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Matthias Lang (LMU Munich)
Simeon Schudy (LMU Munich)

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Ludwig-Maximilians-Universität München | Humboldt-Universität zu Berlin
Spokesperson: Prof. Georg Weizsäcker, Ph.D., Humboldt University Berlin, 10117 Berlin, Germany
info@rationality-and-competition.de

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Abstract

Promise competition is prevalent in many economic environments, but promise keeping is often difficult to observe. We study the value of transparency for promise competition and ask whether promises still offer an opportunity to honor future obligations when outcomes do not allow for observing promise keeping. Focusing on campaign promises, we show theoretically how preferences for truth-telling shape promise competition when promise keeping can(not) be observed and identify the causal effects of transparency in an incentivized experiment. Transparency leads to less promise breaking but also to less generous promises. Rent appropriations are higher in opaque institutions though only weakly so when not fully opaque. Instrumental reputational concerns and preferences for truth-telling explain these results.

JEL codes: C91, C92, D72, D73, D91

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1 Introduction

Competition in promises is ubiquitous (see, for example, Brosig-Koch and Heinrich, 2018; Ellingsen and Johannesson, 2004). Businesses promise high-quality products and services to customers, employers promise careers that offer opportunity and purpose to employees. Workers and managers compete in promises within and between organizations, candidates for company boards promise independent oversight, and CEO candidates compete in promising strategies. Most saliently, in elections, candidates compete in campaign promises about policy actions (Thomson et al., 2017).[1] Promise competition takes place, particularly, when formal contracting is impossible as promises can substitute for commitment. From an economic perspective, promises can be valuable if reputational incentives or preferences for truth-telling allow agents to honor their future obligations. Promises can be used as commitment based on the instrumental value from promise keeping (e.g., voters will not reelect politicians who broke past promises). This instrumental value, however, requires transparency and knowledge about promise keeping, which often cannot be guaranteed. For example, voters' perceptions about promise keeping diverge substantially from actual promise keeping (Naurin, 2011; Thomson, 2011) and voters often lack trust in the media providing such information. The recent rise of populist politicians and illiberal democracies goes hand in hand with distrust in the media – emphasizing the need to understand what shapes promise competition when transparency or trust in information about promise keeping is lacking.

We investigate the important role of transparency for promise competition by focusing on campaign promises. We analyze how promise competition changes if transparency about promise keeping is missing. While several reasons may lead to a lack of transparency (e.g., prohibitively high economic or psychological costs of information acquisition, distrust in the media, or media that leaves people uninformed even if information is in principle available), we operationalize transparency for simplicity by studying whether voters can observe if incumbents keep or break their promises. 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 transparent institutions but not in opaque institutions – the incumbent's past promise keeping before they vote for one of the two candidates. After the

[1] 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.

vote, the elected candidate decides on the allocation. We analyze an infinitely repeated game of such promise competition and consider transparency an exogenous institutional factor. That is, candidates can choose the generosity of promises and how much rents to appropriate but not whether promise keeping is observed.[2]

Our main interest lies in understanding how transparency about incumbents' promise keeping affects electoral competition. Building on a large body of experimental evidence about the heterogeneity of preferences for truth-telling (for recent meta-analyses see Abeler et al., 2019; Gerlach et al., 2019), we theoretically find that transparency systematically shapes the nature of promises, promise breaking, rent appropriations and reelections. With transparency, candidates face instrumental reputational concerns. That is, they are not reelected and loose potential office rents when breaking promises. In equilibrium, candidates make promises that are credible (i.e., promises which allow for sufficiently large office rents). Voters anticipate this behavior and do not trust overly generous promises such that overly generous promises are non-credible. If transparency about promise keeping is missing (opacity), there are no instrumental reputational concerns. Thus, incumbents will break promises according to their preferences for truth-telling. In particular, some dishonest incumbents will break any promise. Therefore, honest candidates want to win the election to avoid large rent appropriations by less honest competitors. Hence, when transparency is missing, even very generous promises are sometimes kept – making such promises credible. Voters anticipate this behavior and put some trust in such generous promises in opaque environments. Consequently, preferences for truth-telling determine the credibility of promises when transparency is missing. Overall, the model predicts promises to be more generous but also more frequently broken when transparency is missing (as well as a lower likelihood of reelection of incumbents). Thereby, more or less rent appropriations may occur in opaque as compared to transparent institutions.

We complement these theoretical findings with clean empirical evidence on the causal effects of transparency. As observational data does not allow for an explicit test of the theoretical predictions and renders the identification of the causal effects of transparency

[2]We consider this approach an important first step as, for example, the quality of media or trust in the media varies across countries and may be exogenous to voters. In Section 6, 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; Serra-Garcia et al., 2011; Alger and Renault, 2007), deniable statements (Khalmetski et al., 2017; Tergiman and Villeval, 2023) or voters' decisions to acquire or ignore information on promise keeping.

difficult, we designed an incentivized experiment that exogenously varies the degree of transparency regarding promise keeping – holding everything else constant. The incentivized experiment provides several advantages. First, it allows us to avoid endogeneity problems regarding the transparency of institutions. Second, it precludes candidates from making vague promises or explicitly disguising information about policy actions and abstracts from endogenous information acquisition by voters. Third, the experiment allows us to explicitly vary potentially relevant additional institutional factors (e.g., whether economic circumstances are observable or to what extent promise-breaking politicians may suffer from non-instrumental social-image concerns). Fourth, the experiment provides insights beyond those of the theoretical model by testing whether theoretical insights on classical preferences for truth-telling (which stem from experimental evidence on misreporting an exogenous state of nature, see e.g. Gneezy, 2005; Fischbacher and Foellmi-Heusi, 2013; Abeler et al., 2019) can be meaningfully applied in promise competition contexts, in which decision makers are dishonest about their expressed intentions when breaking promises. Fifth, the experimental setting allows us to study whether a lack of transparency alters the normative expectation regarding promises and promise keeping. We explicitly measure promise-keeping norms held by decision-makers as well as third-party observers in an incentive-compatible way – adopting methods introduced by Krupka and Weber (2013). Doing so allows us to study whether broken promises are considered actual norm violations.[3]

The causal effects of transparency revealed by the incentivized experiment closely mirror the comparative statics of our theoretical model. In opaque institutions, promises are indeed more generous and also more frequently broken. Rent appropriations are higher in opaque institutions. Lastly, reelection occurs more frequently in transparent institutions. In additional experimental treatments (see Section 5), we highlight that a lack of transparency with respect to promise breaking is less harmful for voters when economic circumstances are observable. Further, we study to what extent self-image and social image concerns shape promises and promise keeping. We find that both these dimensions of preferences for truth-telling are relevant for promise competition. Finally, we show empirically that a lack of transparency does not strongly affect normative expectations about whether promises ought to be kept. In all institutions, a large majority of decision-makers (as well as independent observers) agree that promises ought to be

[3]For the importance of norms in business contexts see also Huck et al. (2012).

kept. Hence, opacity does not change the fact that voters want promises to be interpreted 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 given full transparency. They show that both campaign promises and past promise keeping matter for voting.[4] They study comparative statics in discount factors – pointing out instrumental reputational concerns. Building on their work, we confirm the robustness of their findings in transparent institutions. The main novelty of our paper lies in contrasting transparent and opaque institutions. We show how promise competition changes when voters cannot observe promise keeping (opacity). Our results reveal that – even with unobservable promise keeping – candidates meaningfully compete in promises. As candidates care about being honest as well as being perceived as honest, preferences for truth-telling systematically shape promise competition in opaque institutions.

More generally, our results link to a literature concerned with the effects of transparency and information on politicians' actions (see, e.g., Prat, 2005; Dal Bo, 2007; Levy, 2007; Mattozzi and Merlo, 2007; Hinnosaar, 2023). This literature has documented various changes in transparency. For instance, Brender (2003) shows that better information availability via transparency requirements and the emergence of local media changed voting behavior and affected incumbents' reelection chances in Israel's local elections in 1998. While transparency often appears beneficial at first sight, in some instances, transparency actually makes matters worse. For instance, Gavazza and Lizzeri (2009) show that transparency about government revenues can be counterproductive for fiscal policies. In Benesch et al. (2018), transparency makes voters lose influence over their representatives in parliament. Guriev et al. (2021) show that transparency reduces citizens' trust in the government and the media. Focusing on the positive effects of transparency, Khemani et al. (2016) argue in a recent report by the World Bank that it is essential to understand the exact mechanisms and effect sizes to develop actual policies and to pri-

[4]Feltovich and Giovannoni (2022) provide further interesting insights on how behavior changes in environments with pre-election polls, where candidates can have different (exogenously assigned) types.

oritize. Our contribution clarifies the mechanisms through which transparency affects promise competition – an aspect the literature neglected so far.

The key insight of this study is that promise competition works through different channels depending on the observability of promise keeping. Intuitively, office rents in transparent institutions provide reputational incentives even for less honest candidates to keep their promises. These office rents require that promises are not too generous such that less honest candidates still keep them. In other words, too generous promises in transparent institutions are non-credible. In opaque institutions, reputational incentives do not work. Hence, dishonest candidates break any promises. However, honest candidates want to win the election to avoid large rent appropriations by less honest competitors. Hence, in opaque institutions, it is the behavior of candidates with strong preferences for truth-telling that determines the credibility of promises. Consequently, promises, that are too generous in transparent institutions to be credible, are credible in opaque environments. Thus, competition results in more generous promises in opaque than in transparent institutions. As even in opaque institutions some candidates keep their promises, opaque institutions can result in less rent appropriations. Empirically, voters do suffer to varying degrees from a lack of transparency about promise keeping – highlighting that promise competition can be beneficial to voters even if they cannot observe whether promises are kept.

Our results highlight the importance of (dis)honesty among candidates for the value of transparency. Lying costs appear strong and widespread around the globe (Gächter and Schulz, 2016; Cohn et al., 2019). Janezic and Gallego (2020) study the preferences for truth-telling of Spanish mayors. They find extensive variation in preferences for truth-telling among these politicians. Nonetheless, it appears important to understand self-selection into specific environments of promise competition. Recent work studying 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) highlights that institutions may be prone to positive and negative selection. Self-selection should also matter less for some institutions, e.g., self-selection may be of relatively less concern in elections for local councils and boards within schools or organizations.

