Value for money? Vote-buying and politician accountability in the laboratory*

Jessica Leight[1], Rohini Pande[2], and Laura Ralston[3]

[1]Williams College Department of Economics
[2]Harvard University and NBER
[3]World Bank

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Abstract

This paper reports on the first laboratory study that examines whether and how vote-buying engenders moral hazard among elected politicians. The experiments, conducted with 816 subjects in U.S. and Kenya, test how vote payments influence voters' willingness to discipline a politician for rent expropriation. We report three main findings. First, vote-buying engenders moral hazard: voters who receive payments increase the maximum amount they are willing to allow a politician to expropriate while still voting to reelect, a result inconsistent with a standard model of rational voting behavior. There is, however, a backlash effect among those excluded from payments. Second, the effectiveness of payments is increasing in the number of payments distributed; payments are much more effective in altering a particular subject's behavior when distributed to all voters, rather than to a subset of voters. Finally, we examine the role of voter consent in private information environments, and find the largest increase in the reelection threshold among consenting subjects. Taken together, these findings demonstrate that voter preferences are such that vote-buying may undermine electoral accountability and enable expropriation by the incumbent. They also rationalize the prevalence of *both* secret targeted vote-buying with explicit consent and broad based gift-giving by political parties.

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1 Introduction

The ability of political elites to use financial resources to undermine the one-person one-vote principle of representative democracies – via pork barrel politics, clientelism or straight cash for votes – is widely considered to be a key reason for elite capture of public policy the world over (Acemoglu & Robinson 2012, Khemani 2014). However, unlike pork-barrel and clientelistic politics, little is known about the channels through which vote-buying alters subsequent policy-making outcomes.

A first potential channel is adverse selection: if certain types of politicians are more likely or better able to employ cash to garner votes, then vote-buying may lead to the selection of lower quality candidates, or candidates with preferences that do not match those of the electorate. A second channel is moral hazard. If voters have social preferences such that the receipt of a payment leads them to fail to hold politicians accountable for their subsequent behavior while in office, then vote-buying may undermine mechanisms of electoral accountability for incumbent politicians.

Disentangling politician choices from politician quality is empirically challenging. In this paper, we provide the first empirical evidence of a moral hazard effect of vote-buying by analyzing data from laboratory experiments conducted with 816 subjects in Kenya and the U.S. We deploy a simple model of retrospective voting in which subjects can choose whether or not to reelect an incumbent who expropriates a varying amount of rents from a common treasury, and the reelection choice serves as a tool to discipline rent expropriation. No other dimension of politician quality influences voters' earnings. Hence, voters do not face any selection motive. In equilibrium, the politician always prefers to expropriate an amount less than or equal to the subjects' reelection threshold, and thus to be reelected.

We then augment this game by introducing vote payments as an exogenous external transfer to the voter, described in the baseline game as "a payment in exchange for your vote". The politician has no agency in payment distribution; payments are simply assigned to a certain fraction of voters, and the secret ballot is maintained throughout. If subjects seek only to maximize earnings, the introduction of vote payments should not impact their behavior.

We report three primary findings that demonstrate the existence of a moral hazard effect associated with vote-buying, and are inconsistent with a standard returns-maximizing model of voter behavior. First, voters are responsive to the introduction of payments: voters' maximum expropriation threshold for the re-election of the politician changes once vote-buying is introduced. However, when even minimal information about vote-buying is public, subject behavior varies depending on whether s/he receives a payment. Relative to the game with no payments, subjects who receive a payment increase the threshold of expropriation at which they will reelect the politician. Subjects who do not receive a payment, by contrast, treat the politician more harshly, pointing to a possible phenomenon of backlash at their exclusion.

Second, the effectiveness of vote payments is increasing in the extent to which the hypothetical polity is saturated with payments. When more payments are distributed, the net effect of each payment on the targeted voter's reelection threshold becomes larger.

Third, we explore the relevance of eliciting voter consent for a payment when there is limited public information about vote-buying. When consent for a payment is solicited in advance, we observe significantly more lenient treatment of the politician, with a roughly 15% increase in the effect of vote-payment on the average threshold. This result is consistent with voters' response reflecting social preferences that are activated by the provision of consent for a payment.

Taken together, these findings provide robust evidence of a moral hazard cost to vote-buying: it lowers voters' willingness to use retrospective voting to discipline politicians. However, there is also considerable heterogeneity in voters' responsiveness to payments, suggesting that variations in the structure of vote-buying are an important determinant of its effectiveness.

This paper joins a theoretical and empirical literature in both economics and political science that has sought to analyze vote-buying from both a normative and a positive perspective. The normative conclusion in political science has generally been that vote-buying disables normal democratic mechanisms of accountability; once a vote is purchased, voters no longer exert effective control over general policy decisions and the politician is left free to make those decisions in accordance with his or her own preferences, rather than the preferences of the supposedly represented (Kitschelt 2000, Stokes 2007). However, this literature generally does not distinguish between selection and incentive effects.

A theoretical literature in economics has also analyzed the implications of vote-buying, though focusing primarily on the capture of legislators or committee members rather than the widespread use of monetary incentives for ordinary voters (Bo 2007, Dekel, Jackson & Wolinsky 2008, Groseclose & Snyder 1996, Snyder 1991). These papers generally contend that vote-buying can lead to inefficient outcomes, though in the model developed by Dekel, Jackson & Wolinsky (2008), efficient outcomes are also possible.[1]

Empirically, assembling large-scale empirical data about the prevalence of vote-buying is extremely challenging given that the practice is generally illegal and accordingly underreported. There is a large case-study literature based primarily on interviews or other qualitative data, usefully summarized in Schaffer & Schedler (2007). In addition to case studies, wide-scale survey data on vote-buying has been analyzed from a variety of countries, including Argentina, the Philippines, Sao Tome and Principe, and Nicaragua (Gonzalez-Ocantos, de Jonge, Melendez, Osorio & Nickerson 2012, Schaffer 2002, Vicente & Wantchekon 2009). However, this literature has largely been unable to identify mechanisms and often cannot distinguish between vote-buying and turnout-buying (Nichter 2008).

Our work is closely related to two recent empirical papers. Finan & Schechter (2012) argue that social preferences are a key determinant of vote-buying in Paraguay; they found evidence of greater reciprocity in economic games among those targeted for vote-buying. Our findings suggest that more reciprocal individuals are not only more frequently targeted for vote-buying, but are also more likely to alter their subsequent political behavior. Hicken, Leider, Ravanilla & Yang (2014) provide evidence that a campaign seeking to reduce voters' temptation to sell their votes significantly reduces vote-switching from the candidate ex ante preferred by the

[1] In broad terms, an efficient outcome is possible in this model if the parties place valuations on votes that aggregate the values placed by their supporters.

voter, suggestive again that candidate choice or adverse selection may be an important channel through which vote-buying affects subsequent political outcomes. Our paper builds on these results to show the relevance of social preferences in influencing political outcomes via a distinct and important channel, moral hazard.

Relative to the existing literature, this paper makes a number of contributions. It is the first paper to analyze how subjects respond to vote payments in a laboratory, employing data from both a developed and a developing country and comparing vote payments of varying frequency and with distinct framings. It is also the first paper to clearly identify a potential moral hazard cost to vote-buying.

The remainder of the paper proceeds as follows: Section 2 provides an overview of the model of retrospective voting employed. Section 3 outlines the experimental methods, and Section 4 summarizes the empirical analysis. Section 5 concludes.

2 Modeling retrospective voting

We use laboratory experiments in US and Kenya to examine how vote-buying influences retrospective voting, in a setting that abstracts from elections as a mechanism for politician selection. Below, we describe the basic game structure, and clarify how the nature of subject preferences influence outcomes without and with vote-buying.

2.1 Experimental structure

In our base voting game, five voters and one politician constitute the polity.[2] Each voter receives a country-specific endowment y and is informed that this endowment is taxed at a rate τ of 0.5, with tax revenue held in a collective treasury.

A parameter λ, set at 0.3, defines the fraction of the collective treasury that is available for expropriation by the politician. Hence 15% of each voter's endowment is available for expropriation. Non-expropriated tax revenue is redistributed to voters at the conclusion of the game. Voters' final earnings includes the untaxed portion of their endowment, and whatever revenue is redistributed to them from the common treasury.

Turning to the incumbent politician, subjects are informed that s/he receives a salary y in period 1, but forfeits half this salary ($0.5y$) if s/he fails to win reelection.[3] The intention in framing the second period earnings as a potential loss is to increase the salience of lower payout if not reelected. In addition, politicians receive a small additional bonus $\epsilon \in [0, 0.1y]$ if reelected. A final parameter $\kappa = 0.1y$ defines a transition fee paid by voters if the politician is

[2]Polity size involves a trade-off between budget and time required (since a fixed polity size required a predetermined number of participants in each experimental session), and the objective of realistically simulating an election environment in which a voter has limited information about other voters' decisions and perceives her probability of directly affecting the election outcome as low. Lab experiments analyzing questions such as voter turnout and strategic voting behavior has considered polities ranging from three to 20 voters (Palfrey 2009).

[3]Hence, s/he receives a salary of $0.5y$ in the first period, and an additional $0.5y$ if reelected. The monetary gain from reelection is intended to serve as a proxy for the potential lifetime rents associated with re-election across an infinite number of future periods in a standard moral hazard model.

not reelected. By rendering the failure to re-elect the politician explicitly costly, it discourages subjects from anchoring their threshold of expropriation at zero.

The timing of the game entails simultaneous choices by all subjects. Subjects are not assigned the role of politician or voter ex ante. Instead, they make the following two subjects.

- Each subject specifies as a voter, the maximum amount s/he would allow the politician to expropriate and still reelect him/her.
- Each subject specifies the amount s/he would expropriate as the politician.

If the amount the politician chose to expropriate is less than or equal to the median of the voters' reelection thresholds, s/he is reelected. If the amount the politician chose to expropriate is greater than this median, s/he is not reelected.

Multiple game features minimize the probability that subjects perceive any dimension of politician quality – other than the choice of how much s/he expropriates – as relevant to their voting decision. First, the experimental design provides the politician only a single choice of how much to expropriate. There is no other dimension of quality or ability through which the incumbent's decisions can affect voter earnings. Second, since the game ends immediately after the reelection decision, the hypothetical alternate candidate is afforded no identity or action.

