\ = 1) + \beta_3 (\text{PERSONAL VOTE INTENTION} * \text{MODE OF ELECTION}) + \beta_i \text{CONTROLS})$$

Table 1 reports the results of three logit models. Model 1, without controls, shows the expected treatment effect. Legislators that received a personal vote intention email were more likely to respond than those who received a partisan vote intention one. This gives support to our first hypothesis. In Model 2 we show that this effect remains robust if we include control variables. In Model 3 we test our second hypothesis. While the interpretation of raw coefficients in non-linear and non-additive models is difficult, the significant positive interaction effect corroborates our theoretical expectation ($p < .05$). Model 3 suggests that legislators elected in single-member districts that received a personal vote intention email were more likely to respond than the rest of their colleagues.

⁹ We also reproduce the analysis using OLS regression models instead. The results are very similar and presented in Appendix E. We also reproduce the test of H_2 using a split-sample strategy instead of a regression models with an interaction term between the treatment and the mode of election. The results are very similar and presented in Appendix F.

Table 1. Predicting response to constituency demand (logit)

	(1)	(2)	(3)
Personal Vote Intention	0.32*	0.33*	-0.08
	(0.19)	(0.10)	(0.26)
Mode of Election (1 = Nominal)		0.38	-0.07
		(0.29)	(0.35)
Personal vote Intention * Mode of Election			0.86**
			(0.38)
Gender (1 = Male)		0.11	0.11
		(0.21)	(0.21)
Age		-0.01	-0.01
		(0.10)	(0.01)
Party Fixed Effects	No	Yes	Yes
Constant	0.18	0.31	0.55
	(0.17)	(0.68)	(0.70)
Log-Likelihood	-323.8	-320.1	-317.6
N	494	494	494

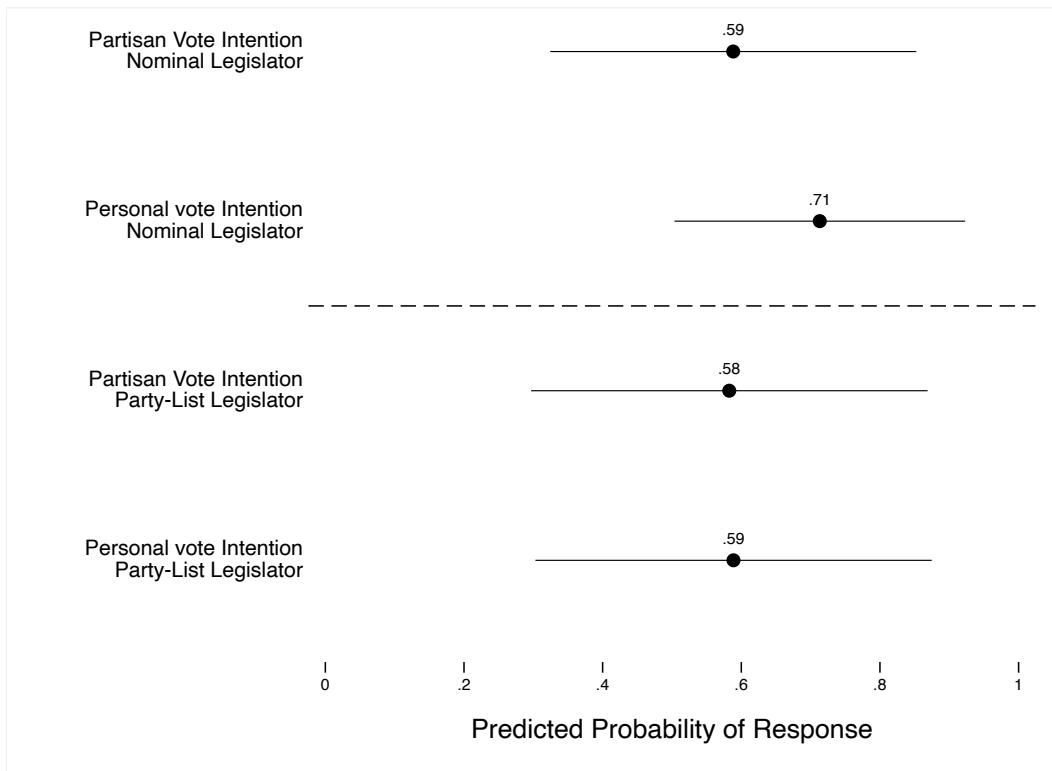
Note: Entries are coefficients estimated from logit regression models. Standard errors in parentheses. * $p < .10$, ** $p < .05$.