We further connect to research on promise keeping in one-shot games and the value of voting (Charness and Dufwenberg, 2006; Vanberg, 2008; Corazzini et al., 2014; Ederer and Stremitzer, 2017; Casella et al., 2018; Koessler et al., 2019; Born, 2020). While these

studies have shown that individuals are willing to keep their promises when the possibility of punishment is missing, we provide novel evidence that candidates are even willing to keep their promises when voters do not learn about promise keeping. Although the literature on preferences for truth-telling (Gneezy, 2005; Fischbacher and Foellmi-Heusi, 2013; Abeler et al., 2019) typically focuses on people reporting a state of the world (e.g,. the result of a dice roll) while the literature on promises focuses on people not following through with statements about their intentions, behavior appears similar. Serra-Garcia et al. (2013) show that usually individuals are more averse to promise breaking than to classical lying. Hence, estimates for lying cost may serve as a lower bound in contexts with promises. Thus, our findings highlight how recent insights on preferences for truth-telling and on the importance of observability of dishonest behaviors (Duffy and Feltovich, 2002, 2006; Houser et al., 2016; Gneezy et al., 2018; Khalmetski and Sliwka, 2019) apply to promise competition.

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 promises, promise keeping, rent appropriations, and voting behavior. We discuss additional treatments and present evidence on how opacity affects norms held by decision-makers in Section 5. Section 6 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.[5] Then the election takes place. Each group member has one vote and – for simplicity – abstention is impossible. 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; Serra-Garcia et al., 2013), 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.[6] Building on the insights in the literature, we assume that

$$C_i(x) = \begin{cases} 0 & \text{for all } x \leq 0 \\ \lambda c_i(x) & \text{for all } x > 0 \end{cases}$$

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 (2022). 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). While unobservable promise breaking can only cause lying costs through self-image concerns ($\lambda = 1$ as a baseline), reduced moral wriggle room or additional social image concerns for observable promise breaking increase this value to $\lambda \geq 1$. Thus, we do not explicitly model social image concerns or moral wriggle room independently but assume that lying costs are higher (by the factor λ) when these are at play compared to only self-image concerns (an example in the

[5] On the one hand, this assumptions allows for a clean analysis and comparability with Feltovich and Giovannoni (2015). On the other hand, it excludes cases of voters recognizing previous incumbents running again after loosing an election.

[6] We 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 $M(s-\pi)$.

next section also illustrates this difference). Notice that these utilities allow for fixed costs of lying as $c(0) > 0$ is feasible.

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 $(c^j)'(\gamma^j) = M$. The value γ_i measures the extent of (unobservable) promise breaking absent instrumental reputational concerns and absent corner solutions. As a normalization, we assume that $\gamma^0 = 0$, that is, an individual i with lying costs $C_i(x) = c^0(x)$ keeps any promise. Further, we assume $\gamma^1 = 1 - 1/n$, that is, an individual i with lying costs $C_i(x) = c^1(x)$ is willing to appropriate all available rents absent instrumental reputational concerns if she promised an equal split and lying is unobservable. Finally, without loss of generality, 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.[7] Each voter i's lying costs $c_i(\cdot)$ are drawn independently from the family $c^j(\cdot)$ according to this distribution $G(j)$. For illustration, we provide an example, which we continue in the next section.

Example 1. *Consider costs* $c^j(x) = \alpha^j M(\exp(x) - 1)$ *with parameter* $\alpha^j = \exp(-j)$ *for* $j \in [0,1]$. *In addition, suppose that* $G(j)$ *is the uniform distribution on* $[0,1]$ *with a mass point of* 0.2 *at zero.*

Relating to our experimental analyses, we study two 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 that voters cannot observe the official's salaries nor their own payoffs in *Opaque* institutions.[8]

Following Feltovich and Giovannoni (2015, Section 2.2), we restrict attention to credible 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 (Elinder et al., 2015; Feltovich and Giovannoni, 2015; Persson et al., 1997). In these equilibria, candidates compete in

[7] This 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.

[8] If 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 additional experimental treatments in Section 5. See also Sections 3 and 5 for more details.

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.[9]

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

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

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 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.

Example 1 continued. *Begin with unobservable lying and $\lambda = 1$.*[10] *Then an individual with lying costs $\lambda c^j(x)$ from Example 1 is willing to break her promises by*

$$\gamma^j = -\ln(\alpha^j) = j \in [0, 1].$$

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

[10] The example has $\gamma^1 = 1$ violating our assumption $\gamma^1 = 1 - 1/n$ to simplify the exposition.

The average or expected lie equals $(1-0.2)1/2+0.2*0=0.4$ *if lying is unobservable, all solutions are interior, and there are no instrumental reputational concerns. Now suppose that reduced moral wriggle room or observable lying implies* $\lambda=\exp(0.3)\approx 1.35$. *Then an individual with lying costs* $\lambda c^j(x)$ *is willing to break her promises by*

$$\max\left\{0,\ln(M/\lambda\alpha^j)\right\}=\max\left\{0,j-0.3\right\}\in[0,0.7].$$

Average lies are $0.8*\frac{0.7}{2}*(1-0.3)\approx 0.2$ *for observable lying.*

2.3 Promises in transparent institutions

In *Transparent* institutions, voters can observe promise keeping as they can compare the incumbent's promised and chosen salaries. In addition, they care about the attractiveness of current promises. Accountability and 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 credible equilibria, voters think about the credibility of the promises in two ways. Voters do *not* vote for the incumbent if she broke her promise in the past. Voters vote for the incumbent if her promise is credible and more generous than a credible promise 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 promise 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 promise, the challenger cannot ensure election.

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 and reduce π_T^*. Vice-versa, low lying costs decrease the range of credible promises and increase π_T^*. Previous analyses, like Persson et al. (1997) and Feltovich and Giovannoni (2015), abstract from lying costs so that their benchmark promises are $1-(n-1)\delta/(n-1+\delta^2) > \pi_T^*$, equivalent to the special case $c^1 \to 0$. Finally, the more patient voters are, the less office rents are required in transparent institutions. In the limit, $\delta \to 1$, equilibrium promises converge to the equal split, $\pi_T^* \to 1/n$. Hence, the value of transparency depends on the discount factor and, in particular, increases in the discount factor δ.

2.4 Promises in opaque institutions

In an *Opaque* institution, voters cannot compare the incumbent's promised and chosen salaries and nobody observes whether promises are broken. Thus, it is impossible to punish incumbents for promise breaking. Voters can only vote based on current promises. As instrumental reputational concerns do not matter in *Opaque* institutions, 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. *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 $s_O^* = \pi_O^* + \gamma_i$. In general, reelection can occur. Promise breaking and chosen salaries follow the distribution G of lying costs.

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,

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

This argument does not involve any intertemporal or reputational considerations. Hence, promises do not depend on time preferences.[11]

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* (voters can observe promise keeping) and *Opaque* (only the official knows whether the promise was kept). These two institutions relate to our main research question and highlight how opacity changes the nature of promise competition. 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* than in *Transparent* institutions.

Prediction 1. *Campaign promises are less generous in* Transparent *institutions.*

Concerning promise breaking, voting disciplines officials to keep their promises in equilibrium in *Transparent* institutions, whereas only self-image concerns matter in *Opaque* institutions. Our model predicts that the additional reputational concerns make promise breaking less likely in *Transparent* institutions than in *Opaque* institutions. Similarly, the extent of promise breaking, i.e., the difference between promises and actual rent appropriations, is predicted to be lower in *Transparent* institutions than in *Opaque* institutions. We summarize these considerations in Prediction 2.

Prediction 2. *Promise breaking is less prevalent in* Transparent *institutions.*

[11] For a recent discussion on the intertemporal aspects of dishonesty see also Bortolotti et al. (2022).

Considering salary choices in *Transparent* and *Opaque* institutions, our model predicts ambiguous effects. According to Prediction 1, there is a clear comparison of promises between *Opaque* and *Transparent* institutions. Nevertheless, the 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.

Prediction 3. *Average salaries can be higher or lower in Transparent institutions.*

Propositions 1 and 2 show that promised salaries in *Opaque* institutions are in some settings much more generous than in *Transparent* institutions, $\pi_T^* >> \pi_O^*$. If preferences for truth-telling are strong on average, these generous promises result in less rent appropriations in *Opaque* institutions. In other settings, in particular, if candidates are patient, promises in both institutions are similar. Then, slightly more generous promises in *Opaque* institutions are offset by more promise breaking in *Opaque* institutions such that rent appropriations are larger in *Opaque* institutions.

Lastly, *Transparent* institutions rely on accountability and instrumental reputational concerns which require reelection of the incumbent if no promise breaking occurs. *Opaque* institutions rely on self-image concerns that do not depend on reelection. Hence, reelection probabilities are predicted to be higher in *Transparent* institutions than in *Opaque* institutions. We summarize these considerations in Prediction 4.

Prediction 4. *Reelection is more likely in* Transparent *institutions.*

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 an infinitely repeated game.

At the beginning of the experiment, participants are randomly matched into groups of $n = 5$ and, to increase the number of statistically independent observations, 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 elect the incumbent or 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.[12] Promises are costless, made simultaneously, and candidates can also refrain from making any promise.[13] 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 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 impossible). 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.

To allow subjects to remember information about the current incumbent, we display the history of previous outcomes within a group at the top of each participant's computer screen within each supergame. In our two main 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, and the election winner (incumbent or challenger).[14] Additionally, in the *Transparent* treatment, participants see information about chosen salaries of the incumbent and the total points they earned.

[12] In the experiment, we speak of announcements, not promises. Even though we abstract from the word promise as well as free-form communication, our norm elicitation shows that announcements are understood as campaign promises (see Section 5.3). Candidates' salary announcements thus resemble "bare" promises (see Charness and Dufwenberg, 2010), which likely induce a lower bound for lying costs.

[13] We decided to allow for not making any promise, such that candidates making a promise do so intentionally. In more than 97 percent of cases, candidates make a promise.

[14] We implemented two additional treatments to disentangle potential mechanisms which we discuss in Section 5. Table A.1 in the Appendix provides an overview of all treatments and participant characteristics.

Choosing a fixed versus a random matching across supergames comes with a trade-off. We decided to use a fixed matching protocol to increase the number of statistically independent observations, which allows for the use of conservative, non-parametric tests at the matching group level. While a partner-matching procedure may generally allow for broader reputational concerns, it is important to note that the experiment does not include any identifiers. Hence, it remains unclear to participants which individual made which choices across supergames, and also within supergames there is no individual information on challengers (e.g. whether they had been the official in previous periods).[15] The only information available to participants within a supergame is for how long an incumbent has been in office and, in the transparent treatment, whether past promises by the incumbent have been kept. Broader reputational and indirect reciprocity effects that emerge outside the context of the infinitely repeated game would likely result in less promise breaking and more generous promises, and thus result in an underestimation of the treatment effects of transparency (which increases promise keeping and reduces generosity of promises).

