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(Dis)honest Politicians and the Value of Transparency for Campaign Promises

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Abstract

Promises are prevalent in many competitive environments, but promise keeping is often difficult to observe. Do promises still offer an opportunity to honor future obligations, if promise keeping is unobservable? Focusing on campaign promises, we study the value of transparency. We show how preferences for truth-telling shape promise competition when promise keeping can(not) be observed. We identify the causal effects of transparency in a laboratory experiment. Transparency leads to less generous promises, but also to less promise breaking. Nonetheless, officials appropriate similar rents as in opaque institutions. Preferences for truth-telling and (instrumental) reputational concerns explain these results.

JEL-Codes: C910, C920, D720, D730, D910.

Keywords: prospective voting, retrospective voting, promises, promise breaking, lying costs, preferences for truth-telling, experiment, political economy.

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1 Introduction

In many economic environments, individuals make promises and compete in promises. If reputational incentives or preferences for truth-telling are at play, promises offer the possibility to honor future obligations, when formal contracting or commitment to future actions is impossible. For example, in markets with asymmetric information, sellers compete in promises about product or service quality (e.g., Beck et al., 2013; Brosig-Koch and Heinrich, 2018) and principals as well as agents compete in promises within and between organizations (e.g., Ellingsen and Johannesson, 2004; Fahn and Hakenes, 2019). Most saliently, in democratic elections, candidates compete in campaign promises about policy actions (Thomson et al., 2017). Seminal work in political economy (Persson et al., 1997; Aragonès et al., 2007) and political science (Key et al., 1966) underlines the importance of campaign promises. Focusing on the instrumental value from promise keeping, this literature argues that politicians can use promises as commitment because voters will not reelect politicians who broke past promises. An important aspect so far neglected in the literature is how a lack of information about promised actions changes promise competition. Promises may fail as commitment when voters lack trust in the media providing such information or voters' perceptions about promise keeping diverge substantially from actual promise keeping; as recent survey evidence suggests (Naurin, 2011; Thomson, 2011). In turn, promises may lose their value when transparency is missing.

Our study investigates this important aspect of promise competition. Focusing on campaign promises, we investigate how promise competition changes if voters can(not) observe whether politicians keep or break their promises (opacity).¹ We consider a simple voting environment in which two candidates compete in campaign promises about how to allocate an endowment among themselves and the citizens. Voters observe promises and – in some institutions – the incumbent's past promise keeping before they vote for one of the two candidates. After the vote, the elected candidate decides on the allocation. We analyze an infinitely repeated game of such promise competition. Our main interest

¹As a first step, we focus on opacity as an exogenously determined institutional factor. For example, the quality of media or trust in the media varies across countries and may be exogenous to voters. In Section 5, we highlight interesting avenues for future research on campaign promises when opacity is endogenously determined, e.g., through intentional vagueness in communication (Blume and Board, 2014; Agranov and Schotter, 2012; Serra-Garcia et al., 2011), deniable statements (Tergiman and Villeval, 2019) or voters' decisions to acquire or ignore information on promise keeping (Matějka and Tabellini, 2017).

lies in understanding how transparency about the incumbent's promise keeping affects promises and rent appropriations. Considering transparency an exogenous institutional factor, we study its causal effect on promises, promise keeping and rent appropriations. Thus, in our setting, candidates can choose the generosity of promises and how much rents to appropriate but not whether promise keeping is observed.

We build on a large body of experimental evidence about preferences for truth-telling (for recent meta-analyses see Abeler et al., 2019; Gerlach et al., 2019), which highlight individuals' preferences for being honest, and being perceived as honest when observed by others. First, we conceptualize theoretically how opacity and preferences for truth-telling affect promise competition in elections. Second, we provide empirical evidence on causal effects of opacity on promise competition. As voters' ability to observe promise keeping may be endogenously determined in observational data, we use a laboratory experiment to identify how opacity changes the nature of promise competition. In the experiment, we vary exogenously whether voters can observe promise keeping, while holding all other aspects constant. Furthermore, our setting enables us to study how promise competition changes, when voters do not observe promise keeping but independent third parties do. Finally, the experiment allows us to measure promise-keeping norms held by decision-makers as well as third-party observers. Adopting methods introduced by Krupka and Weber (2013), we do so in an incentive-compatible way. As we elicit both injunctive and descriptive norms, we are able to discuss how opacity affects whether promises ought to, and are expected to be kept.

Our findings reveal that opacity systematically shapes the nature of promise competition. From a theoretical perspective, when determining optimal promises, candidates compare utilities from being in office with their utilities when not being (re)elected. In the model, politicians are expected to keep their promises in transparent institutions whereas in opaque institutions voters expect promise breaking according to officials' preferences for truth-telling. Hence, in opaque institutions, promises are expected to be more generous but also more frequently broken, which is both clearly born out in our data. The model's predictions about rent appropriations hinge on the shape of preferences for truth-telling which determine the credibility of promises and thereby imply more or less rent appropriations in opaque as compared to transparent institutions. With transparency, the credibility of promises is bound by the fact that voters anticipate very generous promises to be broken and, hence, do not trust such promises. In opaque institu-

tions, honest candidates want to win the election to avoid promise breaking and large rent appropriations by less honest competitors. Hence, even very generous promises occur and are sometimes kept. If so, transparency may not be valuable for citizens. Empirically, opaque institutions indeed do not result in more rent appropriations than transparent institutions. First, opacity yields more generous promises, and some officials indeed keep very generous promises with opacity. Second, some promises are also broken in transparent environments. In line with the instrumental reputational loss for observed promise breaking, candidates, who break their promises, more frequently appropriate all available rents. Both effects reduce the value of transparency.

Further, we decompose the effects of opacity on behavior due to the lack of transparency and accountability, theoretically and experimentally. Lack of transparency refers to institutions in which nobody can observe promise keeping, while lack of accountability refers to institutions in which voters cannot observe promise keeping, but independent observers do. Compared to transparent institutions, lack of accountability makes promises more generous and lack of transparency further strengthens this effect. Finally, we show that opacity does not affect injunctive norms about promise keeping. In all institutions, a large majority of decision-makers, as well as independent observers agree that promises ought to be kept. Hence, opacity does not change the fact that voters interpret promises literally (see also Casella et al., 2018). Instead, opacity influences to what extent preferences for truth-telling rather than the instrumental value from promise keeping shape promise making, promise keeping and rent appropriations.

Our work complements recent findings on promise competition and preferences for truth-telling. Studying an infinitely repeated voting environment (introduced by Persson et al., 1997), our setting relates most closely to Feltovich and Giovannoni (2015). They investigate retrospective-prospective voting in repeated elections. They study comparative statics in discount factors in transparent institutions, in which – without campaign promises – only retrospective voting should matter. Most importantly, they show that retrospective-prospective voting arises when allowing for campaign promises.² Building on their work, we confirm the robustness of their findings in transparent institutions. The novelty of our approach lies in contrasting transparent and opaque institutions. We show how promise competition changes when voters are unable to observe promise keeping

²Feltovich and Giovannoni (2019) provide further interesting insights on how behavior changes in environments with pre-election polls, where candidates can have different (exogenously assigned) types.

(opacity). Our results reveal that – even without retrospective voting based on promise keeping – candidates compete in promises. As candidates care about being honest as well as being perceived as honest, preferences for truth-telling systematically shape the nature of promise competition in opaque institutions.

We further connect to research on promise keeping in one-shot games (Charness and Dufwenberg, 2006; Vanberg, 2008; Geng et al., 2011; Corazzini et al., 2014; Ederer and Stremitzter, 2017; Casella et al., 2018; Agranov et al., 2020; Born, 2020; Ederer and Schneider, 2020). While these studies have shown that individuals are willing to keep their promises, when the possibility of punishment is missing³, we provide novel evidence that candidates are even willing to keep their promises, when voters do not learn about promise keeping. Furthermore, by introducing independent observers in an otherwise opaque institution, we show that the nature of promise competition not only hinges crucially on the existence (or the lack) of reputational concerns, but also on self-image and social-image concerns. These findings highlight how recent insights on preferences for truth-telling (Gneezy, 2005; Mazar et al., 2008; Fischbacher and Foellmi-Heusi, 2013; Abeler et al., 2019; Gneezy et al., 2018; Khalmetski and Sliwka, 2019) and on the importance of observability of dishonest behaviors (Gneezy et al., 2018; Khalmetski and Sliwka, 2019; Agranov and Buyalskaya, 2020) apply to democratic processes.

More generally, our results link to literature concerned with the effects of inferior information on politicians' actions (see, e.g., Prat, 2005; Dal Bo, 2007; Levy, 2007; Mattozzi and Merlo, 2007; Gavazza and Lizzeri, 2009; Ashworth et al., 2017; Fehrler and Hughes, 2018; Hinnosaar, 2019). For instance, Gavazza and Lizzeri (2009) show that transparency about government revenues can be counterproductive for fiscal policies. In a similar vein, we find theoretically that transparent institutions are not necessarily superior. Instead, a lack of transparency about promise keeping even benefits voters if some candidates care very little about promise breaking but average preferences for truth-telling in the population are strong. Intuitively, candidates who care very little about promise breaking make generous promises in transparent institutions non-credible whereas strong preferences for truth-telling ensure that generous promises are often kept in opaque institutions. Empirically, we find that voters indeed do not suffer substantially from a lack of trans-

³See e.g. Brandts and Charness (2003) and Agranov and Buyalskaya (2020) for one-shot games with promises and punishment.

parency about promise keeping, highlighting that promise competition can be beneficial to voters, even if they cannot observe whether promises are kept.

The rest of this article is organized as follows. In Section 2, we present theoretical guidance for a voting environment with and without opacity when preferences for truth-telling are taken into account. In Section 3, we explain our experimental design. Section 4 presents the empirical results on voting behavior, promises, promise keeping and rent appropriations. In Section 4.5, we discuss our main results and present evidence on how opacity affects norms held by decision-makers. Section 5 concludes.

2 Theoretical Guidance

2.1 The Setting

We build on the voting environment introduced in the seminal works of Persson et al. (1997) and Feltovich and Giovannoni (2015) and consider a group of n identical and infinitely lived voters with an odd $n \geq 5$. In each period, the group receives an endowment M that is distributed among its members by an official. In the first period, the official is selected randomly among the group members. In each subsequent period, the incumbent and a randomly chosen challenger contest an election by making a campaign promise $\pi \in [0, 1]$ about the share of the endowment they intend to keep for themselves. The campaign promise is a cheap-talk message without any commitment to certain policies. Apart from the challenger's promise, voters get no additional information about the challenger. In particular, they do not know whether the challenger was an official before, or any previous choices of the challenger. Then the election takes place. Each group member has one vote and – for simplicity – abstention is not possible. The candidate who receives the majority of votes wins the election. Then the elected official chooses her salary $s \in [0, 1]$ as a share of the endowment M . Accordingly, the official obtains sM , and each other group member receives an equal share of the remainder, i.e., $(1 - s)M/(n - 1)$. Finally, we assume a common discount factor $\delta \in [0, 1)$.

Inspired by recent empirical evidence (see Abeler et al., 2019; Gerlach et al., 2019), we assume that individuals have preferences for truth-telling. For this purpose, we define lying costs $C_i(\cdot)$ and individual i 's per-period utilities as an elected official of

$$sM - C_i(s - \pi)$$

if she chooses a salary s after a campaign promise π .⁴ Building on the insights in the literature, we assume that $C_i(x) = 0$ for all $x \leq 0$ and $C_i(x) = \lambda c_i(x)$ for all $x > 0$ with a constant $\lambda \geq 1$ and a non-negative, increasing, and convex function $c_i(\cdot)$; see e.g. Lacker and Weinberg (1989), Maggi and Rodriguez-Clare (1995), Crocker and Morgan (1998), Kartik and McAfee (2007); Kartik (2009), and Deneckere and Severinov (2017). Lying costs are heterogeneous and may result from preferences for being honest (self-image concerns) or being perceived as honest (social-image concerns, see also Abeler et al., 2019; Gneezy et al., 2018; Khalmetski and Sliwka, 2019).⁵ While for observable promise breaking both, self-image and social-image concerns matter ($\lambda \geq 1$), unobservable promise breaking can only cause lying costs through self-image concerns ($\lambda = 1$).

To capture heterogeneity in lying costs, we consider a family of positive, increasing, and convex functions $c^j(\cdot)$, $j \in [0, 1]$. We characterize each lying-costs function $c^j(\cdot)$ by a value $\gamma^j \in \mathbb{R}$ defined by $(c^j)'(\gamma^j) = M$. As a normalization, we assume that $\gamma^0 = 0$, that is, an individual i with lying costs $C_i(x) = \lambda c^0(x)$ keeps any promise. Further, we assume $\gamma^1 = 1$, that is, an individual i with lying costs $C_i(x) = \lambda c^1(x)$ is willing to appropriate all available rents absent instrumental reputational concerns. Finally, we assume γ^j increases in j . The higher j is, the larger is the extent of promise breaking. Preferences for truth-telling in the population follow a distribution $G(j)$ with a mass point at 0 reflecting the empirical evidence that some individuals are honest and do not lie.⁶ Each voter i 's lying costs $c_i(\cdot)$ are drawn independently from the family $c^j(\cdot)$ according to this distribution $G(j)$.

