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Politicians and their promises in an uncertain world: Evidence from a lab-in-the-field experiment in India

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Abstract

In emerging economies, policy implementation failures may be caused by bureaucratic inefficiency, strategic conduct by elected or non-elected office holders or by other hurdles. For local citizens, uncertainty about the true cause of a failure prevails. We examine the effect of a promise as a mechanism to mitigate implementation failures in a laboratory experiment with local politician participants. In our modified dictator game, nature intervenes with positive probability and randomly allocates the endowment to the dictator or recipient. A core feature of our design is that a recipient who receives zero does not know whether nature intervened or not: a dictator's selfish allocation can therefore be hidden. We compare two treatments, (1) baseline: dictators choose how much to give when they, and not nature, decide the outcome; (2) promise: dictators make a non-binding promise to the recipient prior to deciding how much to give. In the baseline, about one third of politicians distribute zero; in the promise treatment, 88 % of politicians promise to give a positive amount and 83 % keep their promise. Giving is significantly more generous and the fraction of zero-giving significantly lower in the promise treatment. These results support our simple theoretical model which predicts that a promise affects the behaviour of politicians who care about their image and who incur a psychological cost from not keeping their word.

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Politicians are the same all over. They promise to build bridges even when there are no rivers. (Nikita Khrushchev)

1. Introduction

In parliamentary democracies, politicians play an obvious and critical role by deciding which policies to implement. In settings where governance is decentralised, the effort and public spirit of *local* politicians may also crucially influence how a policy fares and thus the effectiveness of policy implementation (e.g., Jack and Recalde 2015; Kosfeld and Rustagi 2015; Ferraz and Finan 2011; Besley et al 2004). Over the last few decades, developing countries have gone through 'waves of decentralization' (Bardhan and Mookherjee 2005), usually to bridge information gaps and strengthen accountability by bringing government closer to the people (Bardhan 2016). In these decentralisation initiatives, the responsibility for administration of major public programmes, for infrastructure and other public good investments or for local common property resource management is transferred to local bureaucrats, village councils and other elected or representative rural institutions (Chattopadhyay and Duflo 2004; Baland and Platteau 1996). The downside is that strong vested interests may interfere with such transfers of access, control and power and meritable decentralisation efforts may thus be tampered with, captured or held up. A key question in such implementation failures is whether the failure is caused by deliberate misconduct on the part of local politicians or has other explanations.[1]

For citizens, the question of where to put the blame when policy initiatives are delayed or not implemented as intended is therefore not straightforward: uncertainty about the true state of nature with a hard to mitigate asymmetric information challenge at its core prevails: in such situations, only a politician in power may be able to observe the true state of

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[1]See Anderson et al. (2015) who present evidence of the mechanisms through which elected representatives from an elite minority colluded to undermine policies that redistribute income towards the poor.

nature. For an elected representative with a self-serving agenda, a tempting strategy would be to blame factors outside his control and point the finger at others.

In this paper, we report the results from an innovative lab-in the field experiment that mimics this decision-making problem faced by politicians. Decision makers take actions that affect other individuals' welfare in an uncertain environment in which only the final outcome, and not their actions, can be observed by others. The external validity of laboratory experiments devoted to gaining a better understanding of politicians' behaviour can be critically questioned when non-politicians are used as experimental subjects, because of the incontrovertible selection into a political career. We tackle this issue by recruiting local politicians from two Indian states, West Bengal and Uttar Pradesh, as participants in our experiment. To the best of our knowledge, our paper is among the first to have real politicians as subjects in an experiment. We use a non-anonymous modified dictator game to first examine whether local politicians use private information about the state of nature to benefit themselves at the expense of local citizens. We next examine whether a politician's word – specifically his or her promise – is seen as binding or reneged on in circumstances where it would be easy for a politician participant to not keep his or her promise. A key question is whether the presence of a promise mitigates politician propensity to use private information to benefit themselves.

The first treatment replicates Andreoni and Bernheim's (2009) experimental design: with positive probability a subject (dictator) chooses how to split a monetary endowment with another subject (recipient), and with complementary probability the entire endowment is randomly allocated either to the dictator or to the recipient. The outcome is observed by an audience composed of the recipient, the experimenters, and other participants. While dictators who divide the endowment know that their decision will be implemented for sure, recipients (and the other people in the audience) who receive a zero amount do not observe whether the

outcome was chosen by the dictator or was due to bad luck. The second treatment (promise treatment) is identical to the first, but has the following add-on. Dictators first make a non-binding promise to the recipient: this promise is observed only by the respective recipient. Then, in the instances dictators make decisions, they choose the amount to distribute following the same procedure as in treatment one.

In the experiment, the probability that the endowment is randomly allocated by nature is high (80%). In the first treatment (where politicians can take advantage of the opportunity to hide their action), a non-negligible fraction, around one third of politicians, who played the role of dictator in the game, distribute a zero amount. In the second treatment, we find that forcing politicians to make a non-binding promise significantly affects their behaviour. To start with, politicians are more generous in the promise than in the no-promise treatment, with a 28% increase in the mean amount given. In the promise treatment, we also observe a significant drop in the proportion of politicians giving zero from 28% to 12% and a significant increase in the proportion of politicians giving 50:50, from 32% to 52%. 88 % of politicians promise to distribute a positive amount and 83 % keep their promise. This effect is striking, since we have taken care to ensure that our local politicians are matched with recipients from distant villages that they are highly unlikely to have had any past or will have any future interaction with once the experiment is over.

We offer a simple theoretical model of a dictator game with predictions in line with the experimental results. Dictators vary in their motivation and may be of three types: selfish, inequality averse, and reputation concerned (as in Benabou and Tirole 2006). In the presence of high uncertainty (i.e., a high probability that the endowment is randomly allocated by nature), reputation-concerned dictators may opt for a selfish choice, since they can blame nature for a zero transfer. However, when dictators are forced to make non-binding promises, reputation-concerned individuals change their behaviour. Reputation concerns

4

push them to make generous promises to avoid being regarded as greedy. If a reputation-concerned dictator incurs a sufficiently large psychological or moral cost from not keeping a promise, he or she will keep the promise and distribute a positive amount. The model predicts that reputation-concerned dictators with non-negligible cost of promise-breaking distribute zero in the baseline and a positive amount in the promise treatment. Since inequality-averse and selfish dictators behave similarly in the two treatments, it follows that recipients, on average, will receive a larger share of the endowment in the promise treatment.

Our experimental results confirm that local politicians care about their reputation and incur a positive cost when their promises are not kept. Our findings suggest that institutionalised mechanisms that compel politicians to report to their constituencies what they plan to do help curtail the adverse effects of uncertainty even when there is no monetary cost from reputational loss or punishment for breaking a promise. These reported effects could be stronger outside the laboratory and in situations where politicians endanger their reputation in front of the electorate. Thus, rather than investing resources in screening politicians for honesty, implementation of such mechanisms may act as a discipline device.

The rest of the paper is organised as follows. In Section 2, we briefly discuss the background and related literature. Section 3 outlines the theoretical framework and spells out our main hypotheses. Section 4 presents the research design, including the game and experimental procedures. Section 5 presents the analysis and main findings and Section 6 concludes.

