Title: Incomplete Political Contracts with Secret Ballots: Reciprocity as a Force to Enforce Sustainable Clientelistic Relationships

Kenju Kamei*

Durham University

* Department of Economics and Finance, Durham, UK. Email: kenju.kamei@gmail.com, knju.kamei@durham.ac.uk

Abstract: Clientelism is frequently observed in our societies. Various mechanisms that help sustain incomplete political contracts (e.g., monitoring and punishment) have been studied in the literature to date. However, do such contracts emerge in elections with secret ballots when the interactions are one-shot? How does repetition affect the evolution of incomplete political contracts? Using an incentivized experiment, this paper finds that even during one-shot interactions where monitoring is not possible, candidates form incomplete contracts through vote buying and promise-making. The candidates' clientelistic behaviors are heterogeneous: some target swing voters, whereas others offer the most to loyal voters, or even opposition voters. These tactics distort voting behaviors as well as election outcomes. Repeated interactions significantly magnify candidates' offers and deepen clientelistic relationships. These results underscore the possibility that clientelism evolves due to people's strategic behaviors and interdependent preferences, without relying on alternative mechanisms.

Keywords: experiment, cooperation, vote buying, election, clientelism

JEL codes: C92, D72

Acknowledgement: This project was supported by a grant-in-aid by the Murata Science Foundation. The author thanks John Hey for his hospitality when he conducted the experiments in the University of York. The author also thanks Mark Wilson at the University of York for his support in setting the computers and the z-Tree software in the EXEC laboratory. The author also thanks the editor, Nicola Persico, and three anonymous referees for valuable comments.

1. Introduction

A wealth of research during the last several decades in economics, political science and psychology documents that people have interdependent preferences, such as reciprocity and inequity aversion (e.g., Camerer *et al.*, 2003; Fehr and Schmidt, 2006). In dilemma situations, cooperation is observed under certain conditions even though defecting is the dominant strategy, while some individuals – especially cooperators – punish free riders at a private cost (e.g., Fehr and Gächter, 2000 and 2002). The prevalence of actors with interdependent preferences may go some way to explaining the frequent observation of incomplete political contracts in our societies.

Incomplete political contracts in elections, such as vote buying and clientelism, are recognized in political economy and political science as important influencers of people's voting behaviors. Most theoretical models rely on the assumptions of self-interested political actors and common knowledge of rationality. These assumptions imply that, unless interactions are infinitely repeated (e.g., Robinson and Verdier, 2013), incomplete political contracts should not arise if people cast votes with secret ballots and candidates (e.g., politicians, parties) are unable to commit to their post-election behaviors. This is because rational voters would not comply with such agreements due to the secrecy of ballots. Thus, providing benefits to voters would just decrease the payoffs of the candidates, unless the benefits being provided align with the interests of the candidates. Rational candidates would also renege on their post-election promises after elections. In theoretical models of such political exchanges, additional assumptions are often considered. For example, candidates can fully control individuals' votes if the

individuals accept and receive up-front payment before the election (e.g., Dekel *et al.*, 2008); voters simply select candidates that offer them the highest total payoffs, i.e., the sums of the utility from the election outcome and the amount paid by the candidate (e.g., Groseclose and Snyder, 1996); the candidates are able to commit to promises made before the election (the contracts are enforceable) when the offers take the form of providing benefits contingent on winning (e.g., Dal Bó, 2007; Myerson, 1993).[1]

In democratic societies, secret ballots are often in official use. For example, the Australian ballot system is employed during elections in most US states.[2] Contrary to the predictions of standard game-theoretic models, a large body of work based on surveys, case studies and field experiments has found that incomplete political contracts are common empirical phenomena.[3] Various studies have suggested mechanisms for political actors to overcome the commitment problems behind incomplete political contracts, such as candidates targeting a particular set of citizens (e.g., poor voters [e.g., Calvo and Murillo, 2004; Stokes, 2005], more reciprocal individuals [e.g., Finan and Schechter, 2012], core constituency [e.g., Golden and Picci, 2008]), the monitoring of citizens' votes (e.g., Brusco *et al.*, 2004), voters' lack of political knowledge or information (e.g., Pande, 2011), providing no institutional guarantees for people's security, status or wealth (e.g., Scott, 1972), and employers' political requests to their workers (e.g., Hertel-Fernandez, 2017).[4] This paper asks the following question: how frequently do incomplete political contracts occur under secret ballots in the absence of these mechanisms, when they are not supported as equilibria because of the self-interest of players and their beliefs that others act the same? In doing so, this study contributes to the literature by providing clean

