

Politicians, institutional incentives, and citizen welfare: evidence from a lab-in-the-field experiment in India

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Abstract

We examine how politicians and non-politicians in rural India respond to behavioural incentives. Using a modified dictator game, we vary treatments (and incentives) across the nature of interactions, the visibility of actions, and an upfront promise. Under anonymity, politicians and non-politicians behave selfishly: both become significantly more generous when interactions are personalized. However, while non-politicians respond to greater visibility more strongly than politicians, an upfront promise induces more pronounced politician responses. Whereas promise-breaking appears to be more costly for politicians, visibility, via social image concerns, appears to matter more for non-politicians. This mix of similarity and heterogeneity in response suggests that evidence about the behaviour of real-world politicians is more important for effective policy design than acknowledged so far.

Keywords: asymmetric information; politician behaviour; social preferences; promises; dictator game; India

JEL classifications: D63, C91, C93, H11

1. Introduction

An important finding from behavioural experiments is that individuals are less inclined to use private information to their own advantage than economic theory would lead us to expect (Abeler, Nosenzo, and Raymond 2019). In dictator games, where an individual is given money she can keep but also has the option to share, people typically share. Such sharing becomes more generous when the relationship between the dictator and the recipient shifts from being anonymous to involving simple social cues and relations (Bohnet and Frey 1999; Charness and Gneezy 2008). Other experiments find that people are often unwilling to break a promise even if promise-breaking advances their material interests (e.g. Vanberg 2008).

While these and other experimental results challenge key behavioural assumptions in economics, they also provide valuable insights into the effectiveness of economic and (low-cost) norm-based and other social incentives. In this article, we report on behavioural

experiments in the field that examine how real-world, village-level politicians and local citizens (non-politicians) in rural India behave and respond to such incentives. An important but largely neglected research question is whether politicians are ‘a special breed’ and respond differently to incentives than ordinary members of the public. If this turns out to be the case, such behavioural differences can and should inform evidence-based policy initiatives seeking to incentivize politicians to act in the public interest.

We use a modified dictator game which draws on [Andreoni and Bernheim \(2009\)](#), who examined how the visibility of a dictator’s action affects prosocial behaviour. To capture ‘low visibility’ and thus the extent to which information about a dictator’s action is private¹, we introduce a high exogenous probability (0.8) that the financial endowment will be randomly allocated to the dictator or the recipient.² This leaves a probability of 0.2 that the dictator will make the allocation decision. A selfish dictator can now, with ease, conceal a decision to keep the entire endowment to himself/herself. To capture ‘high visibility’, the exogenous probability that the endowment is randomly allocated to the dictator or recipient is reduced to 0.1, with a probability of 0.9 that the dictator will make the allocation decision. This makes it much harder for a dictator to hide a selfish allocation.

We implement and report on six treatments where we vary (1) the nature of interactions (anonymous to personalized) between dictator and recipient; (2) the extent to which information about a dictator’s action is ‘visible’ (or private); and (3) whether the dictator makes an upfront commitment in the form of a promise to the recipient or not.

In treatment 1, dictators make decisions in an anonymized setting. In treatment 2, interaction is personalized and a dictator meets and greets their matched recipient prior to decision-making. In treatment 3, there is again anonymity and no interaction, but dictators are forced to make an upfront promise, which is then communicated to their matched recipient. In treatment 4, interaction is personalized and dictators make an upfront promise. In these first four treatments, visibility is low. In treatments 5 and 6, the interaction between the dictator and the recipient is personalized and visibility is high. In treatment 5, dictators do not make upfront promises, whereas in treatment 6, they do.

Our findings can be summarized as follows. In our anonymized, no-promise, and low-visibility benchmark, both politicians and non-politicians allocate almost the entire endowment to themselves. Average giving represents around 1 per cent of the endowment, and 87 per cent of politicians and 93 per cent of non-politicians give zero. When the interaction between dictator and recipient is personalized, both politicians and non-politicians become significantly more generous, with average giving increasing to 33%–35% of the endowment. At the same time, the percentage of zero-giving drops to 26 and 19, respectively. For these two treatments, we find no statistically significant differences in the generosity and behaviour of politicians and non-politicians.

However, when an upfront promise is introduced, first under anonymity and then with personalized interactions, behavioural differences emerge. For the former, while both groups largely keep their word, politicians promise and give more. While the difference in giving is not dramatic, it is statistically significant at the 5 per cent level. Further and when compared to anonymity, the behaviours of politicians and non-politicians change more dramatically when interactions are personalized (still with low visibility). For politicians, average giving increases from about 8 per cent to about 50 per cent of the endowment: among non-politicians, the corresponding rise is from about 4 per cent to about 31 per cent. This difference in politician and non-politician behaviour is distinct and statistically significant at the 1 per cent level.

Finally, with personalized interaction and high visibility (a low degree of private information), non-politicians are more generous than politicians, with average giving

¹ We use the terms private information about and low visibility of an action interchangeably throughout.

² Both politicians and citizens play the dictator role.

representing 54.5 per cent of the endowment for non-politicians and 38.9 per cent for politicians. This behavioural difference is statistically significant at the 5 per cent level.

Summing up, while promise-breaking appears to be more costly for politicians, non-politicians respond more strongly to changes in visibility and thus the extent to which information is private. Both respond strongly and positively to more personalized interaction. This combination of similarity (in the response to personalized interaction) and heterogeneity (for promises and to low and high visibility) in politician and non-politician responses, points to the need for evidence-based and contextualized design of reform initiatives that seek to incentivize politicians to act in the public interest.

The rest of the article is organized as follows. Section 2 provides further background and motivation for the study. Section 3 describes the research design, the game, and experimental procedures. Section 4 presents the analysis and reports on the main experimental findings. Section 5 presents further discussions and robustness tests. Section 6 concludes.

2. Background and motivation

In contrast to traditional choice theory, which forms the backbone of the political economy literature (Osborne and Slivinski 1996; Besley and Coate 1997), well-known behavioural and experimental studies show that behaviour often deviates from the *homo economicus* assumption—people and politicians have intrinsic preferences, including for fairness (Kosfeld and Rustagi 2015) and reciprocity (Enemark et al. 2016).

Studies using dictator games in the lab and charitable giving in the field suggest that individuals become more generous when they, for example, see the face of the recipient (Bohnet and Frey 1999), are matched with ‘friends’ (Leider et al. 2009; Goeree et al. 2010), or are provided with seemingly irrelevant additional information, such as the family name of the recipient.

As noted by Bénabou and Tirole (2006), the visibility of an action also affects prosocial behaviour by invoking reputational concerns. The risk of reputational loss has been found to strongly affect norm compliance relating, for example, to voter turnout (Gerber, Green, and Larimer 2008) and voluntary contributions to public goods (Andreoni and Petrie 2004; Milinski et al. 2006; Ariely, Bracha, and Meier 2009). A plausible expectation about politicians, accustomed to scrutiny from voters and peers, would be that they are more likely to respond to visibility to avoid reputational loss (e.g. Callander 2008; Ferraz and Finan 2011; Bobonis, Cámara Fuertes, and Schwabe 2016; Cavalcanti, Daniele, and Galletta 2018).

