Elections and Deceptions:
An Experimental Study on the Behavioral Effects of Democracy
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Abstract

How do democratic elections affect policy making? Traditionally, the virtue of elections has been seen in their role as means of screening and sanctioning shirking public officials. This paper proposes a novel rationale for elections and political campaigns considering that candidates incur psychological costs of lying. These non-pecuniary costs imply that political campaigns constitute an anchor and guide subsequent behavior, even in the absence of reputational or image concerns. Our lab experiments reveal that promises are more than cheap talk. They influence the behavior of both voters and their representatives. Strikingly, the electorate is better off when their leaders are elected democratically rather than being appointed exogenously. In addition, we find that representatives are more likely to serve the public interest when their approval rates are high. Taken together, our results suggest that electoral competition and campaigns confer important benefits beyond their screening and sanctioning functions.

JEL classification: D72, C92, D03.
Keywords: Costs of Lying, Electoral Competition, Laboratory Experiment.
“We have won with an ample margin. But, far from putting us in a position of privilege, this puts us instead in a position of greater responsibility and obligation.”

Argentina’s first lady, Cristina Fernandez de Kirchner, after winning the general election with 45% of all votes - almost twice the number of the runner-up (2007/10/29)

Introduction

Are political campaigns beneficial for society? Expenditures for political campaigns are skyrocketing (e.g. see, Benoit and Marsh 2008; Stratmann 2005) and often described as an inefficient “arms-race” (e.g. see, Abrams and Settle 2004). Although electoral campaigns are anything but cheap, rational choice scholars generally consider their content as cheap talk (e.g. see, Barro 1973; Ferejohn 1986; Austen-Smith and Banks 1989). Candidates can promise almost anything in pre-election campaigns, but voters do not have any direct means to enforce promises (see Manin, Przeworski and Stokes 1999).

We outline a psychological rationale why voters might benefit from elections and electoral campaigns. Our idea is based on the observation that human behavior is not characterized by pure self-interest, but is also driven by other-regarding preferences and intrinsic norm compliance (e.g. Cooper and Kagel 2009; Fowler and Kam 2007; Fehr and Schmidt 2002). Extensive experimental evidence suggests that people tend to tell the truth in strategic situations, even if reputation is not at stake. Scholars usually explain this phenomenon by arguing that people incur psychological costs if they do not live up to their promises. Several reasons for such non-pecuniary costs of lying have been suggested, as for example the desire to maintain a positive self-image (see Fischbacher and Heusi 2008; Mazar, Amir and Ariely 2008). Others have argued that people feel guilty if they do not meet others’ expectations (see Baumeister and Heatherton 1994; Charness and Dufwenberg 2006). Whatever the exact roots of the costs of lying are, their existence implies that electoral campaigns might not just be cheap talk. They have the potential to serve as anchors for actual office behavior. To the extent to which promises anchor the incumbent’s subsequent behavior, the constituency is likely to benefit from electoral competition, where candidates outrun each other with campaign promises.

We test our conjecture with incentivized lab experiments studying both the behavior of

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1Elections serve as an indirect instrument for promoting representation. In repeated elections with non-binding or a complete lack of term limits, voters can threaten to vote dishonest politicians out of office (e.g. see, Key 1966; Ferejohn 1986).
representative and voters in a stylized delegated democracy. In our benchmark “Election”
treatment two candidates competed for office in an election with five voters. Both candidates
simultaneously promised how much money they would distribute to the electorate if they won.
Promises were not enforceable and thus non-binding for the candidates. The electorate was
therefore uncertain about how their representatives would behave once elected. The winner
was determined by majority rule and was entrusted with a budget that she could share with
the electorate or keep for herself. This game captures the elementary tradeoff representatives
face in situations where their personal interests do not coincide with those of the public.\(^2\)

We compare treatment Election with two additional treatments. In treatment “Random”,
we eliminated electoral competition by replacing the election with a random selection mech-
anism. The approval rate was randomly determined by the experimenter using a large die
in front of all participants. By contrasting treatment Election with Random, we are able to
analyze the causal effects of electoral competition on promises and office behavior. In the
second additional treatment “NoCampaign”, voters could choose their preferred candidate as
in treatment Election, but the candidates were not allowed to run electoral campaigns. This
treatment sheds light on the impact of campaign promises on the candidates’ benevolence.

Our experiment provides the following insights. First, electoral competition intensified
campaign promises. Candidates promised significantly more benefits in democratic elections
than if they were randomly appointed. Second, promises served as an anchor for subsequent
office behavior. We find that on average candidates fulfilled their promises to a degree of roughly
60 percent, despite conflicting self-interest and the absence of reputational concerns. This
suggests that lying creates non-pecuniary or psychological costs. Third, eliminating electoral
competition led to a substantial drop in the candidates’ benevolence. Furthermore, we observe
that when candidates were not allowed to make any promises (in treatment NoCampaign),
voters were just as badly off as in the absence of elections. Together, these results suggest
that not only electoral competition matters, but also the ability to make pre-election promises.
Finally, we found that the larger their share of supporters, the more benevolent were the
candidates. This indicates that the costs of lying increase with the share of supporters a
candidate would let down. This relationship was much weaker in the two control treatments

\(^2\)Our setup can easily be reinterpreted to mimic a scenario where politicians have to decide how much costly
effort (e.g. for solving problems, fighting corruption, etc.) they want to provide, where effort is beneficial for the
citizens.
NoCampaign and Random, where the approval rate was less or not informative about voters’ expectations.

We opted for an experimental approach because it allows us to identify the impact of lying aversion under tightly controlled conditions. A separation of intrinsically motivated honesty from reputational motives is inherently difficult with observational field data, as politicians often face looming re-elections, future career plans, or other image concerns. Moreover, voters can weed out dishonest candidates in repeated elections, creating additional potential for selection bias. It is important to disentangle the psychological costs of lying from reputational motives and adverse selection because it improves our knowledge about how elections influence leadership behavior and it provides valuable input for the design of political institutions. We designed our experiment in such a way that we can sidestep reputational concerns and selection effects. Because our election was only for one term, voters could not punish or weed out less benevolent leaders. Moreover, all participants interacted anonymously with each other using a computer interface and their true identities were never revealed to the other participants at any point in time. Another advantage of our experimental approach is that we can exogenously assign constituencies to different democratic institutions. We thus avoid the methodological problems implied by the potential endogeneity of political institutions (e.g. see Besley and Case 2003).³

Related Literature

How do electoral institutions affect policy making? The theoretical literature has predominantly focused on two key mechanisms through which elections influence policy making. According to the accountability or moral hazard view (see Key 1966; Barro 1973; Ferejohn 1986), the threat of looming re-elections disciplines incumbents. Other theories have emphasized the role of elections as a selection device, giving voters the opportunity to weed out incompetent or dishonest politicians (see Fearon 1999; Besley 2005; Ashworth 2005; Gordon, Huber and Landa 2007; Ashworth and de Mesquita 2008).⁴ Disentangling selection from accountability effects is challenging, because both mechanisms often have observationally equivalent implications. A common empirical approach is to take advantage of binding term limits and to analyze how the