evidence that, while candidates' clientelistic behaviors are heterogeneous, incomplete political contracts do emerge even when voters free ride on others' voting behaviors, interactions are perfectly one-shot and there are no deterrent mechanisms (such as monitoring and political operatives) in place. Equally importantly, this study shows that without any other mechanisms, repetition alone – even if it does not change the set of equilibria for rational selfish actors – may deepen the relationships between candidates and voters. The results imply that political actors' interdependent preferences and long-term relationships may be key forces to sustain clientelism in a democratic society.

An established body of experimental work on relational contracts in the context of labor markets suggests that interdependent preferences, such as reciprocity, may be strong enough to enforce incomplete contracts between political actors. The significance of incomplete contracts has been consistently verified in the context of employer-worker, gift-exchange relationships (e.g., Fehr *et al.*, 1993 and 1998; Fehr *et al.*, 1997; Fehr and Falk, 1999). For example, regardless of whether the market is in balance or has an excess supply of workers, employers tend to provide larger surplus to workers; and workers often reciprocate the employers' favors by exerting large efforts even when doing so is costly in one-worker-one-employer environments (e.g., Fehr *et al.*, 1993 and 1998; Fehr *et al.*, 1997; Fehr *et al.*, 1998). The gain from trade is especially large if they successfully foster long-term bilateral relationships (e.g., Brown *et al.*, 2004 and 2012). In addition, these findings have been shown to extend to the case of multi-worker firms (e.g., Charness and Kuhn, 2007; Maximiano *et al.*, 2007; Abeler *et al.*, 2010; Gächter *et al.*, 2012). In contrast to these studies in labor markets, surprisingly little attention has been

given to the possibility that interdependent preferences might lead to incomplete political contracts in elections. Studying incomplete contracts in elections is of great interest to scholars for at least two reasons. First, in elections, a candidate (voter) can form incomplete contracts with multiple voters (candidates) simultaneously, the voters cast their votes with secret ballots and only a subset of candidates win. This is in clear contrast with the prior gift-exchange game experiments, in which each worker makes a contract with one firm (one-to-one or one-to-N relation). Each voter as part of the *M-to-N* relation (where $M > 1$ and $N > 1$) can free ride on the peers' voting acts, without being verifiable by a candidate. This study is, to the best of the author's knowledge, the first to experimentally study political actors' clientelistic behaviors in an M-to-N relation. To achieve this goal, two forms of incomplete contracts, specific to the M-to-N election setup, are examined. Second, each political actor has potentially different political preferences. Controlled laboratory experiments enable researchers to collect data regarding the target with whom candidates attempt to form incomplete contracts.

This paper focuses on the two forms of clientelistic relationships, akin to the theoretical setup by Dekel *et al.* (2008): up-front vote-buying [payment before the election] and promises [payment after the election contingent on the outcome]. Unlike Dekel *et al.* (2008), the experiment is designed so that contracts are incomplete, since the interest of this paper is in how political actors voluntarily comply with incomplete contracts despite vote secrecy. These two forms of candidate-voter relationships differ in the timing by which candidates deliver a favor to voters. Henceforth, this paper uses the term, "vote buying" or "up-front payment," to refer to an exchange of money, goods or

services from a candidate to a targeted voter *before* the election (see also Stokes [2005] and Vicente [2014]). The term, "promise," is used to refer to a promise of post-election favor made by candidates (e.g., monetary or non-monetary rewards, potential access to public resources) in exchange for votes *contingent on winning*. The agreements are not binding. Voters can select whichever candidate they prefer with secret ballots, irrespective of whether they agreed to vote for a specific candidate by forming an incomplete contract. Candidates are also free to decide the amounts to transfer after the election regardless of the promised amount. Candidates can select the target of vote buying or promise selectively.