The outcome of this game is sensitive to the nature of subject preferences. A subject may seek to maximize his/her own earnings E_i, and s/he may also have other-regarding preferences where she seeks to maximize the joint earnings realized by all subjects, or minimize inequality in earnings among subjects. Subjects may have preferences over the earnings of the politician E^{pol}. We will employ the following simple stylized representation of the subjects' objective function.

$$\max \eta_1 E_i + \eta_2 \sum_{i=1}^{N} E_i + \eta_3(-\Delta^{pol} E_i) + \eta_4 E_i^{pol} \tag{1}$$

Individual earnings, joint earnings, the inverse of earnings inequality and the politician's earnings are weighted η_1, η_2 η_3, and η_4 respectively. $\Delta^{pol} E_i$ denotes the difference between politician earnings and average voter earnings.[4]

If all subjects seek simply to maximize their earnings from the game (i.e., $\eta_1 = 1$ and $\eta_2 = \eta_3 = \eta_4 = 0$), they can do so by coordinating on a certain threshold for expropriation (as voters) and specifying a choice as politicians that is less than or equal to this threshold. This generates expected earnings that are identical for all subjects. (However, realized earnings are always higher for the subject who is assigned to be the politician, unless the politician chooses to expropriate zero.)

If subjects have social preferences, then it is no longer possible to predict a single equilibrium outcome. Rather, our focus is on comparative statics. Subjects with higher η_2 (i.e., who place more weight on maximizing total subject group earnings) will set higher thresholds for reelection as voters. At the extreme, they could allow the politician to expropriate an unlimited amount

[4]In the game without vote payments, all voters have identical earnings. This will not be the case when payments are introduced.

in order to ensure that s/he is reelected and thus earns the $10 reelection bonus from the experimenter. As politicians, they should expropriate a lower amount.

Subjects with higher η_3 (i.e., who are more averse to inequity in earnings) will set lower thresholds as voters, and expropriate less as politicians. Subjects with higher η_4 (who place more weight on the politician's earnings) will set higher thresholds as voters and expropriate more as politicians. In Section 3.3, we present evidence that is consistent with subjects placing non-zero weights on their own earnings, total subject group earnings, and inequality in earnings; the positive weight placed on politician earnings will become more relevant when vote-buying is introduced.

2.2 Introducing vote-buying

Our primary objective is to analyze how vote-buying alters subjects' behavior in a retrospective voting game. In our experimental sessions, each subject first played the basic retrospective voting game described above, and then played the voting game in which vote-buying is introduced. In the U.S., subjects generally played the voting game with payments twice, and the number of payments distributed varied in each game round. In Kenya, subjects played the voting game with payments only once in each experimental session.

The voting game including vote payments is structured as follows. First, voters are informed that vote payments will now be introduced into the voting game; all other parameters are maintained as before. The payment size is fixed at $.1y$ or 10% of the voters' endowment ($2 in the U.S.), and the choice of who receives the payment is made by experimenters, not politicians. The secret ballot is maintained, and vote payments are funded separately (i.e., not drawn from voters' income or tax revenue). In the simplest version of the voting game with payments, all of this information is shared with subjects.

We implement four different framings of the vote payment game, and two substantive variations. We also varied the number of payments distributed in the voting game: the number of payments could be one, four or five. The full set of games implemented can be described as follows.[5]

A *Public payment*: All subjects are informed that vote payments have been introduced, and are informed that "the payment is in exchange for your vote". They are informed about the number of payments and the size of the payment. This framing was implemented with one, four and five payments distributed.

B *Public gifts* All subjects are informed that payments have been introduced, but the payments are uniformly described as a gift to the subjects. Again, subjects are informed about the number of payments and the size of the payment. This framing was implemented with one and four payments distributed.

C *Limited information: prior consent* No information is provided about the number, size or nature of payments; the game description only states that some voters may receive

[5]Some combinations of framing and payment structures were omitted due to resource constraints.

payments in exchange for their votes. The size of the vote payment is maintained at $2. Prior to making any choices about their reelection thresholds as voters, subjects are asked whether they would like to accept a payment, if offered. This framing was implemented only with four payments distributed.

D *Limited information: posterior consent* The information and payment structure is identical to the previous framing. However, subject consent is not elicited prior to the subject's choice. After the subject specifies his/her reelection threshold, s/he is asked whether s/he would have chosen to accept the payment. This framing was also implemented only with four payments distributed.

E *Big pot*: The fraction of the treasury vulnerable to expropriation by the politician (λ) is increased from .3 to .5. All other features of the game are identical. This framing was implemented only with five payments distributed.

F *Unequal endowments*: Subject endowments are rendered unequal ex ante to mimic the distribution of wealth induced by the vote payments; i.e., rather than all subjects as voters having an endowment of $20, some have an endowment of $22 (parallel to their total endowment if they had received a $2 vote payment). The standard voting game is then played, without any reference to vote payments. This game will be employed in the robustness checks.

Our main objective is to examine how vote-payment changes subject behavior, primarily employing within-subject variation. The key comparative statics of interest can be formulated as follows.

1. If subjects seek only to maximize individual returns, introducing vote payments will not affect voters' specified thresholds.

2. If subjects are reciprocal, then introducing vote payments will increase the weight that subjects who receive payments place on the politician's payout (η_4 increases), and voters will increase their reelection thresholds.

3. If subjects are averse to inequality (high η_3) while also placing some non-zero weight on the politician's earnings or total earnings, then ceteris paribus they will increase their threshold for reelection when vote payments are introduced. (When the average difference between the politician's and voters' earnings is reduced following the introduction of payments, voters can allow politicians to expropriate more without sacrificing their preference for equity.) In addition, subjects who are averse to inequality will be more responsive to payments when more payments are distributed.

3 Experimental methods

3.1 Laboratory procedures

Between 2013 and 2015, we conducted experiments at the Harvard Decision Science Lab in Cambridge, MA in the U.S. and the Busara Experimental Laboratory in Nairobi, Kenya. In the U.S., 450 subjects participated in 62 sessions, each session consisted of 6 or 12 subjects (usually 6), and each subject participated in only one session. In Kenya, 366 subjects participated in 24 sessions, each session consisted of 12 or 18 subjects, and each subject participated in only one session. In both settings, subjects were recruited through the lab's centralized database.

Upon arriving at the lab, a subject was seated at separate computer terminal. During the session s/he first participated in a set of social preference games and then in multiple iterations of the voting game; each iteration was denoted a game round. A given experimental session includes two to three game rounds. The game rounds were independent of each other: there is no relationship between a decision a subject makes in one game round and another game round. Each subject can make new decisions in each new game round, but they cannot revisit decisions in a previous game round.

As described in the previous section, we had six primary session types. There were also small variations in how each session type was implemented in the U.S. and Kenya. (We denote these country-specific session types by numbers.)[6] Below we describe the typical session structure.

First, all subjects played a set of social preference games, including the dictator, trust and ultimatum games. Appendix A describes the protocols for these games.[7]

Second, instructions on the "retrospective voting game without payment" were provided, and the subjects completed a quiz about the instructions to ensure comprehension. Subjects privately received immediate feedback about whether or not they answered the questions correctly. The instructions emphasized that subjects would make choices as both the voter and the politician. No payments were discussed.

Third, subjects played the "retrospective voting game without payment" as described in Section 2.1. First, s/he was assigned the role of a voter and was asked to specify his/her reelection threshold, and his/her expectation regarding how much the politician would expropriate. Next, s/he was assigned the role of politician and asked to specify how much s/he would expropriate and whether s/he expected to be re-elected.

Next, in some rounds, instructions on the "retrospective voting game with payment" were provided, followed by a second comprehension quiz. The two key points highlighted in the instructions were first, that money for vote payments was provided separate from the subjects' endowments and collective treasury and second, that the vote remained secret and anonymous. In the limited information rounds, this introduction was not provided.

[6] For example, session type A corresponds to public payments; session type A1 was implemented in the U.S., and session type A2 in Kenya. As noted above, in the U.S. subjects participated in two game rounds of the voting game with payment, including varying numbers of payments, and this did not occur in Kenya. This is the difference between session types A1 and A2, B1 and B2, etc.

[7] The ultimatum games were introduced only in the 2014 sessions.

Finally, the subject played one or two game rounds for the "retrospective voting game with payment". In order to maximize power, we did not identify ex ante a subset of the subjects in a given session as those who receive vote payments. Rather, we asked each subject to specify his/her threshold for reelection in the case in which s/he does not receive a payment, and also to specify his/her threshold for reelection in the case in which s/he does receive a payment.[8] In addition, each subject specified how much s/he would expropriate as the politician. As before, subjects made choices simultaneously, without revelation of who would be chosen to be the politician. At the conclusion of the experimental session, the subject completed a brief questionnaire on demographic characteristics and political history.

Subjects were compensated on the basis of their choices during the experimental session. To clarify the compensation structure, consider a session type with two game rounds: the voting game without payment, and the voting game with four payments.[9] Subjects complete the full experimental session without learning about the outcome of either voting game. They specify the choices they would make as both voters and politicians, and do not learn anything about other subjects' choices in any game round. At the conclusion of the experimental session, subjects are provided with the following information.

- Which game round was chosen as the basis of payment: either the voting game without payment, or the voting game with payment.
- What role they were chosen to play: the politician, or a voter who did or did not receive a payment, as applicable. (In experimental sessions with more than six subjects, the experimenter constitutes the subjects into polities of six prior to randomly assigning game roles.)
- What the outcome of the game was: how much the politician expropriated, what the subjects' thresholds for reelection were, and thus whether or not the politician was reelected. (For subjects selected to receive payments, the thresholds they specified conditional on payment are employed in the determination of whether the politician is reelected. For subjects not selected to receive a payment, the thresholds they specified unconditional on payment are employed.)
- Based on the outcome of the game, subjects are informed of their individual earnings, and receive payment.

Thus subjects are not compensated on the basis of their choices in all game rounds, but rather on the basis of their choices in one, randomly selected, game round. In addition, they are compensated based on their choices in one, randomly selected role (politician or voter, and voter who did or did not receive a payment). Subjects are regularly reminded during the experimental session that any choice they make could affect their final earnings.