Further, and in contrast to the prediction that an agent should break a promise when this is consistent with material self-interest [e.g. contract theory (Akerlof 1970); mechanism design theory (Hölmstrom 1979)], behavioural and experimental studies suggest that people incur a psychological cost from promise-breaking (Corazzini et al. 2014; Di Bartolomeo et al. 2019), either because of preferences for keeping their word (Ellingsen and Johannesson 2004; Vanberg 2008) or because of guilt-aversion (Gneezy 2005; Charness and Dufwenberg 2006; Ederer and Stremitzer 2017).³

A substantive literature has explored whether people are averse to lying even when dishonesty increases their monetary payoff. Here, evidence is mixed, with some work suggesting heterogeneity in types: while some never lie, other people lie when the payoff from dishonesty increases or the risk of detection decreases (Gneezy, Rockenbach, and Serra-Garcia 2013; Gneezy, Kajackaite, and Sobel 2018). In recent work, Abeler, Nosenzo, and Raymond (2019) find that both a preference for honesty and a preference for a reputation of being honest can explain aversion to lying.⁴

³ Vanberg’s (2008) findings supported mainly the former explanation.

⁴ Abeler, Nosenzo, and Raymond (2019) combine a meta-analysis with new experiments, finding that people often do not misreport private information even when this cannot be detected and misreporting significantly advances their material self-interest. To investigate further, they conducted four experiments with over 1,600

For politician behaviour, evidence is ambiguous: while a stereotype suggests that promise-breaking belongs to the fine art of political practice (e.g. [ISSP 2008](#); [Thomson 2011](#)), some evidence suggests that politicians try to keep their word ([Thomson et al., 2017](#)). Recent work by [Janezic and Gallego \(2020\)](#), for a sample of Spanish mayors, finds dishonest behaviour to be widespread among elected officeholders, while [Chaudhuri et al. \(2020\)](#) find some dishonesty among newly elected politicians, but also that the same politicians are less dishonest than ordinary citizens.

The dictator game has been widely deployed to capture social preferences in the lab (e.g. [Fehr and Schmidt 1999](#); [Andreoni and Miller 2002](#)). Using a modified dictator game, we draw on [Andreoni and Bernheim \(2009\)](#), who examined how the visibility of a dictator's action affected prosocial behaviour in a setting with personalized interactions. With low visibility, student dictators gave close to zero: giving shifted notably towards 50:50 sharing when visibility increased. We expand on the [Andreoni and Bernheim \(2009\)](#) design, first, by adding an anonymous benchmark to help identify the effect of personalized interactions. Second, we introduce an upfront promise to examine whether a pledge affects prosocial behaviour. Finally, we explore the effect of increased visibility (transparency) on politician and non-politician behaviour.

3. Experimental design and subject recruitment

3.1 Design and experimental procedure

[Table 1](#) presents the treatments used in the design.

Our politician and ordinary citizen participants, from two different and distant locations, form a pair (group) following a random protocol. In each pair, participants are randomly assigned a dictator (D) or recipient (R) role. In the low-visibility treatment, there is a high exogenous probability (0.8) that the endowment will be randomly allocated to the D or the R. When making a decision, the D knows the 'true state of the world', that is, whether her decision will be implemented or whether the endowment will be allocated randomly to the D or the R in the pair. In the low-visibility treatments, each D can choose the allocation once and knows that this decision will be implemented; in contrast, an R who receives zero does not know whether the D or bad luck is responsible. The final outcome is observed by an audience comprising the recipient, the experimenters, and other participants. Notice that a D will reveal a distributional choice whenever any other amount than zero or the full endowment is allocated to the R. A selfish D can therefore—and with ease—conceal a selfish decision to allocate the entire endowment to him/herself.

In the anonymous baseline, dictators and recipients do not meet. When interactions are personalized, participants say 'hello' to each other before playing the game. In the upfront promise treatment, a D makes a promise that is observed by the R before the D knows whether she will make the allocation decision. Finally, in the high-visibility treatment, we reduce the exogenous probability that the endowment will be randomly allocated to the D or the R from 0.8 to 0.1: this makes it much harder for dictators to hide a selfish action. We then follow the same approach as above, with personalized interaction and with and without an upfront promise.

Our experimental design has six treatments modifying the interactions between dictators and recipients along the anonymity, promise, and visibility dimensions. In T1, Ds make decisions in an anonymous setting with low visibility (i.e. a 0.8 probability that the endowment is randomly assigned to the D or the R). In T2, interaction is personalized, while visibility remains low. In T3, interaction is anonymous, but Ds are forced to make an upfront

participants using the [Fischbacher and Föllmi-Heusi \(2013\)](#) approach where participants are paid based on their reporting of a privately observed outcome of a random variable. The study found that higher payoffs resulted in an almost 30 percentage point increase in the number of participants lying. In contrast, changing what people thought others were reporting did not affect behaviour. Finally, when the outcome was made observable to others, participants were less dishonest.

Table 1. Experimental design: treatments.

		No personalized interaction	Personalized interaction
Low visibility	No promise	T1	T2
Low visibility	Promise	T3	T4
High visibility	No promise	—	T5
High visibility	Promise	—	T6

promise that is communicated to their matched R. In T4, the interaction is personalized and Ds make an upfront promise, while visibility remains low. In T5 and T6, visibility is high (the probability that the endowment is randomly assigned to the D or the R is reduced to 0.1). In T5, we add high visibility to T2 (i.e. personalized interaction without promise). In T6, we add high visibility to T4: Ds meet and greet and make an upfront promise to their respective Rs.

In each treatment, experimenters read out instructions and carefully explained the games to participants. We then administered a short quiz to test each participant's comprehension. Experimenters spent more time explaining the game if anyone was unable to answer a particular quiz question. Participants then played two practice rounds to ensure they completely understood and were familiar with the process.

Supplementary Appendix Table A.1 reports on the number of observations per treatment, by politician and non-politician. We have 588 observations in total: 355 for politicians and 233 for non-politicians.

We now describe the experimental procedure and the steps followed in each treatment.

3.1.1 Treatment 1: no personalized interaction, no promise, low visibility

We present the main structure of the design of T1 in **Table 2**. We explain the design in detail below.