To facilitate the interpretation of the estimated coefficients in the model with the interaction term, we calculate the predicted probabilities of responding¹⁰ depending on the mode of election and treatment group. The predicted probabilities together with their 95% confidence intervals¹¹ are represented in Figure 2.

Figure 2. Predicted probabilities of responding to constituency demand (logit)

¹⁰ To calculate predicted probabilities for a ‘typical’ legislator we set the variables age and gender to the respective mean values of our sample. Given that we need to define particular values for all independent variables in the models, we set all the party fixed effects to ‘0’ indicating that we calculate the predicted of the baseline party (anonymized for ethical concerns, see Appendix A).

¹¹ The confidence intervals are based on a parametric bootstrap procedure. To obtain the respective percentiles of a distribution of predicted probabilities, we draw repeatedly from a multivariate normal distribution where the mean is represented by the estimated coefficients of Model 3 (Table 1), and the variance is the estimated variance-covariance matrix. The simulations are done in *Stata* (version 14) using *Clarify* (King et al 2000).



Note: Predicted probabilities are based on estimates of Model 3, Table 1.

Figure 2 corroborates the expected interaction effect of between the personal vote intention email and the mode of election, and most importantly specifies the size of the effect. Nominally-elected legislators in the personal vote intention group show a predicted probability to respond of 71%. This is substantially higher than the predicted probabilities of responding of the other types of legislators (by at least 12% points). A set of first-difference tests show that these differences are always statistically significant at least at $p < .05$.¹²

Timing of responses

To offer a second independent test of our hypotheses, we focus on the number of days that it took for legislators to respond to our email as a second dependent variable. It is important to note that we stopped data collection after 64 days. However, we kept the non-responding legislators in the

¹² In Appendix G, we use a ‘retrospective design analysis’ developed in Gelman and Carlin (2014) to show that the estimated treatments effects are neither grossly exaggerated (type M error), nor likely to be of the wrong sign (Type S error).

analysis, and treated them as right-censored data points. To estimate to what degree legislators of the personal vote intention group responded faster than others we rely on a duration analysis using a Cox proportional hazard model.¹³ A key advantage of these models is that they do not exclude legislators that have not responded. In doing so, they address a common issue with field experiments of the genre: a reduction of the sample to those that did respond, and the post-treatment bias that this might generate (Coppock 2019). The hazard function or hazard rate of the i^{th} legislator has the following form:

$$h(t|x) = h_0(t) \exp(x\beta)$$

In our model, the hazard rate represents the probability that individual legislators responded to our email at a particular point in time given that they had not responded yet. It has two components. The first is the baseline hazard function, $h_0(t)$, that indicates how the hazard of an email response changes over time when all the covariates are zero. The second component consists of the same set of covariates as before that are hypothesized to affect the timing of an email response. This model does not make any assumptions about the shape of the hazard. Thus, $\log \{h(t|x) / h_0(t)\} = x\beta$, and, consequently, the hazard ratio of two different legislators $h(t|x_i) / h(t|x_j) = \exp(x_i\beta) / \exp(x_j\beta)$ is independent of t .

Following our first hypothesis, we expect legislators treated with a personal vote intention email to respond faster than those treated with the partisan vote intention email. We estimate the following model:

$$h(t|x) = h_0(t) \exp(\beta_1 \text{PERSONAL VOTE INTENTION} + \beta_i \text{CONTROLS})$$

¹³ This is the so-called proportional hazards assumption. Several diagnostic checks using Schoenfeld residuals show that the probability to respond is constant over time, i.e., even after, say, two weeks.

A positive coefficient indicates that the covariate in question increases the hazard rate, i.e., it reduces the time a legislator took to respond. Conversely, a negative coefficient implies that the covariate reduces the hazard rate and therefore increased the response time. Similar than what we have done in the logit analysis above, we interact the mode of election of the legislator and the treatment group to test our second hypothesis. We estimate the following model:

$$h(t|x) = h_0(t) \exp(\beta_1 \text{PERSONAL VOTE INTENTION} + \beta_2 \text{MODE OF ELECTION (NOMINAL = 1)} + \beta_3 (\text{PERSONAL VOTE INTENTION} * \text{MODE OF ELECTION}) + \beta_i \text{CONTROLS})$$

Table 2 corroborates the pattern that we already found regarding our first dependent variable. In Model 1 we see that legislators receiving a personal vote intention email responded faster compared to those receiving a partisan vote intention one ($p < .05$). Model 2 shows that this effect remains robust once we include our controls. In Model 3, we test our second hypothesis that nominally-elected legislators that received a personal vote intention email responded faster compared to the rest of their colleagues, and again find statistically significant effects pointing into the expected direction.

