Value for Money? Vote-Buying and Politician Accountability in the Laboratory[*]

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Abstract

Though vote-buying is observed in a diverse set of polities worldwide, relatively little is known about the channels through which it affects subsequent governance outcomes. Using laboratory experiments conducted in the U.S. and Kenya, we show that vote-buying reduces voters' willingness to punish politicians and increases rent-seeking by the incumbent politician. Specifically, we collect data from 816 subjects engaged in a simple voting game in which voters can punish a politician who expropriates rents from a common treasury. Voters who receive "a payment in exchange for your vote" increase the maximum amount they will allow the politician to expropriate while still voting to reelect him. Politicians, in turn, expropriate more when vote-buying is introduced. We provide evidence that social preferences, particularly reciprocity, are an important channel for this response. JEL code: D72

1 Introduction

The ability of political elites to use financial resources to undermine the one-person one-vote principle of democracy – via pork barrel politics, clientelism or straight cash for votes – is widely considered to be an important cause of elite capture of public policy worldwide (Acemoglu and Robinson, 2012; Khemani, 2014). Consistent with this hypothesis, cross-country evidence suggests that vote-buying is detrimental to the perceived functioning of democratic institutions. For example, data from the World Values Survey suggests a negative correlation between the perceived frequency with which rich people buy elections and the level of democratic governance (Figure 1).

Despite the importance of vote-buying, however, relatively little is known about the channels through which it affects policy-making outcomes. One plausible channel is politician selection; if lower quality candidates or candidates with preferences that do not match those of the electorate are more likely or better able to employ cash to garner votes, then vote-buying may lead to a reduction in politician quality. A second, potentially complementary, channel is a decline in politician performance conditional on quality. If the receipt of a payment renders voters less willing to punish a politician for poor performance, then vote-buying may undermine mechanisms of electoral accountability, reducing politicians' incentives to perform well and to minimize rent-seeking.

Disentangling politician quality and politician performance is empirically challenging. In this paper, we provide the first experimental evidence that vote-buying reduces voters' willingness to punish politicians for rent-seeking, employing data from laboratory games conducted in the U.S. and Kenya. We deploy a model of retrospective voting in which subjects choose whether to reelect an incumbent who expropriates rents from a common treasury. The reelection choice thus serves as a tool to punish politicians for excessive expropriation. No other dimension of politician quality influences voters' earnings, and there is no alternative candidate; hence, voters face no selection motive. Voting is secret and costless. In equilibrium, the politician prefers to expropriate (weakly) less than the expected reelection threshold, and hence to be reelected.

We augment this game by introducing vote payments as an external transfer to the voter, described in the simplest, baseline game as "a payment in exchange for your vote." In all games, the politician has no agency in the targeting of payments, and does not pay transfers out of his own endowment; vote payments are distributed to a certain fraction of voters, and the secret ballot is maintained. We examine whether the introduction of vote payments alters subjects' willingness to punish the politician for expropriation by failing to reelect him. If subjects seek only to maximize their own earnings, their behavior should be unchanged following the introduction of vote payments.

Inconsistent with a standard returns-maximizing model of voter behavior, we find that vote-buying does alter the maximum level of expropriation subjects are willing to tolerate while still reelecting the politician. However, when even minimal information about vote-buying is public, subjects' behavior differs depending on whether they receive payments. Relative to the game with no payments, subjects who receive a payment increase the maximum threshold

of expropriation at which they will reelect the politician – i.e., they treat the politician more leniently. By contrast, those who do not receive payments treat the politician more harshly, suggesting potential backlash at their exclusion. In addition, the effect of each payment is larger when more payments are distributed, rendering payments most effective when they are distributed to all subjects.

Moreover, subjects' behavior as politicians is consistent with the patterns observed for subjects as voters. The introduction of vote-buying leads to increased expropriation by politicians, with the largest increases in expropriation observed in precisely those cases in which vote-buying leads to larger shifts in voters' reelection thresholds. This is suggestive of a potential cost of vote-buying in the form of increased rent-seeking.

Do social preferences – in particular, reciprocity – play a role in generating these patterns? Three sources of evidence suggest that reciprocity may be an important channel. First, we experimentally vary game design features that may activate the reciprocal mechanism, particularly the use of secrecy and the solicitation of voter consent, and find that the request for ex ante consent renders subjects more responsive to payments. Second, we demonstrate that experimentally measured social preferences are correlated with the relative effectiveness of vote-buying. Third, we present evidence that cross-country differences in reciprocity between the U.S. and Kenya are consistent with cross-country differences in the effectiveness of payments.

Taken together, our findings – consistent in both a developed and a developing country context – provide robust evidence that vote-buying lowers voters' willingness to punish politicians, and increases rent-seeking by politicians. In addition, by evaluating the effects of vote payments of varying frequency and with distinct framings, we generate empirical findings that are consistent with the widespread incidence of both secret targeted vote-buying and large-scale untargeted gift-giving in the run up to elections.

To the best of our knowledge, we are the first to provide causal evidence on the link between vote-buying and voters' willingness to punish politicians; by doing so, we identify a mechanism that potentially underlies the common observation that voters who have received payments no longer exert effective control over policy decisions (Kitschelt, 2000; Stokes, 2007).[1] Our findings also complement the seminal work of Finan and Schechter (2012), who find that more reciprocal individuals are targeted for vote-buying in Paraguay. Our contribution is to demonstrate that there may be longer term political benefits for politicians who target reciprocal individuals, as these individuals are more likely to alter their subsequent willingness to punish the politician who provided the payments. Moreover, there may be political benefits associated with these transfers even if the payments are not pivotal and thus do not affect the proximate election outcome.

Our findings also contribute to a growing literature in experimental economics analyzing various dimensions of reciprocity. Similar to Abbink et al. (2002) and Malmendier and Schmidt

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[1]In addition, a theoretical economics literature has analyzed the implications of vote-buying, focusing primarily on the capture of legislators or committee members rather than the use of monetary incentives for ordinary voters (Bo, 2007; Dekel et al., 2008; Groseclose and Snyder, 1996; Snyder, 1991). These papers demonstrate that vote-buying can engender inefficient outcomes, though Dekel et al. (2008) shows efficient outcomes are possible if parties place valuations on votes that aggregate the values placed by their supporters.

(2011), we find that subjects respond to the gift itself, even if this response is at the expense of third parties, and even though the gift is transparently provided in order to influence their behavior. Other recent work consistent with our findings includes Pan and Xiao (2014), who report that recipients favor a gift giver over a third party even when the third party has incurred the same cost and signaled the same intention of giving, and Strassmair (2009), who presents evidence that recipients' response to a gift does not significantly vary given experimental variation in the extent to which the gift may arise from selfish motives by the giver. This suggests a more complex view of reciprocity than the classic model of intention-based reciprocity (Rabin, 1993); a model in which subjects respond positively to the kind intention of another subject does not necessarily apply to a context in which gifts are offered partly or exclusively with the intention of influencing the subject's behavior to the benefit of the giver.

Our experimental results on vote-buying suggest that subjects respond robustly to a transfer even when it corresponds to no kind intention – or no intention at all – on the part of the politician, who does not control its distribution. They also respond regardless of whether any *quid pro quo* is specified, and even though that response is costly both to the recipient of the transfer and to other subjects. Thus, even in the context of an experimental design that is not optimized to activate any reciprocal response, the vote payment induces behavior on the part of the voter that decreases his own expected payout. Similar to the argument in Malmendier and Schmidt (2011) that the gift itself creates a bond between the giver and recipient that leads the latter to increase the weight he places on the gift giver's welfare, we suggest that vote-buying increases the weight that voters place on the politician's welfare.

Finally, our findings increase the common ground shared between the literatures on vote-buying and clientelism. Recent papers argue that clientelistic relationships undermine electoral accountability by enabling the elite to easily win the votes of poor citizens and subsequently engage in rent-seeking behavior (Baland and Robinson, 2008; Anderson et al., 2015). One interesting finding in this literature suggests a strong positive correlation between social capital and elite capture of policy-making (Acemoglu et al., 2014). A potential explanation, consistent with our paper, is that clientelistic relationships are also in part sustained by social preferences.

The remainder of the paper proceeds as follows: Section 2 provides some background about forms of vote-buying as observed in the field. Section 3 provides an overview of the model of retrospective voting employed. Section 4 outlines the experimental methods, and Section 5 summarizes the empirical analysis. Section 6 explores channels for the observed effects, and Section 7 concludes.

2 Background

Cross-country data provides robust evidence of the salience of vote-buying; in the 2010-14 World Values Survey, around 40% of respondents stated that voters in their country are bribed or rich people buy elections fairly often or very often. In 37 countries – including low-income countries and large middle-income countries such as Russia and Brazil – more than two thirds of respondents stated that voter bribery was a frequent occurrence. In addition to cross-country

evidence, a large case study literature around vote-buying based primarily on interviews or other qualitative data is summarized in Schaffer and Schedler (2007), and 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 et al., 2012; Schaffer, 2002; Vicente and Wantchekon, 2009).

Against this background, research around vote-buying has focused primarily on understanding why vote-buying succeeds, and how its impact on voter behavior can be reduced. However, less attention has been devoted to understanding why multiple forms of vote-buying co-exist, and analyzing the potentially multifaceted impacts of vote-buying on governance outcomes. These questions are the focus of this paper.

Broadly speaking, two forms of vote-buying have been identified in the literature. One important manifestation, particularly in Latin America, is targeted transfers in which political brokers interact directly and repeatedly with individuals who they know well. This is, for example, the form of vote-buying analyzed by Finan and Schechter (2012), employing data from Paraguay, and by Nichter (2008), employing data from Argentina.

A second common form of vote-buying is widespread saturation of a neighborhood or community with cash or gifts, without any particular targeting or enforcement. Kramon (2011) provides evidence about vote-buying in Kenya that is consistent with this framework. In his survey of 655 individuals, nearly half reported receipt of cash prior to the 2007 presidential election. Of those, 57% reported receiving the money at a rally and 16% at the market, settings where the presumed degree of targeting is low. Half received money from multiple parties. Nearly 90% of respondents reported they did not believe either the person who provided the payment or anyone connected to a political party could ascertain their true vote.

Similarly, in India observers reported significant vote-buying in state elections in Delhi in 2008 and Bihar in 2010. In both cases, gifts including cash, alcohol and food were distributed to all households in targeted neighborhoods on the night before the election. The distribution was managed by youth from the community who are known to the recipients, but who do not have strong social ties to them (Banerjee et al., 2011). Breeding (2011) also reports that in India, parties often target vote payments to whole communities, rather than individuals.

In 2014, the authors administered a brief exit survey to 111 subjects included in experimental sessions in Nairobi. 46% of them reported that they had received cash or gifts from a politician or a politician's representative in the last presidential election (in 2013); more broadly, they estimated that two thirds of Kenyans received such a gift or transfer. Out of those who reported receiving a transfer, 80% said the individual who provided it was previously known to them, but only 20% stated that they interacted again with this individual after the transfer exchange.