Relating to our experimental analyses, we study three different institutional settings. First, we assume that every voter can observe the official's salary choice in *transparent* institutions. Then, we consider *opaque* institutions, that hinder voters from observing promise keeping. For simplicity, we assume in our theoretical considerations that voters cannot observe the official's salaries nor their own payoffs in *opaque* institutions.⁷ Finally, we discuss the relevance of social-image concerns using an opaque institution

⁴We specify the lying costs on the percent deviation from the promise $s - \pi$. The analysis does not change, however, if we were to specify the lying costs on the absolute deviation from the promise $M(s - \pi)$.

⁵These models build on social identity resulting from both "how individuals see themselves and how others see them" (Kranton, 2016, p. 407).

⁶This assumption is also in line with our measures of injunctive norms as a large majority of decision-makers in our sample thinks promises ought to be kept.

⁷If voters can observe their own payoffs and know the size of the endowment they can infer the official's salary from their payoffs. An alternative assumption is unknown endowments, as used in the experiment. See Section 3 for more details.

with independent observers (*opaque with observers*). In the latter, we assume that the official's salary choice is only observed by independent observers, but not by voters.

Following Feltovich and Giovannoni (2015, Section 2.2), we restrict attention to retrospective-prospective equilibria and derive predictions that take lying costs into account. Appendix A.1 describes this class of equilibria in more detail (including voting behavior in such equilibria) which has received empirical and theoretical support (Feltovich and Giovannoni, 2015; Persson et al., 1997). In these equilibria, candidates compete in promises and voters choose the candidate with the most generous offer that is still credible. A promise is credible, if it maximizes the probability that the official keeps such a promise if elected.

2.2 Lying behavior

Solving the optimization backwards, we begin by calculating the salary that maximizes the official's per-period utilities given a promise π .⁸

Lemma 1. *The salary that maximizes official i 's per-period utilities given a promise π equals*

$$\bar{s}_i(\pi) = \begin{cases} \pi + \gamma_i & \text{if } \lambda c'_i(1 - \pi) \geq M \text{ and } c_i(0) \leq \gamma_i M / \lambda - \int_0^{\gamma_i} c'_i(\tilde{x}) d\tilde{x} \\ 1 & \text{if } \lambda c'_i(1 - \pi) < M \text{ and } c_i(0) \leq (1 - \pi)M / \lambda - \int_0^{1-\pi} c'_i(\tilde{x}) d\tilde{x} \\ \pi & \text{otherwise.} \end{cases}$$

with γ_i determined by $\lambda c'_i(\gamma_i) = M$.

The first case reflects the trade-off between costs and benefits from lying. The optimal amount of lying is determined such that the marginal costs of lying, $\lambda c'_i(\cdot)$, equal the marginal benefits of lying, M . Therefore, the optimal salary equals the promise plus a constant γ_i determined by the curvature of the lying costs. The value γ_i measures the extent of promise breaking absent instrumental reputational concerns, i.e., the wedge between promise and salary. The second and third case consider the two corner solutions. The optimal salary is either equal to 1 (maximal lying) if the promise is close to 1 already or equal to the promise (honesty) if the fixed costs of lying, $c_i(0)$, are large. For the further

⁸In our repeated game, officials do not necessarily want to maximize per-period utilities separately (except for myopic officials with $\delta = 0$) but this maximum serves as a useful benchmark.

analysis, we focus on those cases in which the salary choice is interesting and assume $c^j(0) = 0$ for all $j \in [0, 1]$ to avoid unnecessary case distinctions.

2.3 Promises in Transparent Institutions

In *transparent* institutions, voters can observe promise keeping, as they can retrospectively compare the incumbent's promised and chosen salaries. In addition, they prospectively care about the attractiveness of current promises. Instrumental reputational concerns make it possible that some promises are kept by all officials. Hence, a promise is credible if voters expect every official to keep such a promise. Such a promise cannot be very generous because it has to ensure the official earns sufficient office rents to make staying in office more profitable than breaking promises and losing the next election. The less generous a promise is, the higher the implied office rents are for the official.

Proposition 1. *In transparent institutions, promising and, if elected, choosing a salary of*

$$\pi_T^* = \max \left\{ \frac{1}{n}, 1 - (n-1) \frac{\delta + (1-\delta) \frac{\lambda}{M} c^1(1 - \pi_T^*)}{n - 1 + \delta^2} \right\}$$

is optimal for every candidate. The incumbent is reelected each period.

The value π_T^* is the most generous promise that is still credible because this value ensures that every official keeps her promise. Hence, the probability for keeping this promise equals one. In a deviation, the official may appropriate all available rents and gain additional payoffs of $M(1 - \pi_T^*)$ incurring lying costs of $\lambda c_i(1 - \pi_T^*)$. In the following period, she loses the election to her challenger and expects lower payoffs in the next periods. Candidates who care very little about promise breaking value the gains of such a deviation the highest. Thus, the office rents must be sufficiently high to deter such candidates from deviating and the marginal candidate to deter is the candidate with $c_i(\cdot) = c^1(\cdot)$. Hence, candidates who care very little about promise breaking make very generous promises in transparent institutions non-credible.

Intuitively, in prospective-retrospective equilibria, voters think both retrospectively and prospectively. Retrospectively, voters do *not* vote for the incumbent, if she broke her promise in the past. Prospectively, voters vote for the incumbent, if her promise is credible and more generous than credible promises by the challenger. Therefore, it is optimal for the incumbent to make the most generous credible promise. Otherwise, the

incumbent loses the election to her challenger and, thus, the implied office rents. The incumbent keeps her promises because the promise was credible (as implied by the value of the promise π_T^* in Proposition 1). Vice versa, each challenger makes a promise of π_T^* . Changing her promises, the challenger cannot ensure election.

Finally, lying costs affect equilibrium promises and salaries, as they determine the range of credible promises. The higher lying costs are, the lower are the utility gains from deviating to a higher salary for any given promise. Therefore, high lying costs increase the range of credible promises by lowering π_T^* . Vice-versa, low lying costs decrease the range of credible promises by increasing π_T^* . Furthermore, the more patient voters are, the less one-shot effects, like lying costs, matter. In the limit, $\delta \rightarrow 1$, the most generous credible promise converges to the equal split, $\pi_T^* \rightarrow 1/n$.

2.4 Promises in Opaque Institutions

In an *opaque* institution, voters cannot compare the incumbent's promised and chosen salaries. Thus, retrospective voting is impossible. Voters can only vote prospectively on current promises. Thus, instrumental reputational concerns do not matter in *opaque* institutions. Hence, voters cannot guarantee that the official keeps her promise. Nonetheless, preferences for truth-telling ensure that some officials keep their promises. As in the *transparent* institution, voters consider a promise to be credible if it maximizes the probability that the official keeps such a promise. Voters then choose the candidate with the most generous promise that is still credible. Candidates compare the utilities from winning to losing the election. Expected utilities of losing the election are the same for all candidates and do not depend on lying costs. The utilities of winning the election are lower for candidates with stronger preferences for truth-telling because they choose lower salaries for a given promise. Therefore, they would be the first to drop out of the promise competition. To maximize the probability that the official keeps her promise, we have to ensure that everybody stays in the promise competition and prefers winning the election with such a promise. Consequently, an implication of credibility in *opaque* institutions is the following: A promise is credible if and only if every candidate prefers winning the election with such a promise to losing the election. Candidates compare

the utilities from being in office with their utilities when not being elected. If a candidate i wins the election with a promise π_i , her utilities are

$$(\pi_i + \gamma_i)M - C_i(\gamma_i).$$

If a candidate loses the election to a challenger j with a promise π_j , her utilities are

$$\frac{\mathbb{E}(\max\{0, 1 - \pi_j - \gamma_j\})}{n - 1}M$$

Proposition 2. *Suppose $\text{Prob}[\gamma^j > \frac{1}{n}(n - 1 + \mathbb{E}(\gamma^j))] = 0$. In the opaque institution, promising a salary of*

$$\pi_O^* = \frac{1 - \mathbb{E}(\gamma^j)}{n}$$

is optimal. If candidate i is elected, she chooses a salary of $s_O^ = \pi_O^* + \gamma_i$. The proof (in the appendix) contains the general solution. In general, reelection can occur. Promise breaking and chosen salaries are distributed according to the truncated distribution G of lying costs.*

The condition in the proposition is satisfied if the population of voters does not contain very dishonest individuals.⁹ The condition significantly simplifies the exposition of π_O^* but yields the same intuition as the general results in the proof.

Some candidates keep their promises in equilibrium but often promises are more generous than salaries. To ensure credibility, the most honest voter has to be willing to make such a promise. In this case, she prefers winning the election and receiving payoffs of $M\pi_O^*$ to losing the election and receiving payoffs of

$$(1 - \mathbb{E}(\bar{s}_j(\pi_O^*))) \frac{M}{n - 1}.$$

The most generous promise that is still credible is below the equal split because candidates expect their competitor to break their promises and to choose salaries above the equal split, $\mathbb{E}(s_O^*) = \frac{1}{n} + \frac{n-1}{n}\mathbb{E}(\gamma^j)$. This argument does not involve any intertemporal or reputational considerations. Hence, promises do not depend on time preferences.

Opaque institutions with observers If the official's salary is observable by independent observers but not by voters, the above analysis in Proposition 2 remains qualitatively

⁹The condition ensures that the expectation for the distribution G equals the expectation for the distribution truncated above at $1 - \pi_O^* = \frac{1}{n}(n - 1 + \mathbb{E}(\gamma^j))$.

unchanged. The lying costs are higher, however, as lying feels worse and preferences for truth-telling become more important due to social-image concerns. The increased importance of preferences for truth-telling is captured by an increase in λ as detailed in the beginning of this section. Therefore, the amount of promise breaking γ_i decreases for each voter. The values in Proposition 2 have to adjust to this shifted distribution \tilde{G} for $\lambda \geq 1$. Each candidate anticipates these lower salaries by their competitors compared to the *opaque* institution without observers. Therefore, for a given promise, the expected utilities of loosing the election increase. Thus, the most honest voter makes her promise less generous because it is less important for her to win the election. Consequently, the range of credible promises decreases and equilibrium promises increase.

2.5 Comparison of Promises between Institutions

Our theoretical model allows us to predict comparative statics across different institutions. We contrast institutions that are *transparent* (i.e. voters can observe promise keeping), *opaque* (i.e. only the official knows whether the promise was kept) and *opaque with observers* (i.e. only the official and a group of independent observers learn whether a promise was kept). The first two institutions relate to our main research question and highlight how opacity changes the nature of promise competition. The third institution is crucial for two reasons: first, it allows us to identify the role of self-image and social-image concerns for promise keeping, and to put it into perspective (as compared to instrumental reputational concerns in the *transparent* institution). Second, the *opaque* institution with independent observers resembles situations of promise competition, in which internal review boards, compliance departments or internal auditing observe behavior in office, but cannot credibly communicate observed misconduct to voters. Thus it also sheds light on the question, whether and how partial transparency reduces promise breaking.

Intuitively, *transparent* institutions rely on instrumental reputational concerns which require reelection of the incumbent. Opaque institutions rely on self-image and social-image concerns that do not depend on reelection. Hence, reelection probabilities are predicted to be higher in *transparent* institutions as compared to *opaque* institutions. We summarize these considerations in Prediction 1.

Prediction 1. *Reelection is more likely in transparent than in opaque and opaque with observers institutions.*

The most important predictions concern promises and promise keeping. In *transparent* institutions, voters do not trust very generous promises as keeping such promises implies little office rents. For such promises, voters anticipate promise breaking by officials. Hence, very generous promises are non-credible. Promise competition is weak and promises are less generous. In *opaque* institutions, candidates compare their utilities of winning the election to the utilities of not being elected. They anticipate their competitor to break promises – leaving them (potentially) worse off. Thus, winning the election is important. Promise competition is strong and promises are more generous. Therefore, promises are predicted to be more generous in *opaque* as compared to *transparent* institutions.

Prediction 2. *Promises are more generous in opaque than in opaque with observers than in transparent institutions.*

Concerning promise breaking, retrospective voting disciplines officials to keep their promises in equilibrium in *transparent* institutions, whereas in *opaque* institutions *with observers*, social-image concerns matter in addition to self-image concerns. Our model predicts that the additional reputational concerns make promise breaking less likely in *transparent* institutions as compared to *opaque* institutions. The extent of promise breaking is similarly predicted to be lower in *transparent* institutions as compared to both *opaque* institutions. Further, we expect a lower extent of promise breaking in *opaque with observers* as compared to the *opaque* institution due to additional social-image concerns. We summarize these considerations in Prediction 3 and 4.

Prediction 3. *The extent of promise breaking is smaller in transparent than in opaque with observers than in opaque institutions.*

Prediction 4. *Promise breaking is less likely in transparent than in opaque and opaque with observers institutions.*

As our model incorporates social-image concerns by the factor λ , we predict salaries to be larger in *opaque* institutions as compared to *opaque with observers*. Considering comparisons of *transparent* and *opaque* institutions, our model predicts ambiguous effects. The model makes a clear prediction only conditional on promise keeping.