Incentivized laboratory experiments are used to study the prevalence and the mechanism of incomplete political contracts. The laboratory experimental method is a useful and hitherto under-utilized method to study clientelism.[5] The research question has been studied intensively, especially in political science, using field data; however, a number of issues have been reported. First, social desirability bias can be a concern in the field. For instance, it is difficult to elicit true behavior from people in surveys because they are reluctant to accept that they received gifts in exchange for a vote (e.g., Gonzalez-Ocantos *et al.*, 2012). Second, secret ballots are not always fully enforced in developing countries where surveys or field experiments are conducted, nor in developed countries, such as the United States and Japan (e.g., Stokes, 2005; Kitschelt and Wilkinson, 2007). The literature reports that a non-negligible fraction of people do not believe in the secrecy of ballots, and also share vote choices with others (e.g., Dellavigna *et al.*, 2017; Gerber *et al.*, 2012; Bond *et al.*, 2012).[6] Such vote secrecy can instead be easily preserved in a

laboratory.[7] Third, certain empirical findings from past research may be driven by turnout buying, rather than vote buying (e.g., Nichter, 2008; Finan and Schechter, 2012). Thus, a study that excludes the channel of turnout buying is meaningful. Fourth, with field data it is usually difficult to identify people's ability to resolve commitment problems under secret ballots without any repetition effects, because their interactions are often not one-shot. To address this issue, this study consists of two kinds of experiments: one using strictly one-shot interactions; and the other with finitely-repeated interactions.[8]

The experiment is designed based on an electoral competition model where multiple voters are distributed on a one-dimensional policy space. In the experimental election, two candidates, located at the end points on the space, are each given an opportunity to buy votes before the election, to promise to transfer resources to voters after the election, or to do both. The voters have only one vote and can select candidates with secret ballots. A candidate that collects the majority of votes wins the election. Political actors' identities are kept anonymous and no sanctions are associated with their behaviors. Each voter's political preference stays fixed and is common knowledge to both the candidates and the voters (this condition is set as a simplification for the internal validity of the design).

The experiment data reveal that even if the interactions are one-shot and such candidate-voter agreements are not binding, the candidates do offer positive amounts to voters and the voters are significantly affected by such offers when casting votes in an election. Consequently, election outcomes can be distorted. When interactions are repeated, candidates' offers become significantly larger and this trend stays stable over time. A detailed look at the data further reveals that candidates' clientelistic behaviors are

heterogeneous: some candidates target swing voters, whereas others offer more to loyal voters to protect them from the opposition, and still others offer more to opposition voters in an attempt to pry them away from the opposition.

Other than this paper, recently Tonguc and Ozbay (2018) also experimentally studied incomplete contracts between a candidate and a voter. Their experiment is built on a finitely repeated community game under random matching in which there are an equal number of candidates and voters. Each period represents an election, consisting of one candidate and one voter, and the candidate can buy the voter's vote (their setup does not differentiate between turnout and vote buying and also no collective decision-rule is applied as there is only one voter per society). Their focus is different from this paper, however, in that they aim to study which behavioral mechanisms support vote trading in a simple one-candidate-one-voter matching under high or low commitment environments. Since the authors stay close to the one-to-one matching, which has been studied extensively in labor economics, behavioral hypotheses can easily be tested by drawing directly from the previous literature. By contrast, this paper's focus is on incomplete contracting in a richer M-candidates-N-voters matching (where $M > 1$ and $N > 1$) environment, in which voters can free ride on others' voting under secret ballots. With the multiple-voter setup, it is also possible to explore candidates' clientelistic behaviors regarding with which voters they target to form incomplete contracts. For example, as discussed, this paper found candidates' heterogeneous clientelistic behaviors. This result is useful because there is little guidance in the current behavioral literature and it could help advance theoretical work in a competitive election. Further, this study designs two sets of treatments – one with purely one shot, and the other

with finitely repeated interactions – aiming to study (a) not only political actors' incomplete contracts without any repetition effects, but also (b) the impact of repetition. This distinction is important because repetition, along with dyadic relationships, contingency, and hierarchy, has been proposed as key elements of clientelism in the political economy literature (e.g., Hicken, 2011). Tonguc and Ozbay's design precludes them from studying impact of repetition itself, since all of their treatments are constructed based on a repeated community game.