³We discuss the potential limitations of our lab experimental approach in the conclusion.
⁴In the presence of informational asymmetries repeated elections can also create counterproductive incentives for politicians to pander to public opinion (see Maskin and Tirole 2004; Canes-Wrone, Herron and Shotts 2001).
lack of electoral incentives affects policy making in the last term. The evidence is consistent with both accountability and selection effects (see Besley and Case 2003). Alt, de Mesquita and Rose (2011) for example use variation in U.S. gubernatorial term limits across states and time and find that the impact of re-election incentives and selection on government performance are of similar magnitude (see also Ferraz and Finan 2011; Gagliarducci and Nannicini N.d.; Rothenberg and Sanders 2000 for further recent evidence on selection and accountability effects). We rule out reputational concerns in our study by design, and are able to control for selection effects. We therefore contribute to this extensive literature by providing novel evidence that elections convey motivational benefits beyond their role as a selection and sanctioning device.

Our paper further adds to the literature in the following ways. First, scholars of pledge fulfillment analyzed the extent to which elected representatives and political parties live up to their campaign promises. Pétry (2009) surveyed 18 studies from various countries and found that 67 percent of promises are kept on average. The degree of pledge fulfillment is often noted as surprisingly high, because the general population tends to believe that politicians are untrustworthy (see Thomson 2011). While most of the literature has focused on party promises, fewer papers studied promise keeping (or congruence with pre-election issue positions) on the level of a single individual. Individual level studies have drawn similar conclusions (e.g. see Fishel 1985; Sulkin and Swigger 2008; Sulkin 2009; Ringquist and Dasse 2004; Schwarz, Schädel and Ladner 2010). Both reputational concerns and the psychological costs of lying can explain promise keeping in all existing studies. Our controlled lab experiment allows us to disentangle the psychological cost of lying from other pecuniary costs implied by a loss of reputation. Moreover, we exogenously manipulate key features of democratic institution. We are thus able to study the causal effects of democratic institutions on candidates’ behavior, complementing the existing field studies.

Second, random appointment of public officials by lot (also known as “sortition”) was a core feature in the ancient Athenian democracy (see Headlam 1933; Manin, Przeworski and Stokes 1999). Various forms of sortition were also practiced in medieval and renaissance Italian city-states (mostly Florence and Venice), 16th century England, as well as in some of the Landsgemeinden in Switzerland until 1837 (see Engelstad 1989; Carson and Martin 1999; Tridimas 2011). A well known modern form of random selection of social decision makers are juries in

5See also Elster 1989 for a more general discussion of randomization in social decision-making.
court cases. Advocates of sortition have argued that random appointment of public officials achieves a more accurate descriptive representation (e.g. Mueller, Tollison and Thomas 1972; Burnheim 1985). Moreover, random appointment is thought to attenuate the incentives for self-interested rent-seeking activities and promotes political equality (see Lockard 2003; Mulgan 1984). Our results suggest that these potential benefits should be carefully weighed against the potential costs of less motivated representatives.

Third, we add to the growing field of experimental political economy (e.g. see Woon 2011; Grosser and Schram 2010; Morton and Williams 2010; Grosser and Schram 2006; Kube and Puppe 2009). Three related studies analyzed the effects of democratic choice on cooperation and public goods provision. Hamman, Weber and Woon (2011) find higher public goods provision when contribution decisions are democratically delegated rather than decentralized. Hamman et al. complement our results by focusing on the role of elections in selecting pro-social representatives. The experiments by Dal Bó, Foster and Putterman (2010) suggest that the impact of a given rule depends on whether it was democratically chosen or exogenously imposed on the subjects through another mechanism. Olken (2010) conducted a field experiment in Indonesia to study the causal effects of direct democratic participation in local development programs. He found that direct participation in the political decision making process increased satisfaction and the perceived legitimacy of the program. We complement these studies by showing that democratic elections and higher electoral support can increase the leaders’ benevolence.

Finally, a recent strand of experiments analyzed the behavioral implications of leadership (e.g. Hermalin 1998; De Cremer and van Knippenberg 2005; Güth et al. 2007; Glöckner and Normann 2011; Gächter and Sefton N.d.). Leadership in existing experiments is typically assigned exogenously (i.e. randomly). Our results suggest that leaders may behave differently if they have to compete for leadership rather than if their role is exogenously assigned (see also Brandts, Güth and Stiehler 2006; Brandts, Cooper and Weber 2011), particularly when competition promotes promise making.

**Experimental Design**

We conducted laboratory experiments to study democratic elections under controlled conditions. The experiments were conducted at the University of Bonn (BonnEconLab). Subjects
were randomly recruited from the BonnEconLab general subject pool, which consisted of approximately 3000 students from all disciplines and from various stages in their studies; psychology students were excluded. We ran five sessions with a total of 210 subjects. This resulted in 10 independent constituencies for each of the three treatments. Each constituency consisted of seven participants, who were randomly divided into two candidates (A and B) and an electorate of five voters.

Subjects made their decisions on the computer screen using the z-tree interface (Fischbacher 2007). Every computer was located in a private booth ensuring that the participants interacted anonymously with each other. This high degree of anonymity was required in order to rule out any reputational concerns among the participants. At the beginning of the experiment, all subjects received written instructions explaining the different stages of the game and the payoffs (see the Supporting Information for sample instructions). After participants had read the instructions, they answered control questions, ensuring that everyone understood the game. All earnings were computed in tokens and converted into cash using an exchange rate of €4 per 100 tokens at the end of the experiment. Subjects received an additional show-up-fee of €4 for their participation. The entire experiment lasted approximately 40 minutes.

The benchmark treatment “Election” consisted of the following five stages (see Figure 1):

![Figure 1: Timeline: Treatment Election](image)

**Stage 1 Campaigns:** In the first stage, candidates pursued their electoral campaigns. Campaigns were non-binding and consisted of two parts. In the compulsory part, candidates promised citizens how many tokens (between 0 and the maximum budget of 450 tokens) they would distribute equally among the citizens. In addition, candidates had the option of sending a text message (up to 300 characters) to the electorate. Both candidates pursued their campaigns.

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6Table 5 in the Appendix reports the background characteristics of our subjects.
simultaneously and their campaigns were not revealed to the opponent.

**Stage 2 Voting:** Each citizen voted for one of the two candidates in the second stage. The winner was determined by majority rule, received a fixed payment of 30 tokens, and was entrusted with a discretionary budget of 450 tokens. The outcome of the election was only announced later on, in Stage 5 of the game.