Prediction 5. *Average salaries can be higher or lower in opaque as compared to transparent institutions. Conditional on promise keeping, average salaries are higher in transparent than in opaque institutions.*

According to Prediction 2, there is a clear comparison of promises between *opaque* and *transparent* institutions and, hence, also for average salaries conditional on promise keeping. Nevertheless, the (unconditional) comparison of average salaries is ambiguous because it depends on the average extent of promise breaking and whether some candidates care very little about promise breaking. Focusing on the parameterization used in the experiment ($n = 5$ and $\delta = 0.8$) allows for additional predictions. Candidates who care very little about promise breaking make generous promises in transparent institutions non-credible. Then, promises in *transparent* institutions are much higher than in *opaque* institutions, $\pi_T^* \gg \pi_O^*$. A small extent of promise breaking ensures that generous promises result in low salaries in *opaque* institutions. Thus, if some candidates care very little about promise breaking and the extent of promise breaking is not too large, salaries in *opaque* institutions cannot catch up and are lower than in *transparent* institutions. If all candidate care sufficiently much about promise breaking, promises in both institutions are similar. Then the lower promises in *opaque* institutions are made up by a larger extent of promise breaking such that salaries are higher in *opaque* than in *transparent* institutions.

We can also compare differences in salaries to differences in promises. Comparing *transparent* and *opaque* institutions, we predict salaries to differ less than promises if some candidates care very little about promise breaking and the extent of promise breaking is small. Prediction 2 ensures that promises are more generous in *opaque* than in *transparent* institutions. Prediction 4 ensures that promise keeping is more likely in *transparent* than in *opaque* institutions. Salaries equal promises plus the extent of promise breaking. Therefore, salaries differ less between institutions than promises (see Proposition 1 and proof of Prediction 5 in the appendix).

3 Experimental Design

We implement a between-subjects design based on experiments by Feltovich and Giovannoni (2015), in which an (elected) official can allocate an endowment among herself and a group of citizens. In total, our experiment consists of five supergames and each

supergame encompasses a randomly determined number of periods. Participants are informed about the number of supergames but not about the number of periods within each supergame, as we use a infinitely repeated game.

At the beginning of the experiment, participants are randomly matched into groups of $n = 5$ and the group composition stays constant across all periods and super games (partner matching). In each period, the group receives an endowment to be distributed among an official and the other group members. The official chooses her salary s as a share of the endowment and each other member receives an equal share of the remainder, i.e., a share $(1 - s)/(n - 1)$ of the endowment. From the second period on, an election is held between the incumbent and a randomly determined challenger. The group decides on whether to keep the incumbent or elect the challenger. Before each group member votes, both, the challenger and the incumbent have the opportunity to make a campaign promise by announcing what share of the endowment they intent to keep for themselves.¹⁰ Promises are costless, made simultaneously, and candidates can also refrain from making any promise.¹¹ Promises are cheap talk, in the sense that they do not offer any formal commitment to certain policies.

As soon as both candidates have submitted their promises, all members of a group see the promises made (if any) on their computer screen and the election takes place. In the election, each participant chooses between the current incumbent and the challenger (i.e. abstention is not possible). The number of votes for each candidate are displayed and the candidate who receives the majority of votes wins. Finally, the elected official chooses the percentage share of the endowment she wants to keep and the current period ends. Then, a new period begins with a continuation probability of eighty percent. Participants are informed about the continuation probability at the beginning of the experiment. They know that if no new period begins, the current supergame is over and a new supergame begins. After the last period of the fifth supergame, the voting experiment ends.

Importantly for our purposes, participants' payoffs are composed of two components in each period. The first component refers to the share allocated by the official. The second component is determined randomly and independently for each participant by the computer. Participants are informed about the two payoff components, but neither about

¹⁰In the experiment, we speak of announcements, not promises. Nevertheless, our norm elicitation shows that announcements are understood as campaign promises (see Section 4.5).

¹¹Following Feltovich and Giovannoni (2015), we decided to allow for not making any promise, such that candidates making a promise do so intentionally.

the size of the endowment to be allocated by the official nor about the support of the distributions from which the random payoff is drawn. This design allows us to study the causal effect of opacity on promise competition. While the payoff structure stays constant, our experimental treatments simply vary, whether or not voters (or independent observers) see how much rent (in percent of the endowment) an official has appropriated.

To allow for both prospective and retrospective voting motives to matter, we display the history of previous outcomes within a group at the top of each participant’s computer screen within each supergame. In all treatments, participants see for each previous period of the current supergame which role they had (citizen, challenger or incumbent), what shares the incumbent and the challenger promised, the election winner (incumbent or challenger) and the total points they earned. Additionally, in some treatments, participants see information about chosen salaries.

3.1 Treatments

We implement three treatments in a between-subjects design, in which we vary who observes promise keeping. In the *transparent* treatment, the history table on participants screens informs voters in addition about the official’s chosen salary as a share of the endowment while such information is missing in the *opaque* treatments. To investigate the role of social-image concerns, we introduce an additional *opaque with observers* treatment. Here, four independent outside observers see promises and salary choices but voters cannot observe chosen salaries. Hence, as in the *transparent* treatment, officials choose their salary knowing that four other participants observe their promise and salary choice, but there is no instrumental value from keeping a promise as observers do not vote. Participants are informed about the history table and its content at the beginning of the experiment, such that candidates and voters are aware of the information displayed to voters or external observers.

3.2 Procedures

During the experiment, participants payoffs are denoted in points (1 point = 7 eurocents) and the computer randomly selects (with equal probability) one period out of all periods to be payoff relevant. The average income for a group of five participants was roughly 830 points per period, which was mentioned in the instructions to avoid complete ambi-

guity about payoffs and stress the monetary consequences of behavior in the experiment. However, as mentioned earlier, the exact size of the endowment (270 points) and the uniform distribution from which the random component was drawn (87.5 to 137.5 points) was unknown to the participants. This randomness makes it difficult for citizens to infer the salary chosen by the official. Remember that participants know neither the support of the distribution of the random component nor the distributions. Even if they were to know both, it is impossible for them to infer the official's salary with certainty. Furthermore, we designed the experiment such that it is also impossible to learn the exact endowment size by appropriating all rents, as in all treatments, only the own total payoff (i.e. the sum of the share of the unknown endowment and the random component) was shown to participants.

All experimental sessions were conducted at the Munich Experimental Laboratory for Economic and Social Sciences (MELESSA). Participants are part of the MELESSA subject pool which includes undergraduate and graduate students from all fields of study. We recruited in total 290 participants using the online recruiting system ORSEE (Greiner, 2015). The experiment encompasses 15 sessions, with either 15 or 20 decision-makers (plus 4 observers in *opaque with observers*) per session. Each participant took part in the experiment exactly once and received a show-up fee of six euros, which was added to the other earnings from the experiment.¹²

During the experiment, each participant was randomly assigned a separated computer cubicle and provided with a printed copy of the experimental instructions. A translation of the instructions can be found in appendix A.4. No communication between the participants was allowed. We randomized treatment at the session level (i.e. we use a between-subjects design) and read instructions aloud to create common knowledge about all procedures. Also, participants had to answer a series of control questions correctly before the experiment began. If they failed to do so, an experimenter answered the participant's questions in private.

After the voting experiment, we also elicited participants' injunctive and descriptive norms about promise keeping.¹³ For the elicitation, we adopt incentive-compatible

¹²Observers' earnings in *opaque with observers* were not determined by voting decisions. Instead, each observer received 175 points for the main part of the experiment and could earn additional payoffs through the norm elicitation task.

¹³While we elicited norms for passive observers in *opaque with observers* before and after the voting experiment, active decision-makers' (i.e. voters') norms were elicited only after the experiment, to avoid priming effects.

methods introduced by Krupka and Weber (2013). The idea of this method is to measure shared beliefs about desirable (injunctive) and perceived behavior (descriptive). Injunctive norms refer to perceptions of which behaviors are typically acceptable whereas descriptive norms refer to perceptions about others' behavior. In our norm elicitation task, participants are incentivized to guess the modal answer (by all active decision-makers in their session) to two questions.¹⁴ The first question relates to injunctive norms and asks whether "an elected official *ought to* choose a share 'much larger', 'larger', 'equal to', 'smaller' or 'much smaller' than announced". The second question related to descriptive norms and asked "whether an elected official chooses a share 'much larger', 'larger', 'equal to', 'smaller' or 'much smaller' than announced". The order of the two questions was randomized and one of the two questions was randomly selected to be payoff relevant at the end of the experiment. If a participant's guess was correct in the payoff-relevant question, she earned additional 20 points (EUR 1.40).¹⁵ After the norm elicitation, participants answered a short questionnaire and received their payments individually.

The whole experiment took about one hour and fifteen minutes and the average income (including the show-up fee, earnings from one randomly selected period of the voting experiment and the norm elicitation task) was about 18 euros. The experiment was programmed and conducted using z-Tree (Fischbacher, 2007). A computer determined the random income components as well as the random number of periods resulting from continuation probabilities in the first session. We used these values for all subsequent sessions (to keep variation in random payoff components and length of supergames constant). At the end of the experiment, the computer randomly selected for each group one period of the voting game to be payoff relevant with equal probability for all periods.

¹⁴In *opaque with observers*, observers' answers were not included when calculating the modal choice, and voters as well as observers were explicitly told so.

¹⁵Our main aim was to elicit whether opacity affects the shared belief that promises ought to be kept. The original method introduced by Krupka and Weber (2013) would have implied to ask participants how appropriate they consider each of the five possible outcomes. To keep the experiment short and incentives high, we decided to shorten the elicitation procedure for our purposes and ask directly for the shared belief of what ought and is expected to be done.

4 Results

4.1 Voting Behavior and Reelection of the Incumbent

To investigate prospective and retrospective voting in *transparent*, *opaque* and *opaque with observers* institutions, we use votes by all citizens excluding challengers and incumbents in situations in which both candidates made a promise.¹⁶ Further, we focus on all voting decisions for which an elected incumbent was in office for at least one period.¹⁷ Table 1 shows results from random-effects Probit regressions estimating the probability of voting for the incumbent and reports average marginal effects. Results from linear probability models are very similar (see Table A.2 in appendix A.3). For each treatment, we run three main specifications (in which we step-wise add additional controls). In these specifications, we regress voting for the incumbent on a dummy whether the incumbent's promise was more generous to voters than the challenger's promise, a dummy for whether the incumbent broke her promise in the previous period, and the citizen's total income in the previous period (which was observable to all participants in all treatments). Further, we report results from an additional specification, in which we use the absolute values of promises instead of the dummy variable indicating whether the incumbent's promise is more generous. The three main specifications show that irrespective of the treatment condition, making a promise that is more generous than the challenger's promise increases the incumbent's probability of receiving a vote by 13 to 22 percentage points. Specifications (4), (8), and (12) show in addition that the incumbent's probability of receiving a vote increases in the challenger's promised salary and decreases in her own promised salary (as higher salaries imply less generous allocations for voters). Hence, in all institutions, many voters vote prospectively. In *transparent* institutions, we also find evidence for retrospective voting. Here, if the incumbent broke her promise, her probability of receiving a vote decreases by about 30 percentage points.

¹⁶Challengers vote for themselves in 95 percent of elections, and incumbents do so in 96 percent of elections. Further, candidates made promises in 97 percent of the cases.

¹⁷This is necessary as otherwise promise breaking in the previous period cannot be included as an explanatory variable. To highlight the robustness of our results, the appendix A.3 includes additional specifications in which we focus only on situations in which the incumbent's and the challenger's promise differed (Table A.1 and A.3).

Table 1: Probability of voting for the incumbent

	Dependent variable: Indicator for voting for the incumbent											
	Transparent				Opaque				Opaque with observers			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Incumbent's promise more generous	0.142*** (0.042)	0.144*** (0.040)	0.150*** (0.037)	-0.008*** (0.002)	0.134** (0.059)	0.135** (0.057)	0.136** (0.057)		0.208*** (0.047)	0.215*** (0.048)	0.214*** (0.048)	
Promise incumbent								-0.010** (0.004)				-0.008** (0.003)
Promise challenger				0.010*** (0.003)				0.012*** (0.004)				0.004*** (0.001)
Promise broken	-0.290*** (0.070)	-0.294*** (0.070)	-0.304*** (0.067)	-0.293*** (0.067)	-0.018 (0.065)	-0.034 (0.059)	-0.025 (0.060)	-0.030 (0.063)	-0.048 (0.067)	-0.049 (0.068)	-0.038 (0.068)	-0.027 (0.067)
Own total income in previous round	0.003* (0.002)	0.003* (0.001)	0.003* (0.001)	0.003* (0.001)	0.008*** (0.001)	0.008*** (0.001)	0.008*** (0.001)	0.008*** (0.001)	0.008*** (0.001)	0.008*** (0.001)	0.008*** (0.001)	0.007*** (0.001)
# voting decision	432	432	432	432	417	417	417	417	447	447	447	447
Round controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Individual characteristics	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes

The table displays results from a random-effects Probit model (average marginal effects).

Standard errors (clustered on group level) in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Individual characteristics include gender, risk attitudes, age, available income, high school math-grade, political orientation and number of experiments.

Table 2: Reelection of Incumbent (group level analyses)

	Dependent variable: Incumbent reelected		
	(1)	(2)	(3)
<i>Transparent</i>	0.148* (0.080)	0.151* (0.078)	0.185** (0.074)
<i>Opaque with observers</i>	-0.023 (0.081)	-0.019 (0.079)	0.028 (0.083)
Reelection probability in baseline (<i>opaque</i>): 43.7 percent			
<i>N</i>	237	237	237
Round controls	No	Yes	Yes
Group characteristics	No	No	Yes
The table displays average marginal effects from random effects Probit models. Baseline: <i>opaque</i> . Standard errors (clustered on group level) in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$			

In *opaque* institutions voters do not vote retrospectively based on promise keeping, as they do not observe the officials' salary choice. However, in line with the idea that economic conditions serve as a proxy for politicians' performance (Key et al., 1966; Fiorina, 1981; Lewis-Beck and Ratto, 2013) voters react to the total income they received in previous periods. Including additional control variables does not substantially affect our main findings.