3.1 Treatments

We implement our treatments in a between-subjects design, in which we vary whether voters observe promise keeping. In the *Transparent* treatment, the history table on participants' screens informs voters about the total points they earned and the share of the endowment the official chose as her salary while this information is not shown in the *Opaque* treatment. Thus, in *Opaque*, voters cannot base their votes on past promise breaking of the incumbent. At the beginning of the experiment, all participants are informed about the history table and its content such that candidates and voters are aware of the information displayed.

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. Importantly, to allow for a ceteris paribus comparison to additional

[15] As this design feature plausibly lowers instrumental reputational concerns in *Transparent*, any differences identified in the experiment can be considered lower bounds for the true treatment effect.

treatments discussed in Section 5, 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 existence of both payoff components but neither about the exact size of the endowment (270 points) nor about the distributions (uniform on 87.5 to 137.5 points) from which the random payoff is drawn. The average income for a group of five participants was roughly 830 points per period, which was mentioned in the instructions to avoid complete ambiguity about payoffs and stress the monetary consequences of participants' behavior in the experiment.

All 42 experimental sessions were conducted at the Munich Experimental Laboratory for Economic and Social Sciences (MELESSA). Participants were part of the MELESSA subject pool which includes undergraduate and graduate students from all fields of study. We recruited 195 participants for our main experimental treatment conditions (95 in *Opaque* and 100 in *Transparent*) using the online recruiting system ORSEE (Greiner, 2015). With a group size of five, this yields 39 independent observations (19 in *Opaque* and 20 in *Transparent*). Importantly, each participant took part in the experiment exactly once. Participants received a show-up fee of six euros, which was added to the other earnings from the experiment.

During the experiment, each participant was randomly assigned a separated computer cubicle and provided with a printed copy of the experimental instructions. 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. 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 elicited participants' norms about promise keeping (see Section 5) and socio-economic background (including age, gender, risk attitudes, available income, high school mathgrade, political orientation, experience with economic experiments). Then, participants individually received their payments.[16]

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

[16]Table A.1 in the Appendix provides an overview of participant characteristics by treatments.

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 the continuation probability of 80% in the first session. We used these values for all subsequent sessions (to keep variation in random payoff components and length of supergames constant).[17] 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.

4 Results

We structure our main results section in accordance with the predictions derived in Section 2. To keep the paper concise, we focus on (conservative) non-parametric tests in the main text when comparing our experimental treatment conditions.[18] Additional regression analyses are provided in the appendix. We discuss two additional treatments in Section 5, which also adds descriptive information (see Table 1) to the empirical distributions of promises, promise breaking, and salary choices in Figure 1 below.

4.1 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. Panel A in Figure 1 illustrates the cumulative distribution function of promises by candidates (individual means) across our two main treatments *Transparent* and *Opaque*. In line with Prediction 1, promises are significantly more generous (i.e., promised salaries are smaller) in *Opaque* than in *Transparent* institutions (Mann-Whitney tests on group level, p-value $= 0.072$). We conclude with Result 1.

Result 1. *Campaign promises are less generous in* Transparent *institutions.*

4.2 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

[17] For supergames $(1, 2, 3, 4, 5)$, the random draws resulted in $(8, 2, 1, 6, 2)$ rounds.
[18] We thank the associate editor for this useful suggestion.

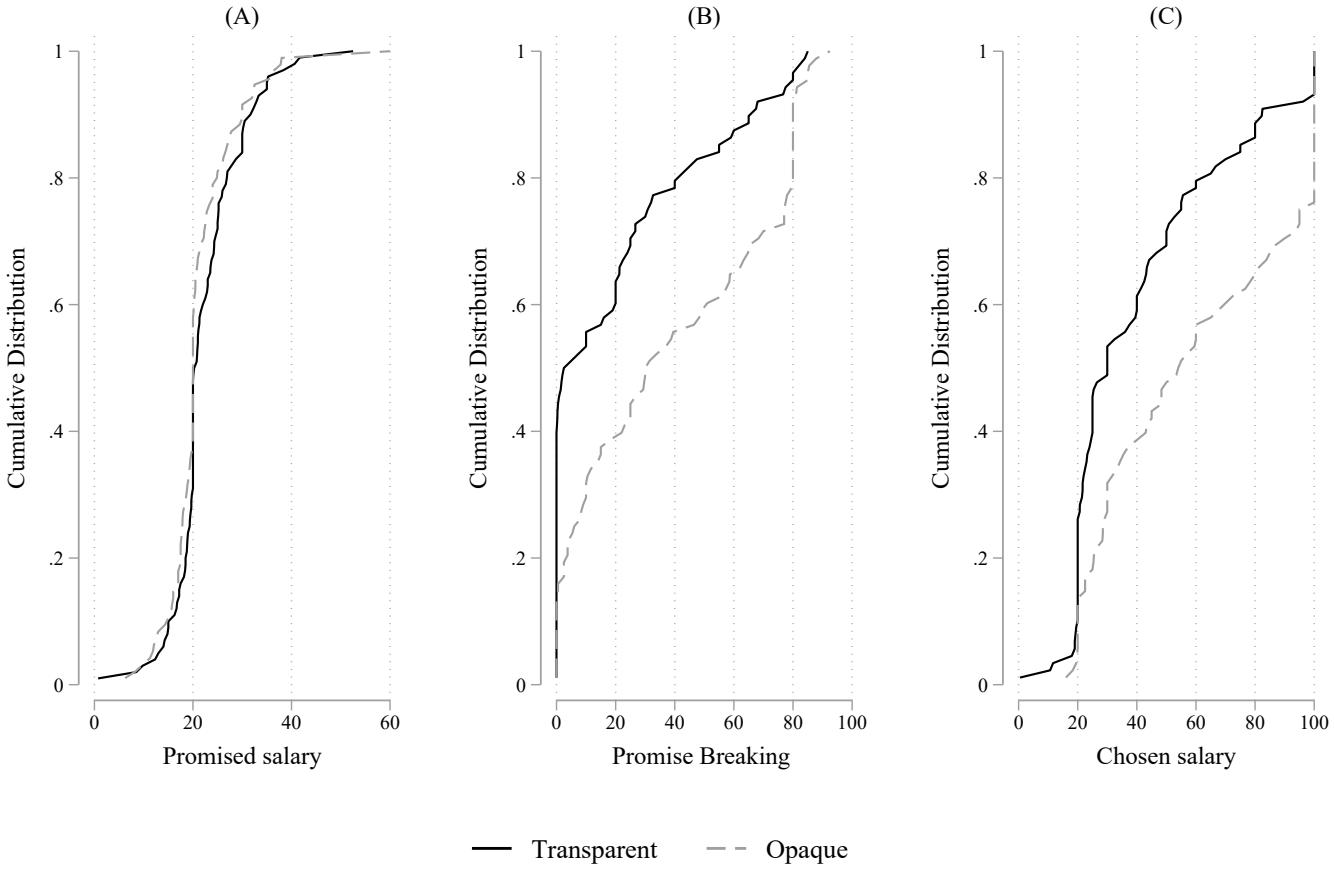

Figure 1: Promises, promise breaking, and salaries across treatments

Cumulative distribution functions (individual means)

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 value of zero to our measure of promise breaking. Figure 1 Panel B shows the cumulative distribution function of promise breaking (individual means) across treatments. In line with Prediction 2, the extent of promise breaking is significantly lower in *Transparent* than in *Opaque* institutions (p-value < 0.001, Mann-Whitney tests on group level). Similarly to the extent, also the likelihood of promise breaking is significantly smaller in *Transparent* institutions (p-value < 0.001, Mann-Whitney tests on group level).

Result 2. *Promise breaking is less prevalent in* Transparent *institutions.*

4.3 Salary choices

For salaries in *Transparent* and *Opaque*, our model predicts an ambiguous comparison. Panel C in Figure 1 illustrates the cumulative distribution function of chosen salaries (in percent of the endowment, individual means) across treatments. We observe significantly less generous salaries in *Opaque* than in *Transparent* institutions (Mann-Whitney tests on group level, p-value < 0.001). Hence, opacity hurts voters if they are unable to observe

promise breaking nor their economic circumstances (as they do not receive feedback regarding payoffs in *Opaque* before the end of the experiment). In Section 5, we discuss what shapes the value of transparency for voters' outcomes.

Result 3. *Salaries are more generous in the Transparent institution.*

4.4 Voting Behavior and Reelection of the Incumbent

For the instrumental reputational concerns in *Transparent* institutions to matter, reelection is essential. In *Opaque* institutions, promise keeping must rely on self-image concerns which do not depend on reelection. Indeed, the incumbent's probability of winning the election is on average 16 percentage points higher and, thus, significantly higher in the *Transparent* than in the *Opaque* institution (see also Table 1 in Section 5).[19]

Result 4. *Reelection is more likely in* Transparent *institutions.*

4.5 Learning and Summary

Building on our theoretical considerations, opacity was expected to systematically shape promise competition. In *Transparent*, voting creates an instrumental value for officials to keep their promises. In *Opaque*, voters have to rely on candidates' preferences for truth-telling. Due to opacity, voters cannot identify honest politicians but preferences for truth-telling shape the range of credible promises (made by both honest and dishonest officials). Empirically, officials indeed make more generous promises in *Opaque*. Promise breaking is more prevalent in *Opaque* so that final salaries are higher in *Opaque*. Reassuringly, these main results are not driven by systematic adjustments due to learning across rounds (see regression specifications in Tables A.2 to A.7 which include the total number of rounds as control variable) and also replicate when considering the last supergame only: promises are more generous in *Opaque* (Mann-Whitney tests on group level, p-value $= 0.087$), the extent of promise breaking differs significantly (Mann-Whitney tests on group level, p-value $= 0.001$) and salaries are larger in *Opaque* (Mann-Whitney tests on group level, p-value $= 0.009$). Nevertheless, we observe that participants in *Opaque* make

[19] We provide further insights on voting behavior in Appendix A.3.6, which confirm the higher reelection probability of incumbents in *Transparent* using regressions (see Table A.7). Further, we show that breaking promises in *Transparent* substantially reduces the incumbent's reelection probability. Interestingly, we do not find a strong correlation between own promise breaking and voting for (other) promise breaking incumbent's (Spearman's $\rho = -0.0344$, $p = 0.753$).

slightly more generous promises across time, but choose higher wages in later rounds. In turn, promise breaking increases across time in *Opaque*. In all other treatments, we do not observe significant changes in promise breaking across time. Finally, we observe no trends in the reelection probability of the incumbent in any treatment (see also Table A.16 in the Appendix). As our setting only allows for a limited scope of learning, asymmetries in the latter unlikely explain the observed treatment differences in promises, promise breaking and salary choices. Clearly, it is an interesting avenue for future research to explore in more detail how learning opportunities shape the value of transparency.

