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Value for Money in Purchasing Votes? Vote-buying and Voter Behavior in the Laboratory

Research and Innovation Grants Working Papers Series

January 15, 2016



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Value for Money in Purchasing Votes?

Vote-buying and Voter Behavior in the Laboratory

Research and Innovation Grants Working Papers Series

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MESSAGE FROM THE DIRECTOR

The DRG Center of Excellence is pleased to share “*Value for Money in Purchasing Votes? Vote-Buying and Voter Behavior in the Laboratory.*” This publication was produced by USAID in partnership with the Institute of International Education as part of the Research and Innovation Grants Working Papers Series.

The *Strategy on Democracy, Human Rights, and Governance*¹ reaffirmed USAID’s commitment to “generate, analyze, and disseminate rigorous, systematic, and publicly accessible evidence in all aspects of DRG policy, strategy and program development, implementation, and evaluation.” This paper, along with the others contained in the series, make a valuable contribution to advancing this commitment to learning and evidence-based programming.

This series is part of USAID’s Learning Agenda for the DRG Sector, a dynamic collection of research questions that serve to guide the DRG Center’s and USAID field missions’ analytical efforts. USAID seeks to inform strategic planning and project design efforts with the very best theory, evidence, and practical guidance. Through these efforts, the Learning Agenda is contributing to USAID’s objective to support the establishment and consolidation of inclusive and accountable democracies to advance freedom, dignity, and development.

The research presented in this paper provides useful insights into the issue of how vote-buying influences voter behavior. The findings suggest that while vote-buying does influence voter behavior, the relationship between a vote payment and the voter’s subsequent choice is complex. For example, research subjects who received a vote payment treated the candidate more leniently and were willing to tolerate higher levels of expropriation. However, there was a backlash among voters who did not receive a vote payment and were aware of the candidate’s vote-buying activities. This finding appears to show that while vote-buying is effective within the target group, there remains an unintended consequence to the approach. If highlighted and publicized, vote-buying may lead to a backlash against the subject candidate. For the DRG practitioner, this insight can help calibrate election- assistance activities, including oversight efforts that seek to uncover vote-buying as well as the inappropriate use of administrative resources.

I hope you find this research enlightening and helpful. As the DRG Center’s Learning Agenda progresses, we will continue our effort to bring forward the latest in relevant social science research to important constituencies for our work, particularly our DRG cadre and implementing partners, but also others. I invite you to stay involved as this enriching, timely, and important work proceeds.

Neil Levine, Director
Center of Excellence on Democracy, Human Rights, and Governance
US Agency for International Development

¹ [https://www.usaid.gov/sites/default/files/documents/1866/USAID%20DRG_%20final%20final%206-24%203%20\(1\).pdf](https://www.usaid.gov/sites/default/files/documents/1866/USAID%20DRG_%20final%20final%206-24%203%20(1).pdf)

EXECUTIVE SUMMARY

Vote-buying is extremely common in developed and developing countries: politicians use a range of tools, from covert or complex to simplistic and blatant, to attempt to purchase votes in democracies around the world. Vote-buying endangers the validity of election results; undermines public trust in the democratic system; and negatively affects post-election politics, government accountability, and public perceptions of that accountability. But how does vote-buying influence individual voter behavior? Does vote-buying change what candidate the voters select on election day? Does it change voters' tolerance of other corrupt behavior demonstrated by the vote-buying politician.

Through an Innovation and Research Grant funded by USAID's Center of Excellence on Democracy, Human Rights, and Governance under the Democracy Fellows and Grants Program, a research team led by Williams College analyzed how people respond to vote payments in a laboratory setting in a developed and a developing country (United States and Kenya, respectively). The team conducted 84 sessions with 816 research subjects, and each research subject participated in two to three "games" per session.

The findings suggest that while vote-buying does influence voter behavior, the relationship between a vote payment and the voter's subsequent choice is complex. The sessions were structured to gauge changes in subject response to 1) different levels of vote payments and of politician expropriation of public funds and 2) variations in the "games," such as whether voters consented to receive a vote payment or knew other voters had received one. The research was also structured to assess differences in voter response between the USA and Kenya; however, interestingly, no country-based differences were observed.

Key policy implications of the findings include:

- Research subjects who received a vote payment treated the politician more leniently: compared to a game in which no vote payments were received, these subjects were willing to tolerate higher levels of expropriation and still vote to reelect the politician.
- In an apparent backlash effect, research subjects who knew that vote payments were being distributed, but did not receive one, tolerated a lower level of expropriation and were less likely to vote to reelect the politician.
- Around 80% of research subjects indicated that they would consent to receive a vote payment if it were offered, and consenting subjects were most willing to reelect a vote-buying politician. In addition, consenting subjects tolerated a 15% higher expropriation threshold than those who did not consent to receive a payment.
- However, if subjects received a payment and did not consent to receive it, their behavior did not change significantly in any way, consistent with the hypothesis that vote-buying is most effective when the targeted voter is actively engaged in the process.

Taken together, these findings provide evidence of the lasting negative effects of vote-buying: it can lead to a decreased willingness by voters to use their vote to hold politicians accountable for corruption. This research is an independent project, and is not intended to provide direct guidance to USAID programming. However, it seems clear that elections support programs that raise awareness of vote-buying can contribute to a backlash effect and ultimately disincentize the practice of vote-buying.

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 a key reason for elite capture of public policy the world over (Acemoglu & Robinson 2012). However, unlike pork-barrel and clientelistic politics, little is known about the channels through which vote-buying alters subsequent policymaking 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 increased 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.

While there is some theoretical literature analyzing adverse selection—or more specifically, the possibility that vote-buying leads to inefficient electoral outcomes (for example, Dekel, Jackson, & Wolinsky 2008)—to the best of our knowledge the potential moral hazard effect has not been examined in the literature. Clearly, it is empirically extremely challenging to disentangle politician choices from politician quality. In this paper, we use laboratory experiments designed to exclude any variation in politician quality to provide the first evidence around the moral hazard effects of vote-buying, analyzing data collected from 816 subjects in Kenya and the United States.