Table 2. Predicting the time to response to constituency demand (Cox proportional hazard)

	(1)	(2)	(3)
Personal Vote Intention	0.24** (0.11)	0.28* (0.11)	-0.02 (0.16)
Mode of Election (1 = Nominal)		0.16 (0.17)	-0.12 (0.22)
Personal vote Intention * Mode of Election			0.49** (0.23)
Gender (1 = Male)		0.02 (0.13)	0.01 (0.13)
Age		-0.00 (0.01)	-0.00 (0.01)
Party fixed effects	No	Yes	Yes
Log-Likelihood	-1804.2	-1801.7	-1799.5
N	494	494	494

Note: Entries are coefficients estimated by Cox proportional hazard regression models. Standard errors in parentheses. * $p < .10$, ** $p < .05$.

To interpret the size of the hypothesized interaction effect we calculate the hazard ratio of the treatment effect (based on Model 3 in Table 2) for two hypothetical legislators that are from the same party and that are similar on all other observed characteristics that we control for. However, these two hypothetical legislators differ in whether they received a personal vote intention email (x^t) or a partisan vote intention one (x^c). The ratio of the two hazard rates for nominally-elected legislators is:

$$h(t|x^t)/h(t|x^c) = h_0(t) \exp(\beta_1 + \beta_2 + \beta_3 + \beta_i \text{CONTROLS}) / h_0(t) \exp(\beta_2 + \beta_i \text{CONTROLS}) = \exp(\beta_1 + \beta_3).$$

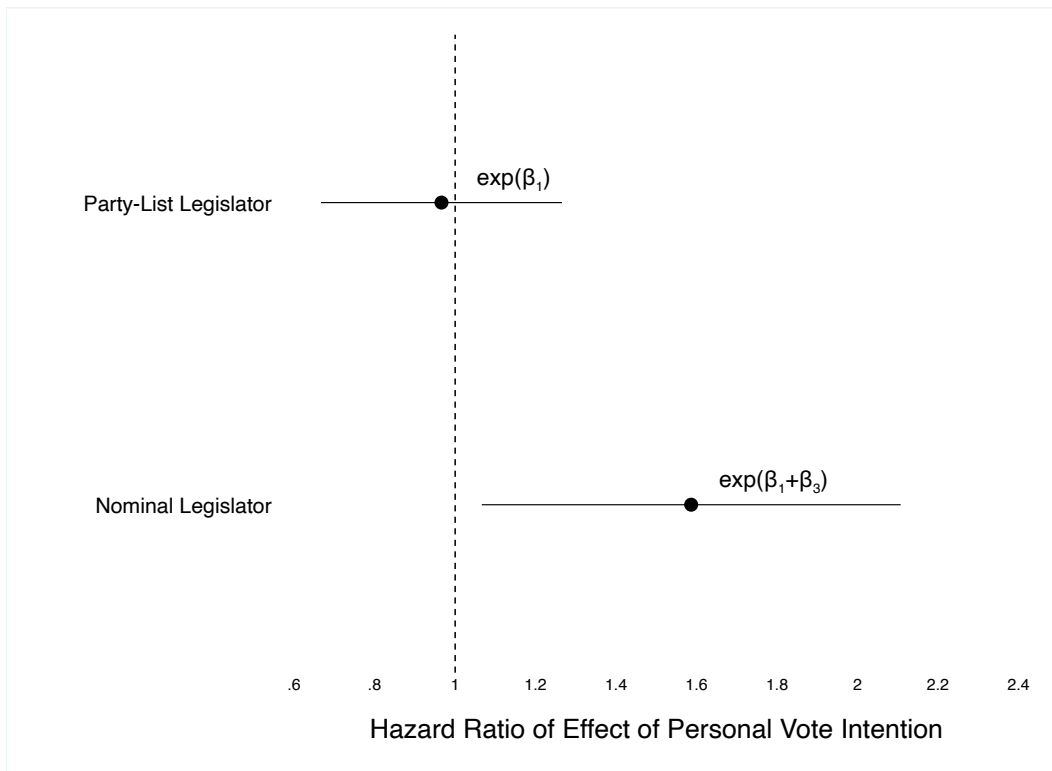
Similarly, the hazard ratio of the treatment effect for two party-list legislators is:

$$h(t|x^t)/h(t|x^c) = \exp(\beta_1).$$

Figure 3 illustrates the estimated hazard ratio of the effect of receiving a personal vote intention email (compared to a partisan vote intention one) for nominal and party-list legislators. It shows that the hazard ratio for nominal legislators is systematically greater than 1 because the 95% confidence interval does not overlap with the reference line. This implies that legislators that received the personal vote intention email responded on average faster than those that received the partisan vote intention one. Their hazard rate increases by 60%.¹⁴ This indicates that the treatment did in fact cause nominal legislators to respond faster. In contrast, there is no systematic effect of receiving the personal vote intention email (compared to the partisan vote intention one) for party-list legislators. These results thus also support our second hypothesis.