Step 1: Twenty participants from the home village (where the venue was located) and twenty from a visitor village (a distant location) took part in each session, with ten politicians and ten non-politicians participating from each village. *Step 2:* Visitor-village and home-village participants arrived separately and were seated in different rooms (rooms V and H). V and H participants did not meet before, during, or after the experiment. *Step 3:* Each participant received their ID randomly matching each participant in room V with a participant in room H to form a pair. *Step 4:* Participants in each pair were randomly assigned the role of dictator (D) or recipient (R) and kept their role throughout the session.⁵ *Step 5:* Each pair received a fixed endowment of INR 1,000 (USD 15.50) in each round. This was common knowledge. In each round, D decided how to allocate the endowment between D and R with the following modification to a standard dictator game. *Step 6:* We asked each dictator, one at a time and in private, to pick one closed chit from an urn on a table. Each chit had a number between one and ten that should be kept strictly confidential and not shown to anybody in the room, not even to the experimenters/research assistants. We called these 'private numbers'. Each D in each room received a decision sheet (see decision sheet example in the **Supplementary Appendix**). *Step 7:* At the start of each round, the experimenter announced two randomly chosen numbers between one and ten. Ds filled in their decision sheets one by one in private. Only Ds whose numbers were announced could choose and record a distribution on the decision sheet: others would just tick a box stating that nature would give zero to either D or R. The dictator could choose any number between 0 and 1,000 only when their private and the announced number

⁵ Given the challenges with recruiting politician participants, we did not follow an equal split when randomly allocating the roles of politicians and non-politicians. Instead, we did the following for each session: randomly chose a number between five and eight and split accordingly (e.g. if number six is drawn, six out of ten politicians played the role of dictator for that session).

Table 2. Experimental design: Treatment 1.

Steps	Procedure
Step 1	Participants from H-village and V-village sit in separate rooms
Step 2	H-village participants do not meet their counterparts
Step 3	Group numbers (or IDs) are given and groups formed randomly in pairs
Step 4	Random role assignments (D or R) in each pair
Step 5	All Ds pick a chit with private numbers from an urn, one by one, and in private.
Step 6	At the start of each round, two randomly chosen private numbers are announced
Step 7	Only Ds with announced private numbers can decide the allocation
Step 8	One by one, Ds write their decisions in private
Step 9	Decision sheets go into an envelope
Step 10	Five rounds with different private numbers (each D can decide only once)
Step 11	One round's envelope is randomly picked and sent to an external person
Step 12	Results are published
Step 13	Nobody else knows who made the decision if 0 or 1,000 was chosen by the D
Step 14	Participants receive earnings in closed envelopes and leave the lab one by one
Step 15	Participants from the visitor village leave the venue first

matched. *Step 8*: All Ds who made a decision or ticked a box folded their decision sheet and put it in an envelope, named, for example, Round 1–Decisions. The Rs, other Ds, and the experimenter knew the probability (i.e. 0.8) but did not know whether nature or D made the decision when the outcome was either zero or INR 1,000. *Step 9*: Steps 7 and 8 were repeated five times (i.e. in each session, participants played five rounds). In each round, the experimenter announced different private numbers. In one session, each D could choose the distribution only once. *Step 10*: At the end of the five rounds, one round was randomly selected to determine payments. The envelope with decision sheets for this selected round was handed to a person outside the venue (an external person) who had no information about the game or the participants. He checked the decision sheets in a separate room and put payments in separate envelopes for each pair. He also decided by flipping a coin whether D or R got INR 1,000 when nature intervened.

The external person gave a result sheet to the experimenter and small sealed envelopes for each participant, with their group number and role in the game written on the top. These envelopes contained their earnings from the game—based on the decisions they, their partners, or nature made—plus a fixed participation fee of INR 300. *Step 11*: The experimenter showed the results to each D and their corresponding R sitting in the other room. *Step 12*: Each participant left the room and venue one by one and received their envelopes with their payment from the experimenter. Participants from the visitor village left the venue first.

3.1.2 Treatment 2: personalized interaction, no promise, low visibility

Table 3 presents the main structure of the design of Treatment 2. Next, we explain the design in detail.

Step 1: As in T1, there were twenty participants from each of the home and visitor villages: ten from each village were politicians. They arrived separately at the venue. *Step 2*: Out of twenty participants from the visitor village, ten were randomly chosen to be seated in one room and the others in another. A similar procedure was followed for home-village participants. In one room, we had ten participants from village H and ten participants from village V. Participants from these two different locations did not meet each other before entering the ‘laboratory’. *Step 3*: In each session and following a random matching protocol, a participant from the visitor village formed a pair with a participant from the home village. *Step 4*: Participants in each group were randomly assigned the role of D or R

Table 3. Experimental design: Treatment 2.

Steps	Procedure
Step 1	Participants from H and V villages sit in the same room
Step 2	Group numbers (or IDs) are given and groups formed randomly in pairs
Step 3	The members of each pair say ‘hello’ to each other
Step 4	Random role assignments (D or R) in each pair
Step 5	All Ds pick a chit with private numbers from an urn, one by one and in private
Step 6	At the start of each round, two randomly chosen private numbers are announced
Step 7	Only Ds with announced private numbers can decide the allocation
Step 8	One by one, Ds write their decisions in private
Step 9	Decision sheets go into an envelope
Step 10	Five rounds with different private numbers (each D can decide only once)
Step 11	One round’s envelope randomly picked and sent to an external person
Step 12	Results are published
Step 13	Nobody else knows who made the decision if 0 or 1,000 is chosen by the D
Step 14	Participants receive earnings in closed envelopes and leave the lab one by one
Step 15	Participants from the visitor village leave the venue first

and kept this role during the entire session. Each pair D and R (from different villages) were asked to stand up and greet each other. *Step 5*: We then followed Steps 5–10 as in T1. *Step 6*: After receiving the result sheet from the external person, the experimenter published the results in the room (writing each pair’s allocation on a board—see the result sheet in the [Supplementary Appendix](#) for details). *Step 7*: Step 12 in Treatment 1 was then followed.

It is important to note that: (1) we did not use the subjects’ names or any other identifiers, other than their IDs, anywhere; and (2) the private numbers remained private throughout. If a dictator whose private number was matched with the announced number decided to take 1,000 or 0, her identity could not be known to others, including the experimenter. However, if she decided to give any other amount, everybody could identify her when the result was published.

3.1.3 Treatment 3: no personalized interaction, promise, low visibility

We added a process of promise-making, described below, by each D after Step 5 in [Table 2](#). We followed Steps 6–15 afterwards, as shown in [Table 2](#).

In T3, we added a *promise* to T1. We followed Steps 1–6 as in T1. Then, each D in each room was asked to write how they would allocate INR 1,000 between herself and R on a promise slip (see the [Supplementary Appendix](#) for an example of a promise slip). They were also asked to write their group numbers, but not the ‘private numbers’. Each D went to an enclosed area and wrote this in private; then they put the folded promise slip into an envelope and returned it to the experimenter. The experimenter carried the promise slip to the corresponding R (without looking at it) seated in the other room. Each R observed the D’s promise in private and then put the slip in an envelope named, for example, Round 1–Promise. We then followed Steps 7–12 of T1. Note that Ds made the promise before the two numbers were announced and, therefore, before knowing whether D or nature would decide how to divide the endowment. They then chose the actual allocation, which, as in T1 and T2, no one could observe. Ds thus had the option to break their promise and increase their private gain without being detected.