We find that promises, promise breaking, and re-election probabilities are significantly different between the *Transparent* treatment and the *Opaque* treatment. Matching the experimental findings to our theoretical guidance, we argue that the observed differences in behavior reflect differences in instrumental reputational concerns across treatments, because reputation can by design only work in *Transparent* but not in *Opaque*. Preferences for truth-telling or inequality aversion alone cannot explain the observed behavioral differences between *Opaque* and *Transparent*, because these preferences should affect behavior similarly in both treatments. Preferences for own material payoffs cannot explain that promises are sometimes kept in *Opaque*. Hence, we conclude that in *Opaque* preferences for truth-telling shape promise competition while instrumental reputational concerns shape promise competition in *Transparent*.

5 Mechanisms

In our main treatment, opacity implies three changes compared to the transparent benchmark. First, voters cannot observe promise keeping. Thus, there is lack of accountability. Second, promise keeping becomes unobservable. Thus, independent of accountability, there is lack of observability of promise keeping. Third, voters and incumbents do not receive feedback about their economic circumstances. Thus, there is lack of observability of economic circumstances. In this section, we study two additional treatments called *Opaque I* and *Opaque II* to examine these effects in isolation.[20] In *Opaque I*, voters can-

[20]Note that the order of treatment presentation was chosen to provide the best conceptual understanding of how the treatments relate to each other. Chronologically, the treatments were run in a different order: the sessions for *Opaque I* and *Transparent* were run in parallel in May, November and December 2018, *Opaque II* was run in December 2019, and *Opaque* was run in July and August 2022 based on the suggestions of an anonymous referee.

Table 1: Promises, Promise Breaking and Salaries across treatments

	Opaque ($N = 95$)		Transparent ($N = 100$)		Opaque I ($N = 90$)		Opaque II ($N = 100$)	
	Mean	(SD)	Mean	(SD)	Mean	(SD)	Mean	(SD)
Promised Salary	20.84	(8.97)	22.58	(10.29)	17.40	(6.46)	20.07	(10.76)
Promise Breaking	40.09	(35.09)	16.24	(28.68)	24.14	(29.42)	23.44	(29.32)
Pr(Promise Breaking > 0)	76%	(0.43)	36%	(0.48)	78%	(0.42)	69%	(0.46)
Chosen Salary	59.81	(33.64)	36.93	(28.84)	40.84	(29.87)	42.25	(28.72)
Pr(Reelection Incumbent)	33%	(0.47)	49%	(0.50)	42%	(0.49)	41%	(0.49)

not observe promise keeping but everyone can observe economic circumstances. Hence, *Opaque I* combines observability of economic circumstances with lack of accountability and lack of observability of promise keeping. *Opaque II* adds independent observers to *Opaque I*. Hence, *Opaque II* combines observability of promise keeping and economic circumstances with lack of accountability, as in *Opaque II*, voters cannot observe promise keeping but everyone can observe economic circumstances. These two additional treatments allow us to shed light on what shapes the value of transparency, as well as whether and how partial transparency affects promise competition. We recruited another 190 participants for these two additional treatments, for which we have 18 independent observations (*Opaque I*) and 20 (*Opaque II*), respectively. In total, the experiment thus encompasses 385 participants (see also Table 1).

5.1 Opaque environments with economic circumstances

This treatment is identical to our *Opaque* treatment except that voters are informed about the total points they earned each round. Remember that 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 existence of both payoff components but neither about 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 randomness makes it difficult for citizens to infer the salary chosen by the official from their own payoffs. Even if they were to know the support of the distributions and the distributions, it is impossible for them to infer the official's salary with certainty. Our empirical analyses of voting behavior in Appendix A.3.6 highlights that this treatment manipulation was successful. In *Transparent*, the incumbent's probability of reelection is significantly smaller when the incumbent breaks her promise. In *Opaque I and II*, there

is no significant effect of promise breaking on reelection probabilities (see Tables A.9, A.11, A.13, and A.15 in the appendix). Furthermore, we designed the experiment such that it is 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. This design allows us to study the causal effect of observability of economic circumstances combined with lack of accountability and lack of observability of promise keeping. This design is akin to many real-world settings in which economic circumstances are observable but voters (not necessarily) can condition their behavior on officials promise keeping behavior.

From a theoretical perspective, the analysis in Proposition 2 remains qualitatively unchanged in such an environment. However, the intensity of the preferences for truth-telling can be expected to differ due to observability of economic circumstances. In *Opaque*, candidates may exploit some moral wriggle room by telling themselves that lying and choosing a higher salary may be offset by a high random payoff component in voters' payoffs.[21] In *Opaque I*, this moral wriggle room is reduced as the incumbent observes their own payoff after every round and thus faces stronger constraints regarding their expectation about the magnitude of the random component in voters' payoffs. Hence, *Opaque I* makes lying more costly as candidates cannot tell themselves so easily that their lie does not substantially hurt anyone. In our model, this increase in lying costs in *Opaque I* as compared to *Opaque* is captured by the parameter λ (as discussed in the beginning of Section 2). Remember, we assumed that $\lambda = 1$ for *un*observable promise breaking with full moral wriggle room in *Opaque* which implies $\lambda_I > 1$ for *Opaque I*. Consequently, our theoretical predictions regarding the comparison to *Transparent* are qualitatively the same as in *Opaque*, that is, more generous promises in *Opaque I* than *Transparent* and more promise breaking. However, promise breaking in *Opaque I* is predicted to decrease as compared to *Opaque*.[22]

Empirically, promises are significantly more generous (i.e., promised salaries are significantly smaller) in *Opaque I* than in *Transparent* (Mann-Whitney tests on group level, p-value $= 0.0001$) as shown in Table 1. Further, the extent of promise breaking is lower in *Transparent* than in *Opaque I* (p-value $= 0.054$, Mann-Whitney tests on group level).

[21] Given that random payoff components in our experiment are independent of salary choices, this argument is theoretically flawed but, e.g., Exley and Kessler (2022) and Dana et al. (2007) show that humans are nevertheless willing to exploit such moral wriggle room.

[22] See Proposition 3 in the appendix for the formal result.

Similarly to the extent, also the likelihood of promise breaking is significantly smaller in *Transparent* (Mann-Whitney tests on group level, p-value < 0.001). For salaries in *Transparent* and *Opaque*, our model predicts ambiguous effects. While we found differences in salaries between *Transparent* and *Opaque* in Section 4.3, differences are much less pronounced between *Transparent* and *Opaque I*. While average salaries tend to be higher in *Opaque I* than *Transparent* (on average 40.84 vs. 36.93), differences across these two treatments are statistically insignificant (p-value $= 0.421$, Mann-Whitney test on group level). Hence, if economic circumstances are observable (*Opaque I*), rent appropriations are not substantially worse compared to *Transparent*. The main reason for this result is that – conditional on promise keeping – generous salaries (i.e., at most 20 percent of the endowment) are more common in *Opaque I* than in *Transparent*. In *Opaque I*, almost all promises kept resulted in salaries of 20 percent or less. In *Transparent*, instead, a substantial fraction of promises kept resulted in salaries larger than the equal share (*Transparent* vs *Opaque I*: p-value $= 0.002$, Mann-Whitney tests on group level, conditional on promise keeping). Hence, even if voters are unable to observe promise breaking, observable economic circumstances may recover a large part of the value of full transparency. Lastly, as expected, the incumbent's probability of winning the election is higher in *Transparent* than in *Opaque I* as the regressions in Table A.7 in the appendix confirm.

5.2 Opaque environments with social image concerns

Introducing *Opaque II*, we study the role of additional social image concerns due to the observability of promise breaking. Akin to *Opaque I*, economic circumstances are observable but voters are not informed about promise keeping. Instead, four independent outside observers see promises and salary choices of candidates. Hence, as in *Transparent*, officials choose their salary knowing that four other participants observe their promise and salary choice (observability of promise keeping and economic circumstances) but there is no instrumental value from keeping a promise (as observers do not vote and voters cannot observe chosen salaries).[23] Compared to *Transparent, Opaque II* removes instrumental reputational concerns whereas compared to *Opaque I, Opaque II* introduces social image concerns due to the observability of promises by observes. This environment

[23]Observers' earnings in *Opaque II* 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.

thus 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 – creating a lack of accountability.

While the theoretical analysis in Proposition 2 remains qualitatively unchanged the observability by independent observers introduces social image concerns which is captured by an increase in λ (as discussed above and in the beginning of Section 2). Remember we assumed $\lambda = 1$ for *un*observable promise breaking with full moral wriggle room and $\lambda_I > 1$ for *Opaque I* which implies $\lambda_{II} > \lambda_I$ for *Opaque II*. Hence, predictions for *Opaque II* are qualitatively similar to *Opaque* and *Opaque I* but promise breaking is predicted to decrease in *Opaque II* compared to *Opaque* and *Opaque I*.[24]

Empirically, we find that an increase in social image concerns indeed seem to play a role for promise competition. In particular, promises are significantly more generous in *Opaque II* as compared to *Transparent* (Mann-Whitney tests on group level, p-value $= 0.016$) and *Opaque I* (Mann-Whitney tests on group level, p-value $= 0.075$). Thus, both the lack of observability and accountability affect promises. The extent of promise breaking in *Opaque II* is higher than in *Transparent* (Mann-Whitney tests on group level, p-value $= 0.066$) but does not significantly differ from promise breaking in *Opaque I* (Mann-Whitney tests on group level, p-value $= 0.759$). Similarly to the extent, also the likelihood of promise breaking is significantly smaller in *Transparent* (Mann-Whitney tests on group level, p-value < 0.001) but does not significantly differ between *Opaque II* and *Opaque I* (Mann-Whitney tests on group level, p-value $= 0.187$). Salaries in *Opaque II* tend to be lower than in *Transparent* but do not differ significantly (Mann-Whitney test on group level, *Transparent* vs. *Opaque II*: p-value $= 0.164$, similar to *Opaque I* and for similar reasons). Salaries do not differ significantly in *Opaque II* as compared to *Opaque I* (Mann-Whitney test on group level, p-value $= 0.682$). As expected, reelection of the incumbent is less likely also in *Opaque II* as compared to *Transparent* (as confirmed by regression analyses in Table A.7 in the appendix). Overall, *Opaque I* and *Opaque II* thus highlight that voters may substantially benefit from the observability of economic circumstances when transparency about promise breaking is missing while additional image concerns due observability by independent outsiders does not further reduce rent appropriations.