Figure 3. Hazard ratios of time to response to personal vote intention (Cox proportional hazard)

¹⁴ We conducted several robustness tests using parametric survival models. The estimated hazard ratio of the treatment effect is even higher assuming a Weibull or an exponential distribution. Thus, no matter what distribution we assume, we get similar results that are consistent with our theory.



Probing contamination effect

If contamination theory is correct (Ferrara et al., 2005; Manow, 2015), any dual candidate looking back on a tight electoral race for a single-member district should be more likely to make an effort and to respond to a constituency demand, independent of their mode of election. Most German legislators prefer being elected in a single-member district as it gives them more electoral security and prestige within their party (Manow 2015; Zittel and Gschwend 2008). Therefore, even legislators who lost the nominal vote in the previous election, and therefore earned their mandate via a party list, should have made an extra effort to cultivate a personal vote and respond to our email. Especially in marginal districts with tight electoral competition, every vote counts and thus extra efforts are needed to secure the few extra votes that might be needed to win next time.

In order to test for the implications of this argument, we divide up our sample in legislators competing in marginal and non-marginal electoral single-member districts and test whether the estimated effects (i.e., the effects estimated in model 3 of table 1) differ across both samples. We

define a district as marginal for a legislator if they either won or lost the district by a margin of less than 5% points in the last election. Using this rule, we identify 93 legislators competing in marginal districts and 401 competing in non-marginal districts. To test for the impact of marginality (and thus contamination) across the two different groups of legislators, we simultaneously run logit regression models predicting the probability of responding for the samples of legislators in non-marginal and marginal districts (we use the regression model set-up of Model 3, Table 1).¹⁵ Such a ‘seemingly unrelated’ bivariate logit set-up allows us to statistically test the difference in predicted probabilities across the samples of marginal and non-marginal legislators.

Table 3 presents the results of our bivariate logit regression analysis. The entries represent the predicted probabilities of responding for nominal and party-list legislators when they received a personal vote intention email depending on whether they were in a marginal or non-marginal district at last election. It shows consistent with contamination theory that party-list legislators competing in non-marginal districts were the least likely to respond while party-list legislators competing in marginal districts were those who are most likely to respond. The difference in predicted probability across those two groups is large (26%-points) and not due to chance ($p < .05$). By contrast, the predicted probabilities for nominal legislators who competed in marginal or non-marginal districts are almost identical.

Table 3. Predicted probabilities to respond among legislators in marginal and non-marginal districts

	(1) Personal Vote Intention Nominal Legislators	(2) Personal Vote Intention Party-List Legislators
Legislators in non-marginal districts (N = 401)	0.72 (0.05)	0.60 (0.04)
Legislators in marginal districts	0.73	0.86

¹⁵ We have to exclude the party dummies from the model because they cannot get identified for the group of marginal legislators. Some parties simply do not have marginal legislators.

(N = 93)	(0.07)	(0.09)
First-Difference (in column)	-0.01 (0.09)	-0.26** (0.10)

Note: Entries are predicted probabilities estimated from bivariate logit regression models. Standard errors in parentheses. N = 494. * $p < .10$, ** $p < .05$ for the test of whether the difference in predicted probabilities is zero.

These results stress in most convincing ways the unique incentives that result from mixed-member proportional systems. Generally, our analysis demonstrates that nominal legislators were more responsive than party-list legislators. However, Table 4 shows that party-list legislators need to be separated in two very different groups. Those that competed in a marginal district previously were very responsive, exceeding even the level of responsiveness of nominal legislators, since they badly wished to win next time. In contrast, party-list legislators competing in non-marginal districts were less responsive, since they saw no chance to win their district next time.