3.1.4 Treatment 4: personalized interaction, promise, low visibility

In this treatment, we added a ‘promise’ to T2. We followed Steps 1–4 as described in T2 and then Steps 5 and 6 as in T1. As in T3, we then asked each D to write how they would

allocate INR 1,000 between himself and R on a ‘promise slip’ and followed the same procedure as for the promise slip in T3. The rest of the process was the same as for T2.

In terms of Table 3, we added *promise* after Step 5. After completing the promise-making procedure, we followed Steps 6–15.

3.1.5 Treatment 5: personalized interaction, no promise, high visibility

We changed the probability of nature’s intervention from 0.8 to 0.1 (i.e. the visibility of a dictator’s action increases). In any session with ten dictators, nine out of ten dictators had to choose how to distribute the endowment. In only one out of ten cases, nature decided the outcome with either D or R receiving the entire endowment.

We followed the same structure presented in Table 3. The only change was introduced in Step 6—instead of two out of ten numbers, the experimenter announced nine out of ten numbers randomly at the beginning of each round. Dictators with those announced numbers could choose the distribution. Other than this, the process was identical to T2. That is, we followed the procedure described in T2 but now with high visibility.

3.1.6 Treatment 6: personalized interaction, promise, high visibility

In this treatment, we added high visibility to T4. The only change to the process described in T4 was that at the start of each round, the experimenter announced nine numbers between one and ten so that nine out of ten Ds would be making decisions.

3.2 Recruitment

For recruitment, we take advantage of India’s decentralized Panchayat system, where Gram Panchayats (GPs, i.e. village councils) are the lowest tier. A GP is subdivided into wards (Samsads). Voters elect representatives for each tier, and elections are held at regular, 5-year intervals.⁶ In our study, we define a politician as a person who has either recently fought or recently won (in the last 10 years) an election for a village council ward member seat.

To create a neutral field-lab environment, and to elicit the true preferences of politician participants, great care was taken to match politicians with ordinary citizen residents from villages at a sufficient distance from their constituencies to instil confidence there was no contact in the past, or before and after the experiment. This was to ensure that politicians were matched with strangers and to avoid generosity in the games that could be dismissed as motivated by re-election prospects. Participants from visitor villages knew they had to travel an average of 25 km to participate, but had no prior information about where they were going and with whom they would be matched. Free transport and refreshments were arranged since no well-connected public transport was available. We also chose the timing of the experiment to avoid overlap with election-related or other political campaigning.

Hooghly district in West Bengal and Varanasi district in Uttar Pradesh were selected because of the researchers’ prior experience working there. From among the administrative blocks in each district, we randomly selected two blocks following a stratified random sampling based on geographical location. For example, from among Hooghly’s eighteen administrative blocks, we randomly selected Singur and Dhaniakhali. In Uttar Pradesh, Badagaon and Sevapuri blocks were selected using a similar procedure. GPs were randomly selected from each block. For each GP, we used publicly available Election Commission data⁷ to prepare a list of individuals who had contested or been elected during the two most recent Panchayat elections. We then randomly selected politicians from the list and

⁶ Each ward represents around 500–800 voters, and they elect at least one, in some cases two, ward members. GPs usually comprise 10–15 wards/villages and 3,000–5,000 voters, although this varies widely. Elected ward members form the village council. The second tier (i.e. block level) comprises 10–12 GPs, while the final tier is the district council (ie Zila Parishad) comprising 15–20 (on average) blocks.

⁷ www.websec.gov.in.

invited them to participate using an invitation letter prepared by the research team. The letter neutrally framed the purpose of the study (e.g. to study challenges of rural development) and explained the random selection of the village/GP and participants (the letter text is provided in the [Supplementary Appendix](#)).

From each village and based on a household census,⁸ we also invited randomly selected ordinary citizens (non-politicians) to participate in the experiment. The presence of non-politicians facilitated the comparisons of interest but also helped reduce experimental demand effects, since a sample comprising only politicians could intensify the feeling of being under scrutiny.

In the invitation, the randomly selected potential participants were informed that (1) their names had been selected through a random process; (2) participation was voluntary; (3) they would receive monetary payments if they agreed to participate (a fixed participation fee of INR 300 with prospects for earning up to INR 1,000), and refreshments; (iv) they would get free transport if they had to travel to a different location; and (v) their identity, personal information, and data on behaviour in the experiments would be strictly anonymized. Each participant was given time to confirm their participation and also knew that they could withdraw at any time without providing any explanation.

4. Results and analysis

This section presents our findings for the 355 politicians and 233 non-politicians in our sample.⁹ We first present the descriptive statistics, followed by the results of the regression analysis.

4.1 Descriptive statistics

[Tables 4](#) and [5](#) present summary statistics and t-tests of differences in giving and promise-making between politicians and non-politicians across treatments. [Figure 1](#) presents histograms of the amount given for politicians and non-politicians in treatments T1, T2, and T5 (where there is no upfront promise). [Figures 2](#) and [3](#) present histograms of the amounts given and kernel density plots of amounts given versus amounts promised for treatments T3, T4, and T6 (where there is an upfront promise) for politicians and non-politicians, respectively.

Starting with [Table 4](#), we find that in T1, 87 per cent of politician dictators give zero, with average giving of INR 13.33 or about 1.3 per cent of the endowment. In comparison, 93 per cent of non-politicians give zero with average giving of INR 5, which represents 0.5 per cent of the endowment. When testing differences in average giving and the proportion of zero-giving, we find no statistically significant differences in the behaviour of politicians and non-politicians (see [Fig. 1](#) and [Table 5](#)).

In T2, when interactions are personalized and politicians and non-politicians meet and greet their recipients before making their allocation decision, we observe notable changes in behaviour. Both groups become more generous, as shown in [Fig. 1](#) and [Table 4](#). Among politicians, average giving increases from 1.3 per cent of the endowment in T1 to 35 per cent (i.e. from INR 13 to INR 357) in T2. For non-politicians, the corresponding increase is from 0.5 per cent to 33 per cent (i.e. from INR 5 to INR 335). Further, zero-giving drops to 26 per cent among politicians and 19 per cent among non-politicians.¹⁰ In T2, politician

⁸ Our research assistants recruited local enumerators to collect participant information. They prepared a list (census) of households, which was always kept with them only, containing basic demographic information (name of household head, sex, education, occupation). Following a blinded, random protocol, the enumerators selected potential participants and invited them, following the same procedure as above.