In 2015, we also surveyed 400 households in rural Bangladesh about their vote-buying behavior. Around 30% of respondents reported payment in cash or in kind in the last election. However, in this context 25% of respondents reported they had rarely interacted with the agent who offered the payment prior to receiving it, and 55% reported they rarely interacted with the person after the payment. Both sources of evidence seem consistent with limited targeting and

limited enforcement.

Turning to the relationship between vote-buying and policy, some indirect evidence on this point can be drawn from a growing experimental literature focusing on strategies to reduce the effectiveness of vote-buying. Vicente (2014) demonstrates that an anti-vote buying campaign in Sao Tome and Principe reduces the challenger's vote share, and argues this reflects the fact that vote-buying is the challenger's preferred response to the incumbent's use of clientelistic policies. Hicken et al. (2014) report on a campaign in the Philippines that sought to reduce voters' temptation to sell their votes, and demonstrate that it significantly reduced vote-switching from the candidate ex ante preferred by the voter. Cruz et al. (2015) analyze the effect of providing information about the incumbent's performance to voters in the Philippines, and find these voters are subsequently targeted more for vote-buying.

In these papers, the effect of the intervention is framed primarily as a shift in the composition of the votes, and thus the identity of the winning politician. However, there may be other effects; payments may have a persistent effect on voters' preferences beyond the targeted election. In addition, if vote payments increase voters' willingness to tolerate poor performance or rent-seeking by politicians, then payments may have an effect on governance outcomes even if the identity of the winning politician is unaffected. This potential relationship between vote-buying and reduced politician accountability will be the focus of this paper.

3 Modeling Retrospective Voting

In this section, we describe the laboratory voting game (without and with vote-buying) and clarify how subject preferences shape the game outcome.

3.1 Basic Voting Game

We analyze subject behavior in a one-shot voting game in the laboratory. A polity is constituted by six subjects, of whom one is assigned the role of politician and the other five are assigned the role of voters. Each voter receives a country-specific endowment y ($20 in the U.S., and 500 shillings or approximately $6 at the prevailing exchange rate in Kenya) and is informed that this endowment will be 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 available for expropriation by the politician; thus 15% of each voter's original endowment is vulnerable to expropriation. To discourage subjects from simply punishing any non-zero expropriation, we also define $\kappa = 0.1y$ as a transition fee paid by all voters if the politician is not reelected. Voters' final earnings consist of the untaxed portion of their endowment and non-expropriated treasury revenue redistributed at the end of the game, minus $0.1y$ if the politician is not reelected.

The politician receives a salary y in period one, but is informed that he will forfeit half this salary ($0.5y$) if he fails to win reelection. Framing the gain from reelection as a potential loss is intended to increase the salience of the lower payout. Re-elected politicians also receive a small bonus of randomly determined value ϵ, drawn uniformly from the interval $[0, 0.1y]$.

In summary, the payoffs of subjects as voters and politicians, $Earn^v$ and $Earn^p$, are:

$$Earn^v = y - \frac{Exp}{5} - .1y(1 - D_{Reelect}) \tag{1}$$

$$Earn^p = .5y + Exp + D_{Reelect}(.5y + \epsilon) \tag{2}$$

Exp denotes the quantity expropriated by the politician, bounded between 0 and .75y, and $D_{Reelect}$ is a dummy equal to one if the politician is re-elected.

Each subject specifies a choice as a voter and a politician in the following order; no information about other subjects' choices is provided at this point.

- As a voter, the subject specifies the maximum amount he would allow the politician to expropriate and still vote to reelect him.

- As a politician, the subject specifies the amount he would expropriate.

Ultimately, if the subject selected to be the politician expropriates an amount less than or equal to the median of the reelection thresholds specified by the voters, then he is reelected. More details about how subjects are assigned to roles and paid on the basis of the reelection outcome are provided in Section 4.

Multiple game features minimize the probability that subjects perceive any dimension of politician quality other than the level of expropriation as relevant to their voting decision. The experimental design provides the politician a single choice of how much to expropriate. There is no other dimension of quality through which the politician can affect voter earnings. The politician's identity is unknown, and the game ends immediately after the reelection decision.

3.2 Equilibrium

Best response strategies are straightforward for a subject seeking to maximize his own earnings: as a politician, he should expropriate an amount equal to his expectation of the median threshold among the other five subjects (voters). As a voter, the probability that his vote is pivotal in determining the politician's reelection is low but non-zero; if he is pivotal, he can maximize returns by setting his threshold equal to (or greater than) the expected level of expropriation by the politician, to avoid incurring the transition fee. There are accordingly multiple subgame perfect equilibria in pure strategies: for any expropriation level strictly greater than $5, there is an equilibrium in which politicians expropriate exactly some amount Exp^*, and voters vote not to reelect if the politician expropriates any more than Exp^*.

The game thus constituted affords purely returns-maximizing subjects few choices of interest. Because a voter's reelection threshold is not revealed prior to the politician's choice, the voter cannot use this choice to discourage expropriation ex ante. He can only punish the politician ex post, a choice made costly by the transition fee.

However, subjects may well also have other-regarding preferences; we capture these prefer-

ences employing the following stylized objective function.

$$\max \eta_1 E_i + \eta_2 |-\Delta^{pol} E_i| + \eta_3 \sum_{i=1}^{N} E_i + \eta_4 D_i E_i^{pol} \tag{3}$$

Specifically, the subject maximizes a weighted sum of own earnings E_i, the inverse of earnings inequality as captured by the difference between politician and average voter earnings $|-\Delta^{pol} E_i|$, joint earnings realized by all subjects $\sum_{i=1}^{N} E_i$, and politician earnings E_i^{pol}. The final term is interacted with a dummy variable D_i, equal to one if the subject receives a vote payment; i.e., it is assumed the subject does not value the politician's payout in the absence of a vote payment. Vote payments will be described in more detail in the next subsection;/.

Our analysis abstracts from the question of whether the voter is pivotal. Put differently, we assume that voters' choices primarily reflect their desire to express a preference, independent of the probability that this preference will be decisive (Brennan and Lomasky, 1993; Brennan and Hamlin, 1998). Hence, we model subjects as choosing a threshold for reelection that is reflective of their objective function. We can therefore analyze comparative statics with respect to the parameters of the objective function.

Subjects with higher η_2 (i.e., who are more averse to inequity in earnings) will generally set lower thresholds as voters, and expropriate less as politicians.[2] This is perhaps the most intuitive case of subjects punishing a politician for excessive expropriation, despite the fact that this choice is costly. This pattern would be consistent with a large body of experimental evidence suggesting subjects are willing to utilize costly strategies to punish other players for perceived unfairness.[3]

Subjects with higher η_3 (i.e., who place more weight on maximizing total subject earnings) will set higher thresholds for re-election as voters. At the extreme, they could allow the politician to expropriate an unlimited amount to ensure that he is reelected and thus earns the $10 reelection bonus from the experimenter. As politicians, they should expropriate a lower amount. Finally, subjects who receive payments will set higher thresholds as voters, unless $\eta_4 = 0$ and the subject has no reciprocal motivations; subjects with higher η_4, who are more reciprocal, will be particularly responsive to payments, increasing their reelection thresholds more steeply. We will present evidence that subjects' choices as voters and politicians are broadly consistent with these comparative statics in Section 4.3.

3.3   Introducing Vote-Buying

In our experimental sessions, all subjects first play the basic voting game, and are subsequently engaged in the same game incorporating vote payments. The payment size is set at $0.1y$ or 10% of the voters' endowment ($2 in the U.S., and 50 shillings in Kenya). The experimenter,

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[2]The exception is if voters expect politicians to expropriate only one or two dollars, in which case inequity is minimized at any threshold that allows for reelection. If the politician expropriates nothing, any voter threshold leads to a zero gap between voter and politician earnings.

[3]Fehr and Gächter (2000) provides a useful summary of this large literature, noting that subjects in the ultimatum game, for example, generally reject offers of less than 30% of the available endowment.

not the politician, chooses who receives the payment, and vote payments are funded separately (i.e., not drawn from the voters' or the politician's endowment).

We implemented four framings and two substantive variations of the vote payments game.[4]

A *Public payment*: Subjects are informed that some subjects will receive a "payment in exchange for your vote", and are informed of the number and value of the payments. This framing was implemented with one, four, and five payments.

B *Public gifts*: Subjects are informed that "one (four) voter(s) will receive a gift of \$2. This gift does not come from the treasury." Again, all subjects are informed about the number and value of the "gifts". This framing was implemented with one and four payments.

C *Limited information, prior consent*: No information is provided about the number, size or nature of payments; the game description states that some voters may receive payments in exchange for their votes. Prior to choosing reelection thresholds, subjects are asked if they would accept a payment, if offered. This framing was implemented with four payments.

D *Limited information, no prior 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 each subject specifies his reelection threshold, he is asked whether he would have accepted the payment. This framing was implemented with four payments.

E *Big pot*: The fraction of the treasury vulnerable to expropriation by the politician (λ) is increased from 0.3 to 0.5. This framing was implemented with five payments.

F *Unequal endowments*: Subject endowments are rendered unequal ex ante to mimic the wealth distribution induced by the vote payments; i.e., subjects have an endowment of either \$20 or \$22 (parallel to their endowment if they had received a \$2 vote payment). The standard voting game is played, without reference to vote payments.

4 Experimental Methods and Data Description

4.1 Laboratory Procedures

Between 2013 and 2015, we conducted experiments at the Harvard Decision Science Lab in Cambridge, MA and the Busara Experimental Laboratory in Nairobi, Kenya. Subjects were recruited through the laboratories' centralized databases, and each subject participated in only one session. In the U.S., 450 subjects participated in 62 sessions, and a typical session consisted of 12 subjects; only around 5% of subjects participated in a session with six subjects. In Kenya, 366 subjects participated in 24 sessions, and each session consisted of 12 or 18 subjects. All experimental sessions were programmed using zTree. Table C2 provides more details on the sample and session structure.

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[4]Some combinations of framing and number of payments were omitted due to resource constraints.

At the beginning of the experimental session, each subject was seated at a separate computer terminal, and played a set of social preference games. This included the dictator, trust and ultimatum games; detailed protocols are provided in Appendix A.

Next, subjects were engaged in multiple iterations of the voting game; we denote each iteration as a game round. A session included two to three independent game rounds. Subjects made new decisions in each round, and could not revisit decisions made in previous rounds.