To isolate the moral hazard effects of vote-buying, we deploy a simple version of a pure moral hazard model. Subjects in the laboratory choose whether or not to reelect an incumbent, employing the reelection decision as a tool to discipline the incumbent’s expropriation of rents. No dimension of politician quality influences voters’ payouts other than the expropriation decision; hence, voters do not face any selection motive.

We then augment this game by introducing vote payments as an exogenous external transfer to the voter that is described in the baseline game as “a payment in exchange for your vote.” The politician has no agency in payment distribution; they are simply assigned to a certain fraction of voters, and the secret ballot is maintained throughout. If voters are rational best-responders, the expropriation threshold they set should be unchanged.

Our first finding is that voters are responsive to the introduction of payments and their response is inconsistent with a returns-maximizing model of voter behavior: the voters’ maximum expropriation threshold for the reelection of the politician changes once vote-buying is introduced. Second, when even minimal information about vote-buying is public, subject behavior varies depending on whether s/he receives a payment. Subjects who receive a payment on average treat the politician more leniently, increasing the threshold of expropriation at which they will reelect the politician compared to the simple election game in which no vote payments were introduced. Subjects who do not receive a payment, by contrast, treat the politician more harshly and set a lower threshold of expropriation. This suggests a possible phenomenon of backlash at their exclusion.

Both of these phenomena are robust to the framing of the transfer as a payment in exchange for a vote, or its framing as a gift with no *quid pro quo*. By contrast, when vote payments are provided to all voters in a hypothetical polity, the backlash effect unsurprisingly disappears and there is a large increase in the voters' reelection thresholds. In addition, we demonstrate that this pattern does not simply reflect a phenomenon of inequality aversion in which subjects who do not receive a vote payment respond by punishing the politician.

Next, we explore the channels through which voters' response is elicited. We find that the average positive response of subjects to vote payments masks significant heterogeneity with respect to another feature of the experimental design. In settings with limited information about vote-buying, if subjects have the opportunity to consent to receiving a payment in exchange for their vote, then they respond with significantly more lenient treatment of the politician. The difference between the effect of a payment on the voter's threshold without consent and the effect with consent is equal to about 15% of the average threshold. This result is consistent with voters' response to vote-buying 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 can lead to a decreased willingness by voters to discipline politicians. However, the evidence of limited response in the absence of consent and the importance of the backlash effect suggests that the institutional environment also meaningfully shapes voters' responsiveness to payments, and that they are not uniformly effective in decreasing politician accountability.

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 with a selective incentive for that voter, 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 those they 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.²

Assembling large-scale empirical data about the prevalence of vote-buying is extremely challenging given that the practice is generally illegal and accordingly under-reported. 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 have been analyzed from a variety of countries, including Argentina, the Philippines, São Tomé and Príncipe, and Nicaragua

² 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.

(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 Finan & Schechter (2012) who find evidence that social preferences were a key determinant of successful vote-buying in Paraguay; they found evidence of greater reciprocity in economic games among those targeted for vote-buying.

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.

METHODS

Our objective is to test a simple model of electoral accountability in conjunction with vote-buying in experimental sessions conducted at the Harvard Decision Science Laboratory in Cambridge, Massachusetts, and the Busara Experimental Laboratory in Nairobi, Kenya. Below, we first describe the experimental design. Next, we will outline the sequence of games in each experimental session and the alternate framings employed. Finally, we will describe the structure of the sample and the primary dependent variable of interest.

A. Experimental Design

The primary focus of the laboratory experiments is to test a simple model of retrospective electoral accountability. We abstract entirely from elections as a mechanism for selecting politicians, and focus purely on their use as a mechanism of retrospective accountability to punish politicians for poor performance.

The polity in the lab is constituted by five voters and one politician.³ Each voter in the polity receives an endowment y (set at a different level in the US and Kenya), and is informed that this endowment is taxed at a rate τ of 0.5. The parameter λ —which is set at 0.3—defines the fraction of tax revenue available for expropriation. Hence 15% of each voter's endowment is available for expropriation. The introduction of the parameter λ allows us to maintain a high tax rate, and thus the salience of the politician's expropriation decision for the voters within the game, while limiting the reelection bonus for the politician. Any tax revenue that is not expropriated by politicians is redistributed to voters at the conclusion of the game and constitutes part of their final payoff.

³ The choice of the polity size involves a trade-off between the budget and time required (given that a fixed polity size required a pre-determined number of participants in each experimental session), and the objective of realistically simulating an election environment in which voters have limited information about other voters' decisions and perceive the probability that they can directly affect the election outcome to be low. Work in experimental political economy analyzing questions such as voter turnout and strategic voting behavior has analyzed voter behavior in hypothetical polities generally constituted by 3 to 20 voters (Palfrey 2009).

A final parameter κ defines a transition fee paid by voters whenever the politician is not reelected. This parameter makes failing to reelect the politician explicitly costly, and discourages subjects from simply anchoring their threshold of expropriation at zero. κ was set to be equal to $0.05y$.

Turning to the politician, s/he does not receive an endowment, but instead a salary of $0.5y$ in the first period, and an additional $0.5y$ if reelected. The reward for reelection is intended to proxy for the potential lifetime rents associated with reelection across an infinite number of future periods in a standard moral hazard model. To implement this, the language of the game stated that the incumbent politician receives a salary y , and forfeits half this salary ($0.5y$) if s/he fails to win reelection. This language was intended to increase the salience of the lower payout if the politician is not reelected.