⁹ [Supplementary Appendix Table A.1](#) presents the number of politicians and non-politicians who participated in each treatment. [Supplementary Appendix Table A.2](#) presents summary statistics for politician and non-politician attributes.

¹⁰ These changes (i.e. average giving and zero-giving in T1 compared to the same in T2) are statistically significant at the 1 per cent level for both politicians and non-politicians—see [Supplementary Appendix Table A.3](#).

Table 4. Summary statistics.

Trt	Mean giving (INR)		Mean promise (INR)		Zero-giving (%)	
	Pol	Non-pol	Pol	Non-pol	Pol	Non-pol
T1	13.33 (34.57)	5.00 (20.13)	—	—	86.67	93.33
T2	356.60 (291.88)	335.48 (256.32)	—	—	26.42	19.35
T3	80.33 (86.40)	41.33 (50.63)	87 (95.23)	41.33 (50.63)	36.67	43.33
T4	504.41 (293.42)	308.69 (245.69)	513.23 (252.68)	476.08 (234.93)	11.76	28.26
T5	428.10 (281.96)	544.68 (236.65)	—	—	6.84	0
T6	474.26 (221.20)	483.67 (267.97)	551.48 (192.67)	567.35 (177.23)	0	0.06

Notes: Standard deviations are in parentheses.

Table 5. T-tests, by treatment, politicians versus non-politicians.

Treatments	Give	Promise	Zero-giving (%)
T1	1.14 (0.26)	—	-0.86 (0.39)
T2	0.33 (0.74)	—	0.72 (0.46)
T3	2.13** (0.04)	2.32**(0.02)	-0.52 (0.61)
T4	3.72*** (0.00)	0.79 (0.43)	-2.25** (0.03)
T5	-2.35** (0.02)	—	1.84* (0.07)
T6	0.23 (0.82)	0.48 (0.62)	-1.71*(0.09)

Notes: z-statistics are in parentheses; ** and ***: significant at the 5 and 1 per cent levels.

and non-politician behaviours are not significantly different: t-tests of differences in average giving and of the proportion of zero-giving show no significant differences in behaviour (Table 5).

In T3 and T4, we observe a difference in the amount that politicians promise and give compared to non-politicians. In the anonymity and promise treatment (T3), politicians promise more than non-politicians—INR 87 versus INR 41—and also give more—INR 80 versus INR 41. These differences in the amount promised and given are both statistically significant (Table 5). A similar pattern is observed for the personalized interaction and promise treatment (T4) (Table 5). Politicians give significantly more (INR 504 vs INR 309 for non-politicians), and the difference is statistically significant at the 1 per cent level. At the same time, there is no statistically significant difference in the amount promised by the two groups in T4. In T3 with anonymity, promises are small and 97 per cent of politicians and 100 per cent of non-politicians keep their promises: there is no statistical difference in promise-keeping between politicians and non-politicians. In contrast, in T4, 81 per cent of politicians keep their promises as compared to 54 per cent of non-politicians (see Fig. 3). The mean difference between the amount promised and the amount given is 8.82 for politicians and 167 for non-politicians in T4.¹¹

When we consider the role of visibility without an upfront promise, the amount given by politicians increases from INR 357 in T2 to INR 428 in T5. The corresponding numbers for non-politicians are INR 335 and INR 545, respectively. Compared to the low-visibility treatment, the proportion of politicians and non-politicians who give zero decreases significantly with high visibility (and information becoming less private)—to 7 per cent for politicians and 0 per cent for non-politicians (see Table 4 and Figs. 2 and 3). However,

¹¹ We ran regressions with a dummy = 1 if the amount given was equal to the amount promised (0 otherwise), with controls and a dummy for politicians, and found that the politician dummy was positive and statistically significant in T4 but not in T3.

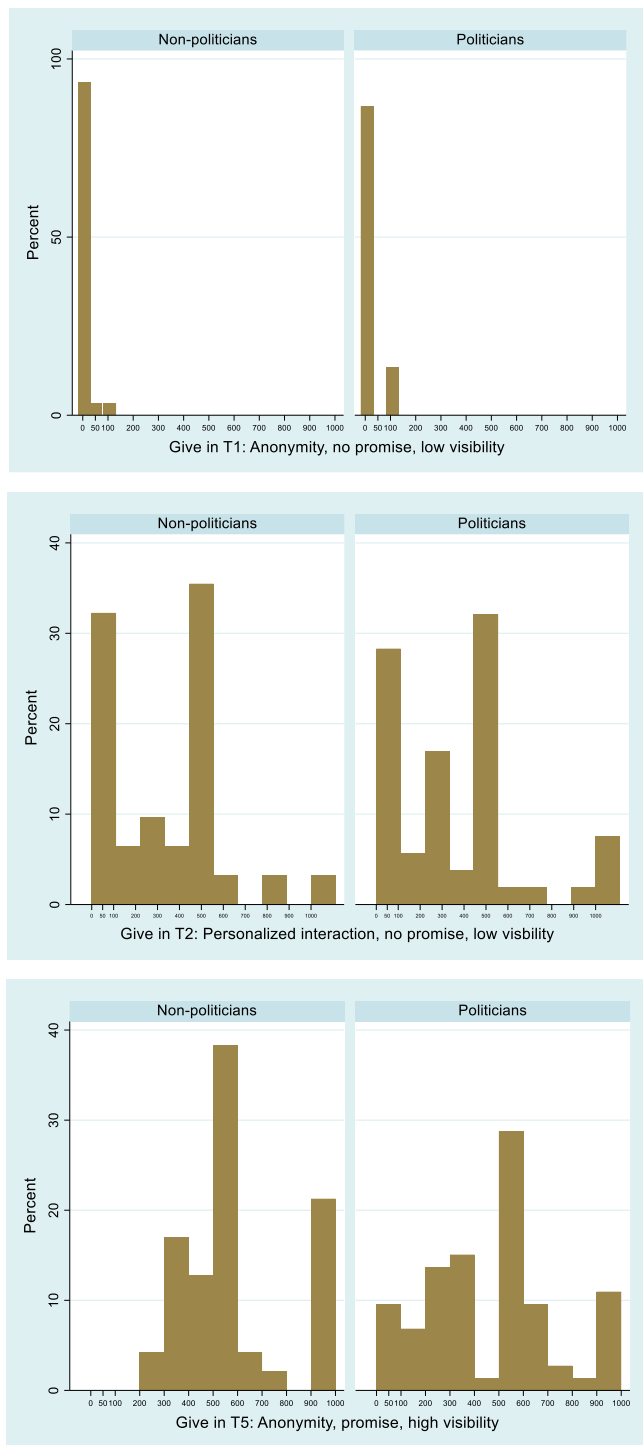


Figure 1. Amount given by politicians and non-politicians in T1, T2, and T5.