[24] See Proposition 3 in the appendix for the formal result.

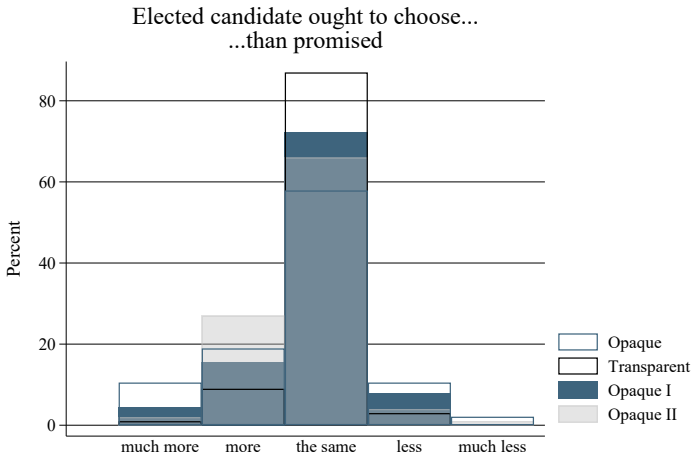

Figure 2: Injunctive norms (across treatments)

5.3 Norms and the meaning of promises

Our analyses show how opacity affects the nature of promise competition when decision-makers have preferences for truth-telling. Opacity may not only affect lying costs by reducing social image concerns and adding moral wriggle room 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 participants' injunctive and descriptive norms about promise keeping after the experiment.[25] For the elicitation, we adopt incentive-compatible methods introduced by Krupka and Weber (2013). The idea of this method is to measure shared beliefs about desirable behavior. Injunctive norms refer to perceptions of which behaviors are typically acceptable. In our norm elicitation task, participants are incentivized to guess the modal answer (by all active decision-makers in their session).[26] We ask whether "an elected official *ought to* choose a share 'much larger', 'larger', 'equal to', 'smaller' or 'much smaller' than announced". If a participant's guess was correct in the payoff-relevant question, she earned additional 20 points (EUR 1.40).[27]

[25] We did so to avoid priming effects for active decision-makers' (i.e. voters'). Passive observers in the *Opaque II* treatment stated their norms twice, before and after the voting experiment.

[26] In *Opaque II*, observers' answers were not included when calculating the modal choice, and voters as well as observers were explicitly told so.

[27] Our 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.

Figure 2 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 $= 0.237$, Mann-Whitney test on group level). 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 II* treatment (see Figure A.3 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).[28] We relegate the discussion of descriptive norms to Appendix A.3.8.

6 Conclusion

Competition in promises occurs in many economic environments, but observing whether such promises are kept may be costly or even impossible. This study asks how such 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 idea that candidates do not necessarily do what they say during their campaigns (Banks, 1990) but have (heterogeneous) preferences for truth-telling (see also Abeler et al., 2019; Gerlach et al., 2019). 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 an incentivized experiment, we show that opacity indeed increases the generosity of promises as well as the frequency of promise breaking. Furthermore, we find that 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.[29] Although differences in rent appropriations are sometimes lower across institutions than

[28] This is also in line with evidence by Galeotti and Zizzo (2018) of voters having a direct preference for honest candidates.

[29] In a similar spirit, Dana and Weber (2007) show that some individuals behave generously in dictator games because they dislike appearing unfair to others.

might be expected, their nature hinges on two distinct underlying factors. In opaque environments, preferences for truth-telling stemming from self-image and social image concerns are the main determinants of behavior, whereas instrumental reputational concerns are the driving mechanism in transparent institutions. The costs of this mechanism are the implied office rents that have to be paid to incumbents. Our theoretical guidance as well as previous literature (both theoretically and experimentally) show that the implied office rents depend on the discount factor. The more patient the incumbent, the lower office rents transparent institutions require. Indeed, in the limit of full patience and certainty of continuation, office rents vanish and salaries as well as allocations approximate the equal split. Therefore, to assess the value of transparency, having the right estimate of the discount factor is essential. While certain institutional details, like term limits, could imply rather low patience of incumbents, patience might be quite high in other settings. Determining exact discount factors or varying discount factors across all possible values is beyond the scope of this paper.[30] Thus, our experiment only allows us to estimate the value of transparency for the chosen parameter values. We find a positive value of transparency as compared to a fully opaque environment, but, surprisingly, demonstrate that observable economic circumstances reestablish a large part of the value of transparency even when voters cannot directly observe whether promises are broken.

From a policy perspective, our conclusion is two-fold. Transparent institutions, on the one hand, generate instrumental concerns for politicians to keep their promises (due to negative consequences when promise breaking is observed). Opaque institutions, on the other hand, are not necessarily characterized by empty campaign promises, or full rent appropriation. Instead, preferences for truth-telling among candidates systematically shape the credibility of promises and promise keeping. Hence, promises still serve as an opportunity to honor future obligations. In turn, the value of transparency hinges crucially on (dis)honesty among candidates and candidates' discount factors.

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 candidates for the value of transparency. Although lying costs appear strong and widespread it appears important to understand self-selection into specific environments of promise competi-

[30] In our experiment, we use a discount factor of 0.8 following the literature, in particular, Feltovich and Giovannoni (2015), to allow for better comparability.

tion.[31] While the extent of self-selection based on preferences for truth-telling may substantially differ across environments, our study highlights how self-selection affects the value of transparency for promise competition. Lying costs determine the credibility of promises in both transparent and opaque environments but instrumental reputational concerns limit the generosity of credible promises in transparent environments. With opacity, instead, honest candidates may keep even very generous promises which they have made to avoid large rent appropriations by their less honest competitors. As in equilibrium candidates will pool on the most generous credible promise, heterogeneity of preferences for truth-telling determines the extent of rent appropriations and thereby the value of transparency.

Further, our study considers opacity an exogenous factor. 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. Thus, we may ask who prefers transparent or opaque institutions in markets with promise competition. Related to recent contributions in the context of investment behavior (Khalmetski et al., 2017; Tergiman and Villeval, 2023), it is interesting to study environments where politicians may themselves decide whether promise breaking is deniable by designing promises in specific ways, as deniability can render retrospective voting ineffective. Alternatively, politicians may use intentionally vague promises (Blume and Board, 2014; Serra-Garcia et al., 2011). Vice-versa, voters may explicitly acquire information about promise breaking, e.g., through costly but trusted media outlets, or rationally ignore available information (Matějka and Tabellini, 2021; Maćkowiak et al., 2023).

[31] See also Andreoni and Serra-Garcia (2021) 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 Credible 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 promise[32] 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 credible 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 credible equilibria then "picks the best possible equilibrium from the voters' point of view in a restricted class of voting rules" (Persson et al., 1997, p.1171) without relying on pivotality but thinking about voters being able to coordinate on the equilibrium.

[32] In 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).

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

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

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

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

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

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.

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.[33] 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^*.[34] Such a deviation is unprofitable for the incumbent if

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

$$\pi_T^* \geq (1-\delta) - (1-\delta)\frac{\lambda}{M}c^1(1-\pi_T^*) + \delta^2\frac{1-\pi_T^*}{n-1} \Leftrightarrow$$

$$\pi_T^* \geq \frac{\delta^2 + (1-\delta)(n-1)(1-\frac{\lambda}{M}c^1(1-\pi_T^*))}{n-1+\delta^2} = \tag{1}$$

$$= 1 - (n-1)\frac{\delta + (1-\delta)\frac{\lambda}{M}c^1(1-\pi_T^*)}{n-1+\delta^2} \in (0,1)$$

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 credible equilibria ensures that in any credible equilibrium reelection of the incumbent occurs with probability one if the incumbent kept her promise and makes the same promise as the challenger. □

[33] If 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 $1 - 1/n$.

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

Proof of Proposition 2: In *Opaque* institutions, voting based on promise keeping is impossible. Voters vote based on promises 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 losing 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 $1-\pi$.

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

Hence,

$$\pi_O^* = \frac{(1-\pi_O^*)(1-\text{Prob}(\gamma^j > 1-\pi_O^*)) - \int_0^{1-\pi_O^*} \gamma^j \mathrm{d}G(j)}{n-1} \quad \Leftrightarrow$$

$$\pi_O^*(n - \text{Prob}(\gamma^j > 1-\pi_O^*)) = 1 - \text{Prob}(\gamma^j > 1-\pi_O^*) - \int_0^{1-\pi_O^*} \gamma^j \mathrm{d}G(j) \Leftrightarrow$$

$$\pi_O^* = \frac{1 - \text{Prob}(\gamma^j > 1-\pi_O^*) - \int_0^{1-\pi_O^*} \gamma^j \mathrm{d}G(j)}{n - \text{Prob}(\gamma^j > 1-\pi_O^*)}$$

The last equality implies that $\pi_O^* \leq 1/n$ because

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

decreases in $\text{Prob}(\gamma^j > 1-\pi_O^*)$. Therefore, the truncation does not matter as we assumed the lowest lying costs to have $\gamma^1 = (n-1)/n$. Consequently, the optimal promise equals

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

In general, $\pi_O^* < 1/n$ is valid if $\text{Prob}(\gamma^j > 0) > 0$. Given a promise of π_O^*, an official i' chooses a salary of $\pi_O^* + \gamma_{i'}$. Thus, salaries follow the distribution G.

If any candidate makes a lower offer, she loses the election. If any candidate makes a higher offer, she loses the election. The expected utilities from losing 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 losing the election. Consequently, it is optimal to promise π_O^*.

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

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

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. □

Proof of Prediction 3: Combining Propositions 1 and 2 yields that promises are expected to be more generous in *Opaque* institutions than in *Transparent*. Conditional on promise keeping, salaries equal promises. Hence, Prediction 1 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

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

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 $\int_0^{1-\pi_O^*} \gamma^j \mathrm{d}G(j)$ 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. □

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

Remember that in Section 5, we define the values $\lambda_{II} > \lambda_I > 1$ for promise breaking if observability of economic circumstances reduces moral wriggle room (λ_I) or observability by independent observers triggers social image concerns (λ_{II}). Denote the corresponding values of the wedge γ by γ_I and γ_{II} with induced distributions G_I and G_{II}.