Conclusion

In times of political dealignment and weakening parties, individual legislators play a crucial role as agents of political representation. In this paper, we report the results of a field experiment, for which we sent standardized emails to German legislators on behalf of a constituent asking for their future political agenda, if re-elected. The randomized treatment was the inclusion a personal vote intention (vs. a partisan vote intention) signal in an actual email. Our evidence shows that legislators responded more often and faster when they were treated with a personal vote intention email. Our analysis further demonstrates the moderating effect of the electoral system on the interactions between constituents and legislators. We find that nominally-elected legislators were more responsive than those elected via a party list, but only when they received the personal vote intention email. This effect is strong and robust. Digging deeper into the contamination effect between electoral tiers in the German mixed-member system, we find that legislators elected via party lists competing in marginal single-member districts were at least as likely to respond than nominal legislators. Thus, consistent

with contamination theory, party-list legislators behave similarly like their nominal counterparts as long as they seem to have a chance to win in their district next time.

The contribution of the paper to the literature on legislators' responsiveness is twofold. First, we contribute to the growing literature that use field experiments with legislators to study this topic. Whereas it is a stylized fact that legislators facing tight electoral competition are more likely to respond to constituency requests, we show that constituents themselves can activate electoral considerations in signaling a personal vote intention in their message. Second, we contribute to the literature that studies the effect of electoral system incentives on legislators' responsiveness. This literature usually focuses on the direct effects of the electoral system. In contrast, we highlight the moderating effect between the type of constituency demand and the electoral context. Only when voters remind legislators explicitly about personal votes, electoral system incentives seem to matter for the responsiveness of legislators.

Our results also have some interesting implications for the future of representation in European democracies at large. They suggest that electoral engineers might consider more candidate-centered electoral arrangements in times of dealignment, since they best secure individual-level responsiveness. Voters that feel that legislators might be responsive to their concerns are also more likely to support the political system rather than falling into the populism trap. Finally, it is important to note that this paper also contributes to opening new avenues for further research on legislators' responsiveness in Europe. We are among the first ones to conduct a field experiment with legislators in a region where parties play a more important role in linking the citizens and the State than in the US. Our results demonstrate that individual legislators are particularly responsive to constituents even in the context of a European party democracy.

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The Importance of Personal Vote Intentions for the Responsiveness of Legislators: A Field Experiment

Appendix

Damien Bol, Thomas Gschwend, Thomas Zittel, Steffen Zittlau¹⁶

Appendix A. Ethical considerations

Appendix B. Original text of email treatments

Appendix C. Randomization checks

Appendix D. Balance tests

Appendix E. Linear probability models

Appendix F. Treatment effects in split samples

Appendix G. Power calculations and retrospective design analysis

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Appendix A. Ethical considerations

Field experiments in which researchers send emails to legislators on behalf of a citizen have important advantages, and thus are more and more common in political science. They however also raise ethical concerns. As a general rule, researchers evidently should not deceive experimental subjects, and should strive for their informed consent. However, we consider field experiments that involve deception important since they allow to pursue conflicting goals that are important and that preclude informed consent. As far as the goals are concerned, we particularly consider it crucial to extract unhindered and objective insights in the basic processes in democratic systems. Under democratic rule, the public has a right to know how they are governed and what kind of biases or blind spots might affect political decision-making. As far as the means are concerned, we consider field experiments crucial to reach these goals. Revealing our real identity for example in a pre-briefing effort would compromise the advantage of randomization, augment the risk to receiving biased reactions from legislators, and thus would result in poor reflections of the actual communications between the constituents and their representatives.

To minimize ethical ramifications, we took several precautions to reduce the harm caused by the experiment on our subjects. First, responding to an email is a task that occurs frequently over the working day of a legislator, even without our intervention (McClendon 2012). Compared to the overall volume of email, our extra email provided a minor nuisance to legislators. Second, in our email we ask a very general question about future projects if re-elected as to minimize time burdens and to also not extract confidential types of information. Third, we completely anonymized the dataset, so that it is no longer possible to identify the individual legislators that took part in our experiment. Fourth, we do not report any effects related to parties to prevent any strategic use of our results in the party-political game and thus to prevent harm to any of the parties involved. Fourth, to further probe our considerations, we subjected our project to an ethics audit at the University of Mannheim. The responsible audit board came to a positive conclusion and found that we conducted an experiment that is relevant for the scientific community and the public and that we took all precautions necessary to minimize harm in a professional way.