The timing of the game entails simultaneous choices by both the politician and the voters, and all subjects specify their choices as both voters and politicians:

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

At the conclusion of the experimental session, each subject is assigned to a role, and subject earnings are determined based on the choices specified earlier by all six subjects. Subjects cannot revisit their previous decisions, and no information about any other subjects' decision is provided prior to the conclusion of the experimental session.

Note that the experimental design affords 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 the voters' payouts. In addition, since the game ends immediately after the reelection decision, the hypothetical alternate candidate is afforded no identity or choice of action. These features of the game design are intended to minimize the probability that the subjects perceive any dimension of quality of the incumbent as relevant to their voting decision, other than the choice of how much s/he expropriates.

Vote-buying. This simple game of voting payment is then augmented by the distribution of vote payments. The secret ballot is maintained, and the vote payments are not drawn from voters' income or tax revenue; they are funded separately. Politicians are not allowed to choose the number of vote payments or the recipients, and voters are (initially) not given the choice of whether or not to accept the payment. A subset of voters simply receive a payment that is described as a payment in exchange for their votes. When distributed, vote payments were set to be equal to $0.1y$, or \$2 relative to a voter endowment of \$20 in the United States.

The timing of the voters' and politicians' decisions does not change when vote-buying is introduced into the model. Subjects are first asked to specify their threshold for reelection, both in the case they hypothetically receive a payment and in the case they hypothetically do not receive a payment. They are then asked to specify how much they would expropriate as the politician. The game then concludes, and subjects are subsequently assigned to roles in order to facilitate payment.

B. Sequence of Experimental Sessions

We followed a consistent structure and sequence of experimental sessions in both experimental sites. Sessions were conducted with 6 or 12 subjects in the United States, and 12 or 18 subjects in Kenya; this was to ensure that the players could be constituted into polities of six (five voters and one politician), though subjects were not matched into a polity until the end of the session. Each experimental session had the following general structure:

1. *Social preferences*: Subjects played the dictator, trust, and ultimatum games.⁴ We provide more details in Appendix A.
2. *Introduction to voting game without payments*: Subjects were then introduced to the voting game and asked to answer a series of questions testing their comprehension of game dynamics and the structure of payoffs. The question-and-answer interface was computerized to enable the monitoring of subject responses. The introduction placed particular emphasis on the fact that subjects would be asked about their behavior both as voters and as politicians in a variety of scenarios; at the conclusion of the session, one scenario would be chosen, players would be matched into a polity of six, one player would be randomly assigned to be the politician, and payoffs would be determined based on this assignment. No payments were discussed.
3. *Voting game*: Subjects were engaged in the voting game, and first asked to consider their behavior as voters. Each subject could specify whether or not s/he would reelect a politician who expropriated a specified amount (though the way in which this information was elicited differed slightly between the United States and Kenya sessions, as described in more detail below). Each subject was then asked about his/her expectations of how much the politician would expropriate, and whether s/he expected the politician to be reelected. Finally, each subject was asked about his/her behavior as a politician: how much s/he would expropriate, and whether s/he believed that s/he would be reelected.
4. *Introduction to voting game with payments*: In some experimental sessions, subjects were then introduced to the idea of payments for votes. The information and comprehension questions provided were much briefer than the first introduction to the overall game. The key points were that some voters would be randomly offered payments, though all subjects would know whether payments were offered and how many, that the money for vote payments was provided separately from player endowments and the budget for public goods, and that the vote remained secret and anonymous. The responses to comprehension questions were again recorded.
5. *Voting game with payments*: The structure of the questions deployed was identical to that employed in the voting game without payments, though in some subgames, including vote payments, voters answered multiple sets of questions as specified below.
6. *Questionnaire*: subjects completed a brief questionnaire regarding their demographic characteristics and political history.

⁴ The ultimatum games were introduced only in the 2014 sessions.

The key parts of the experimental session are the “voting game” and the “voting game with payments.” We define each iteration of the voting game as a “game round.” A given experimental session includes two to three game rounds, in addition to the social games and the informational introductions. (A subject can participate in three game rounds if the voting game with payments is played two times, with varying numbers of payments.)

The game rounds are strategically 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. Each subject specifies his or her choice as both a voter and a politician in each game round.

Importantly, no information about game play in any game round is revealed prior to the conclusion of the experimental session. At the conclusion of the experimental session, a single game round among those played is randomly chosen as the basis for payment. Subjects are randomly constituted into a polity of six, and selected to serve as the politician and voters who did or did not receive payments, as applicable. All subjects are paid on the basis of their decisions in that game round. In all sessions, subjects were repeatedly reminded that any one of their decisions could affect their final payout.

C. Alternate Framings

In both the United States and Kenya, we conducted experimental sessions with four different framings for the vote payment. The first and most basic framing, denoted “public payments,” entailed full specification to the players of the number of payments that were distributed to the players, and the size of those payments. Voters were informed that the payment was in exchange for their vote. The “public payments” framing was implemented with one, four, and five payments distributed.

In the second framing, denoted “public gifts,” the number of payments that were distributed to the players, and the size of those payments, were specified to subjects. However, the payment was described as a gift, and no *quid pro quo* arrangement was described. The “public gifts” framing was implemented with one and four payments distributed only.

In both of the above framings, all participants were asked to respond first about their chosen threshold *vis-à-vis* the politician if they received a payment, and next about their chosen threshold if other subjects received a payment and they did not. (In the games in which all voters received payments, no choice about action in the absence of payment was elicited.)

The third and fourth framings are denoted “Limited information – prior consent” and “Limited information – posterior consent,” respectively. In these framings, no information was provided about the number, size, or nature of payments; a sentence was simply added to the game description stating that some voters may receive payments in exchange for their votes. The “introduction to vote payments” was omitted. The actual size of vote payments was maintained at \$2, and only the subgame including four payments was implemented. Thus while the payment structure is identical to the other games examined, this information is not fully revealed to the players.