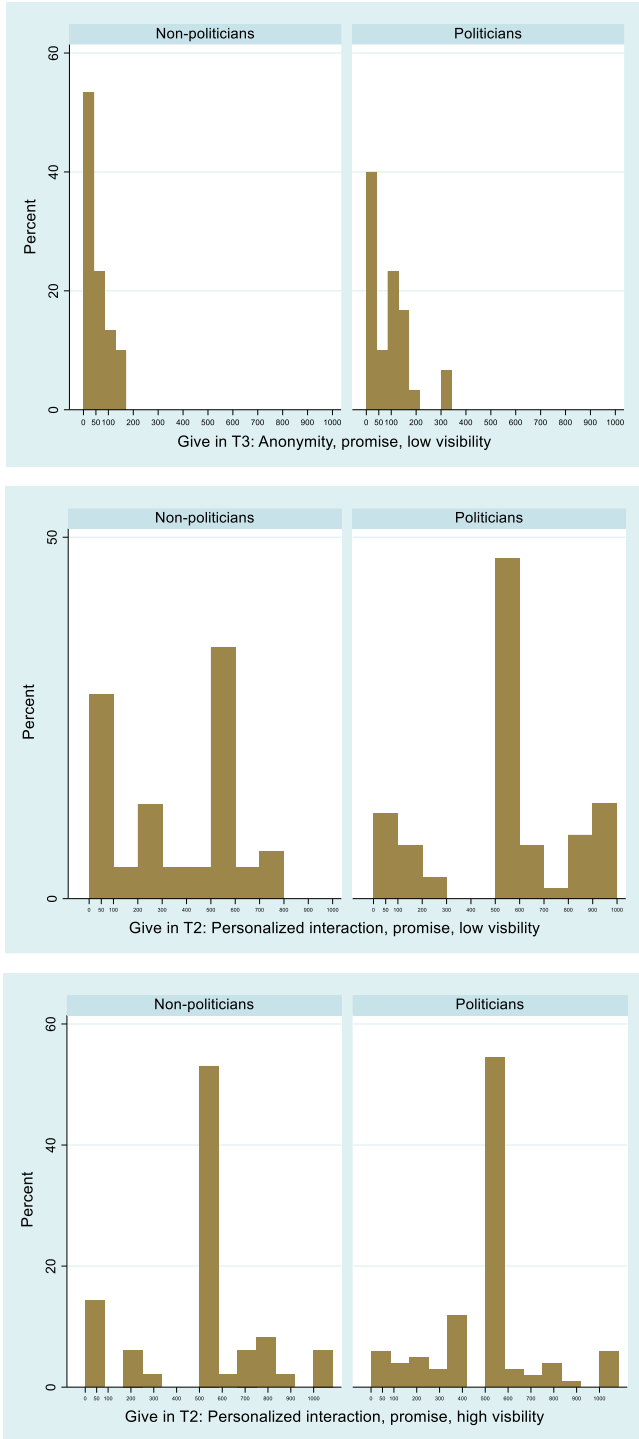


Figure 2. Amount given by politicians and non-politicians in T3, T4, and T6.

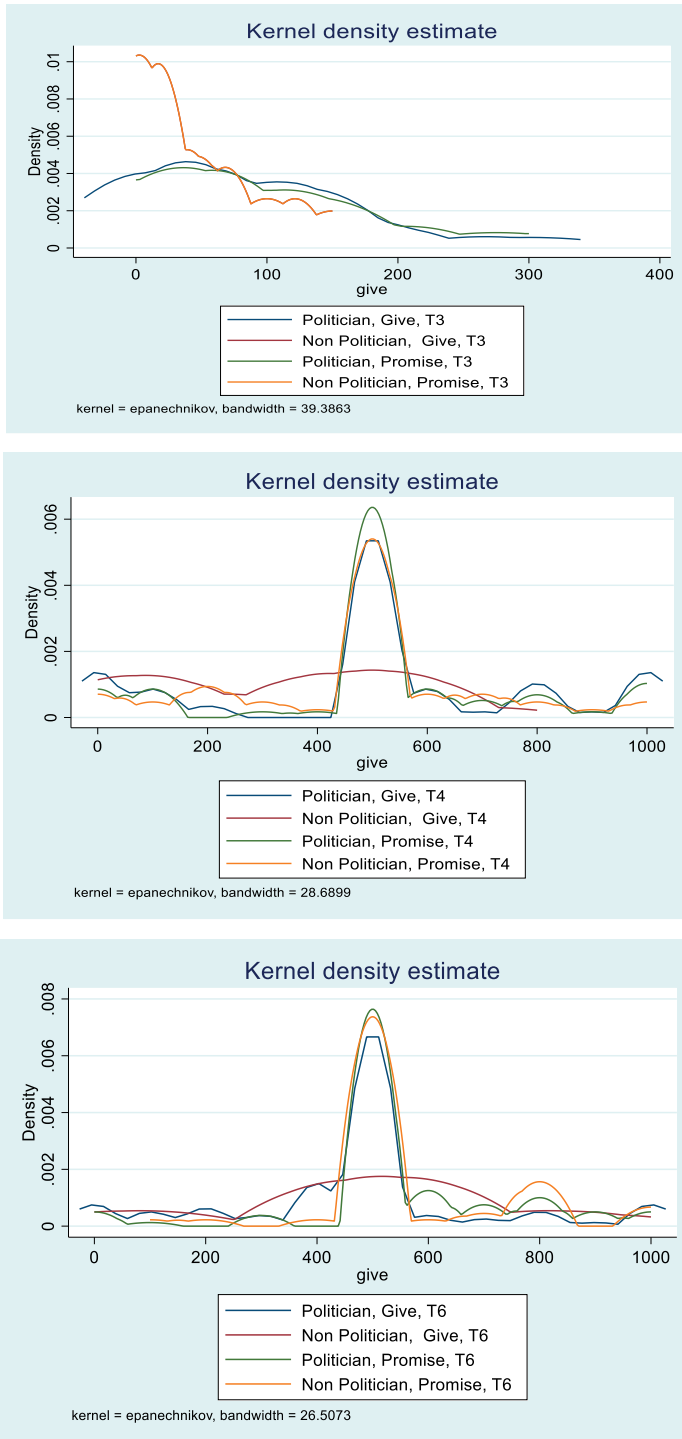


Figure 3. Kernel density plot, amount promised and given, for politicians and non-politicians in T3, T4, and T6.

compared to T2, the overall increase in the average amount given in T5 is not statistically significant for politicians, but is statistically significant for non-politicians (Table 5). Furthermore, the increase in the amount given is far more pronounced among non-politicians than among politicians: the t-test for a difference between politician and non-politician giving in T5 is statistically significant at the 5 per cent level. The t-test of a difference in the proportion of zero-giving between the two groups is statistically significant at the 10 per cent level (Table 5). Non-politicians thus respond more strongly to an increase in the visibility of the dictator's actions than politicians, suggesting, somewhat counter-intuitively, that social image concerns are more pronounced among non-politicians than politicians.¹²

Finally, in T6, when we combine high visibility with an upfront promise, politicians' average giving decreases slightly from INR 504 in T4 (i.e. personalized interaction with promise and low visibility) to INR 474 in T6 (see Table 4 and Fig. 2), while they promise more (i.e. INR 551 in T6 vs INR 513 in T4). Non-politicians give and promise more in T6 as compared to T4 (amount promised and given is INR 567.4 and INR 483.7 in T6, as compared to INR 476.1 and INR 308.7 in T4, respectively). However, the difference in amount given between politicians and non-politicians in T6 is not statistically significant (Table 5). The lack of significance in amount given between politicians and non-politicians in T6, as compared to T5, confirms our earlier results: politicians are more generous with an upfront promise, independent of the level of visibility of their actions.