The first game round began with an overview of the simple voting game, with no reference to vote payments. The instructions emphasized that subjects would make choices as both the voter and the politician, and would be assigned to a game role (and paid on the basis of their choices in that role) at the conclusion of the session. Subjects completed a comprehension quiz, and were required to review the correct responses to the comprehension questions before proceeding. Each subject then specified his choice as a voter, answering the question, "what is the maximum amount you would allow the politician to expropriate and still re-elect him?" He also stated his expectation regarding how much the politician would expropriate. The subject then specified his choice as a politician, answering the question, "what is the amount you would expropriate from the treasury?". He also specified whether he expected to be re-elected.[5]

In all experimental sessions excluding the limited information sessions, the next game round began with an overview of the voting game including payments, followed by a second comprehension quiz. Two key points were highlighted: first, that money for vote payments was separate from subjects' endowments and the politician's salary, and second, the vote remained secret and anonymous even after the introduction of payments. In addition, the number and value of payments to be distributed were fully detailed. In the limited information sessions, this entire introduction outlining the role of payments was omitted, and subjects moved directly to specify their in-game decisions.

To maximize power, each subject was asked to specify his reelection threshold with and without payment. Subjects are presented with the following language: "suppose you are a voter and that you have received \$2 in exchange for your vote. What is the maximum amount you would allow the politician to expropriate and still re-elect him/her, given that you received \$2?" To elucidate preferences in the case of non-payment, a parallel question is posed: "suppose you are a voter and that you have not received \$2 in exchange for your vote. What is the maximum amount you would allow the politician to expropriate and still re-elect him/her, given that you have not received \$2?" The order in which these questions were posed varied; an analysis of this variation can be found in Section 5.1.[6] Finally, subjects specified their choices as politicians, responding to the same question posed in the previous game: "what is the amount you would expropriate from the treasury?" They also specified whether they expected to be reelected.[7]

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[5]The specific wording of these questions follows the games employed in the U.S. sessions. Minor differences between the U.S. and Kenya sessions are detailed in Appendix B. It is also useful to note that the questions do not refer to the possibility that the subject as voter is not pivotal. Thus in some sense we prime subjects to vote expressively, rather than strategically.

[6]The question order never varied across different game rounds for the same subject in the same session. A single subject faced only one ordering of the payment questions.

[7]The questions posed about subjects' expectation of the game outcome were not incentivized.

In the U.S., subjects generally played two vote-buying game rounds, and the number of payments varied across game rounds. In Kenya, subjects played a single vote-buying game round. The experimental sessions concluded with subjects completing a brief questionnaire on their demographic characteristics and political experiences.

Each subject's compensation was based on his choices in one randomly selected role (politician or voter, and voter who did or did not receive a payment), during a randomly selected game round. During the session, subjects were regularly reminded that any choice could affect their final earnings. As an example, consider a session with two game rounds: the voting game without payment, and the voting game with four payments. In each round, subjects specify their choices as both voters and politicians without any information about other subjects' choices. At the conclusion of the session, subjects receive the following information.

- The game round randomly chosen as the basis of payment: either the voting game without payment, or the voting game with payment.

- The game role to which they were randomly assigned: a politician, a voter who did receive a payment, or a voter who did not receive a payment.[8]

- The game outcome: how much the politician expropriated, subjects' reelection thresholds, and whether the politician was reelected. (For subjects selected to receive payments, the thresholds they specified conditional on payment are used to determine whether the politician is reelected. For subjects not selected to receive payments, the thresholds they specified unconditional on payment are employed.)

- Subjects are then informed of their individual earnings, and receive payment.

The unequal endowments game round followed a slightly different structure: rather than specifying choices with and without payment, each subject specified what threshold he would set for the politician if his endowment was \$22, and if his endowment was \$20. Each subject was then asked to specify how much he would expropriate as the politician.

The use of the strategy method to elicit hypothetical subject responses is often described as "cold" decision-making, in contrast to "hot" decision-making in which subjects respond directly to another subject's choice. In general, the strategy and direct-response method yield similar results, though there is some evidence that the strategy method results in lower levels of punishment (Brandts and Charness, 2011). Given that our primary specification entails comparisons across games played by the same subject, all using the strategy method, we do not regard this as a significant source of bias.

The six session types were implemented with some minor country-specific variations, described in Appendix B.[9] To ensure that players face uniform choices, the relative ratios of game

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[8]In experimental sessions with more than six subjects, subjects were randomly constituted into polities of six prior to randomly assigning game roles.

[9]We denote country-specific session types by numbers. For example, session type A corresponds to public payments; session type A1 was implemented in the U.S., and session type A2 in Kenya. A given session can have up to three game rounds, denoted "game round I", "game round II", and "game round III". Appendix Table C1 summarizes the session types implemented and the game rounds included in each session.

parameters were fixed across experimental sites. In addition, the framing of a payment was uniform in a given session: any variation in the framing of the payment is across sessions, and thus across subjects. However, the fact that subjects in the U.S. played the voting game with payments multiple times with different numbers of payments creates both within-session (within-subject) and cross-session (cross-subject) variation in the number of payments.

Minimizing subject learning Typically, in retrospective accountability models voters set a reelection threshold that is revealed to the politician, who then chooses his level of expropriation. 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 is to minimize opportunity for learning during a session, as subjects move from one game round to the next and vote payments are added (or their number is altered). Publicizing voters' reelection thresholds to the subjects (or even only to the politician) would enable subject learning, regardless of whether politician identity is fixed across rounds.[10] While these choices have costs - they render our game less comparable to the literature and create multiple equilibria - they also allow us to estimate the effects of payments using a within-subject design while minimizing the confounding effects of subject learning.

4.2 Data and Descriptive Statistics

Again, our sample includes 450 subjects from the U.S. and 355 from Kenya, characterized by an average age of 33. In the U.S., the subject pool is equally divided by gender, overwhelmingly unmarried, and highly educated. In Kenya, the subject pool is 65% female and has an average of eleven years of education; half are married. Levels of political engagement are high in both subject pools, though higher in Kenya: 72% of U.S. subjects report voting in the last presidential election, while 86% of Kenyan subjects did so. Table C3 in the appendix provides details.

In our analysis, the unit of interest will be the subject-decision. In game rounds with zero or five payments, the subject makes a single decision as a voter, specifying the reelection threshold for the politician. He also makes a single decision as a politician. However, in game rounds with one or four payments, the subject makes two decisions as a voter — the reelection threshold conditional on a payment, and the reelection threshold unconditional on a payment — in addition to a single decision as a politician.

To elicit voters' re-election thresholds, U.S. subjects were asked to specify the maximum amount they would allow the politician to expropriate and still re-elect him, naming any integer between zero and $15, inclusive. In Kenya, we sought to increase comprehension by posing a series of binary choices. Subjects were asked whether they would reelect a politician who expropriated a specified amount (0, 75, 150, 250, 300 or 375 Ksh). In Appendix B.3, we describe

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[10]If the politician were chosen at random in each game round, then some subjects would have prior information about voters' choices in the last round. If politician identity were fixed, then repeated iterations of the game would create the opportunity for learning by the politician about voters' choices, and other subjects would be aware that the politician could re-optimize his expropriation decision in subsequent rounds. In either case, the dynamic effects would confound any effect of variations in game design from one game round to the next.

how we construct a linear variable comparable to the U.S. subjects for the Kenyan data. We drop roughly 7% of observations corresponding to subjects who stated that they would not reelect a politician expropriating a lower amount, but would reelect a politician expropriating a higher amount. This leaves a sample of 755 subjects. (We will also demonstrate that our primary results are robust to the inclusion of subjects exhibiting this form of non-monotonic behavior.)

Panel A of Table 1 reports summary statistics for these observations, where Kenyan subject choices are rescaled to lie on the 0 to \$15 scale employed in the U.S..[11] The average voter reelection threshold in the pooled sample is the equivalent of \$7.33, with significantly higher voter thresholds in the U.S. (\$7.70) than Kenya (\$6.59). Kenyan subjects are also significantly more likely to set their reelection threshold at zero. Again, a higher threshold suggests a greater tolerance by subjects as voters of expropriation by politicians.[12] At \$8, the average level of politician expropriation is above the average voter threshold; this is again significantly higher in the U.S. (\$8.39) than in Kenya (\$7.21).

Panel B of Table 1 reports social preferences, elicited prior to the introduction of the voting game.[13] 66% of U.S. subjects send a positive amount to a partner in the dictator game, compared to 81% of Kenyan subjects. By contrast, U.S. subjects are more likely to send a positive amount in the trust game, but conditional on receiving a positive amount, Kenyan subjects are more likely to return a non-zero amount. These differences are all significant at the one percent level. Kenyan subjects thus seem to show evidence of greater altruism and reciprocity, though they may be somewhat less trusting.

We also construct an additional index of reciprocity Rec_i by calculating the percentage of funds received that a subject would return to the sender in the trust game if 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 he received less than 50%, denoted $Perc_i^{low}$. Rec_i is defined as $Perc_i^{high} - Perc_i^{low}$, censored at zero. In the 2013 sessions and in Kenya, a simpler trust game was employed in which the sender has the choice only to send all or nothing; accordingly, Rec_i can be constructed only for the 2014 U.S. sessions. The mean index of reciprocity is .06.[14]

Consistent with the lower observed expropriation by subject-politicians and a greater willingness to punish expropriation, Kenyan subjects exhibit greater inequality aversion. At 5.72 out of a maximum of \$10, the minimum transfer that Kenyans would accept from a partner in the ultimatum game is roughly double the reported threshold for US subjects (\$2.91). Again, the real endowment in Kenyan sessions was lower, but subjects' choices are normalized relative

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[11]In addition, choices made by subjects in big pot games are re-scaled to lie on the same scale from 0 to \$15.

[12]For graphical evidence about different subject choices in the U.S. and Kenya, Figure C1 shows the corresponding kernel densities and histograms of subject choices as voters in the U.S. and Kenya.

[13]The sample includes 653 subjects (372 in the U.S. and 281 in Kenya); this includes all subjects other than those included in session type F, unequal endowments, for whom social preferences are not observed. In the U.S., these choices were not incentivized, and thus subjects were not paired with a partner in order to calculate a payoff. In Kenya, these choices were incentivized, and subjects were paired with a partner and informed of their payoff. This payoff was added to their earnings in the voting game.

[14]Interestingly, this is extremely close to the average level (.04) reported by Finan and Schechter (2012) for their Paraguay sample.

to the endowment in the U.S. sessions.

4.3   Is Our Model of Subject Behavior Reasonable?

The two key outcomes of interest in this analysis are the reelection thresholds specified by subjects as voters, and the expropriation levels specified by subjects as politicians. We begin by examining whether subjects' choices as voters and politicians in the simple voting game are broadly consistent with our postulated model of subject preferences.