In addition, in the third framing, subjects received the opportunity to state whether or not they would like to accept the vote payment before choosing the reelection threshold for the politician. In the fourth framing, voters’ consent was not solicited at this point. They were subsequently asked whether or not

they would have liked to accept the payment. Again, voters were asked to specify their decisions both with and without receipt of a payment.⁵

Appendix Table A1 summarizes the experiments. In Panel A, each framing is listed on the left; there are four framings and two substantive variations. The substantive variations include the public payments game in which the fraction of the treasury vulnerable to expropriation by the politician (λ) is increased from 0.35 to 0.5 (“big pot”), and a game in which the endowments of the voters are rendered unequal (“unequal endowments”). The latter game will be discussed in the robustness checks.

The cells indicate which games were played for each framing, and in what country and year. In Panel B, each cell—denoted a subgame—is provided with a number that will assist in specifying the set of subgames analyzed in the primary results. It is important to note that all variants of the game in which there are zero payments (excepting unequal endowments and “big pot”) are identical. Accordingly, subgames 1, 5, 8, and 10 are identical in structure. However, they are separately numbered.

Table A2 shows how these various subgames were structured into sessions. Each session included multiple subgames played in sequence by the same subjects, and these sequential subgames are denoted by Roman numerals. It is evident that, in general, the horizontal rows in Table A1 correspond to a given session type.⁶

In much of the analysis, we will be employing data from certain session types. For this reason, we separately number each subgame, even when multiple subgames are identical, to be transparent about the data used. For example, the basic voting game without payments is always identical; however, we number them separately in order to denote both the subgame and the session type from which certain data are drawn.

D. Unit of Analysis

We conducted 84 experimental sessions including 816 subjects—366 in Kenya, and 450 in the United States.⁷ The number of subgames per participant during an experimental session varied between two and three. In Kenya, subjects never participated in more than two subgames given the longer time required for exposition. Table A2 summarizes the session structure including subgames per session. In all, 1,980 subject-subgames were observed.

Our data, therefore, have a four-level hierarchy: session, subject, subject-subgame, and subject-decision. The unit of interest is the subject-decision. In subgames in which zero or five payments are distributed, the subject makes only a single decision; s/he specifies the threshold of expropriation above

⁵ In some sessions, the order in which these questions posed varied. This variation will be discussed in more detail later. Note that there was never a case in which the order of the questions varied across different subgames for the same subject in the same session. A single subject will face only one ordering of the payment questions.

⁶ The unequal endowment game was implemented employing two minor variants in which the simple no-payment game was or was not included in the session; these are denoted J1 and J2, respectively.

⁷ By year, the session breakdown is 30 in 2013, 41 in 2014, and 13 in 2015. Three hundred sixty-six subjects were included in experiments in 2013 (180 in the United States, and 186 in Kenya); 360 subjects were included in experiments in 2014 (180 in the United States, and 180 in Kenya); and 90 subjects were included in experiments in 2015, all in the United States.

which s/he would fail to reelect the politician. However, in subgames 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. A total of 2,862 subject-decisions was observed.

Our analysis below focusses on the behavior of subjects as voters. Subjects in the game sessions also specified their choices as politicians, and we summarize those results in Appendix C.

E. Voters' Reelection Threshold

The primary dependent variable of interest is the voters' reelection threshold; this threshold was elicited differently in the United States and Kenya. In the United States, 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.

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. The reason for this alternate design 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. Only around 7% of subjects exhibited this type of non-monotonic behavior.

The Kenyan subjects' 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.⁸ Observations corresponding to subjects who exhibit non-monotonic behavior are dropped; this leaves a sample of 755 subjects and 2,680 subject-decisions. We will also demonstrate that the primary results are robust to the inclusion of the non-monotonic subjects.

EMPIRICAL RESULTS

A. Voter Behavior

The primary specifications of interest can be written as follows:

$$T_{igd} = \beta_1 P_{ig} + \beta_2 R_{igd} + \epsilon_{igd} + \varphi_i \quad (1)$$

$$T_{igd} = \beta_1 P_{ig}^1 + \beta_2 P_{ig}^4 + \beta_3 P_{ig}^1 \times R_{igd} + \beta_4 P_{ig}^4 \times R_{igd} + \varphi_i + \epsilon_{igd} \quad (2)$$

T_{igd} is subject i 's reelection threshold in subgame g making game decision d . P_{ig} denotes that subject i was playing a subgame g that included distribution of payments to voters (P_{ig}^1 and P_{ig}^4

⁸ For example, if a subject stated s/he would reelect a politician who expropriated 75 shillings, but would not reelect a politician who expropriated 150 shillings, her/his maximum level of expropriation was set to be 112.5.

denote subgames in which one and four payments were distributed, respectively), and R_{igd} denotes a game decision corresponding to the receipt of a payment by subject i in subgame g . All specifications are estimated with and without subject fixed effects φ_i and we cluster standard errors at the subject level.⁹ All subgames are included in this analysis with the exception of the “big pot” and “unequal endowments” subgames (subgames 12 – 14, as denoted in Table A1). The resulting sample includes 2,136 subject-decisions.

Table 1 reports summary statistics for the sample of interest. The average voter reelection threshold in the pooled sample is the equivalent of \$7.07, employing the dollar scale in the United States sessions. 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. We also report summary statistics for simple measures of reciprocity, discussed in more detail later.

Table 2 reports the results of estimating equations (1) and (2); columns (1) and (3) report 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.

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 do not 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.¹⁰

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. This result may be unsurprising, since a voter who knows s/he is in the minority may see no reason to respond to a proffered payment at all.

The bottom row of the table reports the sum of β_1 and β_2 as estimated in equation (1) for columns (1) and (2), and it is significant and positive. 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. This coefficient is positive and significant both with and without subject fixed effects, and the magnitude suggests a relative increase in the reelection threshold of between 4% and 6%. Subsequent analysis will demonstrate that this coefficient masks considerable heterogeneity.