Appendix B. Original text of email treatments

From: [Name]@gmx.de
Object: Bürgeranfrage: **Ihre Arbeit als Abgeordnete[r]**

Sehr geehrte(r) [Name des/der MdB],

Ich heiße [Name] und komme aus [Stadt im Wahlkreis]. Ich sende Ihnen diese E-mail als Bürger, der um seine Zukunft besorgt ist. Normalerweise wähle ich etwas anderes, in letzter Zeit bin ich aber auf **Sie und Ihre Arbeit als Abgeordnete[r]** aufmerksam geworden. Ich finde es gut wie kompetent **Sie sich** für die Belange Ihrer Wähler **einsetzen**. Ich kann mir gut vorstellen, meine Stimme bei der nächsten Wahl **Ihnen** zu geben.

Damit ich eine informierte Wahlentscheidung treffen kann, ist es mir wichtig zu wissen was Sie in der nächsten Zeit machen wollen. Können Sie mir bitte sagen was das wichtigste politische Vorhaben ist, das Sie voranbringen wollen und an dem Sie sich messen lassen wollen bei der nächsten Wahl?

Allerbesten Dank schon einmal im Voraus für Ihre Antwort. Mit freundlichen Grüßen,

[Name]

From: [Name]@gmx.de
Object: Bürgeranfrage: **Arbeit der [PARTEI] im Bundestag**

Sehr geehrte(r) [Name des/der MdB],

Ich heiße [Name] und komme aus [Stadt im Wahlkreis]. Ich sende Ihnen diese E-mail als Bürger, der um seine Zukunft besorgt ist. Normalerweise wähle ich etwas anderes, in letzter Zeit bin ich aber auf **die Arbeit der [PARTEI] im Bundestag** aufmerksam geworden. Ich finde es gut wie kompetent **sich Ihre Partei** für die Belange ihrer Wähler **einsetzt**. Ich kann mir gut vorstellen, meine Stimme bei der nächsten Wahl **der [PARTEI]** zu geben.

Damit ich eine informierte Wahlentscheidung treffen kann, ist es mir wichtig zu wissen was Sie in der nächsten Zeit machen wollen. Können Sie mir bitte sagen was das wichtigste politische Vorhaben ist, das Sie voranbringen wollen und an dem Sie sich messen lassen wollen bei der nächsten Wahl?

Allerbesten Dank schon einmal im Voraus für Ihre Antwort. Mit freundlichen Grüßen,

[Name]

Appendix C. Randomization checks

The analysis below shows that neither the alias (Table C1), nor the wave (Table C2), have a systematic impact on legislator's response behavior.

Table C1. Randomization checks across aliases

Name	No Response	Response	Total
Alexander Müller	38 (31%)	85 (69%)	123 (100%)
Markus Becker	51 (42%)	71 (58%)	122 (100%)
Michael Weber	41 (35%)	77 (65%)	118 (100%)
Thomas Schmidt	53 (40%)	78 (60%)	131 (100%)
Total	183 (37%)	311 (63%)	494 (100%)
<i>N</i>	494		

Note: Entries are absolute number of responses. Row percentages are in parentheses. No systematic differences across names according to a Pearson χ^2 -test for the independence of the rows and columns, with $\chi^2(3) = 4.1$, $p = .25$

Table C2. Randomization checks across waves

Wave	No Response	Response	Total
1	48 (38%)	77 (62%)	125 (100%)
2	40 (34%)	77 (66%)	117 (100%)
3	52 (42%)	71 (58%)	123 (100%)
4	43 (33%)	86 (66%)	129 (100%)
Total	183 (37%)	311 (63%)	494 (100%)
<i>N</i>	494		

Note: Entries are absolute number of responses. Row percentages are in parentheses. Pearson χ^2 -test for the independence of the rows and columns, with $\chi^2(3) = 2.71$, $p = .44$

Appendix D. Balance tests

We demonstrate that the covariates in the dataset are orthogonal to our key experimental treatment. Bivariate tests demonstrate that the type of email (personal or partisan vote intention) is unrelated to other characteristics of the email, namely the wave at which it was sent (Table D1) and the sender's name (Table D2). Also, it is not related to the key independent variable, namely the legislator's mode of election (Table D3), as well as two pre-treatment variables, namely gender (Table D4) and age (Table D5).

These results confirm that our covariates do not systematically predict whether someone got assigned a personal or a party representation treatment.