**Stage 3 Distribution:** Before the candidates knew the outcome of the election, they decided how many tokens they would actually distribute, conditional on winning the election with 60, 80, or 100 percent of votes.\(^7\) This design feature made it possible to analyze the behavior of both winners and losers, therefore circumventing a potential selection bias from citizens electing the more benevolent candidates.

**Stage 4 Belief elicitation:** In stage four, citizens had to guess how many tokens each candidate would distribute, and each candidate guessed (conditional on the outcome of the election) the average amount citizens expect her to distribute. In order to elicit beliefs in an incentive-compatible way, we rewarded correct beliefs with 10 tokens. The reward was reduced by one token for each unit that the stated belief differed from the actual value, down to a minimum of zero tokens.\(^8\)

**Stage 5 Payoff realization:** The winner of the election was announced in the final stage and, depending on the actual approval rate and the choices made in stage 3, the payoffs were realized.

Every participant played this election game only once, and their identities were not revealed at any point in time. This allowed us to rule out reputational concerns and to test whether elections confer benefits beyond their functions as a sanctioning and selection device in repeated settings.

We conducted two additional treatments. Treatment “Random” was identical to treatment Election, except that the electoral outcome was randomly determined by the experimenter using a large die visible for everyone. This was common knowledge among all participants.\(^9\) This

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\(^7\)This approach of eliciting conditional responses is frequently used in the experimental literature and is called the “strategy method” (see Selten 1967). From a game-theoretic perspective, the strategy method does not differ from eliciting direct (i.e. unconditional) responses. Brandts and Charness (2000) surveyed experiments comparing behavior using the strategy and the direct response method. They find no qualitative differences in behavior. Moreover, since we applied the strategy method to all of our treatments, it should not have any impact on differences between treatments.

\(^8\)We report our analysis of second-order beliefs in the Supporting Information.

\(^9\)The rules of the game were transparent to all participants, and the experiment did not involve any form of deception.
procedure eliminated electoral competition and the rationale for making generous promises. In the second treatment, “NoCampaign”, citizens voted for their candidates as in treatment Election, but candidates were not allowed to run electoral campaigns. Together, our three treatments allow us to identify the causal effect of candidates’ promises and electoral competition on the representatives’ behavior.\textsuperscript{10}

**Framework and Hypotheses**

In this section, we provide a simple formal framework of electoral competition and campaign promises, where voters are uninformed about the candidates’ honesty. Recent theoretical approaches, analyzing the influence of costs of lying in models of spatial electoral competition, inspired our framework (Banks 1990; Callander and Wilkie 2007; Callander 2008; Kartik and McAfee 2007). Our framework departs from these models in two dimensions. First, we assume that costs of lying increase with approval. The rationale behind this assumption is that approval rates reflect voters’ expectations. As highlighted in our opening quote by Cristina Fernandez de Kirchner, voters’ expectations can increase feelings of obligation and guilt in elected representatives (see also Baumeister and Heatherton 1994; Charness and Dufwenberg 2006 for a discussion about the relationship between feelings of guilt and expectation). Second, rather than assuming that the preferences of candidates and voters are distributed over a policy space, we model a situation in which the interests of politicians and voters are unambiguously in conflict.

Following our experimental design, we assume that two candidates $i$ (where $i = A, B$), compete for office in a one-shot election with an odd, finite number of voters, $n \geq 3$.\textsuperscript{11} Both candidates simultaneously make their promises, $P_i$, in the first stage of the game. Voters then cast their vote for their preferred candidate. Let us indicate the number of votes for the winner with $k$ and the simple majority with $m = \frac{n+1}{2}$. Conditional on the realized approval rate, $\frac{k}{n} \geq \frac{m}{n} = \frac{n+1}{2n}$, the elected candidate chooses how much money $S_i$ she actually distributes in

\textsuperscript{10}We ran an additional classroom experiment to classify the text messages candidates sent in treatments Election and Random, following Houser and Xiao (2011). We recruited 59 students uninvolved in the experiments above. After providing the verbal instructions for the original experiment, we gave students a list containing the candidates’ messages and asked them to classify each message as a “statement of intent or promise” or “empty talk”. At the end of the classroom experiment, 10 participants were randomly selected and paid according to the following: they earned two euros for each message they classified in the same way as the majority of the other students. Our results remain unchanged if we control for the type of text messages candidates sent. The additional results are available upon request.

\textsuperscript{11}We refer to male voters and female candidates.
total to the voters. We restrict $P_i$ and $S_i$ such that they can take on any value between 0 and a discretionary budget, $I \in \mathbb{R}_+$, assigned to the elected candidate. We normalize the utility of the losing candidate to zero in order to simplify the analysis. The utility of the winning candidate $i$ is given by

$$U_i(P_i, S_i, k, \beta_i) = E + I - S_i - \beta_i C_i(P_i, S_i, k),$$

where $E \geq 0$ is a fixed payment or wage that cannot be distributed to the voters and $C_i(P_i, S_i, k)$ are the psychological costs of lying. In particular, we assume that

$$C_i(P_i, S_i, k) = \begin{cases} \frac{k-1}{2} \frac{(P_i - S_i)^2}{P_i^2}, & \text{if } P_i > 0 \text{ and } S_i < P_i, \\ 0, & \text{otherwise.} \end{cases}$$

$\beta_i \geq 0$ is a parameter that captures how sensitive candidate $i$ is to the psychological costs of lying. If $\beta_i = 0$, then lying is costless for candidate $i$. On the other hand, if $\beta_i > 0$, then candidate $i$ is lying averse. Her utility decreases with the squared difference between her promise and the distributed amount. Moreover, costs of lying increase with the candidate’s approval rate, $k$, and promise, $P_i$.

Candidates may be of two types, $H$ and $L$, differing with respect to $\beta_i$. In particular, let $\beta^H > \beta^L > 1$ be the degree of lying-aversion for type $H$ and $L$, respectively. $\beta^L > 1$ implies that both types of candidates distribute a positive amount when (i) they promise a strictly positive amount and (ii) when they are elected unanimously. The candidates’ types are randomly drawn by nature from the same ex-ante probability distribution. In particular, let $\phi$ and $(1 - \phi)$ be the probabilities that $i$ is of type $L$, respectively of type $H$. We assume that voters are uninformed about the candidates’ type.

Voting is costless and compulsory. Each voter casts his vote for the candidate he expects to be the most benevolent. The ex-post utility of each voter therefore consists of any affine transformation of the amount of money the electorate receives from the elected representative.