⁹ 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 are posed.

¹⁰ Separate tabulations are not reported, for concision, but are available upon request.

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_g is defined to be equal to one if subject i was participating in a game in which five payments were distributed, and the following specification is estimated, again with and without subject fixed effects:¹¹

$$T_{igd} = \beta_1 P_{ig} + \beta_2 R_{igd} + \beta_3 All_{ig} + \varphi_i + \epsilon_{igd} \quad (3)$$

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%. Interpreting the magnitude another way, a vote payment of \$2 leads to an increase in the amount the voters 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 about 75 cents in games in which vote payments are distributed to all subjects.¹² Thus, we can observe that the voter's responsiveness to payments steadily increases in proportion to voters' receiving payments.

Finally, in columns (5) and (6), we re-estimate equation (1) 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.¹³

$$T_{igd} = \beta_1 P_{ig} + \beta_2 R_{igd} + \beta_3 P_{ig} \times Gift_{ig} + \beta_4 R_{igd} \times Gift_{ig} + \varphi_i + \epsilon_{igd} \quad (4)$$

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. These results are consistent with the general anecdotal evidence that pecuniary incentives for voters are often provided as in-kind gifts without specific reference to the desired electoral outcome—a strategy that, these results suggest, may be as effective as a more specific inducement.

Table A3 in the Appendix reports the results from re-estimating all specifications reported in Table 2 employing only the high-comprehension sample.¹⁴ The results are not significantly different, and thus

¹¹ 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.

¹² 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).

¹³ 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.

¹⁴ To evaluate whether there are any systematic differences in game behavior comparing across individuals with

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.

B. Inequality Aversion

One possible interpretation of the results would be that the subjects' responses as voters simply reflect a reaction to receiving or not receiving the payment that is directed at the game organizer, 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.¹⁵

In order to test this hypothesis, we also collected data employing a subgame 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_{igd} , 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 $Ineq_{ig}$ equal to one for the unequal endowment games. The following specification is estimated for the full sample 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.¹⁶

$$T_{igd} = \beta_1 P_{ig} + \beta_2 R_{igd} + \beta_3 Ineq_{ig} \times R_{igd} + \beta_4 R_{igd} + \varphi_i + \epsilon_{igd} \quad (5)$$

The results are reported in Table 3. 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 coefficient is significant with and without subject fixed effects and large in magnitude (around one-half of the

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 (United States/Kenya).

¹⁵ 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.

¹⁶ The sample of games with zero or four payments is subgames 1, 3, 5, 7, 8 – 11, and 14 as designated in Table A1. Subgame 12, the game with zero payments but a "big pot" vulnerable to expropriation, is omitted. 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.

estimated coefficient one receive dummy, R_{igd}).¹⁷ This is consistent with our hypothesis that subjects' observed behavior is not simply a reflection of generalized inequality aversion.

C. 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 a variety of different 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 reframing 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. 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; only following this question were they asked to specify their reelection threshold in case they received a payment, and 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 reframe payments in a context more analogous to the one in which vote payments are usually believed to be most effective: private payments that constitute an implicit contract, with some sort of verbal agreement, between the politician and the voter.

We begin our analysis by re-estimating equation (1) adding interaction terms with the limited information framing; the same sample is employed, including all games except unequal endowments and big pot. The equation of interest can be written as follows, and is again estimated with and without subject fixed effects.¹⁸

¹⁷ 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.

¹⁸ In the specifications without subject fixed effects, a dummy for the limited information framing and additional

$$T_{igd} = \beta_1 P_{ig} + \beta_2 R_{igd} + \beta_3 P_{ig} \times Lim_{ig} + \beta_4 P_{ig} \times R_{igd} \times Lim_{ig} + \varphi_i + \epsilon_{igd} \quad (6)$$

The results are reported in columns (1) and (2) of Table 4, and show that 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 $\beta_1 + \beta_2 + \beta_3 + \beta_4$ are in this case negative and close to zero.

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.

$$T_{igd} = \beta_1 P_{ig} + \beta_2 R_{igd} + \beta_3 P_{ig} \times Lim_{ig} + \beta_4 R_{igd} \times Lim_{ig} + \beta_5 P_{ig} \times Cons_{ig} + \beta_6 R_{igd} \times Cons_{ig} + \varphi_i + \epsilon_{igd} \quad (7)$$

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

The final row of the table reports the linear combination $\beta_3 + \beta_4$; this captures whether the net effect of a payment is different when prior consent is solicited. We can observe that β_3 and β_4 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. However, the linear combination $\beta_3 + \beta_4$ 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.

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 recipient dummy variable with a dummy variable equal to one if consent is provided, $Prov_{ig}$. The specification of interest can thus be written as follows:

$$T_{igd} = \beta_1 P_{ig} + \beta_2 R_{igd} + \beta_3 R_{igd} \times Prov_{ig} + \varphi_i + \epsilon_{igd} \quad (8)$$

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 4. We observe that β_3 is positive and large in magnitude, but not statistically significant at conventional levels; this is perhaps unsurprising, given the

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.

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, prior to the introduction of any voting-related decisions. Subjects are provided with an endowment of \$4 (in the sessions in the United States) 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, 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.)

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). Given the simpler trust game employed in the 2013 sessions, Rec_i can be constructed only for the 2014 sessions.

Table 1 reports summary statistics around social preferences. Around 75% of participants chose to send a positive amount in the trust game; 50% of participants who received a positive amount chose also to return part of it to the sender. The mean index of reciprocity is .06.