[8] In some sessions, the order in which these questions posed varied. This variation will be discussed in more detail in Section 4.1. There was never a case in which the question order varied across different game rounds for the same subject in the same session. A single subject will face only one ordering of the payment questions.

[9] The information provided here was also provided to all subjects at the beginning of the experimental session.

To ensure that players face uniform choices, the relative ratios of key game parameters were fixed across experimental sessions in the U.S. and Kenya. The voter endowment, y, was set at \$20 dollars in the U.S. and 500 shillings (approximately \$5.80) in Kenya. Again, vote payments were set to be equal to $0.1y$, or \$2 relative to a voter endowment of \$20 in the U.S. Appendix B provides more detail about the numerical parameters in the U.S. and Kenya, and the minor differences in session structure across settings.

The six session types were outlined in the previous subsection. Columns (1) and (2) of Panel A in Appendix Table A1 summarizes the session types implemented, and specify the game rounds included in each session. Since a given session can have up to three game rounds, we denote these rounds as "game round I", "game round II", and "game round III". Importantly, there was no within-session variation in framing. Any variation in the framing of the payment is across sessions, and thus across subjects. However, given that subjects in the U.S. (though not in Kenya) played the voting game with payments multiple times with different numbers of payments, there is both within-session (within-subject) and cross-session (cross-subject) variation in the number of payments distributed.

Minimizing subject learning Models of retrospective accountability usually include sequential decision-making with information revelation: i.e., the voters set a reelection threshold and this information is revealed publicly. The politician then chooses his/her level of expropriation. Clearly, the roles of both voters and politicians are fixed. In our experiment, by contrast, decisions are simultaneous, subjects' roles are unspecified ex ante, and no information is revealed until the end of the session.

The principal reason for these features of the experimental design is to eliminate any opportunity for learning during the experimental session, as subjects move from one game round to the next and vote payments are added (or their number is altered). Publicizing voters' reelection threshold to the subjects (or even only to the politician) would allow for subject learning, regardless of whether politician identity is fixed. If the politician were chosen at random in each new voting game, then some subjects would have prior information about voters' choices in the last game round. If the identity of the politician were fixed, then repeated iterations of the game would create the opportunity for learning by the politician about the voters' choices, and other subjects would be aware that the politician could re-optimize his choice of how much to expropriate in subsequent rounds. In either case, the dynamic effects of multiple rounds would confound any effect of variations in game design from one game round to the next.

For this reason, we opt for simultaneous choices by voters and politicians with no revelation of information. In order to maximize power, we also allow subjects to specify their choices as both voters and politicians. While these choices have costs - they render our game less comparable to the literature, and also allow for multiple equilibria even if subjects are purely returns-maximizing - they also allow us to clearly estimate the effects of payments using a within-subject design without any confounding effects of subject learning.

3.2 Data and descriptive statistics

3.2.1 Subjects

We conducted 84 experimental sessions including 816 subjects – 366 in Kenya, and 450 in the U.S.[10] Table A2 summarises the structure of the sample including the number of sessions, subjects, and game rounds observed for each session type. The experimenters determined the schedule of the various session types in advance. Subjects signed up for sessions on a first-come, first-served basis, and did not receive any advance information about scheduled session type. In all, 1980 subject-game rounds were observed, where the number of game rounds per subject during an experimental session varied between two and three.

Our data, therefore, has a four-level hierarchy: session, subject, subject-game round and subject-decision. The unit of interest is the subject-decision, focusing on the choices made by subjects as voters. In game rounds in which zero or five payments are distributed, the subject makes only a single decision; s/he specifies the threshold of expropriation above which s/he would fail to reelect the politician. However, in game rounds in which one or four payments are distributed, the subject makes two decisions: what threshold s/he would select if s/he receives a payment, and what threshold s/he would select if s/he does not receive a payment. 2862 subject-decisions were observed in total.[11]

Panel A of Table 1 presents some summary statistics.[12] The age of subjects is similar in the two experimental sites, around 33. In the U.S., the subject pool was equally divided by gender, overwhelmingly unmarried, and generally highly educated. In Kenya, the subject pool was majority (65%) female, and had an average of eleven years of education; half were married.

Levels of political engagement are relatively high in both subject pools, though higher in Kenya: 72% of U.S. subjects reported voting in the last presidential election, while 86% of Kenyan subjects reported voting in the last presidential election. Around half of subjects at both sites reported attempting to persuade another person to support a particular candidate in the last election, while only a fifth of U.S. subjects reported attending an event in support of a candidate compared to more than half of Kenyan subjects. Around 15% of subjects in both countries reported recently attending a protest or rally.

3.2.2 Summary statistics: re-election threshold and social preferences

Our primary dependent variable of interest is the voters' re-election threshold, but this threshold was elicited in slightly different ways in the two experimental sites. In the U.S., subjects were

[10] 30 sessions were conducted in 2013, 41 in 2014 and 15 in 2015. 366 subjects were included in experiments in 2013 (180 in the U.S., and 186 in Kenya); 360 subjects were included in experiments in 2014 (180 in the U.S., and 180 in Kenya); and 90 subjects were included in experiments in 2015, all in the U.S.

[11] There were several aberrations in conducting experimental sessions. In 2013, a session of type A1 in the U.S. was conducted inadvertently omitting the game round with no payments. In 2014, a session of type C1 in the U.S. was conducted inadvertently omitting the game round with four payments. Also in 2014, four sessions including 24 subjects of type F1 in the U.S. were conducted substituting the "big pot" all payment game for the simple all payment game for game round II. These game rounds were dropped.

[12] Demographic data is missing for one Kenyan subject, and a small number of subjects in the U.S. did not complete questions about their political history due to software errors in the questionnaire administration.

asked to specify the maximum amount they would allow the politician to expropriate and still reelect him/her, naming any integer between zero and $15, inclusive. In Kenya, we sought to increase voter comprehension by instead posing a series of binary choices. Subjects were asked whether they would reelect a politician who expropriated a specified amount, where the amounts were 0, 75, 150, 250, 300 and 375 Ksh. These responses are converted to a linear variable equal to the median of the maximum threshold at which they stated they would reelect the politician and the minimum threshold at which they stated they would not reelect the politician.[13]

We drop observations corresponding to 7% of subjects where we observed non-monotonic behaviour: subjects stated that they would not reelect a politician who would expropriate a lower amount, but would reelect a politician who would expropriate a higher amount. This leaves a sample of 755 subjects, and 2680 subject-decisions. (We show that our primary results are robust to the inclusion of subjects exhibiting non-monotonic behavior).

Panel B of Table 1 reports summary statistics for these 2680 observations.[14] The average voter reelection threshold in the pooled sample is the equivalent of $7.08, employing the dollar scale corresponding to the U.S. sessions.[15] Around 15% of subject decisions include a reelection threshold of zero expropriation. The average level of expropriation by the politician is nearly $8, higher than the average voter threshold; given that each game round includes only one decision by subjects as politicians, the number of reported observations for politician choices is lower.

We also report summary statistics for simple measures of social preferences for 653 subjects; this includes all subjects other than those included in session type F, unequal endowments, for whom social preferences are not observed. On average, subjects send $2.80 out of a maximum of $10 in the dictator game, and 75% of subjects chose to send a positive amount in the trust game. The average threshold in the ultimatum game is $4, relative to a maximum of $10; this is similarly observed only for the 268 subjects who participated in sessions in 2014.

To construct Rec_i, an index of reciprocity, we calculate the percentage of the funds received that a subject would return to the sender in the trust game if s/he received more than 50% of the endowment (i.e., more than $2), denoted $Perc_i^{high}$, and the analogous percentage that the subject would return if s/he received less than 50%, denoted $Perc_i^{low}$. Rec_i is defined as $Perc_i^{high} - Perc_i^{low}$, censored at zero; this measure is parallel to that employed by Finan & Schechter (2012). Given the simpler trust game employed in the 2013 sessions and in Kenya, Rec_i can be constructed only for the 2014 U.S. sessions, and the mean index of reciprocity is .06. This is very close to the average level of the reciprocity index identified by Finan & Schechter (2012), .043.

[13] For example, if a subject stated he would reelect a politician who expropriated 75 shillings, but would not reelect a politician who expropriated 150 shillings, his maximum level of expropriation was set to be 112.5 shillings.

[14] For reporting the summary statistics, the choices made by subjects in the big pot games are re-scaled to lie on the same scale from 0 to $15.

[15] The observations of voter behavior from Kenya are converted to this scale.

3.3 Voter behavior

We first present evidence that subjects' choices as voters are broadly consistent with the model of preferences laid out in Section 2.1. The dependent variable of interest is T_{idgs}, subject i's re-election threshold making game decision d in game round g in session s. The sample is restricted to the game rounds that have no vote payments (i.e., the basic retrospective voting game), as we are interested in understanding subjects' voting behavior in the simple underlying game of political accountability. All specifications include session fixed effects, and standard errors are clustered at the session level.

First, we test whether subjects' reelection threshold is increasing in their belief of how much the politician will expropriate. Conditional on the politician's choice of how much to expropriate, subjects' earnings are always higher if the politician is reelected (i.e., if the subjects' threshold is less than or equal to the amount the politician chooses to expropriate). Thus if subjects believe the politician will expropriate more, they should optimally increase their threshold for reelection.

Second, we examine whether subjects' reallocation threshold is correlated with their level of altruism, trust and inequality aversion. Altruism, denoted Alt_i is proxied by how much the subject sends in the dictator game. Trust, denoted $Trust_i$, is proxied by a dummy variable equal to one if the subject sends any funds in the trust game. Inequality aversion, denoted $Ineq_i$, is equal to the minimum amount out of \$10 the subject reports a willingness to accept in the ultimatum game.

Based on our model of subject preferences, we assume that subjects who are more altruistic and more trusting are characterized by higher η_2, placing greater weight on the total earnings of the subject group. These subjects should set higher thresholds, minimizing the risk of the lost reelection bonus for the politician if the voters fail to reelect. Subjects who are averse to inequality, characterized by higher η_3, should set lower reelection thresholds.