The rest of the paper proceeds as follows: Section 2 describes the experimental design, and then Section 3 discusses political actors' possible behaviors. Sections 4 to 6 present results. Section 7 concludes.

2. Experimental Design

This study consists of three sets of two experiments (3 × 2 design). Each set consists of (a) a treatment with one-shot interactions between candidates and voters and (b) a treatment with repeated interactions between the political actors under fixed matching (Table 1). The one-shot treatments are used to study a possibility of incomplete political contracts without any reputation effects. Part (b) is designed using finite, not infinite, repetition, because of its sharpness of theoretical predictions. If an infinitely repeated game were to be used, the standard theory would predict multiple equilibria for sufficiently patient political actors and so the interpretation of data would be difficult. With finite repetition, the difference between (a) and (b) can be clearly interpreted as being driven by subjects' interdependent preferences and reputation building.[9,10]

The three sets in the design vary by the political exchange process in place between the political actors. In the first set, candidates are given an opportunity to give money to voters before the election. In the second set, they are instead given a pre-election opportunity to promise to give money in case that they win the election. In the third set, they are given both opportunities. The reminder of the experimental design is identical between sets.

Each subject plays the game under only one treatment condition (between-subjects design). In each treatment, two candidates compete against each other to collect votes on a one-dimensional space (on which voters are uniformly distributed). Both candidates can form incomplete contracts with voters through vote buying or promise-making. The design of the one-dimensional space builds on Stokes (2005).

At the beginning of the experiment, each subject is randomly assigned to a group of seven so that there are two candidates (A and B) and five voters in every group. Roles are also assigned at random. The probabilities with which a subject is assigned the role of candidate A, candidate B or a voter are 1/7, 1/7 and 5/7, respectively. After a role is assigned, each political actor is assigned political position x. The positions of candidates A and B are fixed and polarized: $x = 0$ for candidate A and $x = 10$ for candidate B (Figure 1). The political position of voter i, x_i, is drawn uniformly (i.i.d.) from integers on the interval 0 and 10 (end points, 0 and 10, are inclusive). Thus, the probability that a specific integer is assigned to voter i is 1/11. This procedure is common knowledge in the experiment. Voter's x_i can be interpreted as the political taste (e.g., ideology), whereas a candidate's x (0 or 10) can be interpreted as her policy choice.[11] For simplicity, the five

voters' x_i in each group are common knowledge among the seven members including the two candidates. This simplified setup was chosen deliberately since this paper's focus is to explore the mechanism of incomplete political contracting, not how candidates learn the types of voters over time.

<<COMP: Place Figure 1 about here>>

The group assignment, the role assignment and political position $\{x_i\}_{i=1,2,\ldots,5}$ remain the same during the entire experiment in the repetition treatments. Hence, it is possible for the candidates in the repetition treatments to foster long-term relationships with specific voters.

The payoff of each actor depends on the election outcome. The payoff formulas differ between voters and candidates. The payoff of voter *i* is given as follows:

$$\Pi_i\left(x_i; x_e, b_{k,i}, y_{e,i}\right) = \pi_{\mathrm{i}}(x_i; x_e) + \sum_{k\in\{A,B\}} b_{k,i} \cdot 1_{k,i} + y_{e,i}, \qquad (1)$$

where x_e is the political position of the elected candidate *e*, and $\pi_i(x_i; x_e) = 100 - \frac{1}{2}(x_i - x_e)^2$ is the payoff of voter *i* from the election stage, meaning that the voter incurs a loss dependent on the difference between his political taste and $x_e \in \{0, 10\}$. In Equation (1), $b_{k,i}$ is the up-front payment offer from candidate $k\in\{\mathrm{A}, \mathrm{B}\}$ to voter $i\in\{1, 2, 3, 4, 5\}$ ($1_{k,i}$ is an indicator function that equals 1 if *i* accepts the offer from *k*), and $y_{e,i}$ is a post-election transfer from the successful candidate *e* to *i*. $b_{k,i} = 0$ ($y_{e,i} = 0$) in the treatments where candidates do not have an option to make an up-front payment offer (to make a promise and then engage in a post-election transfer).