The specification of interest can be written as follows. We restrict the sample to subgames in which payments were distributed, excluding the big pot subgames; this is subgames 2 – 4, 6 – 7, 9, and 11 as specified in Table A1. Accordingly, the dummy variable P_{ig} can be omitted. All specifications are estimated with and without fixed effects.

$$T_{igd} = \beta_1 R_{igd} + \beta_2 R_{igd} \times Rec_i + \beta_3 Rec_i + \epsilon_{igd} \quad (9)$$

The results are reported in columns (7) and (8) of Table 4. 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 0.20, or a 30% increase relative to the primary effect.¹⁹ This result is again consistent with the evidence that requesting, and providing, consent leads to an increase in voter responsiveness to the payments.

¹⁹ 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.

D. “Big Pot” Games

Finally, to explore the hypothesis that voters’ responsiveness to payments diminishes when they stand to lose more from the politician’s expropriation, in two subgames the amount vulnerable to expropriation was increased, from 35 to 50% of the common treasury. The voting game was then played without any payments (subgame 12) and with five payments distributed (subgame 13). The size of the vote payment is fixed at \$2.

To test this hypothesis, we estimate the following equation using the sample restricted to the no payment and all-payment games.²⁰ Big_{ig} is a dummy variable equal to one if subject i participated in a subgame in which the amount vulnerable to expropriation was increased; this equation is again estimated with and without subject fixed effects.

$$T_{igd} = \beta_1 All_{ig} + \beta_2 All_{ig} \times Big_{ig} + \varphi_i + \epsilon_{igd} \quad (10)$$

The results are reported in Table 5, and show that the coefficient β_2 is negative and large in magnitude, though statistically insignificant. The sum $\beta_1 + \beta_2$ is reported in the final row of the table, and indicates that there is no 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 β_2 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.

CONCLUSION

This paper analyzes evidence about voter behavior under a regime of payments for votes in the laboratory. A simple model of retrospective politician accountability based on Barro (1973) and Ferejohn (1986) 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.

²⁰ Referring again to the labels designated in Table A1, this entails the use of subgames 1, 4, 5, 8, 10, 12, and 13.

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TABLES

Table 1: Summary Statistics

Variable	Mean	St. dev.	Observations
Voter threshold	7.07	4.65	2136
Dummy for threshold at zero	0.15	0.35	2136
Politician expropriation	7.88	4.96	1378
Dummy for zero expropriation	0.14	0.35	1378
Dummy for full expropriation	0.20	0.40	1378
Dummy for sending in trust game	0.74	0.44	558
Dummy for returning in trust game	0.50	0.50	558
Reciprocity	0.06	0.1	126

Table 2: Voter Behavior

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Payment	-0.582 (0.176)***	-0.670 (0.191)***			-0.726 (0.169)***	-0.679 (0.191)***	-0.734 (0.209)***	-0.687 (0.227)***
Payment 1			-0.064 (0.328)	-0.577 (0.243)***				
Payment 4			-0.751 (0.173)***	-0.731 (0.200)***				
Recipient	1.039 (0.142)***	0.964 (0.157)***			0.910 (0.139)***	0.910 (0.162)***	0.961 (0.172)***	0.922 (0.189)***
Recipient x 1			0.278 (0.213)	0.278 (0.249)				
Recipient x 4			1.273 (0.164)***	1.168 (0.181)***				
All payment					0.566 (0.365)	0.466 (0.242)*		
Payment x Gift							0.019 (0.372)	0.057 (0.412)
Recipient x Gift							0.082 (0.293)	0.121 (0.334)
$\beta_1 + \beta_2$	0.457 (0.153)***	0.294 (0.164)*					0.227 (0.180)	0.235 (0.198)
$\beta_1 + \beta_2 + \beta_3$					0.750 (0.338)**	0.697 (0.250)***		
$\beta_1 + \beta_2 + \beta_3 + \beta_4$							0.328 (0.260)	0.414 (0.291)
Mean dep. var.	7.07	7.07	7.07	7.07	7.07	7.07	7.07	7.07
Fixed effects obs.	2136	Subject 2136	2136	Subject 2136	2136	Subject 2136	2136	Subject 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 subgame including any, one, or four vote payments; a dummy for the game 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 subgames 1-11 as labeled in Table A1. Asterisks indicate significance at the 10, 5, and 1% level.

Table 3: Voter Behavior with Unequal Endowments

	Voter reelection threshold			
	(1)	(2)	(3)	(4)
Receive payment	1.047 (0.141)***	0.947 (0.154)***	1.059 (0.160)***	1.071 (0.198)***
Unequal endowment	0.597 (0.560)	-0.168 (0.506)	0.947 (0.645)	
Unequal endowment x payment	-0.530 (0.256)**	-0.609 (0.288)**	-0.534 (0.274)*	-0.609 (0.316)*
Mean dep. var.	7.227	7.227	7.227	7.227
Sample		Full	Four payments	
Fixed effects obs.	None 2490	Subject 2490	None 1902	Subject 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 subgame, and the interaction between the two. The full sample of subgames conducted is employed. Asterisks indicate significance at the 10, 5, and 1% level.

Table 4: Voter Behavior and Consent

	Voter reelection threshold							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Payment	-0.604 (0.217)***	-0.570 (0.247)**	-0.611 (0.218)***	-0.541 (0.229)***	-0.794 (0.392)**	-0.794 (0.486)		
Recipient	0.956 (0.166)***	0.956 (0.193)***	1.097 (0.178)***	0.974 (0.190)***	0.146 (0.905)	0.918 (0.834)	0.713 (0.275)***	0.683 (0.342)**
Payment x Limited info	-0.203 (0.340)	-0.280 (0.389)	-0.300 (0.408)	-0.421 (0.419)				
Recipient x Limited info	-0.162 (0.304)	-0.162 (0.354)	-0.677 (0.410)*	-0.554 (0.467)				
Payment x Consent			0.222 (0.524)	0.147 (0.543)				
Recipient x Consent			0.807 (0.502)	0.807 (0.585)	1.437 (1.050)	0.411 (0.838)		
Recipient x Provided					1.437 (1.050)	0.411 (0.838)		
Recipient x Reciprocity							1.450 (2.467)	2.216 (2.914)
$\beta_3 + \beta_4$			1.029 (0.453)**	0.955 (0.448)**				
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		2014 games including 1 or 4 payments	
Fixed effects obs.	2136	Subject 2136	2136	Subject 2136	291	Subject 291	366	Subject 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 subgame 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 subgames 1 – 11; the sample in columns (5) and (6) includes subgames 8 and 9; the sample in columns (7) and (8) includes subgames 8, 9, 10, and 11 if conducted in the United States. The reciprocity indicator 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 5: Voter Behavior and “Big Pot” Games