The specifications of interest can thus be written as follows, where ν_s denotes game session fixed effects.

$$T_{idgs} = \beta_1 Belief_i + \nu_s + \epsilon_{idgs} \quad (2)$$

$$T_{idgs} = \beta_1 Alt_i + \nu_s + \epsilon_{idgs} \quad (3)$$

$$T_{idgs} = \beta_1 Trust_i + \nu_s + \epsilon_{idgs} \quad (4)$$

$$T_{idgs} = \beta_1 Ineq_i + \nu_s + \epsilon_{idgs} \quad (5)$$

Note again that the sample is restricted to the no-payment game. Thus cross-subject heterogeneity is equivalent to cross-decision heterogeneity: each subject is observed making only a single decision in a single round. 641 subjects are included in this analysis, relative to the total subject population of 755; 102 subjects are excluded because they participated in session type F (unequal endowments) in which games designed to elicit social preferences were not administered, and 12 subjects are excluded because the no-payment game was unintentionally dropped

in their experimental session.[16] For subjects participating in "big pot" sessions (session type F), the threshold for reelection was re-scaled to fall on the same scale of zero to 15 employed in the other sessions.

The results are reported in Table 2. We observe in the first column that subjects who believe the politician will expropriate more do increase their reelection threshold.[17] In the second and third columns, we observe significantly higher thresholds reported by more altruistic and more trusting subjects. In the fourth column, the relationship between inequality aversion and the reelection threshold is noisily estimated, but of the expected negative sign; inequality aversion is observed only in the 2014 games, as the ultimatum game was not included in social games in the other years. These results suggest that our model of voter preferences is approximately accurate in the sense that subjects are both concerned with their own earnings and have social preferences vis-a-vis the earnings of others.[18]

4 Empirical results

4.1 Voter response to payments

Our primary interest in this analysis is in analyzing how subjects' reelection thresholds change when vote payments are introduced. The main specifications of interest can be written as follows.

$$T_{idgs} = \beta_1 P_{gs} + \beta_2 R_{idgs} + \epsilon_{idgs} \quad (6)$$

$$T_{idgs} = \beta_1 P^1_{gs} + \beta_2 P^4_{gs} + \beta_3 P^1_{gs} \times R_{idgs} + \beta_4 P^4_{gs} \times R_{idgs} + \phi_i + \epsilon_{idgs} \quad (7)$$

P_{gs} is a dummy variable equal to one if game round g in session s included the distribution of payments to voters (P^1_{gs} and P^4_{gs} denote game rounds in which one and four payments were distributed, respectively). R_{idgs} is a dummy variable equal to one if subject i's decision in game round g in session s was conditional on the receipt of a payment.

All specifications are estimated with and without subject fixed effects ϕ_i, and standard errors are clustered at the subject level.[19] The sample for this analysis includes all session types except "big pot" and "unequal endowments" (i.e., session types A, B, C and D as specified in Table A1 are included). The resulting sample includes 2136 subject-decisions.

Table 3 reports the results of estimating equations (6) and (7); Columns (1) and (3) report

[16] The subsample of 641 subjects is comprised of 281 subjects in Kenya, and 360 in the United States.

[17] In the U.S., subjects do not directly report the amount they believe the politician will expropriate; rather, they report whether they believe the politician will expropriate more or less than their specified threshold. We set the variable for belief about politician expropriation to be equal to the median of 0 and the threshold if the subject believes the politician will expropriate less than the threshold, or the median of the threshold and 15 if the subject believes the politician will expropriate more.

[18] It is not obvious whether there should be any correlation between reciprocity and the subject's reelection threshold in the simple voting game, and indeed no such correlation is observed. We will further explore the relationship between reciprocity and responsiveness to vote-buying in Section 4.3.

[19] When subject fixed effects are excluded, the specifications include additional control variables for whether the session is conducted in Kenya and the order in which questions about the receipt of vote payments is posed.

results without subject fixed effects, and Columns (2) and (4) report results including subject fixed effects. It is evident in Columns (1) and (2) that subjects playing a voting game that includes payments who do not receive a payment lower their reelection threshold significantly (i.e., they are harsher in their treatment of the politician). We will deem this a "backlash effect". By contrast, the estimated coefficient β_2 is significant and positive, suggesting that subjects who receive a payment increase their reelection threshold relative to subjects who do not. In Columns (3) and (4), a further decomposition of the recipient interaction effect suggests that recipients are responsive to payments only if four voters (a majority) receive them.

Note that given we are employing the strategy method, we observe decisions for all subjects both in the case when they receive a payment, and in the case when they don't receive a payment. Accordingly, we can compare the choice by the same subject in the hypothetical scenario in which s/he receives a payment, and the hypothetical scenario in which s/he does not receive a payment. Nonetheless, we will employ the simpler language referring to subjects who do and do not receive vote payments for ease of comprehension. The sign and significance of coefficients β_1 and β_2 are consistent irrespective of the order in which the "reelection threshold - payment" and the "reelection threshold - no payment" questions are posed.[20]

The bottom row of the table reports the sum of β_1 and β_2 as estimated in equation (6) for Columns (1) and (2), and it is positive, though significant only when estimated without subject fixed effects. This suggests that subjects who receive a payment set a reelection threshold that is higher relative to the threshold in the no-payment game: they are, on average, more lenient in their treatment of the politician, and the magnitude suggests a relative increase in the reelection threshold of between 4% and 6%. Subsequent analysis will demonstrate this coefficient masks considerable heterogeneity.

In Columns (5) and (6), we evaluate the relative impact of payments when they are distributed to all voters in a polity. The dummy variable All_{gs} is defined to be equal to one if subject i was participating in a game round in which five payments were distributed, and the following specification is estimated, again with and without subject fixed effects.[21]

$$T_{idgs} = \beta_1 P_{gs} + \beta_2 R_{idgs} + \beta_3 All_{gs} + \phi_i + \epsilon_{idgs} \tag{8}$$

The results suggest that there is a large increase in the subjects' reelection threshold when payments are distributed to all voters in a polity. The linear combination $\beta_1 + \beta_2 + \beta_3$ is again reported at the bottom of the table and is positive, significant and somewhat larger in magnitude, suggesting an increase in the voters' reelection threshold of around 10%.[22] Interpreting the magnitude another way, a vote payment of $2 leads to an increase in the amount the voters

[20]Separate tabulations are not reported for concision, but are available upon request.

[21]In the specification without subject fixed effects, we also include additional control variables for whether the session is conducted in Kenya and the order in which questions about the receipt of vote payments is posed.

[22]If we test the equality of the net effect of payment comparing the partial-payment case to the all-payment case - i.e., testing the equality of $\beta_1 + \beta_2$ as estimated in Columns (1) and (2) and $\beta_1 + \beta_2 + \beta_3$ as estimated in Columns (5) and (6) - the hypothesis that the two linear combinations are equal cannot be rejected when comparing the specifications without subject fixed effects, Columns (1) and (5). However, it can be rejected when comparing the specifications including subject fixed effects, Columns (2) and (6).

are willing to allow the politician to expropriate of around 30 to 45 cents in games in which some vote payments are distributed, and an increase of around 75 cents in games in which vote payments are distributed to all subjects. Thus we observe that the voter's responsiveness to payments is steadily increasing in the proportion of voters receiving payments.

Finally, in Columns (5) and (6), we re-estimate equation (6) adding interaction terms with the gift framing. The equation of interest can be written as follows, and is again estimated with and without subject fixed effects.[23]

$$T_{idgs} = \beta_1 P_{gs} + \beta_2 R_{idgs} + \beta_3 Gift_{gs} \times P_{gs} + \beta_4 Gift_{gs} \times R_{idgs} + \phi_i + \epsilon_{idgs} \quad (9)$$

The interaction terms are uniformly insignificant and of small magnitude, suggesting that there is no meaningful heterogeneity with respect to the framing of the payment and the specification of a quid pro quo. The linear combinations $\beta_1 + \beta_2$ and $\beta_1 + \beta_2 + \beta_4 + \beta_4$ are reported at the bottom of the table; all four coefficients are positive and of roughly comparable magnitude to the linear combinations estimated in Columns (1) and (2), albeit noisily estimated.

Table A3 in the Appendix reports the results from re-estimating all specifications reported in Table 3 employing only the high-comprehension sample.[24] The results are not significantly different, and thus there is no evidence that poor comprehension by game subjects is a significant source of noise in the results. Similarly, Table A4 reports the same specifications for the sample including subjects who exhibit non-monotonic behavior, and again the same patterns are observed, though the coefficients of interest are more noisily estimated.

Returning to our key predictions, the observed pattern of robust subject responsiveness to payments is inconsistent with a standard returns-maximizing model of voter behavior. The observed pattern is consistent with vote payments leading to an increase in η_4, or the weight placed on the politician's payout, for subjects who receive a payment, and a decrease in η_4 for subjects who do not receive a payment. The role of reciprocity in engendering this subject response will be explored in further detail in Section 4.3.

In addition, the fact that responsiveness to payments increases in the number of payments distributed is consistent with subjects placing some weight on equity in earnings between subjects (i.e., a high η_3): the more payments are distributed, the more the gap between the average politician payout and the average voter payout is narrowed conditional on a certain level of politician expropriation. If subjects place some weight on the politician's payout (due to reciprocal motivations), and some weight on minimizing inequity, they will be more willing to increase their threshold and allow the politician to expropriate more when more subjects have received payments.[25] This hypothesis is also consistent with what we observe in the data: for

[23] In the specification without subject fixed effects, we also include a dummy variable for the gift framing, and additional control variables for whether the session is conducted in Kenya and the order in which questions about the receipt of vote payments is posed.

[24] In order to evaluate whether there are any systematic differences in game behavior comparing across individuals with varying comprehension of the game structure, the subjects' scores on all comprehension questions are compiled. A variable capturing low comprehension is defined to be equal to one if a subject is below the 10th percentile of comprehension compared to other subjects in the same experimental setting (U.S./Kenya).

[25] If subjects are solely interested in maximizing equity, however, they would maintain their reelection threshold

subjects who are more averse to inequality (as proxied by their choices in the ultimatum game), the difference between the net effect of a payment in the four payments game and the net effect in the all payments game is even larger when compared to this difference for a subject who is less averse to inequality.[26]

4.2 Punishing the experimenter

One possible interpretation of the results would be that the subjects' responses as voters reflect simply a reaction to receiving or not receiving the payment that is directed at the experimenter, but is externalized via decisions about reelecting the politician. For example, subjects who receive a payment may experience a warm glow and be more generous; subjects who do not receive a payment may be angry at the inequity they have experienced.[27]

In order to test this hypothesis, we also collected data employing a game denoted "unequal endowments". Unequal endowments is equivalent to the simple voting game without payments, except that the endowments of the voters are rendered unequal. Four voters have endowments of \$22 and one voter has an endowment of \$20, parallel to the endowments that are induced when vote payments are distributed in the four-payment game. All subjects are informed of this distribution of endowments, but there is no explanation of the reason for this discrepancy. We then evaluate whether individuals who receive a higher endowment respond in the same way as subjects who receive a transfer designated as a vote payment.