Table D1. Balance test: Waves

Wave	Partisan Vote Intention	Personal Vote Intention	Total
1	60 (48%)	65 (52%)	125 (100%)
2	57 (49%)	60 (51%)	117 (100%)
3	61 (50%)	62 (50%)	123 (100%)
4	64 (50%)	65 (50%)	129 (100%)
Total	242 (49%)	252 (51%)	494 (100%)
<i>N</i>	494		

Note: Entries are absolute number of responses. Row percentages are in parentheses. No systematic differences across waves according to a Pearson χ^2 -test for the independence of the rows and columns, with $\chi^2(3) = .09$, $p = .993$

Table D2. Balance test: Aliases

Name	Partisan Vote Intention	Personal Vote Intention	Total
Alexander Müller	60 (49%)	63 (51%)	123 (100%)
Markus Becker	59 (48%)	63 (52%)	122 (100%)
Michael Weber	58 (49%)	60 (51%)	118 (100%)
Thomas Schmidt	65 (50%)	66 (50%)	131 (100%)
Total	242 (49%)	252 (51%)	494 (100%)
<i>N</i>	494		

Note: Entries are absolute number of responses. Row percentages are in parentheses. No systematic differences across names according to a Pearson χ^2 -test for the independence of the rows and columns, with $\chi^2(3) = .04$, $p = .998$

Table D3. Balance test: Mode of Election

Mode of Election	Partisan Vote Intention	Personal Vote Intention	Total
List	118 (46%)	136 (54%)	254 (100%)
Nominal	124 (52%)	116 (48%)	240 (100%)
Total	242 (49%)	252 (51%)	494 (100%)
<i>N</i>	494		

Note: Entries are absolute number of responses. Row percentages are in parentheses. No systematic differences across names according to a Pearson χ^2 -test for the independence of the rows and columns, with $\chi^2(1) = 1.34$, $p = .247$

Table D4. Balance test: Gender

	Partisan Vote Intention	Personal Vote Intention	Total
Male	166 (50%)	164 (50)	330 (100%)
Female	76 (46%)	88 (54)	164 (100%)
Total	242 (49%)	252 (51%)	494 (100%)

Note: Entries are absolute number of responses. Row percentages are in parentheses. No systematic differences across gender according to a Pearson χ^2 -test for the independence of the rows and columns, with $\chi^2(1) = .69$ and $p = .407$.

Table D5. Balance test: Age

	Mean (Partisan Vote Intention)	Mean (Personal Vote Intention)	Diff.	Stand. Err.	H: Diff. $\neq 0$
Age	51.75	51.58	0.17	(0.85)	0.842

N = 494. No systematic age differences across treatment groups.

Appendix E. Linear probability models

We reproduce the analysis presented in Table 1 of the main text using a linear probability models instead of logit regression models. The results are similar to those of Table 1 (see Table E1).

Table E1. Linear probability models

	(1)	(2)	(3)
Personal Vote Intention	0.08*	0.07*	-0.02
	(0.04)	(0.04)	(0.06)
Mode of Election (1 = Nominal)		0.08	-0.02
		(0.06)	(0.08)
Personal Vote Intention * Mode of Election			0.19**
			(0.09)
Gender (1 = Male)		0.02	0.02
		(0.05)	(0.05)
Age		-0.00	-0.00
		(0.00)	(0.00)
Party Fixed Effects	No	Yes	Yes
Constant	0.59**	0.58**	0.64**
	(0.03)	(0.16)	(0.17)
N	494	494	494

Note: Entries are coefficients estimated from OLS regression models. Standard errors in parentheses. * $p < .10$, ** $p < .05$.

Appendix F. Treatment effects in split samples

We reproduce the analysis presented in the main text in using a split-sample strategy: we separate the sample into two groups, i.e., nominally-elected legislators and those elected in the party-list, and re-estimate the models.

First, we look at the difference in response rates and perform a difference in means t-test. Among nominally-elected legislators (N=240), those who receive a personal vote intention email are more likely to respond than those who receive a partisan vote intention email by 19% points, i.e. 72% of response rate vs. 53% ($p < .01$, t-test difference in means). Among legislators elected via a party list, (N=254) those who receive a personal vote intention email are as likely to respond than those who receive a partisan vote intention email, i.e. 63% of response rate vs. 64% ($p = 0.75$, t-test difference in means).

Second, we reproduce Model 1 of Tables 1, 2, and E1 in using a similar split-sample strategy. The tables below show that the personal vote intention email increases the probability of responding (logit, OLS), and of responding fast (Cox). See Table F1.

Table F1. Split-sample regression models.

	Nominal Legislators (Logit)	Party-list Legislators (Logit)	Nominal Legislators (OLS)	Party-list Legislators (OLS)	Nominal Legislators (Cox)	Party-list Legislators (Cox)
Personal Vote Intention	0.84** (0.28)	-0.08 (0.26)	0.19** (0.06)	-0.01 (0.06)	0.50** (0.17)	-0.01 (0.16)
Log-Likelihood	-154.0	-166.8			-759.4	-827.6
N	240	254	240	254	240	254

Note: Entries are coefficients estimated from logit regression models (logit), OLS regression models (OLS), and Cox proportional hazard regression models (Cox). Standard errors in parentheses. * $p < .10$, ** $p < .05$.