2. Background

As the introductory quote suggests, politicians are, perhaps, more familiar with promises than most others: stereotypes suggest that making promises is a vital ingredient in election campaigns, while breaking promises belongs to the fine art of political practice. Such

behaviour is consistent with traditional choice theory[2], which forms the basis for the political economy literature (Osborne and Slivinski 1996; Besley and Coate 1997). Political competition and regular elections discipline politicians to act in public interest. Yet and as argued in the introduction, because of uncertainty and asymmetric information in decentralised developing country settings, democratic political institutions may not be enough to align public and private interests.

In contrast to the predictions of traditional choice theory, recent experimental studies show that an individual who breaks a promise may incur an intrinsic psychological cost (Gneezy 2005; Hao and Houser 2010; Charness and Rabin 2002)[3]. The two predominant views suggest that promises induce emotional commitments to fulfil contractual obligations based on a norm of promise keeping (Ellingsen and Johannesson 2004) or will be kept because of guilt when letting down the payoff expectations attributed to others (e.g. guilt aversion) (e.g., Charness and Dufwenberg 2006). Vanberg (2008) investigated whether promise-keeping is due to commitment preferences or guilt aversion and found support for the former. Following Vanberg's (2008) argument, we test whether politicians incur psychological costs when they fail to keep their commitment.

While the role of such social preferences among politicians has been studied for several decades by political scientists and psychologists (see e.g., Calvert 1985; Wittman 1983), the political economy literature has recently started exploring political competition with political actors and/or voters with non-standard preferences. For example, candidates

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[2] In the absence of a mechanism or contract, an agent should always break a promise and extract rents from office if this is consistent with material self-interest (e.g., contract theory (e.g., Akerlof 1970), mechanism design theory (e.g., Holmstrom, 1979)).

[3] At the same time, numerous (mainly) experimental studies show that individuals are not always guided by self-interest —they care about others and may be committed to egalitarian values (Fehr and Schmidt 1999; Dawes et al. 2007). A recent economics literature that draws on the social psychology theory of commitment, suggests that non-binding promises and oath taking significantly affect behaviour by increasing cooperation and coordination in social dilemmas and contributions to public goods (Jacquemet et al 2013; Carlsson 2013). Everyday instances of promise making include a truth-telling oath as part of court protocol and physicians being required to take a Hippocratic oath before they start practicing medicine. According to the social psychology of commitment, the oath or promise works as a "preparatory act" (Burger, 1999): compliance with an initial oath (or, promise)-taking requests changes in behaviour in subsequent decision making situations.

can have heterogeneous motives, e.g., in two dimensions: policy preferences and lying aversion (Callander and Wilkie 2007), or may have a different 'character' (Kartik and McAfee 2007) or skills (Buisseret and Prato 2016). Alternatively, heterogeneity may come from public spirit motives (altruism) or honesty (incorruptibility) (Bernheim and Kartik 2014). Moreover, candidates without such intrinsic motives can signal such unobservable characteristics strategically to voters to gain a good reputation which eventually helps them achieve ulterior self-interested motives (Callander 2008; Benabou and Tirole 2006; Ariely et al. 2009; Dana et al. 2007). The three broad politician types in our theory capture these fundamental motivational ingredients in the literature: intrinsic motivation (e.g., inequality aversion), reputation concerns and selfishness.

Substantial knowledge gaps remain about the respective importance of social and moral motivations for political selection and politicians' behaviour since the extant literature is not well equipped to provide persuasive empirical support to efforts to tackle the challenging disentangling of motivational factors. Put differently, it is hard to isolate one type of politician motivation from another and observed politician behaviour is unlikely to accurately guide such research efforts. While empirical research is well positioned to successfully document politicians' competence (e.g., their education or legislative efforts) (Ferraz and Finan 2011; Dal Bo et al 2017), the empirical study of politician motivation involves tougher identification challenges. In response, economists step back to the lab to disentangle motivations within controlled settings and aided by incentive-compatible mechanisms. In lab-experiments with student-subjects, Corazzini et al. (2014) find that campaign promises may not be pure cheap talk (also see Geng et al. 2011). In their experiments, candidates make promises to voters about how they will split the pie that the election winner receives between themselves and the voters. They find that: (i) candidates make generous promises; (ii) that candidates who promise more receive higher vote shares;

(iii) that a higher amount promised is associated with a larger transfer to voters (i.e., promise-keeping preferences among candidates) (see also, Corazzini, Kube, and Mar´echal 2007).

For the present line of inquiry, the external validity of standard lab experiments with student participants is of limited value since selection into politics and academic studies are incomparable. In democracies, politicians are heterogeneous in type, e.g., selfish or with intrinsic preferences for promise-keeping, and self-select into whether to run for office. Disentangling such contrasts is important for policy since a more incisive understanding of politicians' preferences—self-interested or social —can aid the design of policies and institutions that monitor and incentivize politicians more effectively. Our paper is the first attempt to disentangle politicians' preferences by studying real politicians in a controlled setting. [4]

3. A theoretical framework

Two players - a dictator (D) and a recipient (R) - split a prize normalized to have unit value. Let $x \in [0, 1]$ denote the transfer R receives; D consumes $1 - x$. With probability p, D chooses the transfer, and with probability $1 - p$, nature randomly assigns the prize either to R or to D; then the game ends. The probability $p \in (0, 1)$ is common knowledge, but R cannot observe whether nature intervened.

D players are heterogeneous in their motivations and their utility function has three components:

$$U_i(x) = 1 - x + \alpha\big(min(2x - 1, 0)\big) - \beta \left(min\left\{\hat{x}_{|x} - \frac{1}{2}, 0\right\}\right)^2, \quad (1)$$

where $\alpha \geq 0, \beta \geq 1$ are parameters and $\hat{x}_{|x} \geq 0$ denote R's beliefs about the amount that D has offered, conditional on having observed x. The first component is the material utility

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[4] The only other study of the behaviour of real-world politicians in the lab that we are aware of is Enemark et al. (2016): using a combination of regression discontinuity and experimental design, they examine whether holding office increases reciprocity among politicians. For a sample of politicians from Zambia, they find that office holding politicians exhibit more reciprocity than those who ran for office but narrowly lost the election.

from consuming $1 - x$; the second reflects inequality aversion, and α is the weight of this component, and the third is the concern for reputation: a dictator with parameter $\beta > 0$ cares about R's perception of his fairness, judged by the extent to which the transfer departs from the equal share $\frac{1}{2}$.[5]

We simplify the analysis by assuming that there are three types of dictators $t \in \{s, r, e\}$, each type being identified by one of these components: selfish (s-type) dictators only care about their material utility (the share of the monetary prize they get), so they have $\alpha = 0$ and $, \beta = 0$. Inequality averse (e-type) dictators are characterized by $\alpha > \frac{1}{2}$ so that they always choose to share the prize equally.[6] Finally, reputation-concerned dictators (r-type) are characterized by $\beta > 1$ and $\alpha = 0$. The r-type dictators are not "intrinsically motivated" because they do not suffer from inequality aversion: they care, instead, about the recipient's opinion (beliefs). The probability distribution over the three types, as customary, is assumed to be common knowledge. We denote by $\gamma > 0$ the probability that a dictator is an e-type and $\delta > 0$ the probability of being an r-type, with $1 - \gamma - \delta > 0$. The analysis focuses on reputation-concerned dictators, because, as already mentioned, the behaviour of the other two types directly follows from our assumptions on their utility function. We analyse the existence of perfect Bayesian equilibria in which recipients form beliefs about D's offer. Since nature plays with probability $1 - p$ and distributes either zero or one, if R observes any offer $1 > x > 0$; then $\hat{x}_{|1>x>0} = x$; because R knows that the transfer has been decided by D. However, if R observes $x = 0$ ($or\ x = 1$) then R's beliefs are computed using the Bayesian rule along the equilibrium path.