To test for whether the incumbent is reelected with higher probability in the *transparent* institution (Prediction 1), Table 2 presents results (average marginal effects) from group-level panel Probit regressions. Linear probability models yield very similar results; see Table A.4 in the appendix A.3. We regress whether the incumbent is reelected on the treatment dummies *transparent* and *opaque with observers* only, using *opaque* as baseline. As in all other regressions, we then introduce additional controls (period and average group characteristics) in specifications (2) and (3). The incumbent's probability of winning the election is on average 15 to 18.5 percentage points higher in *transparent* as compared to the *opaque* institution, and also higher as compared to the *opaque with observers* institution (as Wald-tests reject equality of the two treatment coefficients in all three specifications, at p -values < 0.10). Adding observers to the *opaque* institution does not affect the reelection probability. We conclude with Result 1.

Result 1. *Voters vote prospectively in all institutions, and retrospectively in transparent institutions. Reelection of the incumbent is significantly more likely in transparent institutions.*

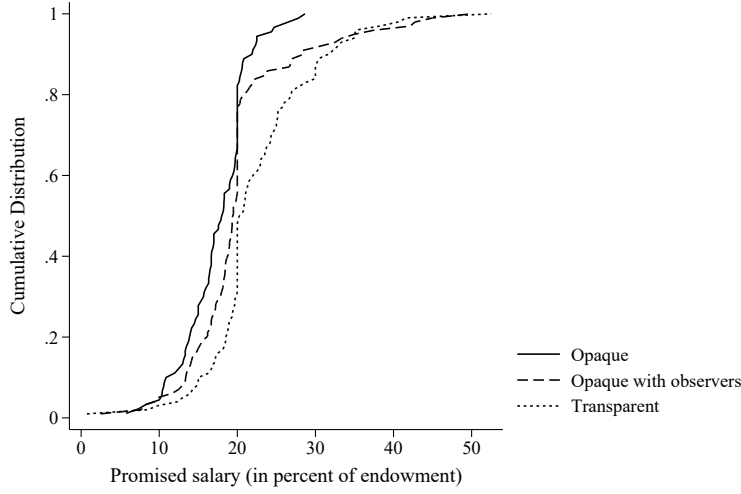


Figure 1: Promises across treatments

Cumulative distribution functions of promised salaries (individual means) across treatments.

4.2 Promise Competition

The opportunity to make campaign promises was used extensively. In more than 97 percent of cases candidates announced the share of the endowment they intended to keep for themselves when being elected. Figure 1 illustrates the cumulative distribution function of promises by candidates (individual means) across treatments. In line with Prediction 2, promises are significantly more generous in *opaque* institutions (Mann-Whitney tests on group level, p -value < 0.001 for *opaque* vs. *transparent*, p -value = 0.016 for *opaque* vs. *transparent*). Figure 1 shows that with *opaque* institutions, more than 80 percent of candidates promise on average to take at most the equal share, while more than 70 percent of candidates promise on average to take at least the equal share in the *transparent* institution. Furthermore, social-image concerns appear to play a role for promise making, as promises are also more generous in *opaque* as compared to *opaque with observers* (Mann-Whitney tests on group level, p -value = 0.075).

Additional regression analyses exploiting the panel nature of our data confirm these findings. Table 3 presents results on promises using random-effects Tobit models (taking censoring at 0 and 100 percent into account). For robustness, Table A.5 in the appendix reports results from random-effects GLS regressions. The dependent variable is the salary

Table 3: Candidates' promised salary (in percent of endowment)

	Dependent variable: Promised salary				
	(1)	(2)	(3)	(4)	(5)
<i>Transparent</i>	5.186*** (0.820)	5.257*** (0.858)	5.307*** (0.871)	5.284*** (0.840)	4.254*** (0.793)
<i>Opaque with observers</i>	2.694*** (0.895)	2.683*** (0.877)	2.702*** (0.841)	2.603*** (0.849)	1.992** (0.979)
Incumbent		1.606*** (0.484)	1.623*** (0.480)	1.604*** (0.476)	0.434 (0.602)
<i>Transparent</i> x Incumbent					2.211** (1.066)
<i>Opaque with observer</i> x Incumbent					1.264 (1.116)
Constant	17.43*** (0.455)	16.64*** (0.500)	17.76*** (0.713)	15.51*** (2.871)	16.14*** (2.861)
<i>N</i>	1583	1583	1583	1583	1583
Round controls	No	No	Yes	Yes	Yes
Individual characteristics	No	No	Yes	Yes	Yes

The table displays results from random-effects Tobit models.

Bootstrapped standard errors in parentheses (1000 repetitions).

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

announced by candidates before the election (ranging from 0 to 100 percent of the endowment). The explanatory variables include our treatment indicators (*transparent* and *opaque with observers*), a dummy for whether the promise was made by the incumbent, a period control and candidates' individual characteristics. The observed treatment effects are robust to additional control variables. Model (5) of our regressions analyses reveals that incumbents announce higher salaries than challengers in *transparent* institutions, as here incumbents can rely on their reputation. We conclude with Result 2.

Result 2. *Campaign promises are significantly more generous in opaque institutions.*

4.3 Promise Breaking

To show both the frequency and extent of promise breaking, we use a simple measure of promise breaking that builds on the distance between the chosen salary s and the promise π . We consider a promise to be broken, if the salary chosen exceeds the winning candidate's announced share and to be kept otherwise. If promises are kept, we assign a

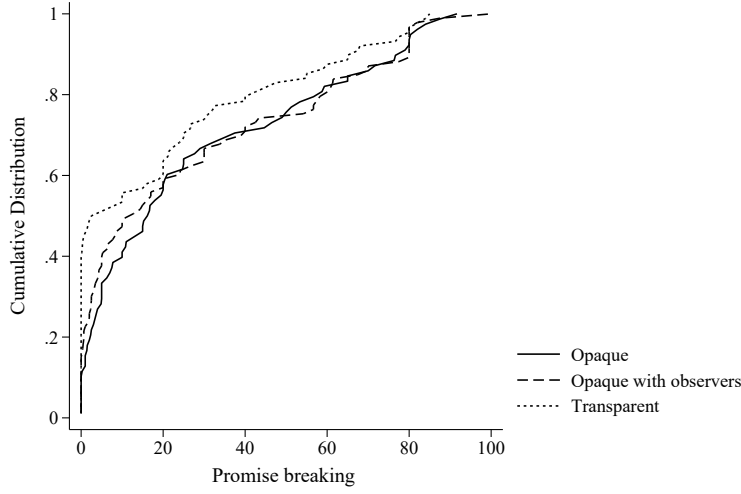


Figure 2: The extent of promise breaking

Cumulative distribution functions of promise breaking (0 if $s \leq \pi$. and $s - \pi$. otherwise, individual means).

value of zero to our measure of promise breaking. Figure 2 shows the cumulative distribution function of promise breaking (individual means) across treatments. In line with Prediction 3, the extent of promise breaking is lower in in *transparent* as compared to *opaque* institutions (*transparent* vs *opaque*: p -value = 0.054, *transparent* vs. *opaque with observers*: p -value = 0.066, *opaque* vs. *opaque with observers*: p -value = 0.759, Mann-Whitney test on group level). These findings are confirmed by results from random-effects Tobit models reported in Table 4. Results from random-effects GLS estimations (Table A.6 in the appendix) yield qualitatively similar results.

Result 3. *The extent of promise breaking is significantly smaller in transparent institutions.*

Focusing on the extensive margin, Figure 2 reveals that, in line with Prediction 4, promise breaking occurs significantly more often in *opaque* institutions. Less than 20 percent of individuals keep their promises in *opaque* institutions. In the *transparent* institution instead, more than 40 percent of officials keep their promises (*transparent* vs *opaque*: p -value < 0.001, *transparent* vs. *opaque with observers*: p -value < 0.001, *opaque* vs. *opaque with observers*: p -value = 0.187, Mann-Whitney test on group level). These results are also confirmed by regression analyses shown in Table A.7 and A.8 in the appendix.

Conditional on promise breaking, the intensive margins of promise breaking ($s - \pi$) amounts on average to 31 in *opaque*, 34 in *opaque with observers* and 45 percent of the en-

Table 4: Promise breaking across treatments

	Dependent variable: Promise Breaking		
	(1)	(2)	(3)
<i>Transparent</i>	-22.57*** (6.534)	-22.80*** (6.702)	-22.98*** (6.580)
<i>Opaque with observers</i>	-4.033 (5.953)	-4.241 (5.685)	-4.653 (5.867)
Constant	21.80*** (4.129)	18.25*** (4.833)	16.90 (17.46)
<i>N</i>	797	797	797
Round controls	No	Yes	Yes
Group characteristics	No	No	Yes

The table reports results from a random-effects Tobit models.
 Bootstrapped standard errors (1000 repetitions) in parentheses,
 * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

dowment in *transparent*. Differences between the *opaque* treatments are not statistically significant (*opaque* vs. *opaque with observers*: p -value = 0.539, Mann-Whitney test on group level), whereas – conditional on promise breaking – the extent of promise breaking is significantly larger in the *transparent* treatment (*transparent* vs *opaque*: p -value = 0.019, *transparent* vs. *opaque with observers*: p -value = 0.039). We discuss this result in more detail in Section 4.4. Additional regression analyses reported in Table A.9 and A.10 in the appendix confirm these findings. We conclude with Result 4.

Result 4. *Campaign promises are significantly less often broken in transparent institutions.*

4.4 Salary Choices

Figure 3 illustrates the cumulative distribution function of chosen salaries (in percent of the endowment, individual means) across treatments. Differences in salaries are much less pronounced than differences in promises. In the *transparent* institution, elected candidates allocate on average 37 percent of the endowment to themselves. In the *opaque* institution, the average salary amounts to 41 percent (and to 42 percent in *opaque with observers*). Salaries tend to be higher in *opaque treatments*, but differences across treatments are statistically insignificant, when using, both, non-parametric tests (*transparent* vs *opaque*: p -value = 0.421, *transparent* vs. *opaque with observers*: p -value 0.164, *opaque*

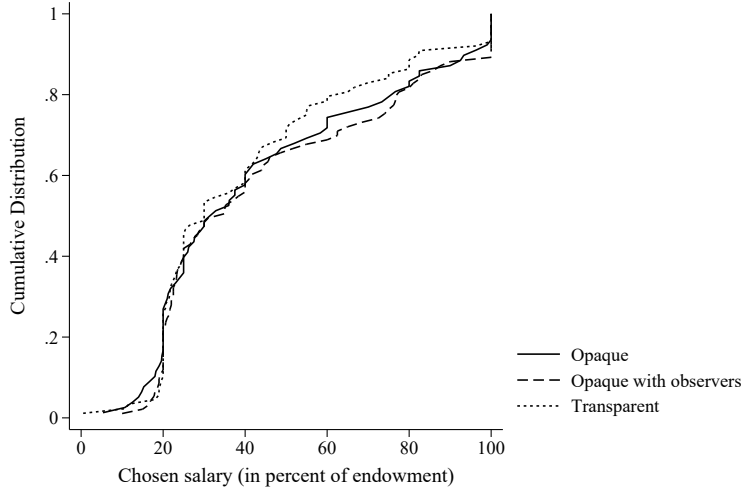


Figure 3: Shares allocated to official across treatments
Cumulative distribution functions of salary choices (individual means) across treatments.

vs. *opaque with observers*: p -value = 0.682, Mann-Whitney test on group level) and regression analyses (see Table 5 and Table A.11 in the appendix). We conclude with Result 5.

Result 5. *Salaries do not differ significantly across institutions.*

To analyze the relationship of preferences for truth-telling, instrumental reputational concerns and salary choices further, Figure 4 shows histograms of chosen salaries across institutions, split by whether a promise was kept (left panel) or broken (right panel). The top-left panel of Figure 4 reveals that in the *opaque* institution, even some very generous promises are kept. Here, more than 40 percent of winning promises kept resulted in salaries of less than the equal share, whereas in *opaque with observers*, most of the promises kept resulted in the equal share. In the *transparent* institution, instead, only 12 percent of kept promises resulted in salaries lower than the equal share, and a substantial fraction of promises kept resulted in salaries larger than the equal share. In line with Prediction 5, conditional on promise keeping, salaries are higher in *transparent* institutions (*transparent* vs *opaque*: p -value < 0.001, *transparent* vs. *opaque with observers*: p -value = 0.038, *opaque* vs. *opaque with observers*: p -value = 0.013, Mann-Whitney tests on group level, conditional on promise breaking).