To do so, we estimate the following specification. R_{idgs}, the dummy for receiving a payment, is set equal to one for individuals of endowment 22 in the unequal endowment games and zero for the other individuals in the unequal endowment games; this is an "artificial" dummy variable. We also define the variable $Ineq_{gs}$ equal to one for the unequal endowment game rounds. The following specification is estimated for the full sample excluding big pot (i.e., session types A-D and F) and the sample of games with zero or four payments in which the base amount, \$15, is vulnerable to expropriation, and both with and without subject fixed effects.[28]

$$T_{idgs} = \beta_1 P_{gs} + \beta_2 R_{idgs} + \beta_3 Ineq_{gs} \times R_{idgs} + \phi_i + \epsilon_{idgs} \tag{10}$$

The results are reported in Table 4. We observe that the coefficient β_3 is consistently negative, suggesting that voters do not respond to an artificial "payment" structured as a higher initial endowment in the same way that they respond to a vote payment. The estimated

at zero.

[26] Tabulations are not reported for concision, but are available upon request.

[27] Note it is possible to rule out the hypothesis that subjects are simply seeking to reach some target level of earnings for the game session. In this case, we would expect to see no change in behavior between the voting game without any payments, and the decisions subjects make in a voting game in which payments are distributed but they do not receive a payment.

[28] This includes game round I (no payments) and any game round including four payments from session types A-D and F as specified in Table A1: A1-III, A2-II, B1-III, B2-II, C1-II, C2-II, D1-II, D2-II, F1-I, F2-I and F2-II. In the specifications without subject fixed effects, we also include the dummy variable for an unequal endowment game, the unequal endowment dummy interacted with a Kenya dummy, and additional control variables for whether the session is conducted in Kenya and the order in which questions about the receipt of vote payments is posed.

coefficient is significant with and without subject fixed effects and large in magnitude (around one half of the estimated coefficient on receive dummy, R_{idgs}).[29] This is consistent with our hypothesis that subjects' observed behavior does not simply reflect a perceived equivalence between the politician and the experimenter.

4.3 Voter consent

The previously reported evidence suggests that subjects are in general responsive to vote payments, that they are responsive to the presence of vote payments both when they do and do not receive a payment, and that this response is robust to alternate framings of the payment. In this section, we seek to further explore the channels through which this response is elicited. In particular, we focus on two additional dimensions of variation in the game design: the provision of limited information about the payment, and the provision of consent.

The objective of re-framing the payment to be quasi-secret is to test whether a transaction that is more private elicits a greater response by subjects who now feel the transfer is targeted specifically to them, and thus there is a greater obligation to respond and a greater increase in the weight the subject places on the politician's payout. Again, in the limited information games, the only information provided about payments is a simple statement in the game introduction noting that some voters may receive payments in exchange for their votes. No information is provided about the number of payments distributed, the targeting mechanism, their size, or their implications.

In the games in which only limited information was provided about payments, the sessions can be further divided. In half the sessions, voters were asked whether or not they would like to accept a vote payment, and only following this question were asked to specify their reelection threshold in case they received a payment. (Even subjects who stated they would not like to accept the payment were asked to specify this threshold.) We denote the sessions as "prior consent." In the other half of the sessions, voters were not asked to provide consent before specifying their reelection thresholds in the case of their payment. After they specified the threshold, they were asked to specify whether or not they would like to accept the payment. We denote these sessions as "posterior consent."

Note that consent is not explored in the public information games or in the games in which all subjects receive payments. That set of games is designed to analyze the response of subjects to payments that mimic vote payments that are generally distributed without regard to the identity of the recipient: for example, cash handed out at a political rally, or gifts provided to all individuals in a neighborhood. In these cases, explicit consent is unlikely. Here, we seek to re-frame payments in a context more analogous to the one in which vote payments are usually believe to be most effective: private payments that constitute an implicit contract, with some sort of verbal agreement, between the politician and the voter.

[29] Note that the sum of coefficients $\beta_2 + \beta_3$ is positive and significant if subject fixed effects are not employed, suggesting that individuals with larger endowments are somewhat more generous in their treatment of the politician compared to individuals with smaller endowments, and consistent with the "warm glow" effect. However, the effect of a vote payment is significantly larger.

We begin our analysis by re-estimating equation (6) adding interaction terms with the limited information framing; the same sample is employed, including all session types except unequal endowments and big pot (session types A through D). The equation of interest can be written as follows, and is again estimated with and without subject fixed effects.[30]

$$T_{idgs} = \beta_1 P_{gs} + \beta_2 R_{idgs} + \beta_3 Lim_{gs} \times P_{gs} + \beta_4 Lim_{gs} \times P_{gs} \times R_{idgs} + \phi_i + \epsilon_{idgs} \quad (11)$$

The results are reported in Columns (1) and (2) of Table 5, and show the interaction terms β_3 and β_4 are negative and of sizeable magnitude, though imprecisely estimated. While there is no robust evidence that limiting information significantly alters subjects' responsiveness to payments, there is suggestive evidence that, inconsistent with our hypothesis, it may render them somewhat less responsive. The linear combinations $\beta_1 + \beta_2 + \beta_4 + \beta_4$ are in this case negative and close to zero, though not reported in the table

Next, we seek to analyze the effect of prior consent. In this case, we estimate a model including interactions with both the limited information and prior consent variables.

$$\begin{aligned} T_{idgs} = {} & \beta_1 P_{gs} + \beta_2 R_{idgs} + \beta_3 Lim_{gs} \times P_{gs} + \beta_4 Lim_{gs} \times P_{gs} \times R_{idgs} \\ & + \beta_5 Cons_{gs} \times P_{gs} + \beta_6 Cons_{gs} \times R_{idgs} + \phi_i + \epsilon_{idgs} \end{aligned} \quad (12)$$

The results are reported in Columns (3) and (4) of Table 5.

We can observe that β_5 and β_6 are both generally positive but noisily estimated, suggesting that the backlash effect is smaller and the positive effect of a payment is larger when prior consent is solicited. The final row of the table reports the linear combination $\beta_5 + \beta_6$; this captures whether the net effect of a payment is different when prior consent is solicited. $\beta_5 + \beta_6$ is positive, significant and generally of substantial magnitude. This suggests that the net effect of a payment is significantly larger when consent is solicited, and the difference is equal to around 15% of the average voter threshold; this result is also consistent in sign and magnitude when the sample is restricted to the sample of limited information sessions. Requesting the voter's consent prior to distributing a payment seems to significantly increase the weight that the subject places on the politician's payout, and thus leads to an increase in the reelection threshold.

Receiving consent from voters In addition to the variation induced by the request for consent, we also have data on whether or not the subjects provided consent for the payment. Around 80% of subjects indicated they would consent to receive a vote payment, offering relatively little variation.

Nonetheless, it is informative to examine the heterogeneity with respect to the provision of consent, rather than merely the request for consent. Accordingly, we estimate a different specification, restricted to the sample of games in which prior consent is requested, interacting the

[30] In the specifications without subject fixed effects, a dummy for the limited information framing and additional control variables for whether the session is conducted in Kenya and the order in which questions about the receipt of vote payments is posed are included.

recipient dummy variable with a dummy variable equal to one if consent is provided, $Prov_{idgs}$. (This is session types C1 and C2 as specified in Table A1.) The specification of interest can thus be written as follows.

$$T_{idgs} = \beta_1 P_{gs} + \beta_2 R_{idgs} + \beta_3 R_{idgs} \times Prov_{idgs} + \phi_i + \epsilon_{idgs} \tag{13}$$

Note that the provision of consent here is endogenous and presumably correlated with subject characteristics. Accordingly, the results of this specification can only be interpreted as evidence of a correlation between the provision of consent and a greater subject response to the payment.

The results are reported in Columns (5) and (6) of Table 5. We observe that β_3 is positive and large in magnitude, but not statistically significant at conventional levels; this is perhaps unsurprising, given the small sample and the limited variation in the provision of consent. However, this evidence is consistent with the hypothesis that the request for (and provision of) consent is correlated with greater voter responsiveness to the payment.

Variation with respect to social preferences The evidence that subjects' response to a vote payment is greater when their consent is solicited suggests that social preferences such as reciprocity may be relevant to their response, consistent with the previous evidence in Finan & Schechter (2012). As already noted, subjects in all sessions first participate in the trust game. Subjects are provided with an endowment of $4 (in the U.S. sessions) and offered the opportunity to send between 0 and $4 to an anonymous partner; they also specify how much they would send back if they received $1, $2, $3, or $4 from their partners. (In the 2013 sessions and in the sessions in Kenya, subjects in the trust game were offered the choice only to send all or none of their endowment, i.e. $0 or $4, and reported how much they would send back if they received $4; the index of reciprocity described below thus cannot be constructed for these subjects.)

To construct Rec_i, an index of reciprocity, we calculate the percentage of the funds received that a subject would return to the sender if s/he received more than 50% of the endowment (i.e., more than $2), denoted $Perc_i^{high}$, and the analogous percentage that the subject would return if s/he received less than 50%, denoted $Perc_i^{low}$. Rec_i is defined as $Perc_i^{high} - Perc_i^{low}$, censored at zero; this measure is parallel to that employed by Finan & Schechter (2012).

The specification of interest can be written as follows. The sample includes all subjects in session types C and D observed in the U.S. All specifications are estimated with and without subject fixed effects.

$$T_{idgs} = \beta_1 R_{idgs} + \beta_2 R_{idgs} \times Rec_i + \beta_3 Rec_i + \epsilon_{idgs} \tag{14}$$

The results are reported in Columns (7) and (8) of Table 5. In Columns (7) and (8), we observe that the interaction with reciprocity is also insignificant, though large in magnitude; the estimate in Column (8) suggests that a one standard deviation in measured reciprocity increases the voter's responsiveness to a vote payment by about .20, or a 25% increase relative to the

primary effect.[31] This is again consistent with the evidence that requesting, and providing, consent leads to an increase in voter responsiveness to the payments.