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[5] For simplicity the cost of reputation is modelled as a quadratic function, but the model can be easily generalized to any convex function.
[6] Since we assume $\alpha > \frac{1}{2}$, β is irrelevant.

By assumptions in equilibrium $\hat{x}_{|x=0} \in \left(0, \frac{1}{2}\right)$ due to (i) the presence of selfish (who choose $x_s^* = 0$) and inequality averse dictators (who choose $x_e^* = \frac{1}{2}$); and (ii) for r-type it is a dominated strategy to distribute more than $\frac{1}{2}$.[7] If an r-type decides to distribute a positive amount $\frac{1}{2} \geq x > 0$, since $\hat{x} = x$, easy calculation shows that the optimal offer is $x_r^* = max\left\{\frac{1}{2} - \frac{1}{2\beta}, 0\right\}$. Alternatively, if an r-type D distributes zero he gets $1 - \beta\left(\hat{x}_{|x=0} - \frac{1}{2}\right)$.

Consider a strategy profile such that both reputation concerned and selfish dictators distribute zero and inequality-averse dictators share the prize equally. R's beliefs about the amount offered by D when observing $x = 0$ are therefore equal to

$$\hat{x}_{|x=0} = \frac{\gamma(1-p)\frac{1}{2}}{(1-\gamma)\left(p+\frac{1}{2}(1-p)\right)+\gamma(1-p)\frac{1}{2}}\frac{1}{2} = \frac{\gamma(1-p)}{(1-\gamma)(1+p)+\gamma(1-p)}\frac{1}{2} \equiv \hat{x}_0. \quad (2)$$

An r-type dictator prefers to keep everything for him to distribute x_r^* if and only if

$$1 - \beta\left(\hat{x}_0 - \frac{1}{2}\right)^2 - \frac{1}{2} - \frac{1}{4\beta} \geq 0 \quad (3)$$

Let $\beta_0 \equiv \dfrac{1+\sqrt{1-\left(\frac{-(1-\gamma)(1+p)}{(1-\gamma)(1+p)+\gamma(1-p)}\right)^2}}{\left(\frac{-(1-\gamma)(1+p)}{(1-\gamma)(1+p)+\gamma(1-p)}\right)^2}$ denote the largest solution of the above polynomial

equation. Simple inspection confirms that $\beta_0 > 1$ for any pair (p, γ) with $p \in (0,1)$ and $\gamma \in (0,1)$, is strictly increasing in γ and tends to infinite for $\gamma \to 1$. It follows that if $\beta \in (1, \beta_0]$, then there exists an equilibrium such that selfish and reputation concerned dictators offer $x = 0$, inequality-averse dictators offer $x = \frac{1}{2}$; receivers' beliefs are such that $\hat{x}_{|x=0} = \hat{x}_0$, $\hat{x}_{|x\neq 0,1} = x$, and $\hat{x}_{|x=1} = \gamma\frac{1}{2}$. Moreover, within this set of parameters, the equilibrium strategy profile is unique for any $\beta \in (1, \beta_0)$. Suppose, by contradiction, that there exists an equilibrium strategy profile in which r-type dictators distribute a positive amount $x' > 0$, and

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[7] For any $\beta \geq 0$, if $x \geq \frac{1}{2}$ the utility is $1 - x$.

let $\hat{x}'$ denote receiver's beliefs when receiving zero according to this equilibrium. Hence

$$\hat{x}'_0 = \frac{\gamma(1-p)^{\frac{1}{2}}}{(1-\gamma-\delta)\left(p+\frac{1}{2}(1-p)\right)+(\gamma+\delta)(1-p)^{\frac{1}{2}}}\frac{1}{2} = \frac{\delta(1-p)^{\frac{1}{2}}}{(1-\gamma-\delta)\left(p+\frac{1}{2}(1-p)\right)+(\gamma+\delta)(1-p)^{\frac{1}{2}}}x' \quad (3),$$

with $\frac{\partial \hat{x}'_0}{\partial \delta} > 0$ and therefore $\hat{x}'_0 > \hat{x}_0$. The payoff of an r-type dictator who distributes x' is

equal or lower than the right hand side of (3), while the payoff in case he deviates and offers

zero is larger than the left hand side of (2) because his reputation is higher in this equilibrium

$(\hat{x}'_0 > \hat{x}_0)$ and therefore the deviation would be profitable. The following proposition

summarizes this discussion.

> **Proposition 1** Suppose $p \in (0,1)$, $\gamma \in (0,1)$ and $\beta \in (1, \beta_0]$. In equilibrium
> reputation-concerned dictators offer zero.

Consider now a dictator game in which dictators are forced to make a non-binding

promise, an announcement $a \in [0,1]$ to receivers about how much they will distribute. If

dictators suffer a psychological cost of promise-breaking, then promises affect R's beliefs

when observing $x = 0$ and, ultimately, increase the distributed amount by r-type dictators.

To convey the intuition, we assume that the costs of promise-breaking are the same for every

dictator and large enough to force dictators to keep their promises; specifically, the utility

function of an r-type dictator is equal to

$$U_r = 1 - x - \beta \left(min\left\{\hat{x}(a) - \frac{1}{2}, 0\right\}\right)^2 - c(max(a-x), 0) \quad (4),$$

with $c > 1$, where we write now beliefs as a function of the promise, $\hat{x}(a)$, to emphasize the

role of promise a in shaping R's beliefs when observing $x = 0$. In fact, if dictators keep

their promises due to a large cost of promise-breaking, a rational recipient believes that

$\hat{x}_{|x=0} = a$. It follows that in equilibrium every D keeps his promise and for any $\beta > 1$, selfish

dictators promise and distribute $a_s = x^*_s = 0$, inequality averse promise and distribute

$a_e = x^*_e = \frac{1}{2}$, and reputation concerned dictators promise and distribute $a_r = x^*_r = \frac{1}{2} - \frac{1}{2\beta} >$

0 and for any a and for any x, $\hat{x}_{|x} = a$. The following proposition highlights the difference in the behaviour of reputation concerned dictators in the two games.

Proposition 2 Let $p \in (0,1)$, $\gamma > 0$, $c > 1$ and $\beta \in (1, \beta_0]$. Reputation-concerned dictators distribute zero in the game with no promise and a positive amount in the game with promise. Selfish and inequality-averse dictators behave the same in the two games.

The results stated in the above proposition still hold if we relax some of the simplifying assumptions we made. In particular they hold if we assume that a positive fraction of r-type dictators have zero costs of promise-breaking and therefore in equilibrium make the promise that gives them the highest reputation. It is not hard to prove that there still exists an equilibrium in which r-type dictators with positive cost of promise-breaking distribute the positive amount x_r^*. The equilibrium strategy profile is such that e-type dictators distribute and promise $a_e = x_e^* = \frac{1}{2}$; s-type dictators distribute and promise $a_s = x_s^* = 0$; and r-type dictators with zero costs of promise-breaking announce $a = \frac{1}{2}$ and distribute zero, r-type dictators with positive costs of lying announce and distribute $a_r = x_r^*$.