Proposition 3. *With observability by independent observers or economic circumstances, the amount of promise breaking is smaller then without such observability:*

$$\gamma_{II} < \gamma_I < \gamma.$$

Obviously, the distribution G first-order stochastically dominates G_I which dominates G_{II}. In Opaque *$t \in \{I, II\}$, promising a salary of*

$$\pi_t^* = \frac{1-\mathbb{E}_{G_t}(\gamma_t^j)}{n}$$

is optimal. If candidate i is elected, she chooses a salary of $s_t^* = \pi_t^* + \gamma_{i,t}$. Promise breaking and chosen salaries follow the distribution G_t of lying costs.

Proof of Proposition 3: We begin by defining γ_t^j for $t \in \{I, II\}$. Define $\gamma_t^j = \max\{0, \alpha\}$ with

$$\lambda_t (c^j)'(\alpha) = M.$$

As lies that materially disadvantage the decision-maker create no lying costs, the maximum operator takes care of corner solutions at zero. The definition of γ_t^j implies

$$M > (c^j)'(\gamma_t^j) \geq M/\lambda_t$$

for all $j \in (0, 1]$. The inequality $\lambda_{II} > \lambda_I > 1$ and the convexity of c^j ensures that $\gamma_t^0 = 0 = \gamma^0$ for $t \in \{I, II\}$ and $\gamma_{II}^j < \gamma_I^j < \gamma^j$ for all $j \in (0, 1]$. Therefore the distribution G first-order stochastically dominates G_I which dominates G_{II}.

The values of optimal promises π_t^* and optimal salaries s_t^* for $t \in \{I, II\}$ are derived analogously to the Proof of Proposition 2. □

A.3 Additional empirical analyses

A.3.1 Participant characteristics

Table A.1 shows participant characteristics across treatments. We observe similar shares of females across treatments, with slightly more females in *Opaque II*. Further, risk attitudes, age, math grades and political opinion appears similar across all treatments. Compared to *Opaque*, participants in the other treatments tend to declare less available income (net of costs for housing and health insurance) and participants in *Opaque II* tend to be more experienced in terms of their self-stated participation in experimental studies. Controlling for multiple hypothesis testing for multiple outcomes and treatments (multiplicity-adjusted p-values, see Theorem 3.1 in List et al., 2019), these differences turn out to be statistically insignificant. Nevertheless, we include this variables as controls in our regression analyses.

A.3.2 Promises

Table A.2 presents results on promises using random-effects Tobit models (taking censoring at 0 and 100 percent into account). The dependent variable is the salary announced by candidates before the election (ranging from 0 to 100 percent of the endowment). The explanatory variables include our treatment indicator (*Transparent*, *Opaque I*, *Opaque II*

Table A.1: Participant characteristics across treatments

	Opaque ($N = 95$)		Transparent ($N = 100$)		Opaque I ($N = 90$)		Opaque II ($N = 100$)	
	Mean	(SD)	Mean	(SD)	Mean	(SD)	Mean	(SD)
Female	0.60	(0.49)	0.57	(0.50)	0.61	(0.49)	0.71	(0.46)
Willingness risk-taking	3.54	(1.58)	3.76	(1.63)	3.50	(1.59)	3.51	(1.65)
Age	22.56	(2.67)	23.27	(5.97)	22.62	(4.85)	23.83	(4.36)
Available Income	636.47	(578.32)	440.55	(314.63)	412.28	(233.91)	436.45	(309.87)
Math grade	2.20	(1.06)	2.25	(1.00)	2.18	(0.96)	2.34	(1.19)
Politically right (1-7)	3.21	(1.10)	3.27	(1.14)	3.20	(1.19)	3.26	(1.24)
Num. experiments	4.68	(5.53)	8.92	(14.89)	6.57	(10.99)	12.09	(22.99)

Notes: Rows report means and standard deviations (in parentheses). Stars indicate significant differences to *Opaque* according to *p*-values adjusted for multiple hypothesis testing following (List et al., 2019, Theorem 3.1), with * = p $<$ 0.10, ** = p $<$ 0.05 and *** = p $<$ 0.01).

– using *Opaque* as baseline), a dummy for whether the promise was made by the incumbent, a period control and candidates' individual characteristics. The observed treatment effects from Figure 1 (Panel A) are mirrored in this analysis. On average, candidates' promised salary is higher in *Transparent* than in *Opaque*. As shown in Model (5), this finding is mainly driven by incumbents, whose promises are less generous in *Transparent*. Incumbents announce higher salaries than challengers, as incumbents can rely on their reputation in *Transparent* institutions.

For robustness, Table A.3 reports results from random-effects GLS regressions (clustering standard errors on the group level). The results are very similar.

Table A.2: Candidates' promised salary (in percent of endowment)

	Dependent variable: Promised salary				
	(1)	(2)	(3)	(4)	(5)
Transparent	1.496	1.543	1.560	0.947	0.106
	(0.960)	(0.981)	(0.995)	(0.981)	(0.935)
Opaque I	-3.691***	-3.706***	-3.747***	-4.330***	-4.155***
	(0.768)	(0.804)	(0.808)	(0.800)	(0.835)
Opaque II	-0.996	-1.021	-1.041	-1.730*	-2.167*
	(0.931)	(0.970)	(1.008)	(0.993)	(1.107)
Incumbent		1.420***	1.441***	1.414***	0.816
		(0.375)	(0.379)	(0.393)	(0.618)
Transparent x Incumbent					1.833*
					(1.067)
Opaque I x Incumbent					-0.370
					(0.846)
Opaque II x Incumbent					0.894
					(1.105)
Constant	21.12***	20.44***	21.77***	21.23***	21.59***
	(0.597)	(0.661)	(0.809)	(2.731)	(2.843)
N	2107	2107	2107	2107	2107
Round controls	No	No	Yes	Yes	Yes
Individual characteristics	No	No	No	Yes	Yes

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

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

	Dependent variable: Promised salary				
	(1)	(2)	(3)	(4)	(5)
Transparent	1.523	1.572	1.588	0.982	0.136
	(1.481)	(1.481)	(1.486)	(1.463)	(1.472)
Opaque I	-3.684***	-3.698***	-3.740***	-4.315***	-4.127***
	(1.258)	(1.260)	(1.265)	(1.236)	(1.237)
Opaque II	-0.976	-0.999	-1.019	-1.698	-2.128
	(1.531)	(1.531)	(1.534)	(1.583)	(1.711)
Incumbent		1.414***	1.435***	1.411***	0.820
		(0.362)	(0.359)	(0.359)	(0.548)
Transparent x Incumbent					1.848*
					(0.951)
Opaque I x Incumbent					-0.397
					(0.789)
Opaque II x Incumbent					0.879
					(0.993)
Constant	21.12***	20.44***	21.77***	21.19***	21.56***
	(1.047)	(1.053)	(1.211)	(3.055)	(3.034)
N	2107	2107	2107	2107	2107
Round controls	No	No	Yes	Yes	Yes
Individual characteristics	No	No	No	Yes	Yes

The table displays results from GLS random effects models.
Standard errors (clustered on group level) in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

A.3.3 Promise Breaking

Below, we present results from random-effects Tobit models on promise breaking (Table A.4a) confirming the non-parametric results reported in the main text (see Section 4.2). In Table A.4b, we consider the same specifications as in Table A.4a, but use random-effects GLS estimations with standard errors clustered on the group level. As in all other regressions, we then introduce additional controls (period and average group characteristics) in specifications (2) and (3). Qualitative results are very similar: transparency reduces promise breaking substantially.

Table A.4: Promise breaking across treatments

	Dependent variable: Promise Breaking		
	(1)	(2)	(3)
Transparent	-33.84***	-33.89***	-33.19***
	(6.468)	(6.558)	(7.144)
Opaque I	-11.54*	-11.21*	-9.752
	(6.039)	(6.087)	(6.245)
Opaque II	-15.47**	-15.51***	-13.96**
	(6.032)	(5.776)	(6.556)
Constant	33.48***	26.97***	20.65
	(4.504)	(4.897)	(15.33)
N	1061	1061	1061
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, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

(a) Tobit

	Dependent variable: Promise Breaking		
	(1)	(2)	(3)
Transparent	-19.69***	-19.84***	-19.84***
	(4.398)	(4.421)	(4.748)
Opaque I	-12.10**	-11.88**	-10.92**
	(4.766)	(4.745)	(4.767)
Opaque II	-13.45***	-13.52***	-12.87***
	(4.628)	(4.632)	(4.937)
Constant	39.49***	34.79***	23.92***
	(3.569)	(3.343)	(8.638)
N	1061	1061	1061
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, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

(b) GLS

A.3.4 Promise Breaking (Extensive Margin)

Focusing on the extensive margin of promise breaking, Figure 1 reveals that, in line with predictions, promise breaking occurs significantly more often in *Opaque* institutions. Less than 20 percent of individuals keep their promises in the *Opaque* institution. In the *Transparent* institution instead, more than 40 percent of officials keep their promises. These results are also confirmed by regression analyses shown in Table A.5a and A.5b. In Table A.5a (Table A.5b) 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.5: Probability of promise breaking (ext. margin)

	Dependent variable: Promise Breaking (ext. margin)		
	(1)	(2)	(3)
Transparent	-0.304***	-0.306***	-0.295***
	(0.0487)	(0.0489)	(0.0509)
Opaque I	0.0353	0.0356	0.0481
	(0.0585)	(0.0583)	(0.0583)
Opaque II	-0.0530	-0.0544	-0.0398
	(0.0508)	(0.0512)	(0.0555)
N	1061	1061	1061
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, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

(a) Probit

	Dependent variable: Promise Breaking (ext. margin)		
	(1)	(2)	(3)
Transparent	-0.350***	-0.351***	-0.340***
	(0.0558)	(0.0560)	(0.0578)
Opaque I	0.0250	0.0263	0.0362
	(0.0531)	(0.0529)	(0.0531)
Opaque II	-0.0596	-0.0600	-0.0469
	(0.0508)	(0.0511)	(0.0544)
Constant	0.769***	0.730***	0.817***
	(0.0345)	(0.0380)	(0.135)
N	1061	1061	1061
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, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

(b) GLS

A.3.5 Salaries

Table A.6a reports results from random-effects Tobit regressions confirming the non-parametric finding mentioned in the main text. For robustness, we further report results from random-effects GLS models in Table A.6b.