4.4 "Big pot" games

In some game sessions, the amount vulnerable to expropriation was increased was increased from 35% of the common treasury to 50% of the treasury. The voting game was then played without any payments and with five payments distributed, with the size of the vote payment remaining fixed at $2. The objective is to test the hypothesis that voters' responsiveness to payments diminishes when they stand to lose more from the politician's expropriation.

Accordingly, the specification of interest can be written as follows, where Big_{gs} is a dummy variable equal to one if the game round was one in which the amount vulnerable to expropriation was increased.[32] The sample included all sessions of types A through D and F, as well as the all payment games from session types E.

$$T_{idgs} = \beta_1 P_{gs} + \beta_2 R_{igds} + \beta_3 All_{gs} + \beta_4 All_{gs} \times Big_{gs} + \phi_i + \epsilon_{idgs} \tag{15}$$

We also restrict the sample to only no payment games and five payment games from sessions in which the five payment game was played. More specifically, this is game rounds CI-I, C1-III, D1-I, D1-III, session type E (all rounds), F2-II and F2-III as specified in Table A1. In this case, we estimate the following simpler specification.[33]

$$T_{idgs} = \beta_1 All_{gs} + \beta_2 All_{gs} \times Big_{gs} + \phi_i + \epsilon_{idgs} \tag{16}$$

The results are reported in Table 6, and show that the coefficient β_2 is negative though statistically insignificant. The linear combinations $\beta_1 + \beta_2 + \beta_3 + \beta_4$ and $\beta_1 + \beta_2$ are reported in the final row of the table, and indicates that there is no significant net effect of payments on voters' reelection thresholds in games in which the vulnerability of the common treasury is greater. While these results must be considered only suggestive given that the interaction effect of interest is noisily estimated, it suggests that vote-buying may become less effective if and when an increase in income increases voters' potential losses from politician expropriation. (This pattern would be consistent with both voter preferences that place a high weight on their own payoffs, and voter preferences that are highly averse to inequality.)

[31] We also estimate comparable specifications testing whether there is evidence of heterogeneity with respect to subject altruism, trust or inequality aversion, employing data from the dictator and ultimatum games as well as the trust game. There is no evidence of meaningful heterogeneity along these dimensions. The tabulations are not reported for concision, but are available upon request.

[32] When the specification is estimated without subject fixed effects, a dummy for the big pot session type and additional control variables for whether the session is conducted in Kenya and the order in which questions about the receipt of vote payments is posed are included.

[33] Again, when the specification is estimated without subject fixed effects, a dummy for the big pot session type and additional control variables for whether the session is conducted in Kenya and the order in which questions about the receipt of vote payments is posed are included.

5 Conclusion

This paper analyzes evidence about voter behavior under a regime of vote-buying in the laboratory. A simple model of retrospective politician accountability was augmented by payments offered to voters and tested with 816 subjects in the Harvard Decision Science Laboratory and the Busara Experimental Laboratory in Nairobi, Kenya. The results suggest that there is a moral hazard cost to vote-buying, in that voters who receive payments in the laboratory are less willing to discipline an incumbent politician for expropriation of rents, in a context in which the selection motive of elections is minimized.

However, this cost is not uniform in all settings. More specifically, we observe the largest response by subjects as voters to vote payments when the payments are distributed to all subjects in the session, or when the payments are distributed in relative secrecy with the subject's prior consent. These findings are consistent with the prevalence of *both* secret targeted vote-buying with explicit consent and broad based gift-giving by political parties.

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6 Tables

Table 1: Summary statistics

Panel A: Demographic characteristics of subjects

	U.S. mean	U.S. obs.	Kenya mean	Kenya obs.
Age	33.46	444	32.53	365
Education	15.58	444	10.55	365
Gender	.50	444	.385	365
Marital status	.11	444	.45	365
Voted in last presidential election	.72	425	.86	366
Attempted to persuade others (in same election)	.46	424	.51	366
Attended event in support of candidate (in same election)	.20	426	.59	366
Joined a protest event in last year	.16	426	.17	366

Panel B: Subject choices in voting game

Variable	Mean	St. dev.	Observations
Voter threshold	7.08	4.55	2680
Dummy for threshold at zero	.13	.33	2680
Politician expropriation	7.85	5.05	1748
Dummy for zero expropriation	.14	.35	1748
Dummy for full expropriation	.20	.40	1748
Dollars sent in dictator game	2.74	2.46	653
Dummy for sending in trust game	.75	.43	653
Reciprocity	.06	.1	192
Threshold in ultimatum game	4.15	2.39	268

Table 2: Stylized facts about voter behavior

	Voter threshold			
	(1)	(2)	(3)	(4)
Belief expropriation	.129			
	(.046)***			
Altruism		.184		
		(.082)**		
Trust			1.057	
			(.424)**	
Inequality aversion				-.174
				(.133)
Obs.	641	641	641	268

Notes: The dependent variable is the maximum threshold of expropriation at which the subject will still reelect the politician. The independent variables are the subject's reported belief about expropriation, and measures of social preferences: the amount the subject sent in the dictator game (altruism), a dummy variable equal to one if the subject sent any money in the trust game (trust), and the minimum amount the subject would accept in the ultimatum game (inequality aversion). The sample is all subject decisions in the no-payment game in session types A through D and F; in Column (4) only subjects who participated in 2014 are included, given that the ultimatum game was not included in the other years. Asterisks indicate significance at the ten, five and one percent level.

Table 3: Voter behavior

	Voter reelection threshold							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Payment	-.582	-.670			-.726	-.679	-.734	-.687
	(.202)***	(.221)***			(.198)***	(.222)***	(.281)***	(.300)**
Payment 1			-.064	-.577				
			(.404)	(.294)**				
Payment 4			-.751	-.731				
			(.187)***	(.221)***				
Recipient	1.039	.964			.910	.910	.961	.922
	(.153)***	(.158)***			(.139)***	(.163)***	(.194)***	(.211)***
Recipient x 1			.278	.278				
			(.205)	(.241)				
Recipient x 4			1.273	1.168				
			(.171)***	(.180)***				
All payment					.566	.466		
					(.375)	(.244)*		
Payment x Gift							.019	.057
							(.394)	(.431)
Recipient x Gift							.082	.121
							(.280)	(.316)
$\beta_1 + \beta_2$	.457	.294					.227	.235
	(.193)**	(.199)				(.215)	(.233)	
$\beta_1 + \beta_2 + \beta_3$					.750	.697		
					(.331)**	(.262)***		
$\beta_1 + \beta_2 + \beta_3 + \beta_4$							.328	.414
							(.325)	(.374)
Mean dep. var.	7.07	7.07	7.07	7.07	7.07	7.07	7.07	7.07
Fixed effects		Subject		Subject		Subject		Subject
Obs.	2136	2136	2136	2136	2136	2136	2136	2136

Notes: The dependent variable is the maximum threshold of expropriation at which the subject will still reelect the politician. The independent variables are dummy variables for the game round including any, one or four vote payments, a dummy for the game round including a gift framing, the interaction of those four dummy variables with a dummy for the subject receiving a payment, and a dummy for the game round including payments distributed to all subjects. $\beta_1 + \beta_2$ reports the sum of the coefficients on payment and recipient. $\beta_1 + \beta_2 + \beta_3$ reports the sum of the coefficients on payment, recipient and all payment. $\beta_1 + \beta_2 + \beta_3 + \beta_4$ reports the sum of the coefficients on payment and recipient and the gift interactions. Fixed effects are as specified in the table; all specifications include standard errors clustered at the subject level. The sample included is session types A through D as labeled in Table A1. Asterisks indicate significance at the ten, five and one percent level.

Table 4: Voter behavior with unequal endowments

	Voter reelection threshold			
	(1)	(2)	(3)	(4)
Receive payment	.966	.934	1.059	1.071
	(.142)***	(.156)***	(.162)***	(.201)***
Unequal endowment	.799	.777	.947	
	(.599)	(.443)*	(.665)	
Unequal endowment x payment	-.448	-.505	-.534	-.609
	(.197)**	(.204)**	(.211)**	(.243)**
Mean dep. var.	7.177	7.177	7.086	7.086
Sample	Session types A-D, F		0 or 4 payments	
Fixed effects	None	Subject	None	Subject
Obs.	2490	2490	1902	1902

Notes: The dependent variable is the maximum threshold of expropriation at which the subject will still reelect the politician. The independent variables are a dummy variable for the subject receiving a payment or a higher endowment, a dummy for an unequal endowment game round, and the interaction between the two. In Columns (1) and (2), all data from session types A through D and F is employed; in Columns (3) and (4), the sample is restricted to game rounds including zero or four payments. Asterisks indicate significance at the ten, five and one percent level.

Table 5: Voter behavior and consent

	Voter reelection threshold							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Payment	-.604	-.570	-.611	-.541	-.794	-.794	-.287	-.323
	(.248)**	(.285)**	(.250)**	(.254)**	(.381)**	(.487)	(.279)	(.328)
Recipient	.956	.956	1.097	.974	.146	.918	.685	.703
	(.158)***	(.186)***	(.180)***	(.182)***	(.719)	(.885)	(.354)*	(.421)*
Payment x Limited info	-.203	-.280	-.300	-.421				
	(.386)	(.455)	(.520)	(.554)				
Recipient x Limited info	-.162	-.162	-.677	-.554				
	(.324)	(.381)	(.510)	(.580)				
Payment x Consent			.222	.147				
			(.605)	(.630)				
Recipient x Consent			.807	.807	1.437	.411		
			(.509)	(.598)	(.857)*	(.898)		
Recipient x Provided					1.437	.411		
					(.857)*	(.898)		
Recipient x Reciprocity							1.978	1.979
							(1.542)	(1.841)
$\beta_3 + \beta_4$			1.029	.955				
			(.415)**	(.372)**				
Mean dep. var.	7.07	7.07	7.07	7.07	7.48	7.48	7.69	7.69
Game sample	All excluding big pot, uneq. endowments				Prior consent		Session types C1, D1	
Fixed effects		Subject		Subject		Subject		Subject
Obs.	2136	2136	2136	2136	291	291	366	366

Notes: The dependent variable is the maximum threshold of expropriation at which the subject will still reelect the politician. The independent variables are dummy variables for the game round including any payments, payments with limited information, or payments with prior consent requested, and the interaction of these three dummy variables with a dummy for the subject receiving a payment; we also include an interaction between a dummy for the subject receiving a payment and a reciprocity index. $\beta_3 + \beta_4$ reports the sum of the coefficients on the interactions with the consent dummy. Fixed effects are as specified in the table; all specifications include standard errors clustered at the subject level. The sample in Columns (1) through (4) includes session types A through D; the sample in Columns (5) and (6) includes session type C; the sample in Columns (7) and (8) includes session types C1 and D1. The reciprocity index is constructed by calculating the percentage of the funds received that a subject would return to the sender if s/he received more than 50% of the endowment (i.e., more than \$2), denoted $Perc_i^{high}$, and the analogous percentage that the subject would return if s/he received less than 50%, denoted $Perc_i^{low}$. Rec_i is defined as $Perc_i^{high} - Perc_i^{low}$, censored at zero.