Let $\tilde{x}_{|x=0,a}$ denote R's beliefs according to this equilibrium when observing a promise a and a distributed amount equal to zero. Recipients' equilibrium beliefs about the amount offered by D when observing zero are now

$$\hat{x}_{|x=0,a=\frac{1}{2}} = \frac{\gamma(1-p)\frac{1}{2}}{\gamma(1-p)\frac{1}{2} + \delta\eta\left(p + \frac{1}{2}(1-p)\right)} \frac{1}{2} \qquad (5),$$

$$\hat{x}_{|x=0,a=x_r^*} = x_r^* = \frac{1}{2} - \frac{1}{2\beta} \qquad (6),$$

$$\hat{x}_{|x=0,a} = 0 \text{ for all } a \neq x_r^*, \frac{1}{2} \qquad (7).$$

This equilibrium exists for some $\beta > 1$ if r-type dictators with zero cost of promise breaking have no incentive to deviate from promising x_r^*, still distributing zero, that is if

$$1 - \beta \left(\frac{\gamma(1-p)\frac{1}{2}}{\gamma(1-p)\frac{1}{2} + \delta\eta\left(p + \frac{1}{2}(1-p)\right)} \frac{1}{2} - \frac{1}{2} \right)^2 \geq \frac{1}{2} + \frac{1}{4\beta}, \text{ and this condition holds for } \eta \text{ small enough.}$$

Our experiment allows us to test our main assumption of whether politicians have a large cost when they deviate from their promise. This is summarised in the following hypothesis.

Hypothesis 1: Politicians keep their promise.

According to our model, there is a non-empty set of parameters such that reputation concerned dictators give nothing in the no-promise treatment and will give a positive amount in the promise treatment. The main predictions from the model that we test in the experiment are therefore the following:

Prediction 1: The fraction of politicians who give zero in the no-promise treatment is larger than in the promise treatment.

Prediction 2: The welfare (i.e., the average amount received) of the recipients is greater in the promise treatment than in the no-promise treatment.

4. Recruitment, experimental design and implementation

Recruitment

We envisaged two main organisational challenges in recruitment: (i) recruiting real politicians as subjects; and (ii) creating a neutral field-lab environment.

For recruitment, we take advantage of India's decentralised and democratic local governance structure, the Panchayat system. This system has three tiers: *Gram Panchayat* (village-level councils), *Panchayat Samiti* (block-level councils), and *Zila Parishad* (district-level councils). A *Gram Panchayat* is divided into *Samsads* (wards). Citizens elect representatives for each tier and elections are held with regular, five-year intervals[8]. Village level elected representatives generally do not have a role in the higher tiers (e,g., block or

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[8] The politicians at the bottom tier of this system (*Samsad* or ward leader) represent around 500-800 voters (around 200-300 households) and are members of a village council or *Gram Panchayat* (GP). GPs usually serve around 3000-5000 voters, although size varies widely. The second tier (i.e., block level) consists of 10-12 GPs and the final tier is the district council (i.e., *Zila Parishad*) which consists of 15-20 (on average) blocks.

district level) unless they are the village council head or hold a key position in the political party they belong to.

Through the 73rd Constitutional Amendment (1993), village councils were given responsibility for implementation of a variety of government-funded development programs and decisions about investments in local infrastructure such as sanitation, drinking water and roads (Chattopadhay and Duflo 2004). The elected representatives of interest here can thus exercise considerable power in their constituencies.

Our definition of a politician is a person who has either recently fought or recently won an election for a village council (*Gram Panchayat* or GP) seat as a ward member[9]. These self-selected politicians' preferences—whether selfish or social —have not been studied in depth. Monetary incentives for holding office are limited (e.g. the official salary of the village head is about USD 50/month, ward leaders are paid even less); but there are potential private returns from political rents and corrupt practices[10]. Elected representatives may also enjoy high social status (e.g., Fehr et al. 2013; Jack and Recalde 2015). Village level politicians are likely to have lower opportunity costs of time and are unlikely to be concerned about their reputation (e.g., to influence the probability of winning elections) when facing an unknown audience they have not previously met and are unlikely to meet in the future.

In West Bengal, Hooghly district and in Uttar Pradesh Varanasi district were selected due to convenience and researchers' prior experience working there. From among the administrative blocks in each of these districts, we randomly selected two blocks

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[9] We purposely avoided recruiting village council heads (pradhans) because of their typically greater and more visible role in their party's political machinery, and their higher likelihood of being known to more villagers. within a district, including among those from distant locations. The opportunity cost of time for village council heads would also be higher than for ward members.

[10] Some evidence suggests that an average candidate spends USD 400 - USD 800 during a village council election (see: http://www.ndtv.com/india-news/the-rs-81-500-crore-lie-565175). The average declared wealth of re-contesting candidates to Parliament and state legislative assemblies in 2004 was 134 percent higher than during the first election (Sastry 2014), suggesting high rents. Fisman et al. (2014) also show that the annual asset growth of winners in state elections is 3-5 p.p. higher than for runners-up. Although similar statistics are not available for village council candidates, the returns are likely to be non-trivial.

following a stratified random sampling based on geographical location[11]. Next, GPs were randomly selected from each block. We randomly selected our politician participants from a carefully assembled list of politicians who had stood for GP elections during the last ten years[12]. We made sure that politician and other participants from one GP should not have any prior knowledge about their matched-counterparts from another GP[13]. We also chose the timing of the experiment carefully to avoid any overlap with election-related or other political campaigning.

From each village and based on the household census, we also invited randomly selected ordinary citizens (non-politicians) to participate in the experiment. This presence of non-politicians aimed to reduce experimental demand effects, since a sample comprising only of politicians could intensify the feeling of being under scrutiny during the experiment. We discuss this issue in more detail after presenting our results in section 5.

Design

Our implementation of the design is described in the following steps. (1) Participants from the home-village (where the venue was located) and the visitor-village (i.e., from distant locations) arrived separately and did not meet each other before entering the lab venue. (2) Following a random matching protocol, a subject from the visitor-village formed a pair/group with a subject from the home-village. Ten politicians and ten villagers participated in each

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[11] For example, from among Hooghly's eighteen administrative blocks, we randomly selected Singur and Dhaniakhali. In Uttar Pradesh, Badagaon and Sevapuri blocks were selected following a similar procedure.

[12] For each GP and to recruit politicians, we prepared a list of individuals who had contested or been elected during the two most recent elections and drew randomly from this list.

[13] 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 to participate with an invitation letter prepared by the research team. The letter neutrally framed the purpose of the study (e.g., we want to study challenges of rural development) and explained the random selection of the village/GP and participants and that participation is voluntary. It also provided other relevant information about the study (e.g., duration, incentives etc) (see letter text in supplementary Appendix). Participants were then given a few days to decide whether to take part. Participants knew that they could change their decision any time, even during the study, without giving any explanation.

session. (3) Pair members were asked to stand up and greet each other. This was done to increase the moral costs of selfish behaviour. (4) The experimenter read out and explained the instructions of the game aloud and answered questions from participants. Each participant was then asked to solve a short quiz. Those who could not answer the quiz properly were given an extra explanation from the experimenter. The experimenter made clear that participant names would not be recorded.[14] (5) Two practice-rounds of the game were played. (6) Each pair received a fixed and known endowment—1000 INR (15.50 USD)[15]—for each round and the dictator had to decide how to allocate the endowment between him/herself and his/her partner (recipient). (7) Their roles (dictator (D) or recipient (R)) were determined randomly and both politicians and non-politicians could be assigned the role of dictator.[16] We did not change their roles in each round—a randomly chosen dictator remained dictator for the entire session. (8) No communication between subjects was allowed (verbal or any other type).