Appendix G. Power calculations and retrospective design analysis

There is always a possibility that the magnitude and the sign of treatment effects that we estimate are due to chance. We follow the recommended approach of Gelman and Carlin (2014) and perform ‘postdata design calculations’ (p.643) to evaluate the reliability of the estimated treatment effects given the sample size.

Where is the problem? In our analysis, we have one estimated treatment effect (t^{est}) of the unknown true treatment effect (t^{true}). Suppose that we conduct a hypothetical replication study and obtain a replicated treatment effect (t^{rep}) using a design and sample size identical to the one the original study and assume that the estimated standard error of t^{rep} is the same as the one of t^{est} . In such a context, Gelman and Carlin (2014, p.643) propose three quantities of interest. First, an important quantity is the probability that t^{rep} is larger (in absolute terms) than the critical value defining “statistical significance” in the original study (*Power*). Second, another important quantity is the probability that t^{rep} has the incorrect sign (*Type S error*). Third, a final quantity of interest is the exaggeration ratio, i.e., a ratio calculating by how much the absolute value of t^{rep} overestimates the value of t^{true} in absolute terms (*Type M error*).

Given that this approach requires unbiased and normally distributed treatment effect estimates, we rely on the results of the linear probability models (Appendix E). Moreover, since we show, in the original study, that the treatment effect is entirely driven by the subsample of nominal legislators (see e.g., Figure 2), we focus on this subsample. The t^{est} derived from the linear probability model applied to nominal legislators is the one that we use in the retrospective design analysis, i.e., 0.19 with a standard error 0.06 (see Appendix F).

By definition, t^{true} is unknown. Gelman and Carlin (2014) suggest turning to external information in the literature (as in traditional power analysis) to determine a range of potential values of t^{true} . Our starting point to find comparable studies is the meta-analysis of field experiments aiming at studying the responsiveness of legislators presented in Costa (2017). One key result of the study is that the levels of responsiveness considerably vary across studies. We thus select one that is reasonably close to ours: Broockman (2013) who studies the responsiveness of elected legislators. In this study, he uses two treatments: whether the sender lives in the legislator’s district or not, and whether they have a ‘putative’ black or white name. We compare our treatment effect to Broockman’s treatment effect of living in or out of the district. In our study, the email of both the treatment and control group feature a sender living in the legislator’s district. Yet, only in the treatment group (*personal vote intention*), there is a clear signal that the voter is intending to vote for the legislator just like a sender living in the district. So, we can see Broockman’s treatment as similar to ours.

Broockman’s (2013) effect of the treatment living in the district vs. out of the district is +26.6%-points. This is our first reference point. Yet, this treatment effect is probably an over-estimation of ours as, even in our control group (*partisan vote intention*), the sender signals a partisan attachment. Hence, as a second reference point, we reduce Broockman’s (2013) sample to the black legislators who received an email from a black sender. In the US context, we can see race as similar to a partisan prime because there is a strong correlation between race and partisan attachment. Looking at Broockman’s (2013) subsample of black legislators receiving an email from a black sender, we observe an effect of the treatment living in the district vs. out of the district of +15.6%-points. This is our second reference points.

Table G1 reports the quantities of interest that Gelman and Carlin (2014) for the two reference points presented above, together with the one we estimate in our study (0.190, see above). First, we observe that the probability of these effects having the wrong sign (Type-S error) is essentially zero. Moreover, we also see that given those hypothetical effect sizes our study is in fact well powered.

Finally, given the sizes of the respectively estimated exaggeration ratio (Type M error) it is very unlikely likely that our t^{est} is a large overestimation of t^{true} . The factor by which the magnitude might have been exaggerated is not larger than 13.5%.

Table G1. Hypothetical Treatment Effect Sizes in Retrospective Design Analysis.

Hypothetical Treatment Effect	Power	Type S-error	Type M-error
0.266	0.99	0.000	1.004
0.190	0.89	0.000	1.069
0.156	0.79	0.000	1.135

Note. *Power* is the probability that the statistical test correctly rejects the null hypothesis; *Type-S error* is the probability of the sign being in the opposite direction of the one in the original study; and, *Type-M error* is the factor by which the magnitude of the effect size in the original study might be exaggerated.

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