Table 5: Candidates' chosen salaries (in percent of endowment)

	Dependent variable: Chosen salary		
	(1)	(2)	(3)
<i>Transparent</i>	-3.333 (4.740)	-3.451 (5.053)	-4.278 (5.217)
<i>Opaque with observers</i>	1.139 (5.076)	1.049 (4.957)	0.498 (5.133)
Constant	46.17*** (3.723)	44.66*** (4.164)	39.77*** (12.18)
<i>N</i>	797	797	797
Round controls	No	Yes	Yes
Group characteristics	No	No	Yes

The table reports results from a random-effects Tobit models.
 Bootstrapped standard errors (1000 repetitions) in parentheses,
 * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

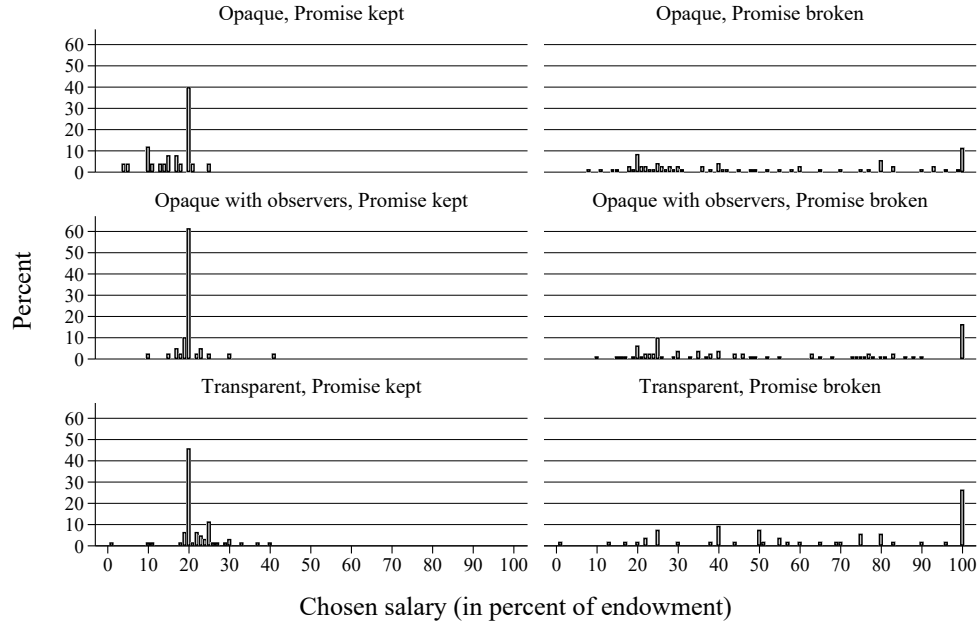


Figure 4: Shares allocated to official across treatments, by promise keeping

Conditional on promises being broken (Figure 4, right panel), officials appropriate on average larger rents in the *transparent* institution (*transparent* vs *opaque*: p -value =

0.007, *transparent* vs. *opaque with observers*: p -value = 0.025, *opaque* vs. *opaque with observers*: p -value = 0.365, Mann-Whitney tests on group level, conditional on promise breaking). The latter effect is mainly driven by reputational (rather than social-image) concerns, as, conditional on promise breaking, the fraction of officials taking the full endowment in *opaque with observers* is substantially lower than in the *transparent* institution (*transparent* vs *opaque*: p -value = 0.037, *transparent* vs. *opaque with observers*: p -value = 0.059, *opaque* vs. *opaque with observers*: p -value = 0.904, Mann-Whitney tests on group level).

The two effects described above help reconcile why salaries do not differ substantially across institutions. Although promise breaking is more frequent in the *opaque* institutions, conditional on promise keeping, salaries are larger in the *transparent* institution. Furthermore, broken promises in the *transparent* institution result more often in full rent appropriations, as with retrospective voting, promise breaking (i.e. any deviation above the promised salary) reduces reelection probabilities substantially (see Table 1).

4.5 Discussion

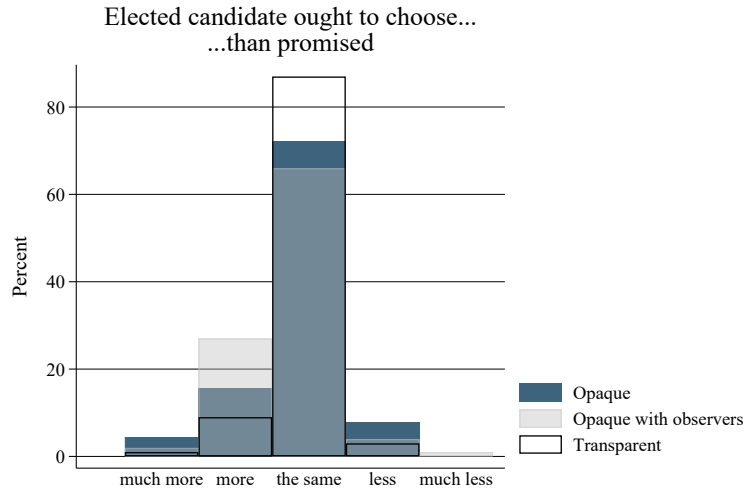
Building on our theoretical considerations, opacity was expected to systematically shape promise competition. In the *transparent* institution, retrospective voting creates an instrumental value for officials to keep their promises. In *opaque* institutions, voters have to rely on candidates' preferences for truth-telling. Due to opacity, they cannot identify honest politicians but preferences for truth-telling (based on self-image and social-image concerns) are expected to shape the range of credible promises (made by, both, honest and dishonest officials). Empirically, officials make the most generous promises indeed in the *opaque* institution, slightly less generous promises in the *opaque with observers* institution (where social-image concerns increase lying costs), and the least generous promises in the *transparent* institution (where social-image concerns and instrumental reputational concerns are at play). While our theoretical considerations predict promises and salaries to be above the equal share in the *transparent* institution, *opaque* institutions allow for more generous promises in prospective-retrospective equilibria, as also observed in the experiment.

Although, based on our theoretical considerations, we did not expect promise breaking in the *transparent* institution, a calibrated version of our model yields surprisingly

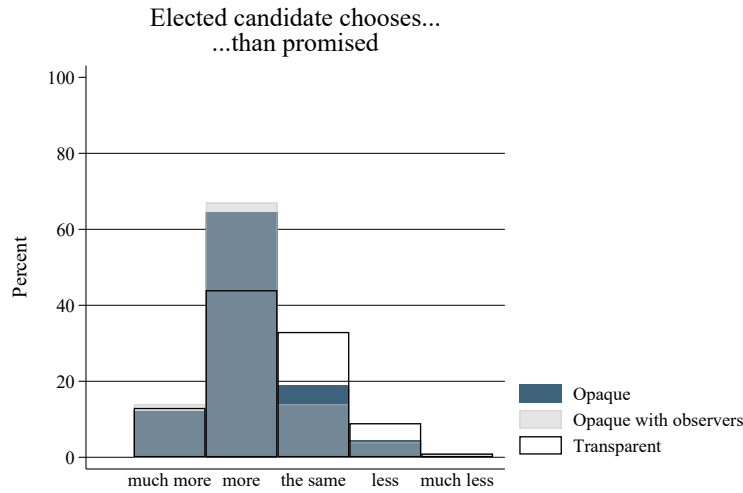
close predictions for promises and salaries. In *transparent* institutions, the model predicts promises between $\max\{1/n, \delta^2/(n-1+\delta^2)\}$ and $\frac{\delta^2+(1-\delta)(n-1)}{n-1+\delta^2}$. The parameterization used in the experiment ($n = 5$ and $\delta = 0.8$) thus implies promises between 20 percent (for high lying costs for the least honest voters) and 31 percent (for low lying costs). In the experiment, candidates promise on average to take 23 percent of the endowment for themselves. The prediction for *opaque institutions* depends on the average extent of promise breaking. If we follow the experimental results and calibrate the average extent of promise breaking ($s - \pi$) to 24 percent (23 percent in *opaque with observers*), the model predicts average promises of 15.2 percent (15.3 percent) and salaries of 39.3 percent (38.8 percent) in *opaque (opaque with observers)* institutions. These values compare to average promises of 17 percent (20 percent) and salaries of 40 percent (42 percent) in the experiment.

Our analyses show how opacity affects the nature of promise competition when decision-makers have preferences for truth-telling and lying costs depend on the size of the lie (i.e. how far an official's salary diverges from her promise). However, opacity may not only affect lying costs by reducing social-image concerns but opacity may also affect the promise-keeping norm itself; and thereby self-image and social-image concerns. The latter aspect hinges crucially on whether *opaque* institutions change participants beliefs on whether promises ought to be kept. To shed light on this question, we elicited injunctive and descriptive norms, for details see Section 3.2. Figure 5a shows a histogram of participants' expectations about the modal choice of whether an official *ought to* choose a higher, the same or a lower salary than promised (on a five-point Likert scale). Irrespective of opacity, a clear promise-keeping norm is apparent. A majority of participants believes promises ought to be kept and treatment differences are small (*transparent* vs *opaque*: p -value = 0.378, Mann-Whitney test on group level).¹⁸

¹⁸Although also in *opaque with observers* a majority thinks promises ought to be kept ("the same" is the modal choice), a few more participants think officials ought to take "more" than promised (as compared to the other institutions, *transparent* vs. *opaque with observers*: p -value = 0.016, *opaque* vs. *opaque with observers*: p -value = 0.069, Mann-Whitney test on group level). However, we cannot exclude that decision-makers, who broke their promise in the *opaque with observers* institution, hold motivated beliefs to avoid lying costs from social-image concerns.



(a) Injunctive norms



(b) Descriptive norms

Figure 5: Injunctive and descriptive norms (across treatments)

Further, we find that more than 80 percent of observers believe that the injunctive norm is to keep one's promise – before and after observing behavior in the voting game in the *opaque with observers* treatment (see Figure A.1 in the appendix). Hence, opacity does not change injunctive norms and thereby the preferred mode of communication (for a discussion see also Krupka et al., 2017; Casella et al., 2018). In line with differences in the frequency of promise breaking across treatments, descriptive norms differ in *transparent* and *opaque* institutions (see Figure 5b, *transparent* vs *opaque*: p -value = 0.052, *transparent* vs *opaque with observers*: p -value = 0.028, *opaque* vs *opaque with observers*:

p -value = 0.847, Mann-Whitney test on group level). Similar to voters' wrong expectations about promise breaking in the real world (Naurin, 2011; Naurin and Oscarsson, 2017), observers' pre-game expectations about the shared belief on promise breaking by officials do not match behavior, and observes substantially update these beliefs after observing promise-breaking behavior (Wilcoxon signed-rank test, p -value < 0.001). Hence, while individuals expect more promise breaking in *opaque* institutions, promises are understood literally and ought to be kept in all institutions.

5 Conclusion

Many economic environments are characterized by competition in promises, but observing whether such promises are kept may be costly or even impossible. This study asks how opacity about promise keeping changes the nature of promise competition. We focus on competition in campaign promises, as empirically, voters' expectations diverge substantially from actual promise-breaking behavior (Naurin, 2011; Thomson, 2011). Our analysis is based on the old idea that candidates do not necessarily do what they say during their campaigns (Banks, 1990) but may have (heterogeneous) preferences for truth-telling (see also Callander and Wilkie, 2007; Abeler et al., 2019; Gerlach et al., 2019). Doing so, we ask how promise competition changes, when voters cannot observe whether promises are kept.

Our theoretical considerations reveal that opacity is expected to systematically affect promises, promise breaking and rent appropriations. Using a laboratory experiment, we show that opacity indeed increases the generosity of promises as well as the frequency of promise breaking. Furthermore, both, self-image and social-image concerns shape promise competition. Individuals care about keeping their word, even when they are not observed, but even more so, when they can appear truthfully to others.¹⁹ Although rent appropriations do not differ substantially in opaque institutions, their nature hinges crucially on preferences for truth-telling stemming from self-image and social-image concerns, as well instrumental reputational concerns in transparent institutions.

From a policy perspective, our conclusion is two-fold. Transparent institutions, on the one hand, generate instrumental concerns for officials to keep their promises by making

¹⁹In a similar spirit, Dana et al. (2007) show that some individuals behave generously in dictator games, because they dislike appearing unfair to others.

reelection conditional on promise keeping. Opaque institutions, on the other hand, are not necessarily characterized by empty campaign promises and full rent appropriations. Instead, preferences for truth-telling shape behavior systematically such that promises still serve as an opportunity to honor future obligations, and promise competition is still be valuable to voters. In turn, the value of transparency may be limited and hinges crucially on (dis)honesty among politicians.

While our study provides clear evidence on the causal effects of opacity in a controlled environment, there are many interesting avenues to extend our analyses in future work. In particular, our results highlight the importance of (dis)honesty among politicians for the value of transparency. Hence, it appears important to understand self-selection into different promise competition environments. Recent work has focused on the important question who runs for office or serves the public (Bernheim and Kartik, 2014; Hanna and Wang, 2017; Barfort et al., 2019; Fehrler et al., 2020). These papers highlight how institutions may be prone to positive and negative selection.²⁰ Our study considers opacity an exogenous factor for voters and politicians. While there are many real world scenarios in which this assumption may hold (e.g. federal governments imposing transparency on behavior of municipal politicians or the presence of local media, see e.g. Ferraz and Finan, 2011), politicians and voters may affect transparency about promise keeping directly. We may thus ask, who prefers transparent or opaque institutions in markets with promise competition? Related to a recent contribution in the context of investment behavior (Tergiman and Villeval, 2019), it is interesting to study environments where politicians may themselves decide whether promise breaking is deniable by designing promises in specific ways. Alternatively, politicians may use intentionally vague promises (Blume and Board, 2014; Agranov and Schotter, 2012; Serra-Garcia et al., 2011). Vice-versa, voters may explicitly acquire information about the extent of promise breaking, e.g., through costly but trusted media outlets, or rationally ignore available information (Matějka and Tabellini, 2017). Finally, we consider it fruitful to pursue our approach further and apply recent insights about preferences for truth-telling and their shape (Abeler et al., 2019; Gneezy et al., 2018; Khalmetski and Sliwka, 2019) to other important economic environments.