Our game proceeds in three stages. In the “Campaign” stage, candidates simultaneously make their promises. In the “Voting” stage, voters simultaneously vote for one of the two candidates by simple majority rule. And finally, candidates observe their approval rates and the winner decides how much money she distributes to the voters in the “Distribution” stage.
We focus on Perfect Bayesian equilibria where voters do not play weakly dominated strategy.\footnote{As shown in the Appendix, this assumption rules out unintuitive equilibria where voters prefer candidates who promise and distribute zero rather than candidates who promise and distribute strictly positive amounts.}

In equilibrium, the model produces the following testable predictions (formal statements and proofs in the appendix):

**H1.** Candidates promise to be more benevolent when they face electoral competition than when they are randomly appointed.

**H2.** The candidates’ promises influence voting behavior.

**H3.** Voters are better off when candidates are democratically elected rather than when leaders are randomly selected.

**H4.** The larger candidates’ promises and the higher their approval rates, the more benevolent are the elected candidates.

We derive these predictions solving the election game using backward induction. When lying is costly, candidates tend to keep their promises. Voters anticipate that candidates partially fulfill their pledges and are therefore more likely to cast their vote for the candidate who promises more. In competitive democratic elections, candidates use higher promises to attract more votes. Taken together, these considerations imply that voters are better off when their representatives are democratically elected rather than appointed exogenously.

### Experimental Results

Our results are presented in three steps. First, we analyze the impact of electoral competition on campaign promises. We then test whether voters consider promises to be cheap talk, and whether electoral outcomes reflect voters’ expectations. Finally, we investigate the extent to which electoral competition and campaigns affect the behavior of office holders.

### Campaigns

Figure 2 shows kernel density estimates for the amount of money the candidates promised, depending on whether they were democratically elected (Election) or appointed by lot (Random).

While candidates frequently promised low amounts of money in treatment Random, most promises in treatment Election were in the top range of the available budget. In comparison with treatment Random, promises in Election were on average twice as high (165 versus 325...
tokens). This difference is statistically significant according to a Wilcoxon rank-sum test ($p = 0.001$).\textsuperscript{13} Strikingly, many candidates in treatment Election do not promise the maximum amount of distributable tokens, but their promises seem to be driven by concerns for equality. The density in Figure 2 peaks at 375 and 400 tokens, which (depending on whether the winner’s fixed payment of 30 is considered in the calculations) imply equal payoffs for the incumbent and voters. The following result summarizes our findings:

**Result 1.** *Electoral competition promotes candidates to make more generous campaign promises.*

**Voting**

A natural question is whether voters take promises into account, even though rational choice theory suggests that they should be considered as cheap talk in our setting. We analyze how promises influence voters’ expectations using the following regression model:

$$E_c[S_i] = \alpha + \beta_1 P_i + \epsilon_{ic},$$

\textsuperscript{13}All reported p-values are based on two-sided tests.
Table 1: Promises and Expectations

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Promise</td>
<td>0.426***</td>
<td>1.173***</td>
</tr>
<tr>
<td></td>
<td>(0.104)</td>
<td>(0.132)</td>
</tr>
<tr>
<td>(Promise)$^2$</td>
<td>-0.002***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.000)</td>
<td></td>
</tr>
<tr>
<td>Constant</td>
<td>89.185**</td>
<td>30.111***</td>
</tr>
<tr>
<td></td>
<td>(32.142)</td>
<td>(6.543)</td>
</tr>
</tbody>
</table>

|                |             |             |
| Obs.           | 20          | 20          |
| $R^2$          | 0.490       | 0.604       |

Notes: This table shows OLS coefficient estimates (standard errors in parentheses are corrected for clustering on the level of each electorate). The dependent variable is the average number of tokens the electorate believed that each candidate would distribute. “Promise”, resp. “(Promise)$^2$” is the (squared) number of tokens the candidate promised. The results remain qualitatively the same if we use a Tobit model as an alternative. Significance levels are denoted as follows: Significance levels are denoted as follows: * p<0.1, ** p<0.05, *** p<0.01.

Where $E_c[S_i]$ is the average number of tokens that the voters in constituency $c$ believe candidate $i$ will distribute. $P_i$ is the promise candidate $i$ makes. The model is estimated using Ordinary Least Squares (OLS). Standard errors are corrected for clustering, accounting for dependency of the error term $\epsilon_{ic}$ within each constituency.\textsuperscript{14} We extend our empirical model and include $\beta_2 P_i^2$ in order to test whether excessively high promises are less credible.

The results from column (1) of Table 1 show that promises have a significant influence on expectations, suggesting that voters do not consider promises to be merely cheap talk. However, the significant coefficient for squared promises in column (2) suggests that the relationship between promises and beliefs is hump-shaped. The decrease in credibility is reasonable, given that fulfilling very generous promises is more costly. According to the regression results, promises which exceed 375.6 tokens become less credible. Strikingly, the peak mentioned above at 375 in the distribution of promises in Figure (2) suggests that candidates correctly anticipated this nonlinear relationship.

We complement these results and estimate the effect of promises on voting behavior using the following linear empirical model:

$$v_{nc} = \alpha + \beta_1 (P_{Ac} - P_{Bc}) + \epsilon_{nc},$$

where $v_{nc}$ is a dummy variable indicating whether voter $n$ in constituency $c$ supported can-

\textsuperscript{14}The results remain qualitatively the same if we use a Tobit model instead.
Table 2: Promises and Voting

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Difference in Promises</td>
<td>0.146</td>
<td>0.321***</td>
</tr>
<tr>
<td></td>
<td>(0.116)</td>
<td>(0.088)</td>
</tr>
<tr>
<td>(Difference in Promises)$^2$</td>
<td>-0.194***</td>
<td>(0.057)</td>
</tr>
<tr>
<td>Obs.</td>
<td>50</td>
<td>50</td>
</tr>
<tr>
<td>$R^2$</td>
<td>0.065</td>
<td>0.180</td>
</tr>
</tbody>
</table>

Notes: This table shows OLS coefficient estimates (standard errors in parentheses are corrected for clustering on the level of each electorate). The dependent variable is a dummy variable indicating whether or not a voter cast his vote for candidate A. “Difference in Promises” respectively “(Difference in Promises)$^2$” is the (squared) difference between the number of tokens candidates A and B promise (in hundreds of tokens). The results remain qualitatively the same if we use a Probit model as an alternative. Significance levels are denoted as follows: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Candidate $A$. $P_{Ac} - P_{Bc}$ is the absolute difference between promises (in hundreds of tokens) between candidate A and B. We used OLS$^{15}$ to estimate the linear probability model in equation 4 and corrected the standard errors for clustering of voting decisions within each constituency. We separately included the squared difference in promises ($\beta_2(P_{Ac} - P_{Bc})^2$) to capture potential nonlinearities. The results in column (1) of Table 2 suggest voters tend (although not statistically significant) to vote for the candidate who promises more than his opponents. However, the results in column (2) show that the moderate effect in column (1) is masked by a statistically significant nonlinear relationship: more generous promises increase political support but only up to a certain point. Based on the parameter estimates electoral success is, ceteris paribus, maximized for promises exceeding the opponents promise by 82.7 tokens.