Table A.6: Candidates' chosen salaries (in percent of endowment)

	Dependent variable: Chosen salary		
	(1)	(2)	(3)
Transparent	-23.78***	-23.94***	-24.47***
	(5.707)	(5.908)	(5.907)
Opaque I	-20.49***	-20.33***	-19.42***
	(6.145)	(5.868)	(5.990)
Opaque II	-19.21***	-19.30***	-18.43***
	(5.758)	(5.844)	(5.976)
Constant	66.90***	62.87***	52.51***
	(4.863)	(4.931)	(12.21)
N	1061	1061	1061
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, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

(a) Tobit

	Dependent variable: Chosen salary		
	(1)	(2)	(3)
Transparent	-18.58***	-18.69***	-18.88***
	(4.509)	(4.525)	(4.889)
Opaque I	-15.34***	-15.18***	-14.39***
	(4.682)	(4.664)	(4.700)
Opaque II	-14.26***	-14.31***	-13.75***
	(4.442)	(4.443)	(4.643)
Constant	59.47***	56.14***	46.72***
	(3.353)	(3.206)	(8.640)
N	1061	1061	1061
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, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

(b) GLS

A.3.6 Voting Behavior and Reelection of the Incumbent

To study reelections, we regress whether the incumbent is reelected on the treatment dummy *Transparent*, *Opaque I*, and *Opaque II* only, using *Opaque* as baseline. The incumbent's probability of winning the election is on average 14-17 percent higher in *Transparent* as compared to the *Opaque* institution, and also higher as compared to *Opaque I* and *Opaque II* (as Wald-tests reject equality of the treatment coefficients in the specifications, at p-values < 0.10). That is, reelection of the incumbent is significantly more likely in *Transparent* institutions. Adding signals about the economic state (*Opaque I*) or observers (*Opaque II*) implies substantially smaller changes in the reelection probability of the incumbent, which are also much less precisely estimated.

Table A.7: Reelection of Incumbent (group level analyses)

	Dependent variable: Incumbent reelected		
	(1)	(2)	(3)
Transparency	0.146***	0.145***	0.162***
	(0.050)	(0.050)	(0.052)
Opaque I	0.096**	0.096**	0.098**
	(0.047)	(0.047)	(0.050)
Opaque II	0.090**	0.090**	0.090*
	(0.043)	(0.043)	(0.048)
Reelection probability in baseline (*opaque*): 33 percent			
N	1061	1061	1061
Round controls	No	Yes	Yes
Group characteristics	No	No	Yes

The table displays average marginal effects from random effects Probit models. Baseline: *opaque*. Standard errors (clustered on group level) in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

(a) Probit

	Dependent variable: Incumbent reelected		
	(1)	(2)	(3)
Transparency	0.160***	0.159***	0.174***
	(0.059)	(0.059)	(0.056)
Opaque I	0.094*	0.094*	0.097*
	(0.054)	(0.054)	(0.056)
Opaque II	0.081	0.081	0.085
	(0.052)	(0.052)	(0.058)
Constant	0.326***	0.305***	0.320
	(0.041)	(0.048)	(0.241)
N	1061	1061	1061
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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

(b) GLS

To further investigate voting behavior in *Transparent* and *Opaque* institutions, we use votes by all citizens excluding challengers and incumbents in situations in which both candidates made a promise.[35] In addition, we focus on all voting decisions for which an elected incumbent was in office for at least one period.[36] Table A.8 and A.9 show results from random-effects Probit regressions estimating the probability of voting for the incumbent and report average marginal effects (results from linear probability models are shown in Tables A.10 and A.11).[37] 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 were both observable to participants in *Transparent* but not in *Opaque*). 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

[35]Both Incumbents and Challengers vote for themselves in 96 percent of elections. Further, candidates made promises in 98 percent of the cases.

[36]This is necessary as otherwise promise breaking in the previous period cannot be included as an explanatory variable. To highlight the robustness of our results, Tables A.12 , A.13, A.14, and A.15 include additional specifications in which we focus only on situations in which incumbent's and challenger's promise differed.

[37]Negative constants in linear specifications for the *Opaque I* and *Opaque II* 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).

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), and (8) 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).[38] Hence, in both treatments, promises shape voting behavior.

In the *Transparent* institution, we additionally find strong evidence for not voting for incumbents who broke their promises. Here, if the incumbent broke her promise, her probability of receiving a vote decreases by about 30 percentage points. In the *Opaque* institution, voters do not observe promise breaking nor their income such that they do not vote based on promise keeping. Importantly, in *Opaque I* and *Opaque II*, voters also do not vote based on promise breaking, as they do not observe officials' salary choices. Although, they appear unable to infer incumbents' promise breaking from the economic circumstances, they react to the latter. Including additional control variables does not substantially affect our main findings.

Table A.12 and A.13 reports average marginal effects of random-effects Probit regressions using the same specifications as in Tables A.8 and A.9 but consider only situations, in which the incumbent and the challenger did not make the same promise (for results from linear probability models see Tables A.14 and Table A.15). These specifications generally mirror the findings from above, in particular the strong reduction in the probability for voting for the incumbent when she breaks her promise in *Transparent*. Note however that although the number of elections in which the incumbent and challenger make different promises does not significantly differ across treatments (*Transparent*: 76 percent, *Opaque*: 73 percent, *Opaque I*: 75 percent, *Opaque II*: 69 percent, pairwise comparisons on group level, p-value > 0.16, Mann-Whitney tests), the voting situations in these specifications may be selective. Hence, these specifications may also capture some spurious correlations (e.g., in *Opaque*, in some specifications show a spurious correlation between voting behavior and promise breaking or past income, which were both not observed by voters.

[38]The latter effect is imprecisely estimated in the *Opaque* condition and thus fails to be statistically significant (p-value$= 0.154$).

Table A.8: Probability of voting for the incumbent (in *Transparent* and *Opaque*)

Dependent variable: Indicator for voting for the incumbent								
	Transparent				Opaque			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Incumbent's promise more generous	0.142***	0.144***	0.150***		0.145**	0.144**	0.143**	
	(0.042)	(0.040)	(0.037)		(0.058)	(0.057)	(0.057)	
Promise incumbent				-0.008***				-0.007
				(0.002)				(0.005)
Promise challenger				0.010***				0.011***
				(0.003)				(0.004)
Promise broken	-0.290***	-0.294***	-0.304***	-0.293***	-0.013	-0.008	-0.019	-0.014
	(0.070)	(0.070)	(0.067)	(0.067)	(0.046)	(0.048)	(0.047)	(0.049)
Own total income in previous round	0.003*	0.003*	0.003*	0.003*	-0.000	-0.000	-0.000	-0.000
	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
# voting decisions	432	432	432	432	444	444	444	444
Round controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Individual characteristics	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.9: Probability of voting for the incumbent (in *Opaque I* and *Opaque II*)

Dependent variable: Indicator for voting for the incumbent								
	Opaque I				Opaque II			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Incumbent's promise more generous	0.134**	0.135**	0.136**		0.208***	0.215***	0.214***	
	(0.059)	(0.057)	(0.057)		(0.047)	(0.048)	(0.048)	
Promise incumbent				-0.010**				-0.008**
				(0.004)				(0.003)
Promise challenger				0.012***				0.004***
				(0.004)				(0.001)
Promise broken	-0.018	-0.034	-0.025	-0.030	-0.048	-0.049	-0.038	-0.027
	(0.065)	(0.059)	(0.060)	(0.063)	(0.067)	(0.068)	(0.068)	(0.067)
Own total income previous round	0.008***	0.008***	0.008***	0.008***	0.008***	0.008***	0.008***	0.007***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
# voting decisions	417	417	417	417	447	447	447	447
Round controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Individual characteristics	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.10: Probability of voting for the incumbent (in *Transparent* and *Opaque*)

	Dependent variable: Indicator for voting for the incumbent							
	Transparent				Opaque			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Incumbent's promise more generous	0.143***	0.143***	0.146***		0.150**	0.149**	0.148**	
	(0.040)	(0.038)	(0.037)		(0.061)	(0.061)	(0.061)	
Promise incumbent				-0.008***				-0.007*
				(0.002)				(0.004)
Promise challenger				0.009***				0.011***
				(0.002)				(0.004)
Promise broken	-0.322***	-0.326***	-0.340***	-0.331***	-0.014	-0.009	-0.019	-0.014
	(0.079)	(0.079)	(0.077)	(0.078)	(0.046)	(0.048)	(0.047)	(0.048)
Total income previous round	0.003*	0.003*	0.002*	0.002*	-0.000	-0.000	-0.000	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Constant	0.175	0.151	0.260	0.290	0.369*	0.395**	0.170	0.112
	(0.252)	(0.257)	(0.290)	(0.282)	(0.194)	(0.191)	(0.255)	(0.279)
# voting decisions	432	432	432	432	444	444	444	444
Round controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Individual characteristics	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.11: Probability of voting for the incumbent (in *Opaque I* and *Opaque II*)

	Dependent variable: Indicator for voting for the incumbent							
	Opaque I				Opaque II			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Incumbent's promise more generous	0.134**	0.134**	0.136**		0.208***	0.214***	0.214***	
	(0.067)	(0.065)	(0.066)		(0.048)	(0.049)	(0.049)	
Promise incumbent				-0.009**				-0.003***
				(0.004)				(0.001)
Promise challenger				0.012***				0.004***
				(0.004)				(0.001)
Promise broken	-0.019	-0.035	-0.028	-0.031	-0.047	-0.048	-0.039	-0.018
	(0.070)	(0.065)	(0.067)	(0.070)	(0.072)	(0.073)	(0.073)	(0.073)
Total income previous round	0.008***	0.008***	0.008***	0.008***	0.008***	0.008***	0.008***	0.007***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Constant	-0.762***	-0.784***	-0.810***	-0.858***	-0.735***	-0.705***	-0.976***	-0.791***
	(0.156)	(0.154)	(0.203)	(0.209)	(0.144)	(0.143)	(0.213)	(0.218)
# voting decisions	417	417	417	417	447	447	447	447
Round controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Individual characteristics	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.12: Probability of voting for the incumbent (when promises differed)