²⁰See also Andreoni and Serra-Garcia (2019) on how promises or pledges themselves can be used as a mechanism for self-selection and targeting in the context of charitable giving.

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A Supplementary appendix

A.1 Retrospective-Prospective Equilibria

Voting games, in general, have large numbers of equilibria. To attain sensible predictions, we restrict attention to a particular class of equilibria. First, voters that do not stand for election use identical voting strategies because they are symmetric. The incumbent and the challenger vote for themselves whenever they (weakly) prefer winning to losing the election. Second, as previous literature, we focus on stationary equilibria in pure strategies. Hence, voters' strategies are time-independent and only depend on current promises and on whether the incumbent's promise matched her chosen salary in the last period if this information is available. In particular, we follow Feltovich and Giovannoni (2015, Section 2.2) and consider the following subset of such equilibria. All voters except the incumbent and the challenger vote for the incumbent, if and only if

- the incumbent kept her promise in the previous period by choosing a (weakly) lower salary and
- either
 - her promise is credible and at least as generous as the challenger's promise²¹
 - or
 - her promise is credible and the challenger's promise is non-credible.

A promise is credible, if and only if the probability of keeping such a promise is maximal among all interior promises. We refer to this class of equilibria as retrospective-prospective equilibria. This equilibrium selection contrasts with the literature on voting to aggregate information or ideological voting, where voters' information or preferences for policy options differ and pivotality of voters plays a crucial role. In our setting, the information and preferences for policy options are the same for all voters except for the two candidates. Focusing on retrospective-prospective equilibria then "picks the best possible equilibrium from the voters' point of view in a restricted class of voting rules"

²¹In this case, we allow for random tie-breaking if both candidates make the same promise and voters are indifferent between the resulting equilibria. See also below description of this class of equilibria by Persson et al. (1997).

(Persson et al., 1997, p.1171) without relying on pivotality but thinking about voters being able to coordinate on the equilibrium²²

A.2 Proofs

Proof of Lemma 1: The chosen wage should be weakly above the promised salary, $\bar{s}_i(\pi) \geq \pi$. Assuming an interior solution, the optimal salary is determined by the first-order condition. Hence, $M - \lambda c'_i(s^* - \pi) = 0$ or, equivalently, $\lambda c'_i(s^* - \pi) = M$. Therefore, $\bar{s}_i(\pi) = \pi + \gamma_i$ with γ_i defined in the lemma. This solution is feasible if $\pi + \gamma_i \leq 1$ or $\gamma_i \leq 1 - \pi$. This condition is equivalent to $\lambda c'_i(1 - \pi) \geq M$. This solution is optimal if it yields higher utilities than keeping the promise. Hence, $M(\pi + \gamma_i) - \lambda c_i(\gamma_i) \geq M\pi$ or

$$M\gamma_i/\lambda \geq c_i(\gamma) = c_i(0) + \int_0^{\gamma_i} c'_i(\tilde{x})d\tilde{x}.$$

If $\lambda c'_i(1 - \pi) > M$, an interior solution is infeasible. Therefore, if the fixed costs of lying are sufficiently low, $\bar{s}_i(\pi) = 1$ is optimal because utilities are increasing in s in this case. This condition is equivalent to $M - \lambda c_i(1 - \pi) \geq M\pi$ or

$$M(1 - \pi)/\lambda \geq c_i(1 - \pi) = c_i(0) + \int_0^{1-\pi} c'_i(\tilde{x})d\tilde{x}.$$

If these conditions are violated, the corner solution at $\bar{s}_i(\pi) = \pi$ is optimal. \square

Proof of Proposition 1: The voters use the following strategy: The incumbent and the challenger vote for themselves. The other $n - 2$ voters elect the incumbent if and only if

- the incumbent chose a (weakly) lower salary than her promise in the previous period and
- her promised salary is credible, and
- either her promise is (weakly) more generous than the challenger's promise or the challenger's promise is non-credible.

²²Notice that this concept of retrospective voting is different from Esponda and Pouzo (2017).

Then the election's winner always get four votes. Thus, a single voter cannot change the result of the election and is never pivotal. Therefore, the strategy is optimal for the voters.

We show that some promises are kept by all officials independent of their lying costs. Hence, a promise is credible if and only if the least honest decision-maker keeps such a promise. Hence, the probability of keeping such a promise is one. Any more generous promises are broken by some officials. Therefore we consider the least honest voters as candidates with $\gamma_i = 1$ and $\bar{s}_i(\pi_T^*) = 1$ here.²³ Given the voters' strategy, it is optimal for the candidates to promise a salary of π_T^* and to keep their promises in case of winning the election. In this case, the incumbent's utilities are

$$M \frac{\pi_T^*}{1 - \delta}.$$

If the incumbent changes her promise, she loses the election and receives utilities of

$$M \frac{1 - \pi_T^*}{(n - 1) * (1 - \delta)}$$

which is lower than her equilibrium utilities as $\pi_T^* \geq 1/n$. If the incumbent promises π_T^* but breaks her promise, she chooses a salary of $\bar{s}_i(\pi_T^*) = 1$ according to Lemma 1 and loses the next election. Then the next challenger wins the election. Hence, this challenger optimally promises and chooses a salary of one before reverting back to π_T^* .²⁴ Such a deviation is unprofitable for the incumbent if

$$\begin{aligned} M \frac{\pi_T^*}{1 - \delta} &\geq \underbrace{M - \lambda c^1(1 - \pi_T^*)}_{\text{utilities of } s=1 \text{ in the current period}} + \underbrace{\delta 0}_{\text{utilities in the next period}} + \underbrace{\delta^2 M \frac{1 - \pi_T^*}{(n - 1)(1 - \delta)}}_{\text{utilities thereafter}} \Leftrightarrow \\ \pi_T^* &\geq (1 - \delta) - (1 - \delta) \frac{\lambda}{M} c^1(1 - \pi_T^*) + \delta^2 \frac{1 - \pi_T^*}{n - 1} \Leftrightarrow \\ \pi_T^* &\geq \frac{\delta^2 + (1 - \delta)(n - 1)(1 - \frac{\lambda}{M} c^1(1 - \pi_T^*))}{n - 1 + \delta^2} = \\ &= 1 - (n - 1) \frac{\delta + (1 - \delta) \frac{\lambda}{M} c^1(1 - \pi_T^*)}{n - 1 + \delta^2} \in (0, 1) \end{aligned} \quad (1)$$

²³If the highest value in the support of the distribution G is below one, replace 1 and $c^1(\cdot)$ by that highest value. The analysis remains unchanged as long as the highest value is above $1 - 1/n$.

²⁴See also Feltovich and Giovannoni (2015, Footnote 16, (3)) for the optimality of this strategy in a model without lying costs.

The definition of π_T^* guarantees inequality (1).

Assume to the contrary that, after promise keeping, reelection of the incumbent would occur with probability less than one. Then the official's utilities of keeping promises decrease because their next challenger wins the election with positive probability. Thus, officials break promises of π_T^* . Hence, the threshold for credible promises increases and equilibrium promises are less generous. Voters jointly, thus, strictly prefer a reelection probability of one. Consequently, the definition of the class of retrospective-prospective equilibria ensures that in any retrospective-prospective equilibrium reelection of the incumbent occurs with probability one if the incumbent kept her promise and makes the same promise as the challenger. \square

Proof of Proposition 2: In *opaque* institutions, retrospective voting is impossible. Voters vote prospectively and choose the candidate with the most generous promise that is credible. They randomize in case of a tie. A promise is credible, if and only if the probability of keeping such a promise is maximal among all interior promises. In any *opaque* institution, all officials break their promises except the most honest officials with $\gamma_i = 0$. If the most honest decision-maker is willing to make a specific promise, all others voters can replicate them receiving larger payoffs. Therefore, the maximal probability of keeping a promise is $G(0)$. Hence, a promise is credible if the most honest decision-maker (weakly) prefers winning the election with such a promise to losing the election. Then the probability of keeping such a promise is equal to the probability $G(0)$ of facing the most honest decision-maker(s) as candidates. The most honest decision-makers do not make more generous promises and, hence, the probability of keeping such more generous promises drops to zero. This voting strategy is optimal by the same arguments as in the proof of Proposition 1.

If the most honest individual wins with a promise of π , her utilities are πM . If she loses, her expected utilities are

$$M \frac{1 - \mathbb{E}(\bar{s}_j(\pi))}{n - 1} = M \frac{1 - \mathbb{E}(\min\{1, \pi + \gamma^j\})}{n - 1}.$$

At the most generous promise that is still credible, the most honest voter is indifferent between winning and losing the election. Indifference guarantees that there are no implied rents of office. Therefore, the continuation values are the same after winning or

loosing the election. Hence, without loss of generality, we focus on the utilities in the current period here. Equilibrium requires

$$\pi M = M \frac{1 - \mathbb{E}(\min\{1, \pi + \gamma^j\})}{n - 1}.$$

According to Lemma 1, the expected salary is the promise and the expected amount of promise breaking. The expected amount of promise breaking is the expectation of γ^j truncated at $1 - \pi$.

$$\mathbb{E}(\bar{s}_i(\pi)) = \mathbb{E}(\min\{1, \pi + \gamma^j\}) = \pi + \int_0^{1-\pi} \gamma^j dG(j) + (1 - \pi)\text{Prob}(\gamma^j > 1 - \pi).$$

Hence,

$$\begin{aligned} \pi_O^* &= \frac{(1 - \pi_O^*)(1 - \text{Prob}(\gamma^j > 1 - \pi_O^*)) - \int_0^{1-\pi_O^*} \gamma^j dG(j)}{n - 1} \Leftrightarrow \\ \pi_O^*(n - \text{Prob}(\gamma^j > 1 - \pi_O^*)) &= 1 - \text{Prob}(\gamma^j > 1 - \pi_O^*) - \int_0^{1-\pi_O^*} \gamma^j dG(j) \Leftrightarrow \\ \pi_O^* &= \frac{1 - \text{Prob}(\gamma^j > 1 - \pi_O^*) - \int_0^{1-\pi_O^*} \gamma^j dG(j)}{n - \text{Prob}(\gamma^j > 1 - \pi_O^*)} \end{aligned}$$

If $\text{Prob}(\gamma^j > (n - 1 + \mathbb{E}(\gamma^j))/n) = 0$, the optimal promise is especially simple and equals $(1 - \mathbb{E}(\gamma^j))/n$. In general, $\pi_O^* \leq 1/n$ holds with a strict inequality, if $\text{Prob}(\gamma^j > 0) > 0$ because

$$\frac{1 - \text{Prob}(\gamma^j > 1 - \pi_O^*)}{n - \text{Prob}(\gamma^j > 1 - \pi_O^*)}$$

decreases in $\text{Prob}(\gamma^j > 1 - \pi_O^*)$. Given a promise of π_O^* , an official i' chooses a salary of $\bar{s}_{i'}(\pi_O^*)$. Thus, salaries are distributed on $[\pi_O^*, 1]$.

If any candidate makes a lower offer, she looses the election. If any candidate makes a higher offer, she looses the election. The expected utilities from loosing are independent of the lying costs of the candidate and, hence, are the same for every voter. The utilities from winning are obviously higher for less honest voters. Therefore, all voters except the most honest voter strictly prefer winning to loosing the election. Consequently, it is optimal to promise π_O^* .

In equilibrium, both candidates make the same promise. Hence, reelection probabilities are arbitrary. \square

Proof of Prediction 1: Combining Propositions 1 and 2 immediately yields the statement of this prediction. \square

Proof of Prediction 2: Combining Propositions 1 and 2 with the remarks at the end of Section 2.4 immediately yields the statement of this prediction. \square

Proof of Prediction 3: Combining Propositions 1 and 2 with the remarks at the end of Section 2.4 immediately yields the statement of this prediction. \square

Proof of Prediction 4: Combining Propositions 1 and 2 immediately yields the statement of this prediction. \square

Proof of Prediction 5 (and related arguments): Combining Propositions 1 and 2 yields that promises are expected to be more generous in *opaque with observers* and *opaque* than in *transparent*. Conditional on promise keeping, salaries equal promises. Hence, Prediction 2 implies that average salaries are higher in *transparent* than in *opaque* institutions, conditional on promise keeping.

Furthermore, the average salary in the *opaque* institution is

$$\begin{aligned}\mathbb{E}(s_O^*) &= \pi_O^*(1 - \text{Prob}(\gamma^j > 1 - \pi_O^*)) + \text{Prob}(\gamma^j > 1 - \pi_O^*) + \int_0^{1-\pi_O^*} \gamma^j dG(j) = \\ &= \frac{1 + (n-2)\text{Prob}(\gamma^j > 1 - \pi_O^*) + (n-1) \int_0^{1-\pi_O^*} \gamma^j dG(j)}{n - \text{Prob}(\gamma^j > 1 - \pi_O^*)}\end{aligned}$$

which is above $1/n$. If the extent of promise breaking is low on average, e.g., for $\text{Prob}(\gamma^j > 1 - \pi_O^*) = 0$ and $\int_0^{1-\pi_O^*} \gamma^j dG(j)$ small, the average salary is above but arbitrary close to $1/n$.