Overall, we find that the electoral outcome reflects voters’ expectations about the candidates’ benevolence. In nine out of ten elections, voters elected the candidate whom they expected to be more benevolent ($\chi^2$ - test: $p = 0.016$). The main findings are summarized in our second result:

**Result 2.** Voters do not treat promises as cheap talk and take them into account when deciding whom to vote for. Election outcomes reflect voters’ expectations about the candidates’ benevolence.

$^{15}$The results are also robust if we use a Probit model.
Benevolence of Representatives

We have shown that candidates promise more if they face electoral competition rather than if they are randomly appointed. But do candidates live up to their promises? We answer this question by creating a measure of promise fulfillment. Our measure consists of the ratio between the actual number of distributed tokens and the candidate’s promise. On average, we find relatively high levels of pledge fulfillment in treatment Election (59.8% with a 95% confidence interval of: 43.8%, 75.7%) as well as in Random (63.4% with a 95% confidence interval of: 42.8%, 84.0%). Strikingly, these rates of pledge fulfillment are comparable with the degree of pledge fulfillment observed in field studies (see Pétry 2009).

Given that promises were partially fulfilled, the more generous promises in Election translated into higher monetary benefits for the voters. The cumulative distribution functions for the number of distributed tokens in Figure 3 show that voters were substantially more likely to be better off in treatment Election than in Random. For example, the probability that a candidate distributed more than a hundred tokens is 0.75 in the Election treatment, but only 0.2 in Random.

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4. Figure 3: Democratic Institutions and Benevolence

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On average (over all approval rates), candidates distributed 197 tokens in treatment Elec-

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16Four candidates distributed a larger amount than what they promised. We set their ratio of pledge fulfillment to 1 in our analysis.
Table 3: Democratic Elections vs. Appointment by Lot

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Election</td>
<td>121.217***</td>
<td>57.169</td>
</tr>
<tr>
<td></td>
<td>(39.392)</td>
<td>(49.205)</td>
</tr>
<tr>
<td>Promise</td>
<td>0.401***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.139)</td>
<td></td>
</tr>
<tr>
<td>Constant</td>
<td>75.500***</td>
<td>9.448</td>
</tr>
<tr>
<td></td>
<td>(26.005)</td>
<td>(14.178)</td>
</tr>
<tr>
<td>Obs.</td>
<td>40</td>
<td>40</td>
</tr>
<tr>
<td>R²</td>
<td>0.199</td>
<td>0.351</td>
</tr>
</tbody>
</table>

Notes: This table shows OLS coefficient estimates (with robust standard errors in parentheses). The dependent variable is the number of tokens (averaged over all three approval rates) that candidates distributed. “Election” is a dummy indicating treatment Election. Random is considered as the reference category. “Promise” is the number of tokens the candidate promised. The results remain qualitatively the same if we use a Tobit model as an alternative. Significance levels are denoted as follows: * p < 0.1, ** p < 0.05, *** p < 0.01.

In treatment, but only 76 tokens in treatment Random. The difference is statistically significant (p = 0.003) according to a Wilcoxon rank-sum test.17

The OLS regression results in column (1) of Table 3 corroborate the nonparametric analysis. We estimated the following linear regression model:

\[ S_i = \alpha + \beta_1 E_i + \epsilon_i, \tag{5} \]

where \( S_i \) is the number of tokens distributed by candidate \( i \) (averaged over all three approval rates) and \( E_i \) is a dummy for treatment Election. The results in column (1) show that candidates distribute 121 tokens more in treatment Election than in Random. In column (2), we additionally control for promises and find that they significantly predict the candidates’ actual office behavior. Remarkably, the coefficient for \( E_i \) is much smaller and is no longer statistically significant when we control for promises, suggesting that the treatment effect is mediated through promises.

In treatment NoCampaign, candidates could not make any pre-election promises. Comparing treatments Election and NoCampaign therefore provides more direct evidence on the influence of promises. As shown in Figure 3, candidates are much more likely to distribute lower numbers of tokens in NoCampaign than in Election. On average, candidates distributed only

---

17We alternatively compared the distributed amount in Election and Random for each approval rate separately and found that all the differences are statistically significant using Wilcoxon rank-sum tests (\( p < 0.05 \)).
41 tokens in NoCampaign. This is significantly less than in treatment Election (Kolmogorov-Smirnov test: \( p < 0.001 \)). Although candidates distributed slightly more tokens in Random than in NoCampaign, the difference is statistically insignificant (0.522).\(^{18}\) We summarize these findings as follows:

**Result 3.** Representatives behave more benevolently when they are democratically elected than when they are randomly appointed by lot. The difference seems to be driven by the less generous promises in the absence of electoral competition. Eliminating the possibility of making campaign promises leads to a corresponding reduction in the monetary payoff for the electorate.

We have shown that the approval rate in an election is a signal of how much voters trust the candidates. The psychological costs of disappointing others should therefore increase with the approval rate in the Election treatment. In contrast, the randomly generated approval rate in treatment Random contains no information about voters’ expectations. And in the NoCampaign treatment, voters have no information about the different candidates, i.e. the voters’ choices can be considered unintentional. We should consequently see a positive correlation between approval rates and the number of tokens distributed in the Election treatment, but not in treatments Random or NoCampaign. Figure 4 provides supporting evidence.

The number of tokens distributed increased markedly with the approval rate in the Election treatment, while it remained much flatter in the other two treatments, where approval rates contained less or no information.

In Table 4, we separately regress the number of tokens distributed by candidate \( i \) on the approval rates \( \frac{k}{n} \) for each treatment. We further included promises \( P_i \) as an additional explanatory variable in our linear regression model for treatment Election and Random (see equation 6). Standard errors are adjusted for clustering of the error term \( \epsilon_i \) on the level of each candidate.

\[
S_i(\frac{k}{n}) = \alpha + \beta_1(\frac{k}{n}) + \beta_2P_i + \epsilon_i,
\]

In the Election treatment (column 1), we find that the coefficient for the approval rate is positive and highly significant (\( p < 0.001 \)). In contrast, the approval rate has a much lower impact on the candidates’ behavior in Random (see column (2)). The coefficient for the approval rate in Random is statistically significant for each approval rate separately (\( p < 0.01 \)), while none of the differences between NoCampaign and Random reach conventional levels of statistical significance.