We estimate OLS regressions in which the two outcome variables are regressed on measures of subject beliefs and preferences; all specifications include session fixed effects and standard errors clustered at the session level. We also include comprehension index fixed effects, where the comprehension index is defined as the percentage of comprehension questions answered correctly by the subject on the first attempt. Table 2 reports the results; the odd and even columns employ the voter's reelection threshold and politician expropriation, respectively, as dependent variables.[15]

We first examine whether subjects' reelection thresholds increase if they believe the politician will expropriate more. Conditional on the politician's choice, voters' earnings are higher if the politician is reelected; thus if subjects expect higher levels of expropriation, they should optimally increase their thresholds for reelection, and we observe this pattern in Column (1). (In the U.S., subjects do not directly report their expectation of politician expropriation; accordingly, this relationship can be estimated only for Kenyan subjects.)

Turning to subjects' choices as politicians, returns-maximizing subjects should exhibit non-linearity in the relationship between their choice of expropriation and the expected median voter threshold. If the threshold for reelection is below four, the politician can maximize earnings by expropriating the full amount available and forfeiting reelection; a politician who expropriates the maximum amount can earn \$25, while a politician who is reelected given such a low threshold will earn no more than \$24. We employ each subject's own choice of threshold as a voter as a proxy for the threshold that he expects he will face as a politician, and regress the subject's level of expropriation on the threshold and a dummy equal to one if this threshold is less than or equal to four. Column (2) shows that politicians expropriate more if they expect a higher threshold, but also expropriate more if they expect the threshold to be extremely low.

Next, we consider social preferences. We first analyze altruism, proxied by a dummy variable equal to one if the subject sends a non-zero amount in the dictator game. If more altruistic subjects place higher weight on total subject earnings (higher η_3), as voters they should set higher thresholds, minimizing the risk of the lost reelection bonus for the politician; conversely, more altruistic politicians should expropriate less. We see these predictions borne out in Columns (3) and (4). Next, we consider trust, proxied by a dummy variable equal to one if the subject sends a non-zero amount in the trust game. We expect trust and altruism to have similar

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[15]We have social preferences data for 641 subjects, including 281 Kenyan and 360 US subjects. 102 of the overall subject population of 755 are excluded because they participated in session type F (unequal endowments) in which social preference games were not administered, and 12 subjects are excluded because the no-payment game was unintentionally dropped in their experimental session.

impacts on voter and politician choices; the expected positive relationship between trust and voter thresholds is observed in Column (5), but the relationship between trust and politician expropriation reported in Column (6) is insignificant.

Finally, we consider inequality aversion, defined as a dummy variable equal to one if the minimum amount the subject reports a willingness to accept in the ultimatum game is greater than or equal to half the endowment. Subjects who are averse to inequality (higher η_2) should set lower reelection thresholds as voters, and expropriate less as politicians. In Column (5) we observe the postulated negative relationship between inequality aversion and the reelection threshold, but the relationship between expropriation and inequality aversion in Column (6) is insignificant. Inequality aversion is observed only in the 2014 experimental sessions.[16]

As previously noted, there are multiple pure strategy equilibria in this game: for any expropriation level strictly greater than \$5, there is an equilibrium in which politicians expropriate exactly some amount Exp^*, and voters vote not to reelect if the politician expropriates any more than Exp^*. We can empirically test whether subjects are employing these best-response strategies. Subjects set low thresholds as voters (\$7.33), but report a significantly higher expectation of the amount other subjects will expropriate as politicians (\$8.16); this is inconsistent with Nash play. If their own choices as politicians are viewed as a reasonable proxy for their expectations, the same pattern is observed: subjects also expropriate more as politicians (\$8.00).

5   Vote-Buying and Subject Behavior: Empirical results

5.1   Voter Response to Payments

To examine how vote payments affect subjects' reelection thresholds, we estimate:

$$T_{idgs} = \beta_1 R_{idgs} + \beta_2 P_{gs} + \phi_i + \epsilon_{idgs} \tag{4}$$

T_{idgs} is the reelection threshold specified by subject i making game decision d in game round g in session s. R_{idgs} is a dummy equal to one if subject i's decision is conditional on the receipt of a payment, and P_{gs} is a dummy equal to one if the game round includes vote payments. All specifications are estimated with and without subject fixed effects ϕ_i, and standard errors are clustered at the session level. Specifications without subject fixed effects also include a Kenya dummy, a control variable for the order in which questions about vote payments are posed, and comprehension index fixed effects. The sample employed for each specification is noted both in the text and in the table notes; again, Table C1 in the Appendix includes an overview of all game sessions and game rounds played.

Table 3 reports the results, employing in the initial sample all session types except "big pot" and "unequal endowments".[17] We observe a significant and positive coefficient β_1, demonstrat-

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[16]At very low levels of politician expropriation, a higher reelection threshold leads to increased equity in subject earnings; this is true if the politician expropriates less than 2. If the politician expropriates zero, then the reelection threshold does not affect equity in payouts. There is no significant change in these estimates if subjects who report an expectation that the politician will expropriate a very low amount are excluded.

[17]This is a sample of 2136 subject-decisions. The unequal endowment session types, F1 and F2, also include

ing that subjects who receive a payment increase their reelection thresholds relative to subjects who do not receive a payment (Columns 1 and 2). By contrast, β_2 is negative, suggesting a "backlash effect": subjects playing a voting game with payments who do not receive a payment are harsher in their treatment of the politician, lowering their reelection thresholds. The sign and significance of 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.[18] The bottom row reports the sum of β_1 and β_2, positive and significant at the 10 percent level when subject fixed effects are included.

Next, we estimate a more complex specification.

$$T_{idgs} = \beta_1 R_{idgs} \times P_{gs}^1 + \beta_2 R_{idgs} \times P_{gs}^4 + \beta_3 P_{gs}^1 + \beta_4 P_{gs}^4 + \beta_5 All_{gs} + \phi_i + \epsilon_{idgs} \tag{5}$$

The specification includes the recipient dummy variable interacted with the dummy variables P_{gs}^1 and P_{gs}^4, denoting game rounds in which payments are distributed to one and four subjects, as well as the dummy All_{gs}, equal to one for game rounds in which all subjects receive payments. Again, this specification is estimated with and without subject fixed effects.

The results in Columns (3) and (4) show that β_1 is insignificant, while β_3 and β_5 are significant and positive: i.e., recipients are responsive to payments only if at least four voters receive them. In addition, the net effect of a payment is increasing in the number of payments distributed. The bottom rows of report the linear combinations $\beta_1 + \beta_3$ and $\beta_2 + \beta_4$, capturing the net effect of a payment when one payment and four payments are distributed, respectively. The net effect of a payment in the one-payment game is negative and insignificant. The net effect of a payment in the four-payment game is positive and significant, of magnitude around .4, and the coefficient β_5, capturing the effect of payments when distributed to all subjects in the polity, is close to .7 and precisely estimated.[19] Put differently, a vote payment of $2 leads to no shift in the amount the recipient voter(s) are willing to allow the politician to expropriate if only one voter receives the payment, an increase of about 40 cents in games in which four voters receive payments, and an increase of about 70 cents in games in which all voters receive payments.

Next, we include interaction terms with the gift framing, yielding the following equation again estimated with and without subject fixed effects.

$$T_{idgs} = \beta_1 R_{idgs} + \beta_2 P_{gs} + \beta_3 R_{idgs} \times Gift_{gs} + \beta_4 P_{gs} \times Gift_{gs} + \phi_i + \epsilon_{idgs} \tag{6}$$

The gift framing dummy variables, in addition to the other framing dummies analyzed subsequently (limited information and prior consent), are defined equal to one for all game rounds in

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some game rounds with five payments. For clarity of the within-subject comparisons, however, data from session types F are omitted. The results are consistent if this data is also included.

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

[19]The hypothesis that the net effect of a single payment, $\beta_1 + \beta_3$, is equal to the effect of five payments, β_5, can be rejected in both specifications, with and without subject fixed effects. The hypothesis that $\beta_2 + \beta_4 = \beta_5$ cannot be rejected, though p=.11 for the specification employing subject fixed effects.

a given session; Table C4 clarifies how dummies are coded.[20] Accordingly, there is no within-subject variation in the gift dummy, and it is omitted from the specification including subject fixed effects.

The results in Columns (5) and (6) show that the interaction terms are insignificant and of small magnitude, demonstrating no heterogeneity with respect to the specification of a quid pro quo for the payment. The sum $\beta_1 + \beta_2$ is again reported at the bottom of the table, and is of comparable magnitude to the linear combinations estimated in Columns (1) and (2).

Finally, we examine whether voters' responsiveness to payments diminishes when they stand to lose more from the politician's expropriation. We expand the sample to include the "big pot" game sessions, in which the fraction of the treasury vulnerable to expropriation was increased from 35% to 50%. The voting game was then played without any payments and with five payments distributed, while the vote payment remained fixed at \$2. We estimate:

$$T_{idgs} = \beta_1 P_{gs} + \beta_2 R_{igds} + \beta_3 All_{gs} + \beta_4 All_{gs} \times Big_{gs} + \phi_i + \epsilon_{idgs} \tag{7}$$

Big_{gs} is equal to one if the session includes a "big pot" vulnerable to expropriation.

Columns (7) and (8) of Table 3 show that the interaction between all payments and the big pot dummy is negative and large in magnitude, but statistically insignificant. This may reflect the fact that the higher losses from politician expropriation are not sufficiently salient. The net effect of a payment in a big pot game is reported as the coefficient sum $\beta_1 + \beta_1 + \beta_3 + \beta_4$; it is positive but insignificant, as contrasted with the robust evidence in Columns (3) and (4) of a significant effect of payments when distributed to all subjects in the context of smaller potential losses from expropriation.

Overall, the observed subject response to payments is inconsistent with a standard returns-maximizing model of voter behavior, and instead suggests that reciprocal behavior may dominate for subjects who receive a payment. In Section 6, we present further evidence that is consistent with the importance of reciprocity in shaping subjects' response to payments.

5.2 Politician Expropriation

Do politicians' expropriation choices respond to vote-buying in a manner that is consistent with the observed voter response? To examine politician behavior, we estimate the following regression:

$$Exp_{igs} = \beta_1 P_{gs} + \phi_i + \epsilon_{igs} \tag{8}$$

The variable of interest is denoted Exp_{igs}, capturing the amount expropriated by subject i as a politician in game round g in session s. Since each subject acting as a politician makes only a single decision in each game round, we omit the subscript d. Parallel specifications will be estimated including dummy variables for various numbers of payments and alternate framings.

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[20]The only exception is for the all payment game rounds that occur following limited information game rounds in the U.S. sessions, types C1 and D1; since information is fully revealed in these game rounds and no consent is sought, the limited information and consent dummies are defined to be equal to zero for these game rounds.

Table 4 presents the results. In Column (1), we can observe that the amount expropriated by the politician increases by \$.45 when vote payments are introduced, an increase of 6% relative to the mean. In Column (2), the results suggest the level of expropriation is larger when payments are distributed to all subjects, but the difference is statistically insignificant. Expropriation is also somewhat higher when payments are framed as gifts, as observed in Column (3).