The average salary in the *transparent* institution is π_T^* . If the least honest individuals in the population have low lying costs at $1 - \pi_T^*$, average salaries are below but arbitrary close to $1 - (n-1)\delta/(n-1+\delta^2) = 1 - 4 * 0.8/(4.64) > 0.3 > 1/5 = 1/n$. Therefore, average salaries are lower in *opaque* than in *transparent*. Vice-versa, if the least honest decision-makers present in the population have high lying costs at $1 - \pi_T^*$, salaries equal

$$\begin{aligned}& \max\{1/n, 1 - (n-1)(\delta + (1-\delta)((n-1)/n))/(n-1+\delta^2)\} = \\ &= \max\{1/5, 1 - 4 * (0.8 + 0.2 * 0.8)/(4.64)\} = 1/5 = 1/n.\end{aligned}$$

If so, average salaries are higher in *opaque* than in *transparent* institutions.

The difference in promises between *transparent* and *opaque* institutions is $\pi_T^* - \pi_O^*$. This difference is positive according to Prediction 2. The difference in chosen salaries between *transparent* and *opaque* institutions equals $\pi_T^* - \mathbb{E}(s_O^*)$ which could be positive or negative as shown above. Therefore, the absolute value of the last difference is smaller than the former difference if average salaries $\mathbb{E}(s_O^*)$ in *opaque* are below or close to salaries in *transparent* institutions. This is the case if lying costs for the least honest individuals are low and the extent of promise breaking is small on average. Consequently, in this case, the difference in salaries between *transparent* and *opaque* institutions are smaller than the corresponding difference in promises. \square

A.3 Additional Empirical Analyses

A.3.1 Voting behavior

Table A.1 reports average marginal effects of random-effects Probit regressions using the same specifications as in Table 1 but considers only situations, in which the incumbent and the challenger did not make the same promise (note that the number of elections in which the incumbent and challenger make the same promise does not significantly differ across treatments (*transparent*: 24 percent, *opaque*: 25 percent, *opaque with observers*: 31 percent, pairwise comparisons on group level, p -value > 0.16 , Mann-Whitney tests).

Analogously to Table 1 (and Table A.1), we present results from linear probability models with standard errors clustered on the group level in Table A.2 (and Table A.3) for robustness. The coefficients of the linear models are similar in size as the average marginal effects from our Probit analyses. Negative constants in linear specifications for the *opaque* institutions result from the fact that own total income in the previous period – which serves as an important proxy (denoted in points) for the official’s performance in *opaque* institutions – amounts at the median to 140 points (i.e. predicted probabilities are positive for the majority of decision makers).

Table A.1: Probability of voting for the incumbent (when promises differed)

	Dependent variable: Indicator for voting for the incumbent											
	Transparent				Opaque				Opaque with observers			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Incumbent's promise more generous	0.182*** (0.056)	0.186*** (0.054)	0.185*** (0.050)		0.169*** (0.058)	0.169*** (0.057)	0.172*** (0.057)		0.254*** (0.065)	0.253*** (0.066)	0.237*** (0.067)	
Promise incumbent				-0.007** (0.003)				-0.010** (0.004)				-0.007*** (0.002)
Promise challenger				0.007** (0.003)				0.008** (0.004)				0.003*** (0.001)
Promise broken	-0.395*** (0.094)	-0.402*** (0.096)	-0.413*** (0.098)	-0.388*** (0.096)	0.001 (0.081)	-0.003 (0.078)	0.022 (0.075)	0.032 (0.083)	-0.122 (0.077)	-0.116 (0.081)	-0.092 (0.081)	-0.046 (0.083)
Own total income in previous round	0.000 (0.002)	-0.000 (0.002)	0.000 (0.002)	0.001 (0.002)	0.008*** (0.001)	0.008*** (0.001)	0.008*** (0.001)	0.008*** (0.001)	0.008*** (0.001)	0.008*** (0.001)	0.008*** (0.001)	0.008*** (0.001)
# voting decision	246	246	246	246	243	243	243	243	237	237	237	237
Round controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Individual characteristics	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes

The table displays results from a random-effects Probit model (average marginal effects).

Standard errors (clustered on group level) in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Individual characteristics include gender, risk attitudes, age, available income, high school math-grade, political orientation and number of experiments.

Table A.2: Probability of voting for the incumbent (all cases)

	Dependent variable: Indicator for voting for the incumbent											
	Transparent			Opaque			Opaque with observers					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Incumbent's promise more generous	0.143*** (0.040)	0.143*** (0.038)	0.146*** (0.037)		0.134** (0.067)	0.134** (0.065)	0.136** (0.066)		0.208*** (0.048)	0.214*** (0.049)	0.214*** (0.049)	
Promise incumbent				-0.008*** (0.002)				-0.009** (0.004)				-0.003*** (0.001)
Promise challenger				0.009*** (0.002)				0.012*** (0.004)				0.004*** (0.001)
Promise broken	-0.322*** (0.079)	-0.326*** (0.079)	-0.340*** (0.077)	-0.331*** (0.078)	-0.019 (0.070)	-0.035 (0.065)	-0.028 (0.067)	-0.031 (0.070)	-0.047 (0.072)	-0.048 (0.073)	-0.039 (0.073)	-0.018 (0.073)
Own total income in previous round	0.003* (0.001)	0.003* (0.001)	0.002* (0.001)	0.002* (0.001)	0.008*** (0.001)	0.008*** (0.001)	0.008*** (0.001)	0.008*** (0.001)	0.008*** (0.001)	0.008*** (0.001)	0.008*** (0.001)	0.007*** (0.001)
Constant	0.175 (0.252)	0.151 (0.257)	0.260 (0.290)	0.290 (0.282)	-0.762*** (0.156)	-0.784*** (0.154)	-0.810*** (0.203)	-0.858*** (0.209)	-0.735*** (0.144)	-0.705*** (0.143)	-0.976*** (0.213)	-0.791*** (0.218)
# voting decision	432	432	432	432	417	417	417	417	447	447	447	447
Round controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Individual characteristics	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes

The table displays results from GLS random-effects models.

Standard errors (clustered on group level) in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Individual characteristics include gender, risk attitudes, age, available income, high school math-grade, political orientation and number of experiments.

Table A.3: Probability of voting for the incumbent (when promises differed)

	Dependent variable: Indicator for voting for the incumbent											
	Transparent				Opaque				Opaque with observers			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Incumbent's promise more generous	0.184*** (0.056)	0.186*** (0.054)	0.182*** (0.050)		0.166*** (0.063)	0.165*** (0.062)	0.174*** (0.062)		0.258*** (0.069)	0.253*** (0.071)	0.232*** (0.075)	
Promise incumbent				-0.006** (0.003)				-0.010** (0.004)				-0.003** (0.002)
Promise challenger				0.007*** (0.002)				0.008** (0.004)				0.003** (0.001)
Promise broken	-0.410*** (0.099)	-0.415*** (0.100)	-0.430*** (0.104)	-0.405*** (0.103)	0.005 (0.088)	0.002 (0.086)	0.023 (0.088)	0.034 (0.096)	-0.117 (0.081)	-0.106 (0.087)	-0.079 (0.085)	-0.017 (0.094)
Own total income in previous period	0.000 (0.002)	0.000 (0.002)	0.000 (0.002)	0.000 (0.002)	0.008*** (0.001)	0.007*** (0.001)	0.008*** (0.001)	0.008*** (0.001)	0.008*** (0.001)	0.008*** (0.001)	0.008*** (0.001)	0.007*** (0.001)
Constant	0.492* (0.290)	0.486* (0.289)	0.597* (0.343)	0.611* (0.317)	-0.803*** (0.190)	-0.820*** (0.198)	-1.155*** (0.272)	-1.047*** (0.277)	-0.719*** (0.115)	-0.677*** (0.126)	-0.775*** (0.299)	-0.581** (0.279)
# voting decisions	246	246	246	246	243	243	243	243	237	237	237	237
Round controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Individual characteristics	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes

The table displays results from GLS random-effects models.

Standard errors (clustered on group level) in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Individual characteristics include gender, risk attitudes, age, available income, high school math-grade, political orientation and number of experiments.

A.3.2 Reelection

Table A.4 reports results from linear probability models using the same specifications as Table 2. Again, we estimate that the *transparent* treatment increases the incumbent's probability of being reelected by about 15 to 19 percentage points.

Table A.4: Reelection of Incumbent (group level analyses)

	Dependent variable: Incumbent reelected		
	(1)	(2)	(3)
<i>Transparent</i>	0.155** (0.074)	0.158** (0.073)	0.189*** (0.068)
<i>Opaque with observers</i>	-0.026 (0.073)	-0.022 (0.070)	0.024 (0.070)
Constant	0.437*** (0.057)	0.380*** (0.075)	0.782** (0.304)
<i>N</i>	237	237	237
Round controls	No	Yes	Yes
Group characteristics	No	No	Yes

The table displays results from GLS random-effects models.

Standard errors (clustered on group level) in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

A.3.3 Promises

Analogously to Table 3, Table A.5 presents results on promises using random-effects GLS models (clustering standard errors on the group level). It becomes clear that our results do not hinge on the model we use.

Table A.5: Candidates' promised salary (in percent of endowment)

	Dependent variable: Promised salary				
	(1)	(2)	(3)	(4)	(5)
<i>Transparent</i>	5.210*** (1.266)	5.278*** (1.270)	5.326*** (1.274)	5.307*** (1.183)	4.268*** (1.188)
<i>Opaque with observers</i>	2.708** (1.320)	2.699** (1.323)	2.716** (1.320)	2.622** (1.325)	2.006 (1.469)
Incumbent		1.583*** (0.445)	1.596*** (0.442)	1.583*** (0.437)	0.404 (0.575)
<i>Transparent</i> x Incumbent					2.227** (0.966)
<i>Opaque with observer</i> x Incumbent					1.273 (1.014)
Constant	17.43*** (0.701)	16.66*** (0.727)	17.78*** (0.900)	15.48*** (3.187)	16.12*** (3.202)
<i>N</i>	1583	1583	1583	1583	1583
Round controls	No	No	Yes	Yes	Yes
Individual characteristics	No	No	Yes	Yes	Yes

The table displays results from GLS random effects models.

Standard errors (clustered on group level) in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

A.3.4 Promise Breaking

In Table A.6, we consider the same specifications as in Table 4, but use random-effects GLS estimations with standard errors clustered on the group level. Qualitative results are very similar: transparency reduces promise breaking substantially.

Table A.6: Promise breaking across treatments

	Dependent variable: Promise Breaking		
	(1)	(2)	(3)
<i>Transparent</i>	-7.602* (4.082)	-7.795* (4.105)	-8.263** (4.038)
<i>Opaque with observers</i>	-1.380 (4.324)	-1.528 (4.334)	-2.170 (4.388)
Constant	27.37*** (3.166)	25.04*** (3.106)	18.79** (8.617)
<i>N</i>	797	797	797
Round controls	No	Yes	Yes
Group characteristics	No	No	Yes

The table displays results from GLS random effects models.

Robust standard errors (clustered on group level) in parentheses,

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

A.3.5 Promise Breaking (Extensive Margin)

In Table A.7 (Table A.8) we present results from random-effects Probit regressions (GLS regressions) on how probabilities of promise breaking differ across treatments. Clearly, promise breaking is less likely in *transparent* institutions.

Table A.7: Probability of promise breaking (ext. margin)

	Dependent variable: Promise Breaking (ext. margin)		
	(1)	(2)	(3)
<i>Transparent</i>	-0.347*** (0.0522)	-0.349*** (0.0521)	-0.346*** (0.0541)
<i>Opaque with observers</i>	-0.0904 (0.0584)	-0.0915 (0.0583)	-0.0935 (0.0607)
<i>N</i>	797	797	797
Round controls	No	Yes	Yes
Group characteristics	No	No	Yes

The table displays average marginal effects from random-effects

Probit models. Standard errors (clustered on group level) in parentheses,

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.8: Probability of promise breaking (ext. margin)

	Dependent variable: Promise Breaking (ext. margin)		
	(1)	(2)	(3)
<i>Transparent</i>	-0.375*** (0.0597)	-0.377*** (0.0598)	-0.371*** (0.0615)
<i>Opaque with observers</i>	-0.0845 (0.0551)	-0.0858 (0.0551)	-0.0864 (0.0565)
Constant	0.794*** (0.0405)	0.767*** (0.0490)	0.871*** (0.156)
<i>N</i>	797	797	797
Round controls	No	Yes	Yes
Group characteristics	No	No	Yes

The table displays results from GLS random effects models.

Standard errors (clustered on group level) in parentheses,

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

A.3.6 Promise Breaking (Intensive Margin)

Conditional on promise breaking, salaries differ more from promises in *transparent* than *opaque* institutions (Table A.9 Tobit regressions, Table A.10, GLS regressions).

Table A.9: Extent of promise breaking (intensive margin)

	Dependent variable: Promise Breaking (int. margin) (if $s - \pi > 0$)		
	(1)	(2)	(3)
<i>Transparent</i>	11.57* (6.207)	11.55* (6.125)	10.44* (6.164)
<i>Opaque with observers</i>	-0.248 (5.010)	-0.201 (4.860)	-0.442 (4.966)
Constant	34.19*** (3.643)	31.10*** (4.105)	21.14 (14.29)
N	404	404	404
Round controls	No	Yes	Yes
Group characteristics	No	No	Yes

The table displays results from random-effects Tobit models.