No-Promise Treatment: (9) Each D received a random (and confidential) private number between 1 and 10—no other person in the room, not even the experimenter - would know this number. (10) At the start of each round, the experimenter announced two numbers between 1 and 10 and only those Ds with the corresponding numbers made a decision, other Ds could not. (11) Each D received a decision sheet. They filled in their decision sheets (e.g., their group number, private number, and round number) in an enclosed area one by one. Only Ds whose private numbers were announced could choose and record a distribution on the

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[14] We did not record participant names. As noted above, only the enumerators had access to participant names and our research assistants or any other member of the research team did not record names during data entry. We reiterated before the experiments that the research team would not have access to participant names.

[15] For example, the minimum agricultural wage for a skilled labourer and the MGNREGS daily wage for a unskilled worker in West Bengal were fixed at 272 INR and 176 INR by the government in 2016 (see, e.g., https://www.wblc.gov.in/synopsys/TmhhWGFaYjBDRTJiWWV2Yks1MVN2QT09). Also, average daily agricultural wages in West Bengal and Uttar Pradesh are around 315 INR and 233 INR in 2017 (see, http://labourbureaunew.gov.in/UserContent/ILJ_March_2017.pdf?pr_id=ejfPiQTrZuY%3d).

[16] As our research focus was on politician behaviour and it was challenging to recruit politician participants, we did not follow an equal split while randomly allocating the roles of politicians and non-politicians. Instead, we did the following for each session: randomly chose a number between 5 and 8 and chose the split accordingly (e.g., if number 6 is drawn, 6 out 10 politicians played the role of dictator for that session).

decision sheet in private, others would just tick a box which stated the nature would give zero to either D or R. (12) All Ds, who made a decision or who ticked a box, folded the decision sheet and put it in an envelope, named Round 1-Decision, themselves. No one (including the experimenter) should be able to identify, during and after the experiment, which individual made a decision and what his or her decision was.[17] The Rs 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 the entire endowment of 1000 INR (this can only be true if D chooses the same division as nature).

(13) We repeated steps (9) to (12) four times more (i.e., five rounds were played) and each time the experimenter announced different private numbers. (14) One of the five rounds was selected randomly to determine the payments. (15) The envelope of decision sheets for that round was given to an external person waiting outside the venue. The external had no information about the game or about the participants. He observed the decision-sheets of different dictators in a separate room and put the payment in a separate envelope for each dictator. He also decided whether D or R got INR 1000 when nature intervened by flipping a coin. (16) Meanwhile, participants filled in a short questionnaire that covered education, occupation and other demographic and related questions. (17) The experimenters received the result and envelopes with cash payments for each subject (each subject's individual id number, same as their seat numbers, written on each envelope) from the external. The experimenters published the result (wrote each pair's earning). (18) Each participant received their envelopes with payments and left the venue one by one. Local participants exited before the subjects from the visitor-village.

Promise Treatment: We followed step (1) to (8). Then all Ds wrote (their pair number and) how they would allocate INR 1000 between him/herself and the R on a 'Promise Slip'.

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[17] The Ds who did not make decisions were asked to put a tick in the decision sheet. This was to ensure that recipients would not know that nature made the decision in these cases.

Each D went to an enclosed area and wrote this in private: the D then folded the promise-slip and returned it to the experimenter who passed it (without seeing it) on to the respective R. Each R observed what his/her partner promised to give him/her in private. The slip was then folded and put in an envelope called Round 1 which is a general envelope specific to a Round. No one in the room except the respective R, not even the experimenter, could observe the promise. The dictator game described above (i.e., Step (9) to (12)) was then played. After that, we repeated the promise-making stage, as described above, and then Step (9) to (12) again four times more and for a total of five rounds. Steps (14) to (18) were then followed.

It is important to emphasise that (i) a D can hide his/her actual decision about allocation with a probability of 0.8 (i.e, in each round 2 of 10 dictators would make allocation decisions) and that this probability is fixed and common knowledge; (ii) a D's promise is only seen by the respective R. If a D wants keep everything for her/himself without losing his/her image in front of the respective R, he could simply make a generous promise to R and then give 0. Then nobody—except the D in question—would know whether nature intervened or the D decided. Accordingly and to reiterate , the D can behave selfishly without being 'found out' by the 'audience'. Notice that any other distribution would reveal the identity of the D who made the decision.

5. Analysis

Data

Our sample contains 96 politicians. In Table A1, we present the summary statistics of the observable characteristics of politicians, by gender, educational level, age, caste, and occupations. We note that 41 per cent of politicians are female. Politicians have 9.3 years of education and are 43 years old on average. The proportion of politicians from forward caste background is 41 per cent. About 52 per cent of politicians are farmers.

Results

We first provide a summary of our main results before discussing the key findings from the two treatments separately. We find evidence of all three types of politicians – selfish (s-type), inequality averse (e-type) and reputation concerned (r-type) in the data. Selfish politicians – that is, politicians who give a zero amount – represent 12 % of the sample in the promise treatment. Around 80% of them (who give zero in the promise treatment) actually promise a zero-amount. The 28% of the sample who give zero in the no-promise treatment, could either be selfish or reputation concerned politicians. The remaining 72% of politicians in the no promise treatment show some level of inequality aversion and distribute a positive amount: around 31% of dictators distribute exactly 50% of the endowment. Finally, we find a non-negligible presence of reputation concerned politicians – that is, those who care about the audience's beliefs – as the proportion of dictators who give zero drops markedly from 28% per cent in the no promise treatment to 12% in the promise treatment. Moreover, the proportion of dictators who distribute half of the endowment, increases from 31.5% in the no promise treatment to 52.3% in the promise treatment.[18] We find that 83% of politicians keep their promise: politicians are also more generous when they have to make a promise. This suggests that politicians face a psychological cost of reneging on a promise, even in contexts in which its breach cannot be observed.

We now report our detailed results by treatment.

Result 1: Politicians keep their promise.

In Table 1, we provide the amount promised and the difference between amount given and amount promised, by different levels of the amount promised. Politicians promise Rs 478 on average, with 57.1 per cent of politicians promising exactly half of the amount allocated (Table 1). We see that they are highly likely to keep their promise – which 83 % do -with the

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[18] As we will report later, these differences are statistically significant.

mean difference between amount given and amount promised being Rs. -30.95 for the whole

sample, and Rs. -12.5 for those who promised 50 per cent of their initial allocation. This is

also evident from Figure 1 where we plot the amount promised versus the amount given. If

dictators maintained their promise, observations in the figure would be at the 45 degree line.

We see that the line of best fit is very close to the 45 degree line.