\(^{18}\)The difference between Election and NoCampaign is statistically significant for each approval rate separately (\( p < 0.01 \)), while none of the differences between NoCampaign and Random reach conventional levels of statistical significance.
proval rate is almost four times smaller than in Election and only reaches marginal significance ($p < 0.1$). We find no significant relationship between approval rates and benevolence in the NoCampaign treatment (see column (3) in Table 4). To test whether the relationship between the approval rate and the voters’ payoff is significantly stronger in Election than in NoCampaign and Random, we pooled the data from all three treatments and added interaction terms between the treatment dummies and the approval rate in column (4). The interaction term for the Election treatment is large and statistically significant. A Wald tests rejects the null hypothesis that the coefficients for “Approval*Election” and “Approval*Random” are equally large ($p = 0.012$). Our last result summarizes these findings:

**Result 4.** *The higher candidates’ approval rates are, the more benevolently do they behave. This relationship is absent or much less pronounced when approval rates are based on random or uninformed voting.*

**Conclusion**

Are campaign promises merely cheap talk? How do democratic elections affect policy making? This paper sheds novel light on these questions using an experiment where the behavior of both representatives and voters can be studied under controlled conditions. Our findings show
Table 4: Approval Rates and Benevolence

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Approval (in %)</td>
<td>2.390***</td>
<td>0.613*</td>
<td>0.163</td>
<td>0.163**</td>
</tr>
<tr>
<td></td>
<td>(0.562)</td>
<td>(0.325)</td>
<td>(0.197)</td>
<td>(0.063)</td>
</tr>
<tr>
<td>Promise</td>
<td>0.427***</td>
<td>0.387*</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.138)</td>
<td>(0.201)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Election</td>
<td>-22.233</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(52.367)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Random</td>
<td>-1.250</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(29.146)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Approval*Election</td>
<td>2.227***</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.602)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Approval*Random</td>
<td>0.450</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.295)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Constant</td>
<td>-133.088**</td>
<td>-37.233</td>
<td>27.750</td>
<td>27.750</td>
</tr>
<tr>
<td></td>
<td>(60.224)</td>
<td>(26.297)</td>
<td>(22.631)</td>
<td>(16.708)</td>
</tr>
</tbody>
</table>

Sample Election Random NoCampaign Full
Obs. 60 60 60 180
R² 0.188 0.258 0.002 0.296

Notes: This table shows OLS coefficient estimates (standard errors are given in parentheses and corrected for clustering on the level of each candidate). The dependent variable is the number of tokens candidates distributed to the electorate for each approval rate. The variable “Approval (in %)” indicates the approval rate. “Election” and “Random” are dummy variables indicating the Election and the Random treatments, respectively. “NoCampaign” is considered the reference category in column (4). The results remain qualitatively the same if we use a Tobit model as an alternative. Significance levels are denoted as follows: * p<0.1, ** p<0.05, *** p<0.01.

that electoral competition promoted candidates to make more generous campaign promises, and that promises are partially fulfilled. Voters were therefore better off in competitive elections rather than if their leaders were randomly appointed – but only if candidates were given the opportunity to make campaign promises. Our analysis of voting behavior showed that voters did not consider promises as cheap talk and that they cast their votes for candidates who promised more. However, this relationship was nonlinear, as voters considered extremely generous campaign promises to be implausible.

These findings have important implications for the advancement of both theoretical work on and the design of democratic institutions. Our empirical evidence is supportive for nascent theoretical approaches considering lying aversion and character in formal models of campaign promises and policy making (see Banks 1990; Callander and Wilkie 2007; Callander 2008; Kartik and McAfee 2007). More generally, our results are informative for the design of social decision-making mechanisms. They suggest that exogenous rather than the democratic assignment of
decision rights, as in office rotation schemes, for example, might produce unwanted side-effects due to less intrinsically motivated representatives. Non-pecuniary motivational effects provide a novel explanation for recent empirical findings showing that elected regulators or judges behave differently than appointed ones (see Besley and Case 2003).

Although our controlled experiment allows for straightforward causal interpretations, this approach also entails limitations. For example, in order to properly identify the psychological cost of lying, we had to impose a higher degree of anonymity than that typically present in the field. How re-election and image concerns interact with psychological motivations is an interesting open question. Some experimental evidence suggests that reputational concerns are complementary and amplify pro-social behavior in social dilemmas (e.g. Brown, Falk and Fehr 2004). Another potential concern is that our student subject pool is not necessarily representative for professional politicians. However, the existing evidence comparing students with non-standard subject pools such as CEOs, public servants, or representative populations suggests that student samples tend to provide a lower bound for the relevance of pro-social behavior (see Fehr and List 2004; Alatas et al. 2009; Falk, Meier and Zehnder N.d.; Cappelen et al. 2011). Moreover, Dawes, Loewen and Fowler (2011) provide evidence suggesting that the relationship between social preferences and political participation is positive. We believe that our experiment provides a simple and parsimonious framework that can be further enriched, opening interesting avenues for future research. For example, one could analyze self-selection by adding a stage where each participant is given the choice of running for office. Other extensions include comparing the influence of high and low fixed wages on office holders or modifying the distribution mechanism such that representatives can discriminate between different types of voter.

Appendix A: Theoretical Framework

In this appendix, we present the main results of the theoretical framework. We assume that the utility of candidate $i$ when she promises $P_i$ in the Political Campaign stage, wins the electoral competition with an approval rate that is equal to $\frac{k}{n} \geq \frac{m}{n} = \frac{n+1}{2n}$, and distributes $S_i$ to voters in the Distribution stage is given by:

$k$ indicates the number of votes for the winner and $m = \frac{n+1}{2}$ is the simple majority.
\[ U_i \left( P_i, S_i, \frac{k}{n}, \beta_i \right) = E + I - S_i - \beta_i C_i \left( P_i, S_i, \frac{k}{n} \right), \quad (7) \]

where \( E \geq 0 \) is the ego rent and \( \beta_i \geq 0 \) is the sensitivity of candidate \( i \) to the costs of lying. \( P_i \) and \( S_i \) are restricted to be positive and lower than the monetary budget, \( I \in \mathbb{R}_+ \). The psychological costs of lying are expressed by

\[ C_i \left( P_i, S_i, \frac{k}{n} \right) = \begin{cases} \frac{k}{n} \frac{1}{2} \frac{(P_i - S_i)^2}{P_i}, & \text{if } P_i > 0 \text{ and } S_i < P_i, \\ 0, & \text{otherwise}. \end{cases} \quad (8) \]

Notice that if \( P_i = 0 \), then the utility of candidate \( i \) strictly decreases in the distributed amount, independent of the approval rate, \( \frac{k}{n} \), and the sensitivity parameter, \( \beta_i \). If \( P_i > 0 \) and \( S_i < P_i \), then it follows that:

1. \( \frac{\partial C_i \left( P_i, S_i, \frac{k}{n}, \beta_i \right)}{\partial S_i} > 0 \): the higher the approval rate of the winning candidate, \( \frac{k}{n} \), the higher are the costs of lying;

2. \( \frac{\partial C_i \left( P_i, S_i, \frac{k}{n}, \beta_i \right)}{\partial P_i} > 0 \): the higher the promise of the winning candidate, \( P_i \), the higher are the costs of lying;

3. \( \frac{\partial C_i \left( P_i, S_i, \frac{k}{n}, \beta_i \right)}{\partial S_i} < 0 \): the higher the amount distributed by the winning candidate to voters, \( S_i \), the lower the costs of lying are;

Candidate \( i \) maximizes (7) with respect to the distributed amount \( S_i \). In any interior solution of the maximization problem it must be that

\[ 1 = \beta_i \frac{\partial C_i \left( P_i, S_i, \frac{k}{n} \right)}{\partial S_i}. \quad (9) \]

Intuitively, in equilibrium the winning candidate chooses the promise, \( P_i \), and the distributed amount, \( S_i \), such that the marginal cost of distributing positive amounts to voters is equal to the corresponding marginal benefit of reducing the costs of lying.