In Column (4), we interact with the all payment dummy with the dummy for a large pot vulnerable to expropriation. Interestingly, politicians do not expropriate significantly more when the amount vulnerable to expropriation is increased. This result is consistent with the fact that voters also do not significantly alter their reelection thresholds in this case.

Importantly, the magnitudes of the observed coefficients are consistent with the observed changes in voter behavior. The average increase in the voter threshold when payments are introduced is between \$.30 and \$.50, while the increase in politician expropriation is \$.45. The increase in the voter threshold when all voters receive payments is around \$.70, while the increase in politician expropriation is \$.80. This pattern is also consistent with subjects' beliefs about the probability that they will be reelected as politicians. We observe no significant shift in subjects' reelection expectations once payments are introduced, suggesting that subjects accurately infer that both politician expropriation and voter thresholds are increasing.

Turning to the overall welfare of subjects as voters, voters who do not receive payments are clearly worse off when vote payments are introduced, as the payments lead to increased expropriation. Given that the value of the vote payment is \$2, we do not observe an increase in expropriation that exceeds this level: i.e., subjects who do receive payments are better off when payments are introduced. In the real world, the potential losses due to politician expropriation are presumably several orders of magnitude larger than a typical voter incentive, but budget constraints did not allow us to replicate this gap in the laboratory.

5.3 Robustness Checks

Alternate samples   In the appendix we reproduce the core results around voter and politician behavior reported in Table 3 and Table 4 employing two alternate samples. Tables C5 and C6 expand the sample to include Kenyan subjects who exhibit non-monotonic behavior, and Tables C7 and C8 limit the sample to exclude subjects who score in the bottom decile of game comprehension. In both cases, our results are robust.[21]

We also test for order effects. Controlling for the number of payments, we find no significant variation in subject behavior with respect to the position of a game round in the experimental session. However, we lack variation in the order of the no-payment/payment game rounds; this choice was made deliberately, in order to observe subjects' choices in the no-payment game without any priming. In addition, there is limited variation in whether a game round with a certain number of payments was played second or third.

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[21]There is some evidence in each case that the positive effect net effect of a payment is smaller and not necessarily statistically significant in the pooled sample or in the games in which subjects received four payments; it remains consistently significant for games in which all subjects received payments.

Psychological effects of increased earnings An alternative interpretation of the results is that the subjects' responses as voters simply reflect a reaction to the receipt of a payment that is directed at the experimenter, but externalized via decisions about re-electing the politician. For example, subjects who receive a payment may be gratified and feel more generous; subjects who do not receive a payment may be angry.[22]

To test this hypothesis, we use the "unequal endowments" game. This game is equivalent to the simple voting game without payments, except that four voters have endowments of $22, and one voter has an endowment of $20, parallel to the endowments that are induced in the four-payment voting game. All subjects are informed of this distribution of endowments.

To examine whether high endowment individuals respond in the same way as subjects who receive a transfer designated as a vote payment, we consider the full sample of games excluding big pot (session types A-D and F). In addition, we examine the smaller sample of games in which zero or four payments are distributed, in order to minimize any confounding effects of variation in the number of payments. The specification is also estimated with and without subject fixed effects.[23] The estimation equation can be written as:

$$T_{idgs} = \beta_1 R_{idgs} + \beta_2 High_{idgs} + \beta_3 P_{gs} + \beta_4 Ineq_{gs} + \phi_i + \epsilon_{idgs} \tag{9}$$

The dummy variable $High_{idgs}$ is equal to one if a subject has a high endowment, and zero otherwise; $Ineq_{gs}$ is equal to one for the unequal endowment game rounds. The objective is to test whether the coefficients β_1 and β_2 are identical.

The results in Table 5 show a consistently positive coefficient β_1, and a coefficient β_2 that is small in magnitude and insignificant, suggesting the effect of having a large endowment is not identical to the effect of receiving a payment. The bottom of the table reports the p-values corresponding to the tests $\beta_1 = \beta_2$ and $\beta_1 + \beta_3 = \beta_2 + \beta_4$: i.e., whether the effect of a payment is the same as the effect of a high endowment, and whether the net effect of a payment in a payment game is the same as the net effect of a high endowment in the unequal endowments game. In all four specifications, we can reject both hypotheses, generally at the 5 percent level, suggesting that the effect of receiving a payment is not the same as the effect of a high initial endowment.[24]

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[22]Note it is possible to rule out the hypothesis that subjects are simply seeking to reach some target level of earnings for the experimental 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.

[23]The sample includes game round I (no payments) and any game round including four payments from session types A-F as specified in Table C1: 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 include a Kenya dummy, a control variable for the order in which questions about the receipt of vote payments is posed, and comprehension index fixed effects.

[24]Interestingly, in the games in which four subjects have higher endowments, the reelection thresholds of all subjects seem to increase substantially vis-a-vis the simple game with equal endowments, as observed in the positive coefficient on the dummy for unequal endowments. However, there is no heterogeneity in this pattern with respect to whether or not the subject has individually received the higher endowment. In the games including payments, we do observe such heterogeneity with respect to payment status.

6 Analyzing Social Preferences

Given the evidence previously presented about subjects' responsiveness to payments as voters, we now seek to test the hypothesis that this response is driven primarily by a sense of reciprocity toward the politician. We present three sources of evidence on this point, analyzing heterogeneity across sessions in the experimental design, heterogeneity across subjects in their social preferences, and heterogeneity across the two experimental sites.

6.1 Variation in the Solicitation of Consent

First, we explore how voter responsiveness to payments varies with dimensions of the experimental design intended to activate a more robust reciprocal response: the provision of limited information about the payment, and the solicitation of consent. The objective of re-framing the payment as quasi-secret is to test whether a more private, and therefore more targeted, transaction is more effective in altering subjects' behavior. 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, the payments' value, or their implications.

We distinguish between two types of limited information sessions. First, in the "prior consent" sessions, subjects were asked whether or not they would like to accept a payment, and only then 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.) Second, in the "no prior consent" sessions, subjects were not asked to provide consent before specifying their reelection thresholds in the case of receiving a payment.

Here, we seek to re-frame payments in a context more analogous to the one in which vote payments are usually believed to be most effective: private payments constituting an implicit contract between the politician or the politician's agent and the voter. Consent is not explored in the public information game rounds or the rounds in which all subjects receive payments, given that these games are designed to mimic vote payments that are distributed widely and without targeting (for example, cash handed out at a political rally, or gifts provided to all individuals in a neighborhood). In these contexts, explicit consent is unlikely.

We re-estimate equation (4) adding interaction terms with the limited information framing; the same sample is employed, including all session types except unequal endowments and big pot. The equation of interest, estimated with and without subject fixed effects, is:

$$T_{idgs} = \beta_1 R_{idgs} + \beta_2 P_{gs} + \beta_3 R_{idgs} \times Lim_{gs} + \beta_4 P_{gs} \times Lim_{gs}$$
$$+ \beta_5 All_{gs} + \beta_6 Cons_{gs} + \phi_i + \epsilon_{idgs} \tag{10}$$

Given within-subject variation in the dummy variables All_{gs} and $Cons_{gs}$, these dummies are included even in the specifications with subject fixed effects.[25]

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[25]In the specifications without subject fixed effects, a Kenya dummy, a control variable for the order in which

In Columns (1) and (2) of Table 6, we observe that the coefficients β_3 and β_4 are negative and of sizeable magnitude, though imprecisely estimated. There is no robust evidence that limiting information increases subjects' responsiveness to payments, and the results suggest that in fact it may render them somewhat less responsive. The linear combinations $\beta_1 + \beta_2 + \beta_3 + \beta_4$ are negative and close to zero.

Next, to analyze the effect of prior consent we estimate a specification including interactions with both the limited information and prior consent variables.

$$T_{idgs} = \beta_1 R_{idgs} + \beta_2 P_{gs} + \beta_3 R_{idgs} \times Lim_{gs} + \beta_4 P_{gs} \times Lim_{gs}$$
$$+ \beta_5 R_{idgs} \times Cons_{gs} + \beta_6 P_{gs} \times Cons_{gs} + \beta_7 All_{gs} + \beta_8 Cons_{gs} + \phi_i + \epsilon_{idgs} \qquad (11)$$

In Columns (3) and (4) of Table 6 we observe that β_5 and β_6 are both positive, 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 $\beta_5 + \beta_6$, the increase in the net effect of a payment when prior consent is solicited. $\beta_5 + \beta_6$ is positive, significant and large in magnitude (around 15% of the average voter threshold). This suggests that the net effect of a payment is significantly larger when consent is solicited; this result is also consistent when the sample is restricted to the limited information sessions.

Roughly 80% of subjects indicated they would consent to receive a vote payment. Compared to those who declined to provide consent, we also observe a greater increase in voter threshold among subjects who stated they would accept the payment, though clearly providing consent may be endogenous to other unobserved subject characteristics.[26] Requesting, and obtaining, voters' consent prior to distributing a payment thus appears to render payments more effective in altering subjects' choices.

6.2 Variation with Respect to Social Preferences

In addition, we can directly test for evidence of heterogeneity with respect to variation in social preferences; our model of subject preferences suggests more reciprocal individuals should be more responsive to payments. First, we employ the index of reciprocity Rec_i and re-estimate the primary specification, interacting the recipient dummy with the index of reciprocity. Additional control variables include the recipient dummy interacted with a dummy equal to one if the subject sent a non-zero amount in the trust game, and dummies for the game round including all payments or prior consent. The sample includes all subjects in session types C and D

questions about the receipt of vote payments are posed, and comprehension index fixed effects are also included.

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

observed in the U.S. All specifications are estimated with and without subject fixed effects.[27]

$$T_{idgs} = \beta_1 R_{idgs} + \beta_2 P_{igs} + \beta_3 R_{idgs} \times Rec_i + \beta_4 R_{igds} \times Sent_i$$
$$+ \beta_5 All_{gs} + \beta_6 Cons_{gs} + \phi_i + \epsilon_{idgs} \tag{12}$$

The results are reported in Columns (1) and (2) of Table 7. The interaction with reciprocity is large in magnitude and significant when estimated without subject fixed effects. The estimate in Column (1) suggests that an increase of 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.[28] (The coefficient on the trust game-sending dummy is negative and insignificant, but is not reported for concision.)

In Columns (3) and (4), we report analogous results employing a simpler, linear measure of reciprocity, capturing how much the subject chooses to return when the partner sends all of the endowment in the all or nothing trust game. The estimated coefficient β_3 is again positive but insignificant, suggesting a more nuanced measure of reciprocity may be more effective in capturing this dimension of subject behavior.