The descriptive statistics suggest that a non-binding promise, coupled with personalized interaction in the presence of low visibility of the dictator's actions, significantly affects politicians' behaviour and mitigates selfishness: they keep less for themselves, give more to citizen recipients, and keep their promises. Such a behavioural change is not observed for non-politicians. In contrast, politicians do not respond more strongly to an increase in the visibility of the dictator's actions than non-politicians. Social image concerns thus appear to be more pronounced among non-politicians than among politicians.

4.2 Regression results

We now investigate whether this behaviour across treatments from unconditional summary statistics is sustained under conditional regression analysis or whether the results, instead, may be driven by differences in the observable characteristics of politician and non-politician participants. In separate regressions, reported in Table 6, we regress the amount given, and a dummy for zero-giving on a politician dummy, treatment dummies, interactions between politician and treatment dummies, and politician and non-politician characteristics (gender, age, and years of education along with dummies for caste and occupation). We estimate equations using the following specifications:

$$Y_{is} = a_0 + a_1 \times D_{is} + \sum_{j=2}^6 \delta_{js} \times T_{ijs} + \sum_{k=2}^6 \varphi_{ks} \times T_{iks} \times Pol_{iks} + \sum_{m=1}^n \omega_{ms} \times Z_{ims} + u_i \quad (1)$$

where Y is the amount given by the dictator or a dummy for zero-giving, i stands for dictator, either politician or non-politician, and s for session. T_{ijs} are Treatments 2–6, with T1

¹² One possible concern is whether the promise results are caused by an experimenter demand effect (EDE) which could occur, first because politicians could have felt they were under 'special' scrutiny when they received the invitation to participate. Second, in the lab, a politician-dictator could respond to the explicit presence of the 'audience', including the experimenters (a deliberate feature to heighten the social-image effect (as in Andreoni and Bernheim, 2009)). We are confident that this was not the case since (1) the invitation to take part in the study followed a neutral narrative; (2) both politicians and their neighbour-non-politicians received the invitation following a random protocol (see Section 5.1 for details); (3) participation was voluntary; and (4) a politician concerned about being scrutinized could simply opt-out. In addition, the results in T2, where one-third of the dictators gave zero, suggest that scrutiny did not interfere with or discourage selfish behaviour. Since EDEs would be expected to be consistent across treatments, this limited impact on behaviour in T2 suggests limited impacts on behaviour in the promise treatment as well.

Table 6. Regression results.

	Column 1	Column 2	Column 3
Constant	15.25* (0.38)	15.25 (0.38)	0.66*** (7.32)
Politician	8.33 (0.80)	1.01 (0.38)	-0.25 (0.12)
T2	330.48*** (7.21)	320.82*** (4.97)	-2.09 (1.35)
T3	36.33** (2.87) 23.51 (1.22)	-1.50 (1.02)	
T4	303.70*** (7.42)	293.36*** (5.70)	-1.81 (1.30)
T5	539.60*** (10.39)	531.58*** (10.27)	-6.85*** (4.45)
T6	478.67*** (7.69)	467.53*** (6.15)	-2.37* (1.65)
Pol*T2	12.79 (0.31)	20.25 (0.37)	0.49 (0.23)
Pol*T3	30.67* (1.84)	34.51 (1.46)	0.17 (0.08)
Pol*T4	187.38** (2.92)	196.79** (2.73)	-0.46 (0.23)
Pol*T5	-124.91** (2.03)	-122.82*** (2.06)	4.45** (2.23)
Pol*T6	-17.75 (0.33)	-8.99 (0.13)	-0.19 (0.09)
Controls	No	Yes	Yes
R ²	0.37	0.38	0.35
N	588	588	588

Notes: Controls are caste and occupation dummies, years of education, age, gender. z-statistics are reported in parentheses for columns (1), (2), and (3). ***, **, and * indicate level of significance at 1, 5, and 10 per cent, respectively. Column (1) and (2), dependent variable: amount given; column (3), dependent variable: dummy = 1 if give = 0, 0 otherwise. The estimator is ordinary least square for columns (1) and (2), and probit for column (3). Standard errors are bootstrapped and clustered at session level.

being the baseline treatment. D_{is} takes the value 1 if the dictator is a politician and the value 0 if the dictator is a non-politician. Z_{ims} is a vector of control variables (gender and age of the dictator, years of education, as well as caste and occupation dummies). The coefficients δ_{is} show how much the dictator gives in Treatments 2–6, relative to T1 (and the propensity of zero-giving in T2 to T6 relative to T1). The coefficients φ_{ks} show the amount given and the propensity of zero-giving by dictator type (e.g. politicians relative to non-politicians), in T2 to T6 relative to T1. u_i is the error term. We use bootstrapped standard errors clustered at the session level.

Columns (1) and (2) in Table 6 present the results where the dependent variable is the amount given, first with no controls and then with controls included. Column (3) presents the results with the dependent variable being the dummy variable for zero-giving. The politician dummy is insignificant across all three columns, suggesting that there is no observable difference between politicians and non-politicians in average giving and zero-giving across all treatments. The coefficients on all treatments T2–T6 are positive and significant for average giving, suggesting that politicians and non-politicians give more in all treatments relative to the baseline treatment—T1 (anonymous, no promise, low visibility). This changes marginally when controls are added, with the T3 coefficient turning insignificant.