[Insert Figure 1 here]

[Insert Table 1 here]

> *Result 2a: There is a significant decrease in the proportion of zero giving and a significant increase in the proportion of 50:50 giving in the promise treatment.*
> *Result 2b: Politicians give more in the promise treatment. Politicians do not treat promises as cheap talk and take their promises into account in their allocation decisions.*

The kernel density plot for the amount given in the promise treatment as compared to the no-

promise treatment shows a spike at 50:50 split, suggesting more politicians moved to a 50:50

sharing norm in the promise treatment (Figure 2) - 52.3 per cent of politicians give 50:50 in

the promise treatment as compared to 31.5 per cent in the no promise treatment. We also find

a sizeable decline in zero giving among politicians in the promise treatment as compared to

the no promise treatment – 11.9 per cent as compared to 27.8 per cent (Table 2). Both

differences are statistically significant. The z statistic for the test of difference in proportions

in zero giving in the promise treatment compared to the no promise treatment is 1.89 with a p

value of 0.06, and in 50:50 giving is -2.07 with a p value of 0.04 (Table A2). We also find an

increase in the amount given in the promise treatment as compared to the no-promise

treatment - from Rs 350 in the no promise treatment to Rs 447.6 in the promise treatment,

with the Wilcoxon Rank Sum test statistic of 1.89 significant at the 10 per cent level with a p-

value of 0.06 (Table A2).[19]

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[19] The t statistic on difference in means is significant only at the 11 per cent level.

[Insert Table 2 here]

[Insert Figure 2 here]

We next investigate whether this difference in behaviour across treatments remains evident after we control for observable politician characteristics. We run separate regressions where we regress the amount given, a dummy for zero giving and a dummy for 50:50 giving on the treatment dummy (equals 1 if promise treatment, 0 if no promise treatment) and politicians' characteristics (the politician's gender, age, and educational level, and dummies for occupation, caste and the state from where politicians are recruited).

We report the results for the amount given, zero giving and 50:50 giving in Cols. (1), (2) and (3) of Table 3. We find that the coefficient on the promise treatment is significant at the 10 per cent level or higher for all the regression estimates, and with the right signs − controlling for their observable characteristics, politicians give significantly more in the promise treatment. They are also less likely to give zero amounts (16 per cent less), and more likely to give 50:50 (25 per cent more) in the promise treatment.

[Insert Table 3 here]

Since the probability that nature decides the outcome is identical in the two treatments, our findings on the higher amounts given in the promise treatment as well as the significant move away from zero giving is attributable to the commitment role of promises in the second treatment.

One possible concern is whether the promise results are caused by an experimenter demand effect (EDE). There are two possible channels through which an EDE could matter. Firstly, politicians could feel under "special" scrutiny when they received the invitation to participate in the experiment. Secondly and in the lab, a politician-dictator could respond to the explicit presence of the 'audience', including the experimenters (this was in fact a deliberate feature of our design and intended to heighten the social image effect (as in

Andreoni and Bernheim (2009)). However, we are confident that this not the case, firstly, because of the emphasis throughout that participation was voluntary. A politician concerned about 'special scrutiny' could simply opt out. Second and more importantly, our results in the no-promise treatment, where one third of the dictators gave zero, suggest that scrutiny did not interfere with and discourage selfish behaviour. Since experimenter demand effects would be expected to be consistent across the two treatments, their limited impacts on behaviour in the baseline suggest limited impacts on behaviour in the promise treatment as well.

Adding to this, the audience, including the experimenters, could only observe the amount distributed by the politician-dictator (and only in instances where a positive amount is distributed), but not the promise made. The promised amount could only be observed by the corresponding recipient and never by the experimenters or the other participants because of the blinding mentioned above. A politician-dictator could also hide his/her distributed amount by choosing zero-giving, which no-one, including the experimenters, would be able to identify.

Using the data collected of non-politician dictators' behaviour we can also make some preliminary inference about the behaviour of non-politicians.

Result 3a: Non-politicians are less likely to keep their promise

Result 3b: Non-politicians are not likely to give more in the promise treatment compared to the no promise treatment

Results about non-politicians should be consider cautiously, due to the small sample size: we only have 30 observations in the no promise treatment and 22 in the promise treatment. We see from Figure 3 that non-politicians are unlikely to keep their promises – the line of best fit is significantly different from the 45 degree line. We also find that there is very little difference in the allocation decisions of non-politicians across the two treatments (Figure 4 and Table 4) – 36.7 per cent of non-politicians gave Rs. 500 in the no promise treatment which marginally increased to 40.1 per cent in the promise treatment. Further, the test

statistics on differences in amount given, zero giving and 50:50 giving across the two treatments all lack statistical significance (Table A2). While the small sample size, makes sharp inference harder, the patterns in the data suggest that there is no evidence of promises playing a similar commitment role for non-politicians as they did for politicians. This is consistent with our theoretical predictions, if ordinary citizens in our sample are either selfish or inequality averse individuals subjects, and only among politicians there is a non-negligible size of reputation-concerned individuals.[20]

[Insert Table 4 here]

[Insert Figure 3 here] [Insert Figure 4 here]

Another interesting issue that we have not discussed up to now is whether the costs of promise-breaking are a moral/psychological fixed cost, as in our simple model, or they are also reputation driven. In our model, reputation is defined as a dictator's concerns about the audience's beliefs about his greediness, i.e., beliefs about the chosen amount compared to the 50-50 fair reference allocation. In contrast, the costs of promise-breaking, may vary among dictators and may not depend on audience beliefs. Alternatively, the cost of promise-breaking could also depend on audience beliefs about the sincerity of the dictators. However, in our design with a high value of p (which corresponds to a very uncertain environment), one third of dictators distribute zero in the no-promise treatment. We observe that dictators remarkably keep their promises even in an environment where it could be easy to renege their pledges without a strong reputational loss. This suggests that the cost of promise-breaking is based more on intrinsic preferences for promise-keeping than shame or reputational from promise breaking (see Cohen et al. (2011,2012) for some psychological studies on shame and guilt)[21].

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[20] The reason why only among politicians there is a sizeable proportion of reputation-concerned individuals could be either due to self-selection into politics by those types or due to an attitude induced by their status: the answer to this question is beyond the scope of this paper.

[21] Future research could explore this issue by explicitly testing whether people care more their reputation of being sincere or they have true intrinsic preferences to keep their words.

6. Concluding Remarks

In this paper, we investigate whether non-monetary and non-binding commitments (e.g., a promise) can act as a useful device to discipline politicians. Despite a common negative view among voters about the extent to which politicians keep their promises, the political science literature argues that politicians tend to fulfil election pledges. According to a recent comparative study by Thomson et al. (2017) on election pledges made during 57 electoral campaigns in twelve countries, the fulfilment of election pledges ranged from 80% to 60%, depending on institutional differences (e.g. the percentage of fulfilment is higher for single-party government than for coalition). Our lab-in-the-field experiments with local politicians provide further and direct evidence of the effect that non-binding but accurate promises have on the choices made by real politicians. The fact that politicians make generous promises suggests that they care about their reputation. The fact that politicians keep the same promises is consistent with the hypothesis that they incur psychological costs when breaching them.

Compared to our subject-politicians in a controlled lab environment, in reality politicians will interact with known voters and peers repeatedly and face electoral competition. One should expect them to care about their own reputation and bear the cost of promise-breaking more than our subject-politicians. Even a selfish politician would make a generous promise to win an election and should care about his reputation because of the disciplining induced by re-election. Our paper shows that even in absence of material incentives induced by repeated interactions with voters, politicians may intrinsically care about their social image and suffer a psychological cost from not keeping their word. Our study suggests that more transparent and informative communication between politicians and their constituencies, not only during electoral campaigns but also when they are in office could help in providing incentives to politicians to act in favour of their citizens.