6.3 Variation by Experimental Site

A comparison of findings across our two experimental sites provides further suggestive evidence about the relevance of social preferences, and the importance of these preferences in facilitating vote-buying in developing country democracies. Kenyan subjects, on average, demonstrate much greater reciprocity compared to the U.S. subjects when engaged in a simple trust game in which the sender has the option to send all or none of an endowment of $4 or 120 shillings, and whatever is sent is tripled prior to the partner's choice of how much to return. In the case of a positive transfer, Kenyan subjects return on average $.50 more to the sender, and are 14 percentage points more likely to return a non-zero amount. (The magnitudes here are normalized with respect to the endowment in the U.S. sessions.)

Needless to say, this is not the only difference between the two subject pools. Kenyan subjects are also significantly more averse to inequality, have lower levels of education and income, and have significantly greater personal experience of vote-buying. Accordingly, any difference in subject behavior cannot be interpreted as only reflective of variations in reciprocity, but the comparison is nonetheless informative.

To examine heterogeneity in the response to payments across our experimental sites, we estimate the following simple specification, interacting the recipient dummy with a dummy variable K_s equal to one if the session was implemented in Kenya. This specification is again

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[27]The specification without subject fixed effects also includes a control variable for the order of questions about the receipt of vote payment, a linear control for the sent dummy, comprehension index fixed effects, and reciprocity index fixed effects. The reciprocity index is observed only for U.S. subjects.

[28]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.

estimated with and without subject fixed effects.[29]

$$T_{idgs} = \beta_1 R_{idgs} + \beta_2 P_{gs} + \beta_3 R_{idgs} \times K_s + \beta_4 All_{gs} + \beta_5 Cons_{gs} + \phi_i + \epsilon_{idgs} \qquad (13)$$

The results are reported in Columns (5) and (6) of Table 7. Kenyan subjects show a significantly more positive response to a payment: β_3 is positive and significant, of magnitude around .5. This suggests the response to payments is up to 70% larger among Kenyan subjects compared to their U.S. counterparts, consistent with the evidence of greater reciprocity in the Kenyan sample. There is, however, is little evidence of a heterogeneous response to the other, more specific variations of the game; in some cases, the comparison may have limited power given that Kenyan subjects participated in fewer game rounds per experimental session, and thus there is less within-subject variation.

Given that our experiment had only two sites, we must be cautious in interpreting the U.S.-Kenya comparison. However, the direction of this comparative static is consistent with a broader observation in the literature that there is a positive correlation between social capital and elite capture of the policy-making process (Acemoglu et al., 2014; Anderson et al., 2015). Our results suggest one possible reason for this phenomenon may be that strong social capital renders clientelistic mechanisms such as vote-buying more effective, even when vote payments are relatively anonymous and unenforceable.

Returning to the links between our paper and the growing literature on the complexity of reciprocity, in this context we see evidence of a reciprocal response that is activated by a transfer even when the ostensible giver (the politician) had no agency in the targeting of the transfer, and even though this response is costly to the recipient as well as to other subjects (other voters). These results complement previous evidence in the literature that reciprocity may be a much broader phenomenon than originally hypothesized. In addition, the evidence earlier presented that each individual payment is more effective when more payments are distributed could reflect a greater response by recipients if the transfer is viewed as part of a broader or more meaningful gesture by the gift-giver (politician). Alternatively, the payments may simply be more salient in the games in which more payments are distributed.

7 Conclusion

Vote-buying is an important phenomenon in polities around the world. Despite its prevalence, however, relatively little is known about the channels through which it may affect voter behavior, beyond the effect on the selection of elected officials.

This paper analyzes evidence about voter behavior under a regime of vote-buying in the laboratory. A simple model of retrospective voting was augmented with vote payments and tested with 816 subjects in the Harvard Decision Science Laboratory and the Busara Experimental Laboratory in Nairobi, Kenya. The results suggest that voters who receive payments

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[29]The specification without subject fixed effects also includes a Kenya dummy, a control variable for the order in which questions about the receipt of vote payments are posed, and comprehension index fixed effects.

are less willing to punish a politician for expropriation of rents. At the same time, we observe greater expropriation by subjects as politicians when vote payments are introduced. Taken together, these findings suggest that vote-buying may result in a decline in politician performance, conditional on politician quality.

However, the responsiveness of subjects as voters is not uniform. More specifically, we observe the largest response to vote payments when the payments are distributed to all subjects, or when the payments are distributed in relative secrecy with the subject's prior consent. These findings are consistent with the prevalence of *both* broad-based gift-giving by political parties and secret vote-buying with explicit consent. They also suggest the importance of enhancing other, non-electoral mechanisms of politician accountability in new democracies.

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8   Figures and Tables

Figure 1: Evidence from the World Values Survey
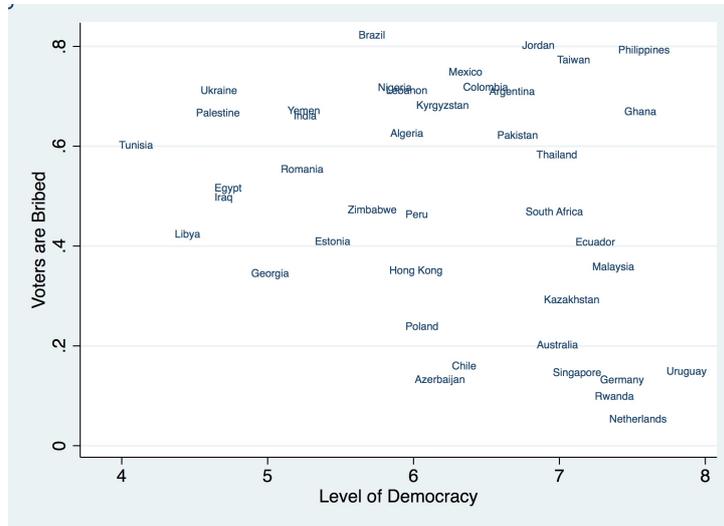
Notes: The variable on the y-axis correspond to the mean subject response to the following question: in your view, how often do these things occur in this country's elections? We employ data from the questions in which subjects rate on a scale from one to five the frequency with which "rich people buy elections." The variable on the x-axis corresponds to the mean subject response to a question in which subjects are requested to rate how democratically their country is being governed today on a scale from one to ten.

Table 1: Summary statistics

	U.S. mean	Kenya mean	U.S. obs.	Kenya obs.
Panel A: Subject choices in voting game				
Voter threshold	7.70	6.59	1794	886
Dummy for threshold at zero	.11	.16	1794	886
Politician expropriation	8.39	7.21	1236	610
Dummy for zero expropriation	.09	.26	1236	610
Dummy for full expropriation	.20	.22	1236	610
Panel B: Experimental measures of social preferences				
Dummy for sending in the dictator game	.66	.81	372	281
Dummy for sending in trust game	.83	.64	372	281
Dummy for returning in trust game	.42	.58	372	281
Reciprocity	.06		222	
Threshold in ultimatum game	2.91	5.72	150	118

Notes: Each panel reports means of the specified characteristics by experimental site. The data reported is restricted to subjects who exhibit non-monotonic behavior and are thus included in the primary sample. Panel A reports summary statistics for subjects' choices as voters and politicians, including the reelection threshold as a voter, a dummy for the threshold being at zero, the amount expropriated as a politician, and dummy variables for expropriating the minimum or maximum amount. This data is reported at the level of the subject-game round-game decision.

Panel B reports summary statistics for subjects' social preferences; this sample includes all subjects other than those included in session type F (unequal endowments). The measures reported include a dummy variable for sending a positive amount in the dictator game, a dummy variable for sending a positive amount in the trust game, a dummy variable for whether they returned any funds in the trust game, a reciprocity index, and the threshold in the ultimatum game; the reciprocity index can be calculated only for U.S. subjects in 2014 and 2015, and the ultimatum game threshold is available only for subjects in 2014. The reciprocity index is defined as $Perc_i^{high} - Perc_i^{low}$, censored at zero. All variables are normalized with respect to the dollar scales employed in the U.S.

Table 2: Voter and politician behavior in the simple voting game

	Threshold (1)	Exprop. (2)	Threshold (3)	Exprop. (4)	Threshold (5)	Exprop. (6)	Threshold (7)	Exprop. (8)
Belief expropriation	.007 (.003)**							
Voter threshold		.453 (.112)***						
Low threshold dummy		2.707 (1.116)**						
Altruism			1.125 (.381)***	-1.966 (.593)***				
Trust					1.383 (.376)***	-.205 (.514)		
Inequality aversion							-1.314 (.788)*	.380 (1.305)
Mean dep. var.	6.91	7.99	7.45	7.99	7.45	7.99	7.85	7.74
Obs.	281	641	641	641	641	641	268	268

Notes: The dependent variable is the voter's reelection threshold or the amount the subject will expropriate as a politician, as specified. The independent variables are the subject's reported belief about politician expropriation, the subject's choice of reelection threshold as a voter, a low threshold dummy defined as a dummy variable equal to one if the specified threshold is less than or equal to four, and measures of social preferences: a dummy variable equal to one if the subject sent a non-zero amount in the dictator game (altruism), a dummy variable equal to one if the subject sent any money in the trust game (trust), and a dummy variable equal to one if the minimum amount accepted in the ultimatum game is greater than or equal to half the endowment (inequality aversion). The sample is all subject decisions in the no-payment game in session types A through E; the subject's reported belief about politician expropriation is reported only for Kenyan subjects, and inequality aversion is reported only for subjects observed in 2014. All specifications include game session fixed effects, comprehension index fixed effects, and standard errors clustered at the experimental session level. Asterisks indicate significance at the ten, five and one percent level.

Table 3: Voter behavior

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Voter reelection threshold				
Recipient	.963 (.138)***	.964 (.135)***			.920 (.185)***	.922 (.179)***	.910 (.139)***	.910 (.139)***
Recipient x one payment			.278 (.205)	.278 (.205)				
Recipient x four payments			1.116 (.163)***	1.116 (.163)***				
Payment	-.703 (.196)***	-.670 (.188)***			-.702 (.279)**	-.687 (.255)***	-.704 (.197)***	-.679 (.189)***
One payment			-.595 (.313)*	-.597 (.253)**				
Four payments			-.752 (.189)***	-.733 (.188)***				
All payments			.593 (.301)**	.729 (.224)***			.498 (.328)	.466 (.208)**
Recipient x gift					.123 (.271)	.121 (.269)		
Payment x gift					-.003 (.393)	.057 (.367)		
Big pot x all payments							-.472 (.426)	-.465 (.368)
$\beta_1 + \beta_2$	.260 (.173)	.294 (.170)*			.218 (.212)	.235 (.199)		
$\beta_1 + \beta_3$			-.318 (.270)	-.319 (.224)				
$\beta_2 + \beta_4$			.364 (.195)*	.383 (.191)**				
$\beta_1 + \beta_2 + \beta_3 + \beta_4$							.232 (.292)	.232 (.292)
Sample			Session types A-D				Session types A-E	
Mean dep. var.	7.07	7.07	7.07	7.07	7.07	7.07	7.28	7.28
Fixed effects		Subject		Subject		Subject		Subject
Obs.	2136	2136	2136	2136	2136	2136	2326	2326

Notes: The dependent variable is the maximum threshold of expropriation at which the subject will vote to reelect the politician. The independent variables are a dummy variable for receiving a payment and receiving a payment in a game with one or four payments; the dummy variables for the game including payments or including one or four payments; a dummy for the game including payments for all subjects; the recipient and payment dummies interacted with a dummy for the gift framing; and the all payment dummy interacted with a big pot dummy. $\beta_1 + \beta_2$ reports the sum of the coefficients on recipient and payment. $\beta_1 + \beta_3$ and $\beta_2 + \beta_4$ report the sum of the recipient and payment dummies interacted with the one payment and four payment dummies, respectively. $\beta_1 + \beta_2 + \beta_3 + \beta_4$ reports the sum of the recipient and payment dummies, the all payment dummy, and the big pot interaction. Fixed effects are as specified in the table; specifications without subject fixed effects include a Kenya dummy, a dummy for ordering of the payment questions, and comprehension index fixed effects. Column (8) also includes the big pot dummy, for which the coefficient is not reported. All specifications include standard errors clustered at the experimental session level. Asterisks indicate significance at the ten, five and one percent level.