	Voter reelection threshold			
	(1)	(2)	(3)	(4)
All payments	0.660 (0.290)**	0.552 (0.520)	0.999 (0.360)***	0.794 (0.504)
Big pot x all payments			-.797 (.600)	-0.562 (1.127)
$\beta_1 + \beta_2$.232 (.480)	0.232 (1.008)
Mean dep. var.	7.93	7.93	7.93	7.93
Fixed effects obs.	964	Subject 964	964	Subject 964

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 includes subgames 1, 4, 5, 8, 10, 12, and 13, or all subgames in which zero or five payments are distributed. Asterisks indicate significance at the 10, 5, and 1% level.

APPENDIX 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 also were 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.

APPENDIX B: COMPARING GAME SESSIONS IN THE UNITED STATES AND KENYA

The relative ratios of key game parameters were maintained fixed across experimental sessions in the US and Kenya to ensure that the choices faced by players were uniform. The voter endowment, y , was set to be \$20 dollars in the United States and 500 shillings (approximately \$5.80) in Kenya. Thus all parameters in the voting game in the United States 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 United States and 350 shillings in Kenya. The politician salary was also \$20 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 United States 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 United States 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.²¹

In choosing the level of payoffs in Kenya relative to the United States, 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 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 sessions in the United States and Kenya. 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 United States, 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 the United States 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.²²

²¹ In both cases, the reelection bonus was chosen from a uniform distribution between 0 and the upper limit, rounded to the nearest \$0.25 in the United States and the nearest 10 shillings in Kenya.

²² The endowment in the ultimatum game is also slightly higher in Kenya.

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

Introduction to the voting game Subjects in the United States and Kenya were not provided with identical introductory materials and comprehension questions. In the United States, subjects began with an overview of the game described onscreen 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 60 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.

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 United States and Kenya was in the specification of the choice made by the voter and the politician. As described above, in the United States, 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 onscreen in the United States and orally and using graphics in Kenya. The structure of comprehension questions followed the model described above: in the United States, 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 United States 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 United States, and around 180 minutes in Kenya. In the United States, 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.

APPENDIX 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 subgame 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.

$$E_{ig} = \beta P_{ig} + \varphi_i + \epsilon_{ig} \quad (11)$$

$$E_{ig} = \beta_1 P_{ig}^1 + \beta_2 P_{ig}^4 + \varphi_i + \epsilon_{ig} \quad (12)$$

$$E_{ig} = \beta_1 P_{ig} + \beta_2 All_{ig} + \varphi_i + \epsilon_{ig} \quad (13)$$

$$E_{ig} = \beta_1 P_{ig} + \beta_2 P_{ig} \times Gift_{ig} + \varphi_i + \epsilon_{ig} \quad (14)$$

$$E_{ig} = \beta_1 P_{ig} + \beta_2 P_{ig} \times Lim_{ig} + \varphi_i + \epsilon_{ig} \quad (15)$$

$$E_{ig} = \beta_1 P_{ig} + \beta_2 P_{ig} \times Lim_{ig} + \beta_3 P_{ig} \times Cons_{ig} + \varphi_i + \epsilon_{ig} \quad (16)$$

The results of estimating equations (11) through (16) 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' reelection thresholds, politicians correspondingly increase their expropriation (presumably with approximately no change in their probability of reelection).

APPENDIX D: ADDITIONAL TABLES

Table A1: Subgame Structure

Panel A: Framing and number of payments				
Framing	Number of payments			
	0	1	4	5
Public payments	US and Kenya (2013)	US (2013)	US and Kenya (2013)	US and Kenya (2014)
Public gift	US and Kenya (2013)	US (2013)	US and Kenya (2013)	
Limited information, prior consent	US and Kenya (2014)		US and Kenya (2014)	
Limited information, posterior consent	US and Kenya (2014)		US and Kenya (2014)	
Public payments – larger treasury	US and Kenya (2014, 2015)			US and Kenya (2014, 2015)
Unequal endowments	US and Kenya (2014, 2015)			
Panel B: Subgame numbers				
Public payments	Subgame 1	Subgame 2	Subgame 3	Subgame 4
Public gift	Subgame 5	Subgame 6	Subgame 7	
Limited information, prior consent	Subgame 8		Subgame 9	
Limited information, posterior consent	Subgame 10		Subgame 11	
Public payments – larger treasury	Subgame 12			Subgame 13
Unequal endowments	Subgame 14			