We recognise the following limitations of our study that future research could explore. Firstly, one may question how representatives our village-level politicians are. We argue, however, that they become politicians by following a standard election process and exercise substantial power—financial and decision making—in their decentralised everyday setting. Secondly, our sample size is small. However, it is difficult to get politicians in the lab, even at the village-level. Future research should investigate politicians' motivations and promise keeping behaviour with a larger data set and at different levels (e.g., block/district level leaders, municipal-council leaders) and in different countries.

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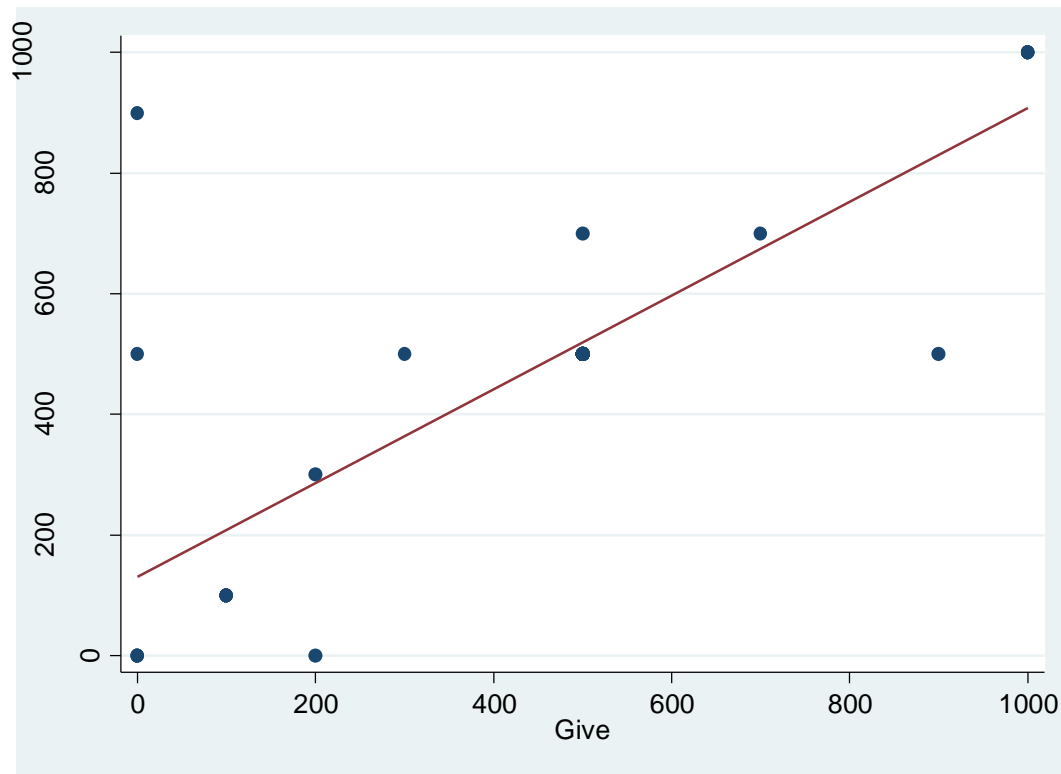
Figure 1. Promise Versus Amount Given (Give_, Politicians

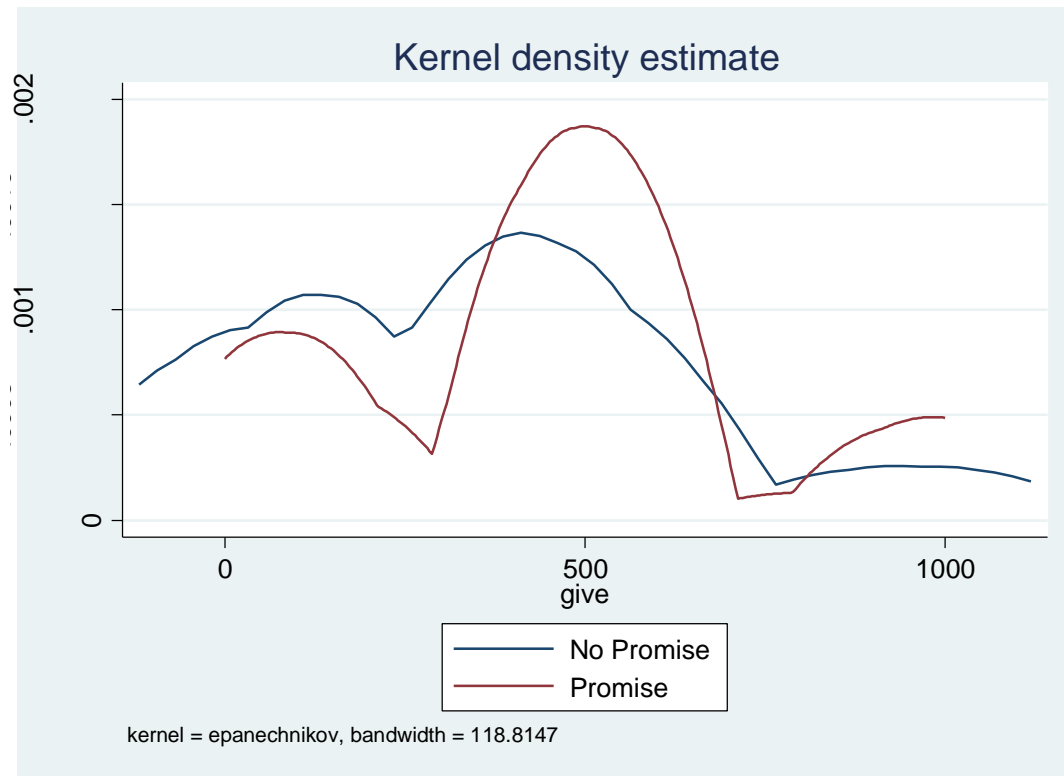**Figure 2. Amount Given, No Promise and Promise Treatments, Politicians**

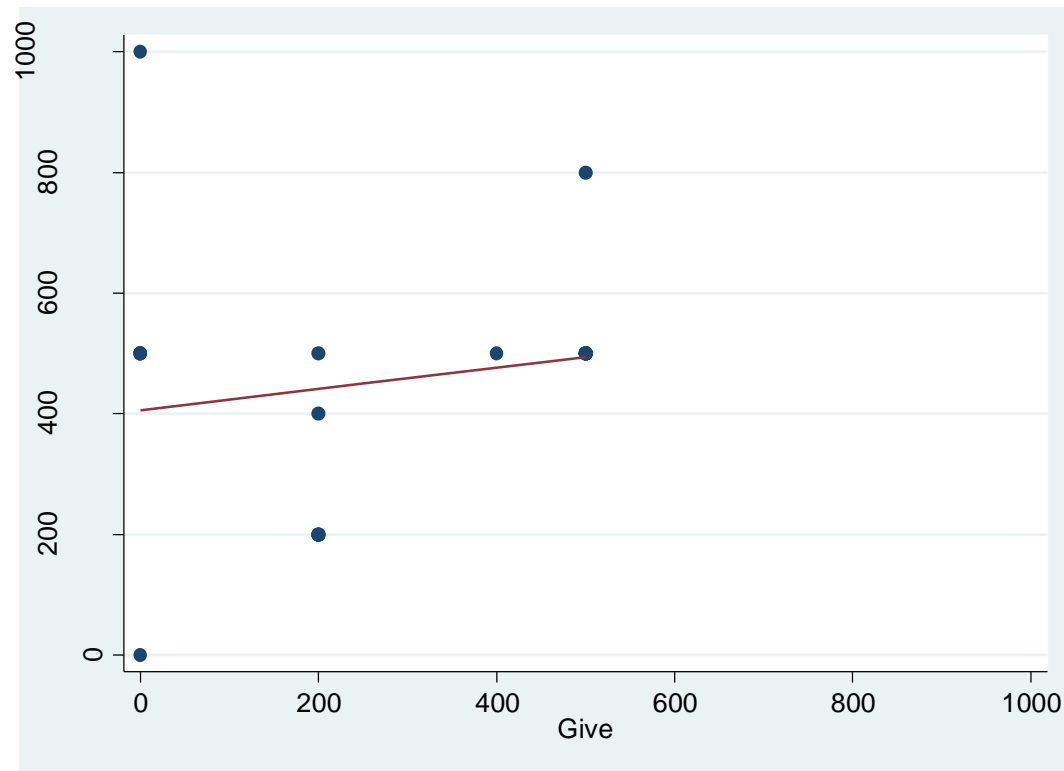**Figure 3. Promise Versus Amount Given (Give), Non Politicians**