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Table 4: Politician expropriation

| | | Expropriation | | |
	(1)	(2)	(3)	(4)
Payment	.553 (.202)***		.427 (.277)	.514 (.215)**
One payment		.478 (.212)**		
Four payments		.523 (.239)**		
All payment		.806 (.256)***		.288 (.252)
Payment x gift			.381 (.356)	
All payment x big pot				.261 (.884)
Sample		Session types A-D		Session types A-E
Mean dep. var.	7.82	7.82	7.82	8.28
Obs.	1404	1404	1404	1594

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 payments, one or four payments, or payments to all subjects; the interaction of the payment dummy variable with the gift framing dummy; and the interaction of the all payment dummy with the big pot dummy. All specifications include subject fixed effects and standard errors clustered at the experimental session level. Asterisks indicate significance at the ten, five and one percent level.

Table 5: Voter behavior with unequal endowments

| | Voter reelection threshold | | | |
	(1)	(2)	(3)	(4)
Recipient	.989	.960	1.116	1.116
	(.142)***	(.133)***	(.163)***	(.163)***
High endowment	.216	.216	.216	.216
	(.162)	(.162)	(.162)	(.162)
Payment	-.717	-.676	-.789	-.730
	(.201)***	(.183)***	(.196)***	(.189)***
Unequal endowment	1.316	.877	1.476	.997
	(.531)**	(.364)**	(.595)**	(.401)**
Test $\beta_1 = \beta_2$	.000	.000	.000	.000
Test $\beta_1 + \beta_3 = \beta_2 + \beta_4$	.028	.025	.051	.081
Mean dep. var.	7.18	7.18	7.09	7.09
Sample	Session types A-D, F		Zero or four 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 vote to reelect the politician. The independent variables are a dummy variable for the subject receiving a payment, a dummy for the subject receiving a high endowment, a dummy for the game round including payments, and a dummy for an unequal endowment game round. Fixed effects are as specified in the table; specifications without subject fixed effects include a Kenya dummy, a dummy for ordering of the payment questions, and comprehension index fixed effects. All specifications include standard errors clustered at the experimental session level. Asterisks indicate significance at the ten, five and one percent level.

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Table 6: Voter behavior and consent

	(1)	(2)	(3)	(4)
	Voter reelection threshold			
Recipient	.956	.956	.956	.956
	(.158)***	(.158)***	(.158)***	(.158)***
Payment	-.695	-.570	-.597	-.570
	(.271)**	(.242)**	(.246)**	(.242)**
Recipient x limited information	-.162	-.162	-.536	-.536
	(.324)	(.324)	(.493)	(.493)
Payment x limited information	.065	-.277	-.314	-.341
	(.510)	(.387)	(.521)	(.519)
Recipient x consent			.807	.807
			(.509)	(.509)
Payment x consent			.264	.131
			(.615)	(.592)
$\beta_5 + \beta_6$			1.071	.939
			(.424)**	(.394)**
Sample	Session types A-D			
Mean dep. var.	7.07	7.07	7.07	7.07
Fixed effects		Subject		Subject
Obs.	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 the recipient and payment dummy variables, and these dummy variables interacted with dummy variables for the game round including limited information or prior consent. Fixed effects are as specified in the table; specifications without subject fixed effects also include a Kenya dummy, a dummy for ordering of the payment questions, the all payment and prior consent dummy variables, and comprehension index fixed effects. All specifications include standard errors clustered at the experimental session level. Asterisks indicate significant at the ten, five and one percent level.

Table 7: Voter behavior and variation in social preferences

| | Voter threshold | | | | | |
	(1)	(2)	(3)	(4)	(5)	(6)
Recipient	.591 (.393)	.591 (.408)	1.081 (.287)***	1.102 (.272)***	.727 (.138)***	.726 (.135)***
Payment	-.301 (.272)	-.324 (.273)	-.664 (.194)***	-.680 (.189)***	-.700 (.195)***	-.703 (.187)***
Recipient x reciprocity	2.520 (1.502)*	2.205 (1.531)				
Recipient x rec. (linear)			.690 (.649)	.671 (.573)		
Recipient x Kenya					.532 (.278)*	.535 (.275)*
Sample	Sessions C1 & D1		Sessions A-D		Sessions A-D	
Mean dep. var.	7.69	7.69	7.07	7.07	7.07	7.07
Fixed effects		Subject		Subject		Subject
Obs.	492	492	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 include the recipient and payment dummy variables, the interaction of the recipient dummy with two reciprocity indices, and the interaction of the recipient dummy with a dummy for Kenya. The first reciprocity index is constructed by calculating the percentage of the funds received that a subject would return to the sender if 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 he received less than 50%, denoted $Perc_i^{low}$. Rec_i is defined as $Perc_i^{high} - Perc_i^{low}$, censored at zero. The second reciprocity index is defined as the amount a subject returns if the partner chooses to send the maximum amount.

Fixed effects are as specified in the table; specifications without subject fixed effects also include a Kenya dummy, a dummy for ordering of the payment questions, the all payment and prior consent dummy variables, comprehension index fixed effects, and linear controls for the specified measure of social preference. Columns (1) through (4) also include the recipient dummy interacted with a dummy for the subject sending any funds in the trust game, and the sent dummy entered linearly. All specifications include standard errors clustered at the experimental session level. Asterisks indicate significance at the ten, five and one percent level.

Appendices

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 both senders and receivers. 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 were asked to specify how much they would propose to send to a partner, and the minimum amount they would accept when sent by a partner. The ultimatum game was implemented only in 2014.

In 2014 and 2015 in the U.S., the trust game was implemented with a larger set of choices: the subject could choose to send any integer amount of the endowment ($4) to the partner; this amount was again tripled. The subject could then specify how much of the endowment he would return if he received each hypothetical level of transfer.

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 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.[30]

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

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[30]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.

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.

Recruitment in each experimental site employed the labs' centralized database. In the U.S., subjects were contacted by email with information about the study and an invitation to sign up; in Kenya, they were contacted by text message. In both cases, the experimental session was described as focusing on political behavior. Subjects could sign up on-line (in the U.S.) or by text message (in Kenya). In the event the number of subjects who showed up for a particular session was not divisible by six (the polity size), excess subjects were paid the show-up fee and invited to sign up again.

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. Differences in the game session are described in the order in which activities were conducted.

B.1 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 voting game at this point in the experimental session.[31]

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 sent in

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

the dictator and trust games.

B.2   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 the 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 pose questions directly to the supervising research staff, they were not required to interact with anyone else.

In Kenya, information was provided primarily orally and graphically given the more limited literacy of the subject 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. The written explanatory materials were provided in English, as this is standard practice in the laboratory, and subjects would have been exposed to English-language instruction in school. However, staff members could speak Swahili in providing explanations as necessary.

B.3   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.

These responses are employed to construct a variable corresponding to the subject's maximum threshold for reelection that is equal to the median of the maximum threshold at which the subject stated he would reelect and the minimum threshold at which he stated he would not reelect: for example, the reelection threshold for a subject who would reelect a politician who expropriated 75 shillings, but not a politician who expropriated 150 shillings, was set at 112.5 shillings.

B.4   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.

B.5   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.

B.6   Questionnaire

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

B.7   Subject Compensation

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 received at the conclusion of the session 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   Appendix Figures and Tables

Figure C1: Subject choices in the U.S. and Kenya

(a) Voters: Simple voting game

(b) Voters: Full sample

(c) Voters: Simple voting game

(d) Voters: Full sample
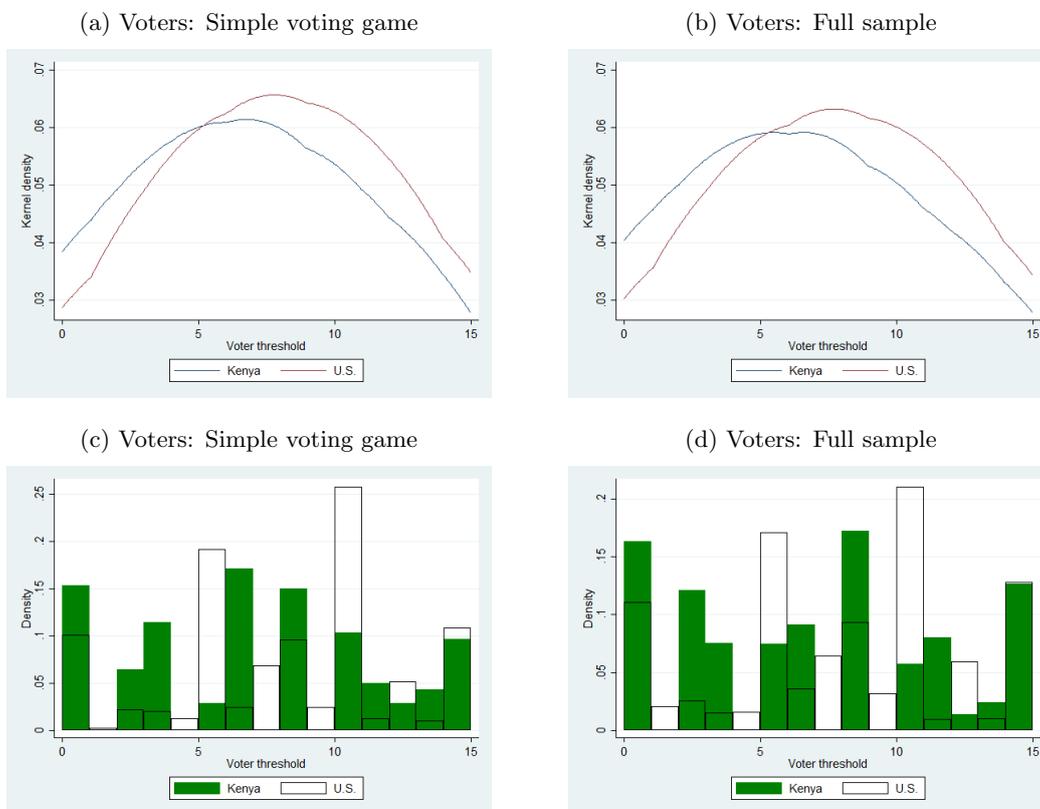
Notes: These kernel densities and histograms show the subjects' choice of a maximum threshold for politician expropriation in the U.S. and Kenya. All subject choices are normalized to lie on the scale employed in the U.S. sessions, from 0 to $15. In Figures C1a and C1c, the sample is restricted to the simple voting game with no payments. Figures C1b and C1d employ data from all game rounds observed.