If \( \beta_i = 0 \) or \( P_i = 0 \), then candidate \( i \) distributes nothing in equilibrium \( S_i = 0 \). If \( P_i > 0 \) and \( \beta_i > 0 \), then the optimal distributed amount is

\[ S_i \left( \frac{k}{n}, P_i, \beta_i \right) = \max \left\{ P_i \frac{\beta_i \frac{k}{n} - 1}{\beta_i \frac{k}{n}}, 0 \right\}. \quad (10) \]
Candidates can be of a two types, $H$ and $L$. The two types of candidates differ in the magnitude of the sensitivity parameter, namely $\beta^H > \beta^L > 1$.\textsuperscript{20} This assumption implies that both types distribute a positive amount if elected unanimously when they make strictly positive promises. Let $\phi$ and $(1-\phi)$ be the probabilities that $i$ is an $L$–type and an $H$–type candidate, respectively. Without loss of generality, let us assume $E = 0$. We focus on Perfect Bayesian equilibria, where voters do not play weakly dominated strategies. The following results define the four testable predictions presented in the experimental design. The first proposition states that in electoral competitions, lie-averse candidates use promises strategically to increase their approval rate.

**Proposition 1** In any equilibrium of the electoral game with political campaign, the winning candidate promises a positive amount in equilibrium.

**Proof.** Suppose that there is an equilibrium in which both candidates promise nothing and, therefore, distribute nothing if elected. In this equilibrium, candidate $i$ wins the elections with a probability that is equal to or less than $\frac{1}{2}$. If she deviates and promises $\varepsilon > 0$, then, regardless of her type, she distributes a positive amount when elected unanimously. Hence, voting for the candidate who promises zero is a weakly dominated strategy. All voters vote for candidate $i$, who wins the elections unanimously, $k = n$. Therefore, deviating is profitable for candidate $i$ if and only if $\frac{1}{2} I < I - \varepsilon \frac{\beta_i - 1}{\beta_i} - \frac{1}{2\beta_i} \varepsilon$ which holds as long as $\varepsilon < I \frac{\beta_i}{2\beta_i - 1}$. Finally, suppose that there exists an equilibrium in which one candidate promises zero, her opponent promises a positive amount, and the former is elected. This equilibrium contradicts the assumption that voters do not play weakly dominated strategies. \hfill \blacksquare

By proposition 1, candidates make positive promises in the political campaign. By combining this result with equation (10), it follows that voters receive positive payoffs in equilibrium.

**Corollary 2** In any equilibrium of the game with a political campaign, the winning candidate partially fulfills her promises and distributes a positive amount.

Of course, the elections are beneficial for voters if and only if candidates truly compete through promises in the political campaign. If either the political campaign stage is removed or the winner of the elections is randomly selected, then voters’ equilibrium payoff is 0. This is formally stated in the next proposition.

\textsuperscript{20}We discuss the extension where some candidates are purely selfish later in the text.
Proposition 3 If either the winning candidate is randomly appointed or the political campaign stage is removed from the electoral game, then candidates promise nothing and, in equilibrium, the winning candidate distributes nothing to voters.

Proof. If the winning candidate is randomly appointed, then the promise made in the political campaign does not influence the probability of winning the elections and neither candidate promises anything. Similarly, if the political campaign stage is removed, \( P_i = 0 \) for both \( i = A, B \). Therefore, by equation (10), the winning candidate distributes nothing to voters in either situation. ■

Corollary 4 Voters are better off when candidates compete for appointment by making promises in the political campaign stage.

Now, let us describe a standard pooling equilibrium of the electoral game in which voters do not play weakly dominated strategies. Regardless of their type, both candidates promise \( I \). If candidate \( i \) is elected with \( k \) votes, she distributes \( I \frac{\beta_i^{k-1}}{\beta_i^k} \), with \( \beta_i = \beta^H \) if \( i \) is an \( H \)-type and \( \beta_i = \beta^L \) if \( i \) is an \( L \)-type. Each voter casts his vote for the candidate who makes the largest promise, while they vote randomly if both candidates make the same promise. Each voter assigns a probability of \( \phi \) to \( i \) being an \( H \)-type candidate when she promises \( I \), while he assigns a probability of 1 to \( i \) being an \( L \)-type candidate when she makes any other promise. In equilibrium, the expected payoff of candidate \( i \) is
\[
\frac{1}{2} \sum_{k=m}^{n} \frac{1}{n-k+1} I \frac{1}{\beta_i^k} > 0.
\]

If she deviates, all voters vote for the other candidate and her payoff is null. When both candidates promise the same amount, the deviation of a voter is irrelevant. If candidate \( -i \) promises less than \( I \), then deviating and voting for \( -i \) reduces voters’ payoffs. Indeed, candidate \( i \) that promises \( I \) wins with \( n-1 \) votes and distributes \( \max \left\{ I \frac{\beta_{-i}^{n-1}}{\beta_i^{n-1}}, 0 \right\} < I \frac{\beta_{-i}^{n-1}}{\beta_i^{n-1}} \). In the pooling equilibrium, both candidates make the same promise. However, candidates in our experiment make different promises and voters vote with higher probability for the candidate who makes the largest promise. These empirical findings are consistent with our model if we introduce a natural assumption on voters’ behavior: If both candidates make the same promise, then each voter casts his vote randomly. This assumption rules out unreasonable separating equilibria such as a situation in which, regardless of their type, candidate \( A \) promises \( 0 < P_A < I \), candidate \( B \) promises \( P_B = I \) and all voters vote for candidate \( A \) although this
is detrimental for their expected payoff. We now turn our attention to separating equilibria in which candidates make different promises.

**Proposition 5** In any symmetric separating equilibrium, each voter votes for the candidate who makes the largest promise with probability greater than $\frac{1}{2}$.