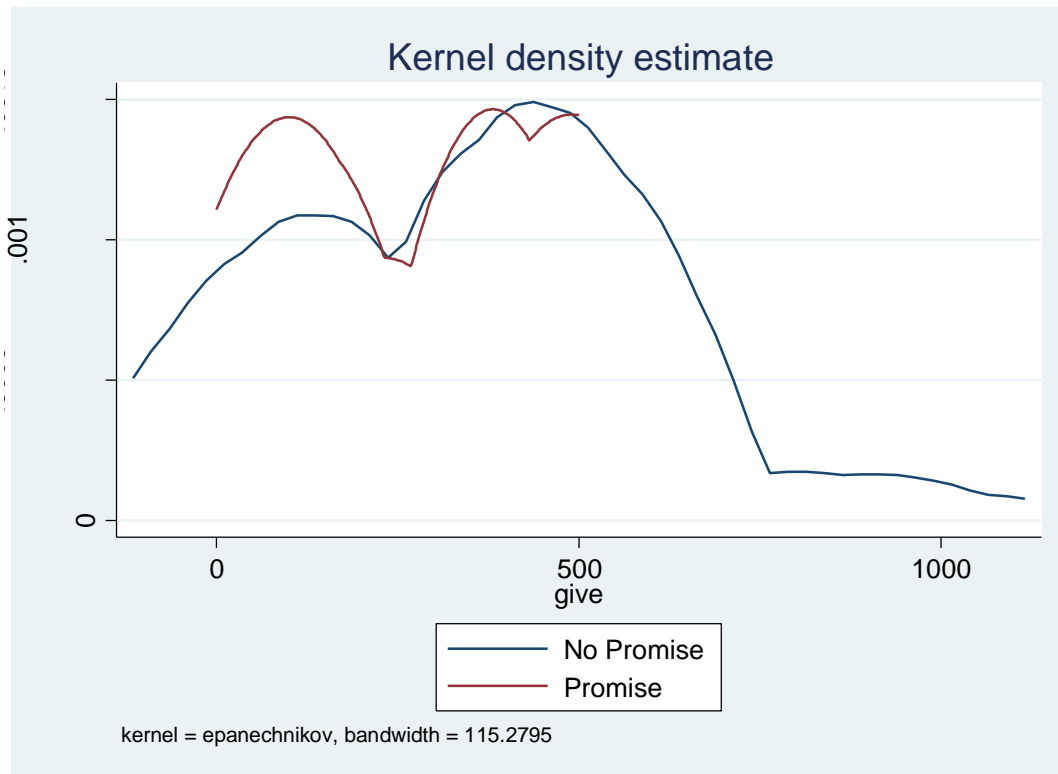**Figure 4. Amount Given, No Promise and Promise Treatments, Non Politicians**

Table 1. Amount Promised and Mean Amount Given-Amount Promised

Amount Promised	Amount Promised			Amount Given-Amount Promised
	Number	%	Cum.	Mean
0	4	9.52	9.52	50
100	5	11.9	21.42	0
300	1	2.38	23.8	-100
500	24	57.1	80.9	-12.5
700	2	4.76	85.66	-100
900	1	2.38	88.04	-900
1000	5	11.9	100	0

Table 2. No Promise and Promise Treatments, Politicians

Amount Given	No Promise			Promise		
	Number	%	Cum.	Number	%	Cum.
Equal to 0	15	27.8	27.8	5	11.9	11.9
Greater than 0 and less than or equal to 100	1	1.9	29.7	5	11.9	23.8
Greater than 100 and less than or equal to 200	3	5.6	35.2	2	4.8	28.6
Greater than 200 and less than or equal to 300	9	16.7	51.9	1	2.4	31
Greater than 300 and less than or equal to 400	2	3.7	55.6	0	0	31
Greater than 400 and less than 500	0	0.0	55.6	0	0	31
Equal to 500	17	31.5	87.1	22	52.3	83.3
Greater than 500 and less than or equal to 600	1	1.9	88.9	0	0	83.3
Greater than 600 and less than or equal to 700	1	1.9	90.8	1	2.4	85.7
Greater than 700 and less than or equal to 800	0	0.0	90.8	0	0	85.7
Greater than 800 and less than or equal to 900	1	1.9	92.6	1	2.4	88.1
Greater than 900 and less than or equal to 1000	4	7.4	100.0	5	11.9	100
Number of Observations	54			42		

Table 3. Regression Results

	(1)	(2)	(3)
Promise Treatment	123.71*	-0.16*	0.25**
	(65.85)	(0.08)	(0.10)
Controls?	Yes	Yes	Yes
Adjusted R-square	0.60	0.24	0.40
No of Observations	96	96	96

Notes: Controls: Caste and Occupation Dummies, Years of Education, Age, Gender, State Dummy; t-statistics in brackets. ***,** and * indicate level of significance at 1, 5 and 10 per cent respectively; Col (1): Dependent variable: amount given, Col (2): Dependent variable: Dummy if give=0, 0 otherwise; Col (3): Dependent variable: Dummy if 50:50 share is 1, 0 otherwise. The estimator is OLS, with no intercept term.

Table 4. No Promise and Promise Treatments, Non-Politicians

Amount Given	No Promise			Promise		
	Number	%	Cum.	Number	%	Cum.
Equal to 0	5	16.7	16.7	6	27.3	27.3
Greater than 0 and less than or equal to 100	4	13.3	30.0	0	0	27.3
Greater than 100 and less than or equal to 200	2	6.7	36.7	6	27.3	54.6
Greater than 200 and less than or equal to 300	3	10.0	46.7	0	0	54.6
Greater than 300 and less than or equal to 400	2	6.7	53.4	1	4.6	59.2
Greater than 400 and less than 500	0	0.0	53.4	0	0	59.2
Equal to 500	11	36.7	90.0	9	40.1	100
Greater than 500 and less than or equal to 600	1	3.3	93.4	0	0	100
Greater than 600 and less than or equal to 700	0	0.0	93.4	0	0	100
Greater than 700 and less than or equal to 800	1	3.3	96.7	0	0	100
Greater than 800 and less than or equal to 900	0	0.0	96.7	0	0	100
Greater than 900 and less than or equal to 1000	1	3.3	100.0	0	0	100