Table 6: Voter behavior and "big pot" games

	Voter reelection threshold			
	(1)	(2)	(3)	(4)
Payment	-.725	-.706	.133	
	(.204)***	(.224)***	(.485)	
Receive payment	.910	.910		
	(.139)***	(.167)***		
All payments	.546	.306	.648	.534
	(.349)	(.235)	(.272)**	(.303)*
Big pot x all payments	-.499	-.279	-.416	-.303
	(.424)	(.419)	(.433)	(.553)
$\beta_1 + \beta_2 + \beta_3 + \beta_4$	.232	.232		
	(.292)	(.353)		
$\beta_1 + \beta_2$			.232	.232
			(.292)	(.470)
Mean dep. var.	7.30	7.30	8.28	8.28
Fixed effects		Subject		Subject
Obs.	2452	2452	657	657

Notes: The dependent variable is the maximum threshold of expropriation at which the subject will still reelect the politician. The independent variables are dummy variables for the game including payments for all subjects and the interaction of the all payment dummy with a dummy equal to one if the game includes a "big pot" subject to expropriation. $\beta_1 + \beta_2$ reports the sum of the coefficients on all payments and the big pot interaction. Fixed effects included are as specified; all standards errors are clustered at the subject level. The sample in Columns (1) and (2) includes all game rounds except for the unequal endowment game rounds; the sample in Columns (3) and (4) includes zero payment and five payment game rounds from sessions in which the five payment game round was played. Asterisks indicate significance at the ten, five and one percent level.

A Structure of social preference games

In the first part of the experimental session, social games, subjects were told they possessed a hypothetical endowment in the dictator game E_d and allowed to freely choose how much to send to another, unidentified, subject. Next, they were provided with a (different) hypothetical endowment for the trust game, E_t, and they could choose whether to send E_t or zero to another, unidentified subject; they were advised that this amount would be tripled, and the recipient would then have the opportunity to choose how much to return to the sender. Subjects were allowed to specify their behavior as receivers and choose how much they would send back to a hypothetical sender. They were also asked to estimate how much, on average, subjects would send in both the dictator and trust games.

Finally, they were provided with a new hypothetical endowment in the ultimatum game, E_u, and could specify how much they would propose to send to a partner, and the minimum amount they would accept when sent by a partner.

B Comparing game sessions in the U.S. and Kenya

The relative ratios of key game parameters were maintained fixed across experimental sessions in the U.S. and Kenya to ensure that the choices faced by players were uniform. The voter endowment, y, was set to be $20 dollars in the U.S. and 500 shillings (approximately $5.80) in Kenya. Thus all parameters in the voting game in the U.S. can be multiplied by 25 to yield the corresponding parameter (in shillings) in Kenya.

Half of the endowment was taxed away, and 30% of tax revenue was vulnerable to expropriation. Accordingly, the common treasury was equal to 15% of the total endowment of the five voters, $15 in the U.S. and 350 shillings in Kenya. The politician salary was also $20 shillings or 500 shillings, of which half was forfeited if the politician was not reelected. Vote payments were set to be equal to 10% of the voters' endowment: $2 in the U.S. and 50 shillings in Kenya. The reelection bonus was set to be between 0 and 10% of the politician's salary, again $2 in the U.S. and 50 shillings in Kenya. Subjects were not informed of the distribution of the reelection bonus, but were simply informed that it was a positive amount between 0 and the specified upper limit.[34]

In determining subject earnings in Kenya relative to the U.S., the objective was threefold: first, to comply with the Busara lab's policies on minimum subject payments, which is around $3-$6 for a (maximum) four-hour experimental session, depending on the distance traveled by subjects; second, to ensure that incentives in the game (particularly the vote payment) were large enough to be salient to the player; and third, to maximize the subject pool relative to available funds. The subject pool at Busara is predominantly drawn from nearby informal settlements, particularly the Kibera slum, where 50 shillings is the price of a bag of maize flour or one-way transportation to the city center; 50 shillings is also the lowest available denomination of paper

[34] In both cases, the reelection bonus was chosen from a uniform distribution between 0 and the upper limit, rounded to the nearest $.25 in the U.S. and the nearest 10 shillings in Kenya.

money. Accordingly, this was viewed as an important psychological break point above which a payment would be regarded as significant, and all other game parameters were set relative to this minimum vote payment.

There were, however, some minor differences in the structure of the U.S. and Kenya sessions. These differences were largely dictated by the requirements of adopting a relatively complex game protocol to accommodate a population with more limited literacy and numeracy in Kenya. Here, any relevant differences in the game session are described in the order in which activities were conducted.

Social games The dictator endowment E_d was equal to \$10 in the U.S., the trust endowment E_t was equal to \$4, and the ultimatum game endowment was equal to \$10; in Kenya, the comparable magnitudes were 100 Ksh, 40 Ksh, and 120 Ksh. Thus while the relative endowments in the trust and dictator games are comparable across U.S. and Kenya, the ratio of the endowment in the social games to the voter's endowment in the subsequent voting game is lower in Kenya. This choice was made primarily to maximize the sample size given budget constraints, and is presumed to have limited relevance given that subjects have no information about the subsequent voting game at the time they make their choices in this stage of the protocol.[35]

In both the U.S. and Kenya, subjects were required to choose an amount that was an integer (in the U.S.) or divisible by 10 (in Kenya), i.e. the choice was not fully continuous. Subjects in the U.S. inputted their choice directly, while Kenyan subjects selected a button from an interactive touch screen.

In the U.S., subjects were not paid on the basis of their choices in social games and were aware of this fact. In Kenya subjects were paid on the basis of their choices in this game; they were randomly assigned to one of four or six roles (dictator sender, dictator receiver, trust sender or trust receiver in 2013, and dictator sender, dictator receiver, trust sender, trust receiver, ultimatum sender, or ultimatum receiver in 2014) and paid their earnings from that role. They also received 50 Ksh bonuses if they correctly estimated the average amount send in the dictator and trust games.

Introduction to the voting game Subjects in the U.S. and Kenya were not provided with identical introductory materials and comprehension questions. In the U.S., subjects began with an overview of the game described on screen in the experimental terminal. They answered simple questions about game structure, and were then asked to consider a number of game scenarios, identify whether or not the politician would be reelected in that scenario, and calculate the associated payoffs. After each set of comprehension questions, they were shown the correct responses and were required to remain on the associated screen for a minimum of sixty seconds. Subjects were also provided with a scripted oral explanation of the game and an explanatory graphic. While they were free to ask questions directly of the researchers, they were not required to interact with anyone else.

[35]The endowment in the ultimatum game is also slightly higher in Kenya.

In Kenya, information was provided primarily orally and graphically given the limited literacy of the population. While the same comprehension questions about basic game structure were employed, subjects were not asked to calculate a full set of payoffs given specific scenarios. The focus in comprehension questions was clarifying the structure of the payoffs, the available choices faced by both voters and politicians, and the use of a majority vote in determining reelection outcomes. Subjects were also asked to calculate how much would be redistributed to voters given various expropriation choices by the politician.

All comprehension questions were posed using multiple-choice touch screens. If a subject answered a question incorrectly, a supervising staff member was required to unlock the screen in order to allow the subject to make a new selection, and would use this opportunity to discuss the question and clarify any misconceptions. Ultimately, the number of incorrect choices made by the subject prior to the correct choice was recorded.

Voting game without payments The only difference between the voting game without payments as played by the subjects in the U.S. and Kenya was in the specification of the choice made by the voter and the politician. As described above, in the U.S., subjects could specify the maximum amount they would allow the politician to expropriate and still reelect him/her, naming any integer between 0 and $15, inclusive. Similarly, they could specify the amount that they would expropriate if acting as a politician.

In Kenya, subjects were asked to respond to a series of questions inquiring whether or not they would vote to reelect a politician who expropriated a specified amount, where the amounts were 0, 75, 150, 250, 300 and 375. Ksh. As politicians, they were allowed to choose how much to expropriate from the same set of choices. The reason for this alternate design, particularly for the voters' decision, was to increase comprehension by presenting the voters with a series of binary choices.

However, it also allowed subjects, intentionally or unintentionally, to exhibit behavior that was "non-monotonic", i.e. state that they would not reelect a politician who would expropriate a lower amount, but would reelect a politician who would expropriate a higher amount.

Introduction to voting games with payments There were no major differences in the overview material and comprehension questions provided here. The same information was delivered on-screen in the U.S. and orally and using graphics in Kenya. The structure of comprehension questions followed the model described above: in the U.S., subjects were required to review the correct answers independently, while in Kenya, subjects were required to interact with a laboratory staff member following any incorrect response.

Voting game with payments Here, subjects again made their choices as voters and as politicians by specifying an integer choice in the U.S. and responding to a series of questions about thresholds in Kenya.

Questionnaire The questionnaire was generally parallel in both countries, though slightly shorter in the Kenya sessions given the time required for experimental activities.