Bootstrapped standard errors (1000 repetitions) in parentheses,

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.10: Extent of promise breaking (intensive margin)

	Dependent variable: Promise Breaking (int. margin) (if $s - \pi > 0$)		
	(1)	(2)	(3)
<i>Transparent</i>	11.54* (5.966)	11.52* (5.986)	10.43* (5.894)
<i>Opaque with observers</i>	-0.383 (5.363)	-0.339 (5.329)	-0.603 (5.268)
Constant	34.19*** (3.740)	31.12*** (3.872)	21.10 (14.11)
N	404	404	404
Round controls	No	Yes	Yes
Group characteristics	No	No	Yes

The table displays results from GLS random-effects models.

Standard errors (cluster on group level) in parentheses,

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

A.3.7 Salaries

Table A.11 reports results from random-effects GLS models, analogously to Table 5 in the main text.

Table A.11: Candidates' chosen salaries (in percent of endowment)

	Dependent variable: Chosen salary		
	(1)	(2)	(3)
<i>Transparent</i>	-3.271 (4.460)	-3.376 (4.481)	-3.940 (4.480)
<i>Opaque with observers</i>	1.042 (4.385)	0.962 (4.399)	0.410 (4.370)
Constant	44.12*** (3.278)	42.86*** (3.260)	37.30*** (8.551)
<i>N</i>	797	797	797
Round controls	No	Yes	Yes
Group characteristics	No	No	Yes

The table displays results from GLS random effects models.

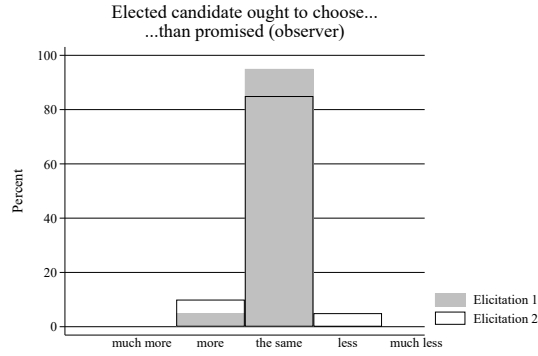
Standard errors (clustered on group level) in parentheses,

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

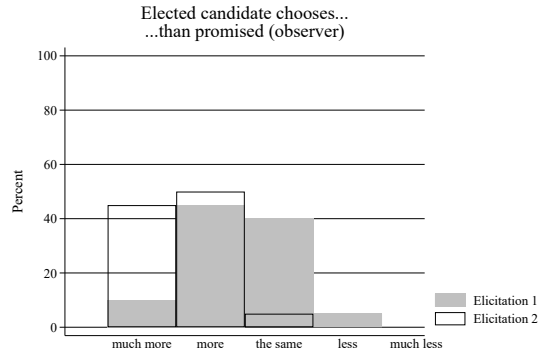
A.3.8 Norms Held by Independent Observers

Figure A.1a reveals that also observer hold very clear descriptive norms: promises ought to be kept (and only 4 observers change their response from the first to the second elicitation). However, injunctive norms (Figure A.1b) are much less clear before observing behaviors.

Before observing promises and promise breaking, 40 percent of observers believe that the modal response to our norm question is "an elected candidate chooses the same as promised", 45 percent expect the modal response to be "...more than...promised", and 10 percent expect that it is "... much more...than promised". After observing behavior in the experiment, these shares amount to 5 percent, 50 percent and 45 percent, respectively. Hence, descriptive norms held by observers differ significantly when learning about promise breaking (Signed-rank test (before-after comparison), p -value = 0.0002).



(a) Injunctive norms



(b) Descriptive norms

Figure A.1: Norms about promise keeping held by observers (before and after the experiment) in *opaque with observers*

A.4 Details on the Design of the Experiment and Instructions

Section 3 provides all details concerning the experimental design and procedures. As explained, participants were undergraduate and graduate students from the MELESSA subject pool. All students of the pool were eligible to participate and we drew a random sample of students who were invited for the experiments. Below we present the written instructions (translated from German) for the voting experiment. We highlight differences in treatments by [**in *treatment(s)* ...**]. The last two pages present additional instructions distributed to observers in the treatment *opaque with observers* only. The norm elicitation task was explained on screen. We provide a translated version of these instructions in the section "On-screen instructions for norm elicitation".

Instructions

You are taking part in an economic experiment today. You receive **6 euros** for showing up in time. In addition, you may receive additional payoffs, depending on your choices. Please read the following instructions carefully.

For the entire duration of the experiment, you are not allowed to communicate with other participants. Violating this rule will result in exclusion from all payments. If you have problems in understanding the instructions, or any questions, please give us a clear hand signal or press the red button on your keyboard. An experimenter will come to you and answer your question personally.

During the experiment we do not denote your payoffs in Euros, but in points. The number of points is converted into euros as follows:

1 point = 7 eurocents

At the end of today's experiment, you will receive a **6 Euro** show-up fee plus the points you earned (in one randomly determined round) converted into Euros in cash.

On the next pages, we explain the procedures of the experiment. First, we introduce you to the general decision-making situation. To do so, we will also show some comprehension questions on your screen. The experiment will only start if all participants understood the instructions and answered the comprehension questions correctly. Note: your answers to the comprehension question will not affect your payoffs.

General procedures

Main part: periods and "years"

The main part of today's experiment consists of a total of five periods. Each of these 5 periods consists of an unknown number of rounds. From now on, we refer to these rounds as "years". The number of years in each period is randomly determined by the computer. That is, at the end of each year, a number drawn by the computer determines whether another year follows. This number is an integer between 1 and 100 (including 1 and 100), randomly drawn with equal probability the computer for each year. If the drawn number is less than or equal to 80, another year follows. If the drawn number is greater than 80, the current period ends. Each period can therefore contain a different number of years. If a period ends, the next period begins with year 1. At the end of the fifth and last period, the main part of the experiment ends.

Expectations, questionnaire and payment

In addition to the main part, you can earn further points through guesses [in *opaque with observers*: and additional decisions in further experiments]. At the end there is a short questionnaire. After the questionnaire, one year of the main part from one of the five periods and one of your guesses is randomly drawn, for which we will pay you the points earned converted into euros. Important: Regardless of how many years a period has, every year has the same probability to be payoff relevant.

In the following, we explain the main part of the experiment in more detail. More detailed explanations of the guesses [in *opaque with observers*: and other decisions] appear directly on the screen.

Summary of main part

In *opaque with observers*: [Four participants are assigned the role of observers for the main part. All other participants...]

In *opaque and transparent*: [In this experiment, you are...]

are randomly assigned to a **group of five participants**, in which it has to be decided how a budget of unknown size is allocated to the five participants in the group. Each year, the group consists of **four citizens** and **an official** who decides how the budget is allocated.

In the first year of a period, the official is randomly selected by the computer. The official decides what percentage of the budget she keeps for herself and thereby, what share she

allocates to the other four participants in the group. The share allocated to the four other members is divided equally among the four citizens in the group (each of the 4 citizens receives a one fourth of share the budget the official did not allocate to herself).

From the second year onward, an election is held at the at beginning of each year, in which two candidates compete to become the official. The incumbent of the previous year competes against a challenger, who is randomly selected from the group by the computer. In the election, all 5 group members (the incumbent, the randomly chosen challenger and the 3 citizens who were not chosen as challenger) vote on which of the two candidates will be the elected official for the current year.

Before the election, the incumbent and the challenger can announce what percentage of the budget they intend to keep for themselves. The two candidates make this announcement simultaneously and all participants in the group observe the announcements.

The candidate who receives the majority of votes in the election becomes the new official. The elected official then decides who to allocate the budget within the group. After the official's allocation decision, the current year ends (and, as described above, a random number drawn by the computer determines whether another year follows).

The **points that each participant receives in a year** are composed of two components: you receive **a share of the budget determined by the official's allocation decision and a random payoff component**. The points determined by the random component result from a number that the computer draws individually, independently and randomly for each participant. As a participant, you neither know how large the budget in the allocation decision is, nor from which range of numbers the random payoff component is drawn. However, you can assume that each group of five participants receives on average about 830 points per year, but these points vary due to the random payoff component.

At the end of each year, all group members see the total points they received in that year (i.e. the sum of their share of the budget allocated to them and the random component).

[in transparent: Additionally, all voters observe the share chosen by the official (and thus also, what share of the endowment is allocated to voters)].

[in *opaque* and *opaque with observers*: However, voters do not observe the share chosen by the official (and hence voters also do not observe what share of the endowment was allocated to them.)]

[in *opaque with observers*: The four observers are shown the share chosen by the official (and thus also, what share of the endowment is allocated to voters). However, the observers make no decisions that can affect the points of the 5 group members in the main part. Also, in the further course of the experiment, observers have no possibility to relate decisions of the main part to individual participants.]

The value of the random component and the number of points in the budget are not shown to any group member **[in *opaque with observers*:** , nor to the observers].

Procedures in the first year of each period

In the first year of each period, there is no election. The computer randomly selects an official and you can directly see on your screen whether you are the official or a citizen in year 1. The randomly chosen official chooses her share, i.e. what percentage of the budget she keeps for herself. The remainder is then divided equally among the four citizens. Then, the first year then ends. All group members see the total points they received in that year (i.e. the sum of their share of the budget allocated to them and the random component).

[in *transparent*: Additionally, all voters observe the share chosen by the official (and thus also, what share of the endowment is allocated to voters)].

[in *opaque* and *opaque with observers*: However, voters do not observe the share chosen by the official (and hence voters also do not observe what share of the endowment was allocated to them.)]

[in *opaque with observers*: However, the four observers see the share chosen by the official.]

As already explained, a randomly drawn number then determines whether another year will follow. The number drawn by the computer (1 to 100) is displayed to the participants on their screen and another year follows, if this number is less than or equal to 80. If the number is 81 or larger, the current period ends.

Procedures in all subsequent years of each period

From the second year, at the beginning of each year an election takes place, where two candidates compete to become the official who decides on the allocation of the budget. As a reminder, the official from the previous year, i.e. the incumbent, competes against a challenger who is randomly determined by the computer. Decision-making is structured in four steps.

- **Step 1 – Candidates’ announcements**

On the first screen of each year, **citizens**, the **incumbent** and the **challenger** are informed about their role. In addition, the **incumbent** and the **challenger** can announce what percentage of the budget they intend to choose as their own share. **This announcement is not binding**. The two candidates make their announcements simultaneously (i.e. they cannot see the other candidate's announcement until they have made their own announcement).

- **Step 2 - display of point announcements and voting**

In Step 2, all participants in a group are shown what percentage of the budget each of the two candidates announced to keep to themselves. Then, all participants decide for which candidate to vote. The outcome of the election is announced, as soon as all participants in a group cast their votes. The candidate who received the majority of votes becomes the **official** and decides in step 3 on the allocation of the budget.

- **Step 3 – Allocation of the budget by the official**

After the election, the elected **official** determines what percentage of the budget she chooses as her share. The remainder is divided between the non-elected candidate and the three citizens. The non-elected candidate and the three citizens each receive one fourth of the remainder.

- **Step 4 - Information at the end of the year**

In Step 4, each voter sees her total points received in the current year (i.e. the sum of their share of the budget allocated to them and the random component).

[in transparent: Additionally, all voters observe the share chosen by the official (and thus also, what share of the endowment is allocated to voters)].

[*in opaque and opaque with observers*: However, voters do not observe the share chosen by the official (and hence voters also do not observe what share of the endowment was allocated to them.)]

[*in opaque with observers*: However, the four observers see the share chosen by the official.]

Table with information (from year 2 onward)

From year 2 onward, the screen of each participant is divided into two parts. The lower part shows steps 1 to 4 (discussed above). The upper area shows a table with the decisions from previous years. For each past year, each participant is shown the following information in the table:

- Own role (i.e. whether she was a citizen, incumbent or challenger in the respective year)
- announcements by the two candidates
- information about which candidate was chosen
- [*in transparent*: share of the budget (in percent) the official allocated to herself]
- and own total points, which are composed of the points from the official's allocation decision and the random component drawn independently for each participant.

Before we start the experiment, we ask you to answer a few comprehension questions on your screen. **Your answers to the comprehension questions will not affect your payment.** However, the experiment only begins after all participants have answered the questions correctly.

If something is unclear to you, please press the red button on your keyboard, we will come to you and answer your question (s).

On-screen instruction for norm elicitation task for voters (translated instructions)

In the following you have the chance to earn more money through two assessments. The computer randomly selects one of the two assessments for which you receive 20 points if your assessment is correct.

[Injunctive norm elicitation]

Right now, we ask all participants which option in the following statement they expect to be selected by most of the participants:

An elected candidate ought to choose...

<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
"much more"	"more"	"the same"	"less"	"much less"

... than she/he announced.

[Descriptive norm elicitation]

Right now, we ask all participants which option in the following statement they expect to be selected by most of the participants:

An elected candidate chooses...

<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
"much more"	"more"	"the same"	"less"	"much less"

... than she/he announced.

If you choose the option chosen by most participants in today's experiment, you will receive additional 20 points, if the computer selects this assessment. If you choose another option, you will not receive any points for this part.

Also every other participant receives 20 points, if she/he clicks on the option that is chosen by the majority [in *opaque with observers*: (excluding observers)] of participants.

Additional remarks for observers

[only distributed to observers in the treatment *opaque with observers*]

You are an observer in the main part. You do not make any decisions in the main part. Instead, you observe the decisions made in the groups of five.

You will see an overview screen.

Your task is to enter the data from the overview screen in the tables available to you.

For this task you will receive 175 points (this is more than the number of points each participant in the groups of five receives on average).

You can earn additional money by making further assessments and decisions in other parts of the experiment.