	Dependent variable: Indicator for voting for the incumbent							
	Transparent				Opaque			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Incumbent's promise more generous	0.182***	0.186***	0.185***		0.208**	0.207**	0.205**	
	(0.056)	(0.054)	(0.050)		(0.082)	(0.081)	(0.084)	
Promise incumbent				-0.007**				-0.010*
				(0.003)				(0.006)
Promise challenger				0.007**				0.017***
				(0.003)				(0.005)
Promise broken	-0.395***	-0.402***	-0.413***	-0.388***	-0.122*	-0.116*	-0.119	-0.112
	(0.094)	(0.096)	(0.098)	(0.096)	(0.067)	(0.066)	(0.073)	(0.072)
Own total income previous round	0.000	-0.000	0.000	0.001	-0.002*	-0.002*	-0.002*	-0.003*
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
# voting decisions	246	246	246	246	237	237	237	237
Round controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Individual characteristics	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.13: Probability of voting for the incumbent (when promises differed)

	Dependent variable: Indicator for voting for the incumbent							
	Opaque I				Opaque II			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Incumbent's promise more generous	0.169***	0.169***	0.172***		0.254***	0.253***	0.237***	
	(0.058)	(0.057)	(0.057)		(0.065)	(0.066)	(0.067)	
Promise incumbent				-0.010**				-0.007***
				(0.004)				(0.002)
Promise challenger				0.008**				0.003***
				(0.004)				(0.001)
Promise broken	0.001	-0.003	0.022	0.032	-0.122	-0.116	-0.092	-0.046
	(0.081)	(0.078)	(0.075)	(0.083)	(0.077)	(0.081)	(0.081)	(0.083)
Own total income previous round	0.008***	0.008***	0.008***	0.008***	0.008***	0.008***	0.008***	0.008***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
# voting decisions	243	243	243	243	237	237	237	237
Round controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Individual characteristics	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.14: Probability of voting for the incumbent (when promises differed)

	Dependent variable: Indicator for voting for the incumbent							
	Transparent				Opaque			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Incumbent's promise more generous	0.184***	0.186***	0.182***		0.217**	0.215**	0.214**	
	(0.056)	(0.054)	(0.050)		(0.090)	(0.089)	(0.092)	
Promise incumbent				-0.006**				-0.010*
				(0.003)				(0.005)
Promise challenger				0.007***				0.017***
				(0.002)				(0.005)
Promise broken	-0.410***	-0.415***	-0.430***	-0.405***	-0.130**	-0.122*	-0.124*	-0.121*
	(0.099)	(0.100)	(0.104)	(0.103)	(0.065)	(0.065)	(0.075)	(0.071)
Own total income in previous period	0.000	0.000	0.000	0.000	-0.002*	-0.002*	-0.002	-0.003*
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Constant	0.492*	0.486*	0.597*	0.611*	0.713***	0.722***	0.748**	0.745**
	(0.290)	(0.289)	(0.343)	(0.317)	(0.222)	(0.226)	(0.352)	(0.357)
# voting decisions	246	246	246	246	237	237	237	237
Round controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Individual characteristics	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.15: Probability of voting for the incumbent (when promises differed)

	Dependent variable: Indicator for voting for the incumbent							
	Opaque I				Opaque II			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Incumbent's promise more generous	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				-0.010** (0.004)				-0.003** (0.002)
Promise challenger				0.008** (0.004)				0.003** (0.001)
Promise broken	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	0.008*** (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	-0.803*** (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	243	243	243	243	237	237	237	237
Round controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Individual characteristics	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.

We further analyze whether our data reveals any indication for a relationship between participants' own promise breaking behavior and their preference for voting for a promise breaking incumbent (when they are not running for office). To do so, we calculate for each participant the fraction of promises kept (i.e. the number of times they chose a salary choice that was at least as generous as their promise over the number of times they made a choice as an official), and test, whether more honest participants act differently when learning that an incumbent broke her promise in the *Transparent* treatment. To avoid other reasons for not voting for the incumbent, we focus on situations in which the promise breaking incumbent makes the more generous promise. Hence, not voting for the incumbent (due to potential preferences for honest candidates) comes at a cost. Figure A.1 shows a (jittered) scatter plot that illustrates that a large fraction of participants in *Transparent* either keep their promises always or always break them and that there is no strong correlation between own promise breaking and not reelecting a promise breaking incumbent (Spearman's $\rho = -0.0344$, $p = 0.753$).

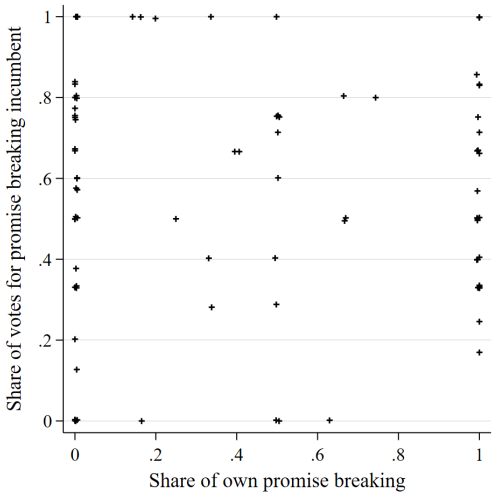


Figure A.1: Own promise breaking and votes for dishonest incumbents (*Transparent*)

A.3.7 Learning across time

Table A.16 shows results from GLS specifications that regress our main outcomes variables on the total round variable. It becomes apparent, that participants in *Opaque* make more generous promises across time, but choose higher wages in later rounds, such that promise breaking increases. In all other treatments, we do not observe significant changes in promise breaking across time. Finally, we observe no trends in the reelection probability of the incumbent.

Table A.16: Trends within treatments

	Transparent	*Opaque*	*Opaque I*	*Opaque II*
		Promises		
Total Round	-0.107	-0.183*	-0.164***	-0.0778
	(0.134)	(0.0973)	(0.0592)	(0.0989)
Constant	23.73***	23.00***	19.03***	20.91***
	(1.811)	(1.574)	(0.780)	(1.190)
# decisions	540	524	497	546
		Promise Breaking		
Total Round	0.191	1.097***	0.226	0.323
	(0.257)	(0.315)	(0.294)	(0.236)
Constant	17.58***	28.54***	25.24***	22.93***
	(3.544)	(3.042)	(3.362)	(3.663)
# decisions	274	264	250	273
		Salaries		
Total Round	0.101	0.882***	0.0892	0.211
	(0.239)	(0.305)	(0.282)	(0.275)
Constant	39.59***	50.68***	43.28***	43.23***
	(3.720)	(3.153)	(3.468)	(3.966)
# decisions	274	264	250	273
		Reelection of incumbent		
Total Round	-0.000	-0.005	0.008	0.005
	(0.006)	(0.005)	(0.005)	(0.006)
Constant	0.458***	0.374***	0.340***	0.359***
	(0.070)	(0.054)	(0.072)	(0.058)
# decisions	274	264	250	273

The table displays results from GLS random effects models.
Standard errors in parentheses * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

A.3.8 Descriptive Norms and Norms Held by Independent Observers

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.[39] The first question related to injunctive norms

[39]In *Opaque II*, observers' answers were not included when calculating the modal choice, and voters as well as observers were explicitly told so.

as discussed in the main text. 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).

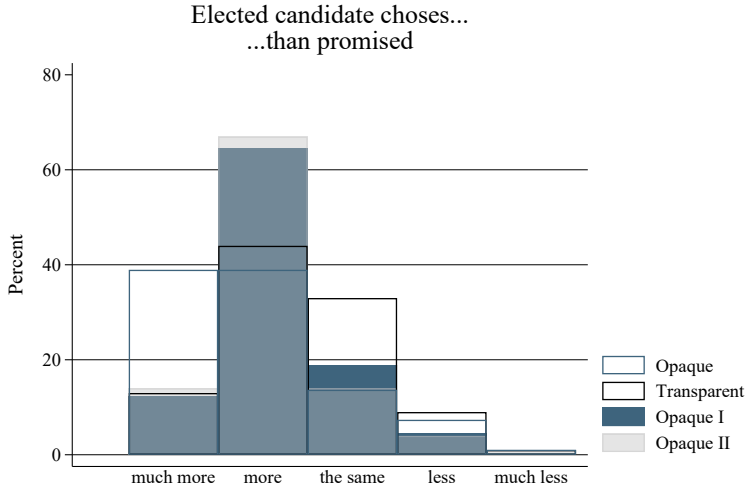
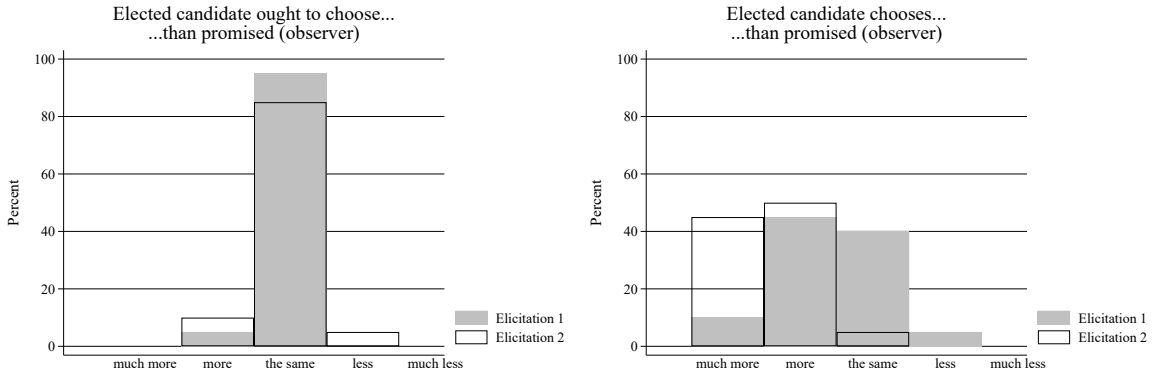


Figure A.2: Descriptive norms (across treatments)

In line with differences in the frequency of promise breaking across treatments, descriptive norms differ in *Transparent* and *Opaque* institutions (see Figure A.2, *Transparent* vs *Opaque*: p-value $= 0.002$, *Transparent* vs *Opaque I*: p-value $= 0.052$, *Transparent* vs *Opaque II*: p-value $= 0.028$, Mann-Whitney test on group level). Hence, while individuals expect more promise breaking in *Opaque* institutions, promises are understood literally and ought to be kept in all institutions.

Figure A.3a reveals that also observers hold very clear injunctive norms. More than 80 percent of observers believe that the modal response to our injunctive norm question is "an elected candidate ought to choose the same as promised" and only 4 percent of observers change their response to this question from the first to the second elicitation. There is much less agreement with respect to descriptive norms (Figure A.3b). 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. 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).



(a) Injunctive norms

(b) Descriptive norms

Figure A.3: Norms about promise keeping held by observers (before and after the experiment) in *Opaque with observers*