Table A2: Sessions Conducted

Panel A: Sessions and types of subgames by description								
Session type	Subgame I	Subgame II	Subgame III	Location	Sessions	Subjects	Subject-subgames	Subject-decisions
A	No payments	1 public payment	4 public payments	US only	8	78	234	378
B	No payments	1 public gift	4 public gifts	US only	10	102	306	510
C	No payments	4 public payments		Kenya only	6	96	192	288
D	No payments	4 public gifts		Kenya only	6	90	180	270
E	No payments	4 limited, prior consent	5 public payments	US only	11	66	192	252
F	No payments	4 limited, posterior consent	5 public payments	US only	9	60	180	240
G	No payments	4 limited, prior consent		Kenya only	4	60	120	180
H	No payments	4 limited, posterior consent		Kenya only	4	60	120	180
I	No payments	5 public payments (“big pot”)		US, Kenya	13	96	192	192
J1	No payments (“uneq”)	5 public payments		US, Kenya	7	60	120	180
J2	No payments	No payments (“uneq”)	5 public payments	US only	8	48	144	192
Panel B: Sessions and types of subgames by number								
Session type	Subgame I	Subgame II	Subgame III	Location	Sessions	Subjects	Subject-subgames	Subject-decisions
A	Subgame 1	Subgame 2	Subgame 3	US only	8	78	234	378
B	Subgame 5	Subgame 6	Subgame 7	US only	10	102	306	510
C	Subgame 1	Subgame 3		Kenya only	6	96	192	288
D	Subgame 5	Subgame 7		Kenya only	6	90	180	270
E	Subgame 8	Subgame 9	Subgame 5	US only	11	66	192	252
F	Subgame 10	Subgame 11	Subgame 5	US only	9	60	180	240
G	Subgame 8	Subgame 9		Kenya only	4	60	120	180
H	Subgame 10	Subgame 11		Kenya only	4	60	120	180
I	Subgame 12	Subgame 13		US, Kenya	13	96	192	192
J1	Subgame 14	Subgame 5		US, Kenya	7	60	120	180
J2	Subgame 1	Subgame 14	Subgame 5	US only	6	42	126	168

Table A3: Voter Behavior—High Comprehension Sample

	Voter reelection threshold							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Payment	-0.587 (0.176)***	-0.764 (0.191)***			-0.747 (0.168)***	-0.774 (0.191)***	-0.626 (0.206)***	-0.694 (0.228)***
Payment 1			0.115 (0.343)	-0.635 (0.258)**				
Payment 4			-0.809 (0.171)***	-0.837 (0.198)***				
Recipient	1.079 (0.150)***	1.010 (0.164)***			0.951 (0.146)***	0.951 (0.170)***	0.956 (0.177)***	0.958 (0.194)***
Recipient x 1			0.200 (0.234)	0.200 (0.273)				
Recipient x 4			1.345 (0.169)***	1.242 (0.186)***				
All payment					0.440 (0.363)	0.472 (0.244)*		
Payment x Gift							-0.347 (0.372)	-0.199 (0.410)
Recipient x Gift							0.143 (0.311)	0.140 (0.355)
$\beta_1 + \beta_2$	0.492 (0.155)***	0.246 (0.167)					0.330 (0.182)*	0.264 (0.207)
$\beta_1 + \beta_2 + \beta_3$					0.643 (0.335)*	0.648 (0.250)**		
$\beta_1 + \beta_2 + \beta_3 + \beta_4$							0.125 (0.258)	0.205 (0.283)
Mean dep. var.	7.12	7.12	7.12	7.12	7.12	7.12	7.12	7.12
Fixed effects obs.	1966	Subject 1966	1966	Subject 1966	1966	Subject 1966	1966	Subject 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 subgame including any, one, or four vote payments; a dummy for the game 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 subgames 1 – 11 as labeled in Table A1. Asterisks indicate significance at the 10, 5, and 1% level.

Table A4: Voter Behavior—Including Non-Monotonic Subjects

	Voter reelection threshold							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Payment	-0.749 (0.186)***	-0.803 (0.205)***			-0.848 (0.179)***	-0.812 (0.206)***	-0.891 (0.222)***	-0.844 (0.248)***
Payment 1			-0.283 (0.336)	-0.666 (0.248)***				
Payment 4			-0.888 (0.186)***	-0.868 (0.217)***				
Recipient	1.047 (0.148)***	0.990 (0.167)***			0.936 (0.147)***	0.936 (0.172)***	1.004 (0.182)***	0.980 (0.204)***
Recipient x 1			0.278 (0.213)	0.278 (0.250)				
Recipient x 4			1.260 (0.172)***	1.185 (0.194)***				
All payment					0.551 (0.368)	0.495 (0.246)**		
Payment x Gift							0.124 (0.392)	0.125 (0.441)
Recipient x Gift							0.010 (0.306)	0.033 (0.352)
$\beta_1 + \beta_2$	0.298 (0.165)*	0.188 (0.181)					0.113 (0.195)	0.137 (0.219)
$\beta_1 + \beta_2 + \beta_3$					0.639 (0.345)*	0.620 (0.255)**		
$\beta_1 + \beta_2 + \beta_3 + \beta_4$							0.247 (0.282)	0.295 (0.320)
Mean dep. var.	7.17	7.17	7.17	7.17	7.17	7.17	7.17	7.17
Fixed effects obs.	2298	Subject 2298	2298	Subject 2298	2298	Subject 2298	2298	Subject 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 subgame including any, one, or four vote payments; a dummy for the game 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 subgames 1 – 11 as labeled in Table A1. Asterisks indicate significance at the 10, 5, and 1% level.

Table A5: Politician Expropriation

	Expropriation					
	(1)	(2)	(3)	(4)	(5)	(6)
Payment	0.536 (0.248)**		0.422 (0.253)*	0.420 (0.299)	0.593 (0.270)**	0.589 (0.270)**
Payment 1		0.490 (0.329)				
Payment 2		0.545 (0.261)**				
All payment			0.447 (0.438)			
Payment x Gift				0.388 (0.536)		
Payment x Limited Info.					-0.286 (0.377)	-1.099 (0.397)***
Payment x Consent						1.555 (0.661)**
$\beta_1 + \beta_2$			0.869 (0.446)*	0.808 (0.445)*	0.307 (0.362)	-0.511 (0.364)
$\beta_1 + \beta_2 + \beta_3$						1.045 (0.574)*
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 subgame 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 subgames 1 – 11 as labeled in Table A1. Asterisks indicate significance at the 10, 5, and 1% level.