**Proof.** In a symmetric separating equilibrium, an $H$-type candidate promises $P^H$ and an $L$-type candidate promises $P^L$, with $P^H \neq P^L$. By contradiction, suppose that voters vote for the candidate who makes the lowest promise with probability $\pi > \frac{1}{2}$. Without loss of generality, suppose $P^H > P^L$. If candidate $i$ is an $H$-type, she makes the largest promise $P^H$ and her expected payoff is given by:

$$
(1 - \phi) \sum_{k=m}^{n} \binom{n}{k} (1 - \pi)^n \left[ I - P^H + \frac{1}{2\beta_H^H} \frac{P^H}{n} \right] + \phi \sum_{k=m}^{n} \binom{n}{k} \frac{1}{2^n} \left[ I - P^H + \frac{1}{2\beta_H^H} \frac{P^H}{n} \right].
$$

(11)

If candidate $i$ deviates and promises $P^L$, she gets:

$$
(1 - \phi) \sum_{k=m}^{n} \binom{n}{k} \frac{1}{2^n} \left[ I - P^L + \frac{1}{2\beta_L^L} \frac{P^L}{n} \right] + \phi \sum_{k=m}^{n} \binom{n}{k} \pi^n \left[ I - P^L + \frac{1}{2\beta_L^L} \frac{P^L}{n} \right].
$$

(12)

Since $\pi > \frac{1}{2}$ and $P^L < P^H$, the deviation is profitable. $\blacksquare$

It is easy to show that separating equilibria exist for a non empty set of parameters. For instance, when $\beta^H$ is large enough, $\beta^L$ is small enough and $\phi$ is large enough, there exists a separating equilibrium in which: (i) $L$-type candidates promise $I$ and distribute $I \beta^L_{k-1} \frac{k-1}{k}$ when they win with $k$ votes; (ii) $H$-type candidates promise $P^H < I \beta^H \frac{1}{(\beta^H - 1)}$ and distribute less than $L$-type candidates for any approval rate; (iii) voters’ beliefs are such that they assign a probability of 1 to a candidate being an $L$-type when she promises strictly more than $P^H$, and a probability of 1 to a candidate being an $H$-type when she promises less than $P^H$; (iv) voters vote for the candidate they expect (conditional on their beliefs) to be the most benevolent if elected. If voters expect the two candidates to distribute the same amount, they vote for the candidate who makes the larger promise. Finally, if the two candidates make the same promise, voters cast their votes randomly.

The previous equilibrium has a simple intuition. An $L$-type candidate wins against an $H$-type, while a candidate wins with a probability of 1/2 against an opponent of the same
type. If $\beta^H$ is large enough, for an $H-type$ candidate, competing against an $L-type$ candidate is "too costly": she promises the entire budget, $I$, and, if elected, distributes a large amount to voters. Therefore an $H-type$ candidate prefers to reduce her promise and win the elections with lower probability. Namely, she wins the elections with a probability of $1/2$ when she competes against an opponent of the same type (a situation that occurs with a probability of $1 - \phi$). In contrast, if $\phi$ is large enough and $\beta^L$ is small enough, an $L-type$ candidate competing against an opponent of the same type prefers to promise the entire budget, $I$, and win the elections with a probability of $1/2$.

We conclude with two remarks. First, some candidates in the Election treatment distribute more than they promised. Moreover, some candidates in the other two treatments, Random and NoCampaign, distribute positive amounts (which are nevertheless significantly lower than the distributed amounts in Election). Also, the amounts candidates distribute are not correlated with the approval rate in NoCampaign and weakly correlated with promises in Random. Our model can be easily extended to account for these empirical results by assuming that candidates (also) exhibit preferences for egalitarianism. For instance, consider the following extension of the utility function of candidate $i$:

$$U_i(P_i, S_i, \frac{k}{n}, \beta_i, \alpha_i) = I + E - S_i - \beta_i C_i \left( P_i, S_i, \frac{k}{n} \right) - \alpha_i \max \left[ 0, \frac{1}{2} (I - \frac{n}{n+1} - S_i)^2 \right], \quad (13)$$

where $\alpha_i \geq 0$ is the sensitivity of candidate $i$ to egalitarianism. By (13), when either $\beta_i = 0$, or $P_i = 0$, or under random appointment, the winning candidate distributes $S_i = \max \left\{ I \frac{n}{n+1} - \frac{1}{\alpha_i}, 0 \right\}$ for egalitarian concerns.

Second, we find in our experiment that excessively high promises generate distrust. In order to rationalize this evidence, our model can be extended to include a third type of (selfish) candidate, denoted $O-type$, such that $\beta^O = 0$. Selfish candidates always distribute zero if they win the elections and their promises are mere cheap talk. Consider a model with three types: $O, H$ and $L$. As follows, we provide the intuition of how introducing the $O-type$ candidates can change the previous results. A separating equilibrium cannot exist because voters never vote for a selfish candidate. If the probability that a candidate is an $O-type$ is sufficiently
high, then there pooling equilibria exist such that, regardless of their type, candidates promise a positive amount $\hat{P} \leq I$ and voters assign a probability of 1 to a candidate being selfish if she promises more than $\hat{P}$. On the other hand, if the probability that a candidate is an $O-type$ is sufficiently low, there also exist semi-pooling equilibria in which $L-type$ and $O-type$ candidates promise $P^{L,O} \leq I$ and $H-type$ candidates promise $P^H$, with $P^H < P^{L,O}$. Voters vote for the candidate who promises $P^{L,O}$ if the other candidate promises $P^H$, and voters assign probability 1 to a candidate being selfish when she promises more than $P^{L,O}$. Hence, making an excessively large promise generates distrust and reduces the probability of winning the elections.\footnote{Indeed, it is easy to show that assigning probability 1 to a candidate being selfish when she promises more than $\hat{P}$ ($P^{L,O}$) in the pooling (semi-pooling) equilibria is the unique profile of voters' beliefs that satisfies standard refinement criteria for Bayesian equilibria, such as the D1 criterion.}
Appendix B: Summary Statistics

Table 5: Subjects’ Background Statistics

<table>
<thead>
<tr>
<th>Variable</th>
<th>Mean</th>
<th>Std. Dev.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Age</td>
<td>24.819</td>
<td>6.509</td>
</tr>
<tr>
<td>Male</td>
<td>0.543</td>
<td>0.499</td>
</tr>
<tr>
<td>Natural sciences</td>
<td>0.271</td>
<td>0.445</td>
</tr>
<tr>
<td>Economics</td>
<td>0.237</td>
<td>0.426</td>
</tr>
<tr>
<td>Law and politics</td>
<td>0.217</td>
<td>0.413</td>
</tr>
<tr>
<td>Other humanities</td>
<td>0.275</td>
<td>0.448</td>
</tr>
</tbody>
</table>

Notes: This table shows the summary statistics for the subjects age, gender and field of study. The total sample size is 210. Three subjects did not provide their field of study.
References