Payment Total time required for the game session was around 75-90 minutes in the U.S., and around 180 minutes in Kenya. In the U.S., subjects were paid in cash at the conclusion of the game; payments were distributed in envelopes to maintain confidentiality and ensure that subjects could not compare their payoffs. In Kenya, subjects were paid the show-up fee of 200 Ksh in cash as well as a bonus of 50 Ksh if they arrived on-time or early on the day of the experimental sessions. The full payoff from their choices in the experimental session was subsequently distributed (within 1-2 days) via the electronic money transfer system Mpesa.

C Additional results: politician behavior

Table A5 reports the results of estimating the following equations, where the dependent variable E_{ig} refers to how much of the common treasury subject i chose to expropriate as the politician in game round g. The independent variables include the dummy for a game including payment, the dummy variables for the game including one payment, four payments and all payments, and the interaction of P_{ig} with dummy variables for the gift framing, limited information framing, and the provision of consent. We estimate the specifications only with subject fixed effects for concision; standard errors are again clustered at the session level.

$$E_{ig} = \beta P_{ig} + \phi_i + \epsilon_{ig} \quad (17)$$

$$E_{ig} = \beta_1 P^1_{ig} + \beta_2 P^4_{ig} + \phi_i + \epsilon_{ig} \quad (18)$$

$$E_{ig} = \beta_1 P_{ig} + \beta_2 All_{ig} + \phi_i + \epsilon_{ig} \quad (19)$$

$$E_{ig} = \beta_1 P_{ig} + \beta_2 P_{ig} \times Gift_{ig} + \phi_i + \epsilon_{ig} \quad (20)$$

$$E_{ig} = \beta_1 P_{ig} + \beta_2 P_{ig} \times Lim_{ig} + \phi_i + \epsilon_{ig} \quad (21)$$

$$E_{ig} = \beta_1 P_{ig} + \beta_2 P_{ig} \times Lim_{ig} + \beta_3 P_{ig} \times Cons_{ig} + \phi_i + \epsilon_{ig} \quad (22)$$

The results of estimating equations (17) through (22) can be found in Table A5. The bottom row of the table reports the linear coefficient $\beta_1 + \beta_2$, equal to the sum of the payment dummy and interaction variables estimated. We observe an increase in the politician's choice of expropriation of small magnitude in Column (1); interestingly, this coefficient is similar in magnitude to the average increase in the voter's reelection threshold observed in games with payment. In Column 2, we observe the increase is insignificant in games in which one payment is distributed and positive and significant in games with four payments. In Column (3), we observe the overall increase is larger in games in which all subjects receive payments; $\beta_1 + \beta_2$ is nearly .9.

In Columns (4) and (5), we observe that there is little meaningful variation with respect to the gift or limited information framing. In Column (6), however, we see a significant decline in the amount expropriated when limited information is shared, and an increase when voter consent is sought.

Comparing these results to the evidence about voter behavior, subjects as politicians seem to be acting approximately rationally. When distributing vote payments increases voters' re-election thresholds, politicians correspondingly increase their expropriation (presumably with approximately no change in their probability of reelection).

D Appendix tables

Table A1: Sessions conducted

Session type	Framing	Game round I	Game round II	Game round III	Location
A1	Public payments	0 payments	1 payment	4 payments	U
A2	Public payments	0 payments	4 payments		K
B1	Public gift	0 payments	1 payment	4 payments	U
B2	Public gift	0 payments	4 payments		K
C1	Limited - prior	0 payments	4 payments	5 payments	U
C2	Limited - prior	0 payments	4 payments		K
D1	Limited - posterior	0 payments	4 payments	5 payments	U
D2	Limited - posterior	0 payments	4 payments		K
E	"Big pot"	0 payments	5 payments		U, K
F1	Unequal endowments	0 payments ("uneq")	5 payments		U, K
F2	Unequal endowments	0 payments ("uneq")	0 payments	5 payments	U

Table A2: Sessions conducted

Session type	Framing	Location	Sessions	Subjects	Subject-game rounds	Subject-decisions
A1	Public payments	U	8	78	234	378
A2	Public payments	K	6	96	192	288
B1	Public gift	U	10	102	306	510
B2	Public gift	K	6	90	180	270
C1	Limited - prior	U	11	66	192	252
C2	Limited - prior	K	4	60	120	180
D1	Limited - posterior	U	9	60	180	240
D2	Limited - posterior	K	4	60	120	180
E	"Big pot"	U, K	13	96	192	192
F1	Unequal endowments	U, K	7	60	120	180
F2	Unequal endowments	U	8	48	144	192
Total			86	816	1980	**2862**
Excluding non-monotonic			86	755	1858	**2680**

Table A3: Voter behavior - high comprehension sample

	Voter reelection threshold							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Payment	-.587	-.764			-.747	-.774	-.626	-.694
	(.208)***	(.233)***			(.204)***	(.234)***	(.266)**	(.301)**
Payment 1			.115	-.635				
			(.412)	(.317)**				
Payment 4			-.809	-.837				
			(.198)***	(.233)***				
Recipient	1.079	1.010			.951	.951	.956	.958
	(.164)***	(.170)***			(.150)***	(.176)***	(.196)***	(.218)***
Recipient x 1			.200	.200				
			(.239)	(.281)				
Recipient x 4			1.345	1.242				
			(.180)***	(.189)***				
All payment					.440	.472		
					(.368)	(.246)*		
Payment x Gift							-.347	-.199
							(.413)	(.472)
Recipient x Gift							.143	.140
							(.305)	(.350)
$\beta_1 + \beta_2$	.492	.246					.330	.264
	(.199)	(.209)					(.214)	(.243)
$\beta_1 + \beta_2 + \beta_3$					.643	.648		
					(.328)**	(.265)**		
$\beta_1 + \beta_2 + \beta_3 + \beta_4$							.125	.205
							(.350)	(.403)
Mean dep. var.	7.12	7.12	7.12	7.12	7.12	7.12	7.12	7.12
Fixed effects		Subject		Subject		Subject		Subject
Obs.	1966	1966	1966	1966	1966	1966	1966	1966

Notes: The dependent variable is the maximum threshold of expropriation at which the subject will still reelect the politician. The independent variables are dummy variables for the game round including any, one or four vote payments, a dummy for the game round including a gift framing, the interaction of those four dummy variables with a dummy for the subject receiving a payment, and a dummy for the game including payments distributed to all subjects. $\beta_1 + \beta_2$ reports the sum of the coefficients on payment and recipient. $\beta_1 + \beta_2 + \beta_3$ reports the sum of the coefficients on payment, recipient and all payment. $\beta_1 + \beta_2 + \beta_3 + \beta_4$ reports the sum of the coefficients on payment and recipient and the gift interactions. Fixed effects are as specified in the table; all specifications include standard errors clustered at the subject level. The sample included is session types A through D as labeled in Table A1. Asterisks indicate significance at the ten, five and one percent level.

Table A4: Voter behavior - including non-monotonic subjects

	Voter reelection threshold							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Payment	-.749	-.803			-.848	-.812	-.891	-.844
	(.186)***	(.205)***			(.179)***	(.206)***	(.222)***	(.248)***
Payment 1			-.283	-.666				
			(.336)	(.248)***				
Payment 4			-.888	-.868				
			(.186)***	(.217)***				
Recipient	1.047	.990			.936	.936	1.004	.980
	(.148)***	(.167)***			(.147)***	(.172)***	(.182)***	(.204)***
Recipient x 1			.278	.278				
			(.213)	(.250)				
Recipient x 4			1.260	1.185				
			(.172)***	(.194)***				
All payment					.551	.495		
					(.368)	(.246)**		
Payment x Gift							.124	.125
							(.392)	(.441)
Recipient x Gift							.010	.033
							(.306)	(.352)
$\beta_1 + \beta_2$	.298	.188					.113	.137
	(.165)*(.181)				(.195)	(.219)		
$\beta_1 + \beta_2 + \beta_3$					.639	.620		
					(.345)	(.255)		
$\beta_1 + \beta_2 + \beta_3 + \beta_4$							.247	.295
							(.282)*	(.320)**
Mean dep. var.	7.17	7.17	7.17	7.17	7.17	7.17	7.17	7.17
Fixed effects		Subject		Subject		Subject		Subject
Obs.	2298	2298	2298	2298	2298	2298	2298	2298

Notes: The dependent variable is the maximum threshold of expropriation at which the subject will still reelect the politician. The independent variables are dummy variables for the game round including any, one or four vote payments, a dummy for the game round including a gift framing, the interaction of those four dummy variables with a dummy for the subject receiving a payment, and a dummy for the game including payments distributed to all subjects. $\beta_1 + \beta_2$ reports the sum of the coefficients on payment and recipient. $\beta_1 + \beta_2 + \beta_3$ reports the sum of the coefficients on payment, recipient and all payment. $\beta_1 + \beta_2 + \beta_3 + \beta_4$ reports the sum of the coefficients on payment and recipient and the gift interactions. Fixed effects are as specified in the table; all specifications include standard errors clustered at the subject level. The sample included is session types A through D as labeled in Table A1. Asterisks indicate significance at the ten, five and one percent level.

Table A5: Politician expropriation

	Expropriation					
	(1)	(2)	(3)	(4)	(5)	(6)
Payment	.536 (.275)*		.422 (.281)	.420 (.364)	.593 (.281)**	.589 (.278)**
Payment 1		.490 (.284)*				
Payment 4		.545 (.301)*				
All payment			.447 (.469)			
Payment x Gift				.388 (.475)		
Payment x Limited info					-.286 (.481)	-1.099 (.497)**
Payment x Consent						1.555 (.790)**
$\beta_1 + \beta_2$			.869 (.482)*	.808 (.304)***	.307 (.499)	-.511 (.498)
$\beta_1 + \beta_2 + \beta_3$						1.045 (.644)
Game sample	All excluding big pot, uneq. endowments					
Obs.	1670	1670	1670	1670	1670	1670

Notes: The dependent variable is the level of expropriation chosen by the politician. The independent variables are dummy variables for the game round including any, one, four or five vote payments, and the interaction of the payment dummy variable with the gift, limited information and prior consent framings. $\beta_1 + \beta_2$ reports the sum of the coefficients on payment and the reported interaction term. $\beta_1 + \beta_2 + \beta_3$ reports the sum of the coefficients on payment, payment interacted with limited information, and payment interacted with consent. All specifications include subject fixed effects and standard errors clustered at the subject level. The sample included is session types A through D as labeled in Table A1. Asterisks indicate significance at the ten, five and one percent level.