Table C1: 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	0 payments ("uneq")	5 payments	U

Notes: 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. In addition, politician choices in the unequal endowments sessions are not analyzed.

Table C2: Sessions conducted

Session type	Framing	Location	Sessions	Subjects	Subject-game rounds	Subject-decisions	Monotonic only
A1	Public payments	U	8	78	234	378	378
A2	Public payments	K	6	96	192	288	258
B1	Public gift	U	10	102	306	510	510
B2	Public gift	K	6	90	180	270	231
C1	Limited - prior	U	11	66	192	252	252
C2	Limited - prior	K	4	60	120	180	111
D1	Limited - posterior	U	9	60	180	240	240
D2	Limited - posterior	K	4	60	120	180	156
E	"Big pot"	U, K	13	96	192	192	190
F1	Unequal endowments	U, K	7	60	120	180	162
F2	Unequal endowments	U	8	48	144	192	192
Total			86	816	1980	**2862**	**2680**

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

Table C3: Demographic characteristics of subjects

	U.S. mean	Kenya mean	U.S. obs.	Kenya obs.	p-value
Age	33.46	32.53	444	323	.349
Education	15.58	10.55	444	365	.000
Gender	.50	.62	443	365	.000
Marital status	.11	.45	444	365	.000
Voted in last presidential election	.72	.86	425	366	.000
Attempted to persuade others (in same election)	.46	.51	424	366	.202
Attended event in support of candidate (in same election)	.20	.59	426	366	.000
Joined a protest event in last year	.16	.17	426	366	.779

Notes: For each characteristic, the mean is reported by experimental site; the p-value reports a test for equality of the specified covariate comparing across the U.S. and Kenyan sample. 450 subjects are observed in the U.S.; questionnaire data was missing for six subjects, and some subjects failed to provide responses to other questions. 366 subjects are observed in Kenya, but age data for some subjects was missing from the laboratory's subject database.

Table C4: Definition of dummy variables

Session type	Framing	Game round I	Game round II	Game round III	Location
A1	Public payments	All zero	$P^1_{gs}=1$	$P^4_{gs}=1$	U
A2	Public payments	All zero	$P^4_{gs}=1$		K
B1	Public gift	$Gift_{gs}=1$	$Gift_{gs}=1, P^1_{gs}=1$	$Gift_{gs}=1, P^4_{gs}=1$	U
B2	Public gift	$Gift_{gs}=1$	$Gift_{gs}=1, P^4_{gs}=1$		K
C1	Limited - prior	$Lim_{gs}=1, Cons_{gs}=1$	$Lim_{gs}=1, Cons_{gs}=1$	$All_{gs}=1$	U
C2	Limited - prior	$Lim_{gs}=1, Cons_{gs}=1$	$Lim_{gs}=1, Cons_{gs}=1$		K
D1	Limited - posterior	$Lim_{gs}=1$	$Lim_{gs}=1$	$All_{gs}=1$	U
D2	Limited - posterior	$Lim_{gs}=1$	$Lim_{gs}=1$		K
E	"Big pot"	$Big_{gs}=1$	$Big_{gs}=1, All_{gs}=1$		U, K
F1	Unequal endowments	$Ineq_{gs}=1$	$All_{gs}=1$		U, K
F2	Unequal endowments	All zero	$Ineq_{gs}=1$	$All_{gs}=1$	U

Table C5: Voter behavior - including non-monotonic subjects

				Voter reelection threshold				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Recipient	.988 (.140)***	.990 (.137)***			.968 (.189)***	.980 (.184)***	.936 (.143)***	.936 (.143)***
Recipient x one payment			.278 (.205)	.278 (.205)				
Recipient x four payments			1.132 (.166)***	1.132 (.166)***				
Payment	-.831 (.196)***	-.803 (.190)***			-.858 (.290)***	-.844 (.267)***	-.826 (.198)***	-.812 (.191)***
One payment			-.681 (.314)**	-.686 (.254)***				
Four payments			-.887 (.189)***	-.871 (.188)***				
All payments			.507 (.304)*	.645 (.228)***			.496 (.329)	.495 (.213)**
Recipient x gift					.045 (.273)	.033 (.271)		
Payment x gift					.093 (.387)	.125 (.344)		
Big pot x all payments							-.398 (.447)	-.412 (.378)
$\beta_1 + \beta_2$	.157 (.190)	.188 (.188)			.110 (.250)	.137 (.237)		
$\beta_1 + \beta_3$			-.403 (.272)	-.408 (.228)*				
$\beta_2 + \beta_4$			.245 (.217)	.261 (.214)				
$\beta_1 + \beta_2 + \beta_3 + \beta_4$							.208 (.302)	.208 (.302)
Obs.	2298	2298	2298	2298	2298	2298	2490	2490

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 receiving a payment and receiving a payment in a game with one or four payments; the dummy variables for the game including payments or including one or four payments; a dummy for the game including payments for all subjects; the recipient and payment dummies interacted with a dummy for the gift framing; and the all payment dummy interacted with a big pot dummy. $\beta_1 + \beta_2$ reports the sum of the coefficients on recipient and payment. $\beta_1 + \beta_3$ and $\beta_2 + \beta_4$ report the sum of the recipient and payment dummies interacted with the one-payment and four-payment dummies, respectively. $\beta_1 + \beta_2 + \beta_3 + \beta_4$ reports the sum of the recipient and payment dummies, the all payment dummy, and the big pot interaction. Fixed effects are as specified in the table; specifications without subject fixed effects include a Kenya dummy, a dummy for ordering of the payment questions, and comprehension index fixed effects; Column (8) also includes the big pot dummy, for which the coefficient is not reported. All specifications include standard errors clustered at the experimental session level. Asterisks indicate significance at the ten, five and one percent level.

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Table C6: Politician expropriation - including non-monotonic subjects

| | Expropriation | | | |
	(1)	(2)	(3)	(4)
Payment	.563 (.201)***		.504 (.272)*	.529 (.213)**
Payment 1		.486 (.212)**		
Payment 4		.537 (.234)**		
All payment		.813 (.255)***		.280 (.252)
Payment x gift			.182 (.364)	
All payment x big pot				.087 (.978)
Sample		Session types A-D		Session types A-E
Mean dep. var.	7.95	7.95	7.95	8.37
Obs.	1512	1512	1512	1704

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 payments, one or four payments, or payments to all subjects; the interaction of the payment dummy variable with the gift framing dummy, and the interaction of the all payment dummy with the big pot dummy. All specifications include subject fixed effects and standard errors clustered at the experimental session level. Asterisks indicate significance at the ten, five and one percent level.

Table C7: Voter behavior - high comprehension sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Voter reelection threshold				
Recipient	.968 (.149)***	1.010 (.145)***			.897 (.191)***	.958 (.186)***	.951 (.150)***	.951 (.150)***
Recipient x one payment			.200 (.239)	.200 (.239)				
Recipient x four payments			1.187 (.171)***	1.187 (.171)***				
Payment	-.706 (.200)***	-.764 (.199)***			-.576 (.264)**	-.694 (.256)***	-.705 (.201)***	-.774 (.199)***
One payment			-.398 (.286)	-.655 (.272)**				
Four payments			-.808 (.198)***	-.839 (.199)***				
All payments			.365 (.270)	.683 (.227)***			.231 (.265)	.472 (.210)**
Recipient x gift					.201 (.298)	.140 (.298)		
Payment x gift					-.397 (.413)	-.199 (.402)		
Big pot x all payments							-.276 (.450)	-.448 (.400)
$\beta_1 + \beta_2$	.262 (.178)	.246 (.178)			.321 (.214)	.264 (.207)		
$\beta_1 + \beta_3$			-.198 (.261)	-.455 (.246)*				
$\beta_2 + \beta_4$			.379 (.199)*	.348 (.199)*				
$\beta_1 + \beta_2 + \beta_3 + \beta_4$							.200 (.330)	.200 (.330)
Mean dep. var.	7.30	7.30	7.30	7.30	7.30	7.30	7.30	7.30
Fixed effects	Subject	Subject	Subject	Subject	Subject	Subject	Subject	Subject
Obs.	1966	1966	1966	1966	1966	1966	2096	2096

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 receiving a payment and receiving a payment in a game with one or four payments; the dummy variables for the game including payments or including one or four payments; a dummy for the game including payments for all subjects; the recipient and payment dummies interacted with a dummy for the gift framing; and the all payment dummy interacted with a big pot dummy. $\beta_1 + \beta_2$ reports the sum of the coefficients on recipient and payment. $\beta_1 + \beta_3$ and $\beta_2 + \beta_4$ report the sum of the recipient and payment dummies interacted with the one-payment and four-payment dummies, respectively. $\beta_1 + \beta_2 + \beta_3 + \beta_4$ reports the sum of the recipient and payment dummies, the all payment dummy, and the big pot interaction. Fixed effects are as specified in the table; specifications without subject fixed effects include a Kenya dummy, a dummy for ordering of the payment questions, and comprehension index fixed effects; Column (8) also includes the big pot dummy, for which the coefficient is not reported. All specifications include standard errors clustered at the experimental session level. Asterisks indicate significance at the ten, five and one percent level.

Table C8: Politician expropriation - high comprehension sample

| | Expropriation | | | |
	(1)	(2)	(3)	(4)
Payment	.544 (.216)**		.436 (.288)	.501 (.231)**
Payment 1		.506 (.240)**		
Payment 4		.500 (.257)*		
All payment		.795 (.260)***		.294 (.256)
Payment x gift			.336 (.390)	
All payment x big pot				.820 (1.219)
Sample		Session types A-D		Session types A-E
Mean dep. var.	7.77	7.77	7.77	8.23
Obs.	1299	1299	1299	1429

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 payments, one or four payments, or payments to all subjects; the interaction of the payment dummy variable with the gift framing dummy, and the interaction of the all payment dummy with the big pot dummy. All specifications include subject fixed effects and standard errors clustered at the experimental session level. Asterisks indicate significance at the ten, five and one percent level.