Considering the coefficients for the interaction between the politician and treatment dummies, we first note that, as above, both politicians and non-politicians respond strongly to the change from anonymity to personalized interactions. Further, as in Table 4 and a key result, there are no differences between politician and non-politician responses. Next and for the personalized interaction–promise treatment (T4), the coefficient is positive and significant at the 5 per cent level when the dependent variable is the amount given, both without and with controls: politicians give significantly more in T4 relative to non-politicians and relative to the baseline treatment (for T3, the interaction term is positive and significant when the model is run without controls, but the significance disappears when controls are included). Politicians give significantly more than non-politicians when they meet and greet their unknown recipients and make an upfront promise, even when a decision to allocate the entire endowment to themselves would be easy to hide. In contrast, the coefficient for the interaction of the politician dummy and the personalized interaction,

high visibility, and no promise treatment (T5) are negative and significant, suggesting that non-politicians respond to high visibility more strongly than politicians. However, there is no discernible difference in the generosity of politicians versus non-politicians in T6 when all behavioural instruments are implemented simultaneously. Finally, and considering zero-giving and the column (3) results, the likelihood of zero-giving is reduced only in the high-visibility treatments (T5 and T6). In the absence of a promise (T5), politicians are more likely than non-politicians to give zero: this difference disappears once a promise is introduced (T6).¹³

The regression results confirm the findings from the unconditional comparison of politicians and non-politicians behaviour (and in particular, the t-tests presented in Table 5). While both politicians and non-politicians are strongly selfish under anonymity, both respond similarly and strongly to a change from anonymized to personalized interactions. In contrast, promise-making induces more generous behaviour among politicians than among non-politicians, while non-politicians are more generous than politicians when their actions are more visible, but promise-making is not part of the treatment. The role of promise-making in inducing more generous behaviour from politicians as compared to non-politicians suggests that breaking a promise is more costly for politicians than for non-politicians. On the other hand, social image concerns seem to matter more for non-politicians than for politicians.

5. Further discussion and robustness tests

5.1 Selection bias

Our research team carefully followed the recruitment protocol for politician and non-politician participants to minimize the risk of selection bias. We are confident that our approach to the random selection of politician and non-politician participants created an unbiased sample.

As discussed in Section 3.2, we created a neutral lab environment in the field to ensure that politicians would not feel they were targeted. We randomly matched politicians with ordinary citizen residents from villages at a sufficient distance from their constituencies to instil confidence about no contact in the past, and before and after the experiment. We followed a random process to select politician and non-politician participants, through (1) random selection of administrative blocks from each district following a stratified random sampling; (2) selecting politicians randomly from publicly available lists of village-level politicians; and (3) selecting non-politicians randomly from a census prepared by our research team. We purposely avoided recruiting village council heads (pradhans) because of their typically greater and more visible role in their party's political machinery, and the higher likelihood of being known to more villagers within a district.

While the invitation letter provided relevant information about the study (e.g. anonymous, voluntary, and incentivized), it was framed neutrally. It was not easy for a politician participant to understand that they had been selected because of their politician status since they could easily find out that their non-politician neighbours had also been invited. There is nothing in the invitation letter or in the implementation of the experiment that should make politicians feel different. After the experiment, all participants filled in a short questionnaire collecting simple demographic and socio-economic information, where they were also asked about their political identity for the first time.

In each session, on average, we experienced attrition rates of about 10 per cent among politicians and 5 per cent among non-politicians. That is, on average, two out of twenty invited politicians and one out of twenty invited non-politicians did not show up. We had sessions with zero attrition rates and sessions with a maximum of 15 per cent attrition. In

¹³ A limitation of our results is that we are not able to adjust the probability levels for multiple hypothesis testing, as the number of observations for each treatment is not sufficiently large.

post-experiment informal discussions with some participants, it was clear that the incentives—monetary (receiving more than a day's average wage, refreshments, and the possibility to earn almost five times the average daily wage) and non-monetary (that they had been randomly selected by reputed national and international universities/institutions in a study of rural development)—were attractive enough to take part. Anticipating a few dropouts, we had over-sampled and invited more participants than required. In cases of full attendance, we randomly chose who could play the game and paid the participation fee to those who were sent home without participating in the experiment.

5.2 Robustness tests

One potential concern with our results is that our sample of non-politicians may not be directly comparable with the sample of politicians, as the latter are more educated than the former, and have different occupational and caste structures (see [Supplementary Appendix Table A.2](#)). To a large extent, we control for the possibility that differences in observable characteristics across politicians and non-politicians may drive our results, by including such observable characteristics (gender, years of education, caste, and occupation) as controls in our regressions. Here, we examine whether heterogeneity within the non-politician sample on education, caste, and occupation dimensions can explain the giving and promise-making behaviour of non-politicians. We test whether more or less educated non-politicians behave the same (where we split the sample by non-politicians whose educational levels are above and below the median), as well as non-politicians from different castes and occupations. We present the results of t-tests on differences in giving and promise-making across sub-samples of non-politicians in [Supplementary Appendix Table A.4](#). Further, for politicians, we run t-tests on giving and promise-making by sub-samples of politicians who were successful in elections and those who were not, with the results presented in [Supplementary Appendix Table A.4](#). Overall, we find that differences in observable characteristics across non-politicians and whether a politician is elected or not do not play an important role in their giving and promise-making behaviour (the only statistical difference in means is for more educated non-politicians who promise more than less educated non-politicians and where non-politicians who are agricultural labourers/farmers give less than those who are not).

In a final robustness test, we estimate [Equation \(1\)](#) using a Tobit model, as the dependent variable is right-censored at 1,000. We present the results, both without and with controls, in [Supplementary Appendix Table A.5](#). We find the results to be similar to those obtained using ordinary least squares, presented in [Table 6](#). Politicians give more than non-politicians in T4, while the opposite is true in T5, with the interaction terms for $\text{Politicians} \times \text{Treatment4}$ and $\text{Politicians} \times \text{Treatment5}$ being positive and negative, respectively, and statistically significant. For all other treatments, the interaction terms with the politician dummy are not statistically significant.

6. Concluding remarks

Our work provides a first attempt to use behavioural experiments to investigate how politicians and non-politicians (ordinary citizens) respond to institutional incentives in a low-income, rural setting with a view to shed new empirical light on whether politicians are 'a different breed'. Using a modified dictator game, we varied treatments—and institutional incentives—focusing on the effects of visibility, the nature of interactions, and an upfront promise.

Under anonymity and with low visibility, both politicians and citizens allocate almost the entire endowment to themselves: both were significantly more generous when interactions were personalized. However, non-politicians were more generous than politicians when actions became more visible. In contrast, a promise induced greater generosity among

politicians than among non-politicians: while breaking a promise appears to be more costly for politicians, social image concerns (and transparency) appear to matter more for the non-politicians in our sample. Our regression results confirm the findings from t-tests of differences in means.

These findings provide new insights about the design of cost-effective mechanisms to prevent politician capture in environments where the actions of officeholders are less visible and hard to observe. Our results also show that, even in the absence of the incentives induced by repeated interactions with voters and electoral competition and campaigns, and even when selfishness and promise-breaking are easy to conceal, it is possible to significantly improve citizen welfare by leveraging politicians' social preferences embedded in local norms. Our study suggests that regular and personalized interactions between politicians and their constituents may be more effective than orthodox accountability initiatives focusing on visibility and transparency for incentivising prosocial behaviour among politicians.

Supplementary material

[Supplementary material](#) is available at the Oxford Economic Papers Journal online. These are the data and replication files and the [online appendix](#).

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