The payoffs for the successful and unsuccessful candidates of the election are 300 ECUs (Experimental Currency Units) and 150 ECUs, respectively, from the competition. The loser also incurs a disutility due to the difference in the political stance ($1/2 \cdot 10^2 = 50$). In sum, the unsuccessful candidate receives a payoff of 100 (=150 – 50) ECUs – see Figure 1.

There is only one election stage in the three one-shot treatments. Each voter has only one vote. A majority rule is used in the election: whichever candidate received at least three votes becomes the winner. All group members are informed of two outcomes: (a) which candidate won and (b) how many votes the elected candidate collected. They are not informed of who voted for whom. The three treatments are named as the "Vote Buying, One-Shot" (BUYING), "Promise, One-Shot" (PROMISE), and "Choice, One-Shot" (CHOICE) treatments.

There are ten election stages with fixed matching in the three repetition treatments. They are named as the "Vote Buying, Repetition" (BUYING-R), "Promise, Repetition" (PROMISE-R), and "Choice, Repetition" (CHOICE-R) treatments. The number of interactions is common knowledge for all players.

2.1. The Vote-Buying Procedure

In the BUYING treatment, candidates A and B simultaneously decide how many ECUs they want to give to each voter before the election. Each offer $b_{k,i}$ must be a non-negative integer that satisfies:

$$\sum_{i=1}^{5} b_{k,i} \leq 300.$$

Here, 300 is the winner's payoff.[12] Candidates are not informed of their competitor's offers.

Once both candidates have made their offers, each voter is presented with the offers made by candidates A and B. They are not informed about the candidates' offers to the other four voters in their group. Voters then decide which offer to accept. They can accept both, only one, or no offers (see Section B.4 in Appendix B for a computer screen image in the experiment). No one except the involved two parties is aware of whether a candidate and a voter formed an agreement. A voter can thus accept both of the offers if the voter is a material payoff maximizer. If a voter accepts an offer from a candidate, the voter will immediately receive the amount and his/her intention to support is conveyed to that candidate. However, these exchanges are *not* binding. The voter can vote for whichever candidate he prefers with a secret ballot. The BUYING treatment ends immediately after the election.

In the BUYING-R treatment, there are a total of ten periods. The structure in the first period is the same as the BUYING treatment. Each period after period 1 also consists of the vote-buying and election stages. The requirement in the vote-buying stage, however, differs by period: the sum of five up-front payment offers in period t must not exceed the candidate's maximum possible accumulated payoff at the end of that period (300 + total payoff up to period $t - 1$). In case that a candidate's payoff earned for a given period is negative, the loss is deducted from her positive payoffs from other periods. Hence, the more frequently a candidate wins and the more resources the candidate accumulates over time, the greater the advantage that the candidate has over the

opposition in later periods and the closer to a monopsony the system becomes. This is a common feature in all repetition treatments.

2.2. The Promise Procedure

In the PROMISE treatment, there are two stages other than the election stage. Before the election, candidates can privately propose to give some amount to each voter in exchange for their vote on condition that they win the election. Two candidates in each group make such promise decisions simultaneously. Each promise to a voter i, $pm_{k,i}$, must satisfy:

$$pm_{k,i} \leq 300.$$

Here, $pm_{k,i}$ must be a non-negative integer. Notice that $pm_{k,i} \leq 300$, instead of $\sum_{i=1}^{5} pm_{k,i} \leq 300$, is imposed as a requirement, because it is just a promise and each voter is not informed of promises made to the other four voters. It is possible for a candidate to make infeasible promises to their five voters if they so wish.

After both candidates have made their decisions, each voter is presented with the promises and then decides whether to accept the offers. Neither candidate knows how much their competitor promised. Voters are not informed about candidates' proposals to the other four voters or whether they formed agreements. These agreements are not binding. Voters can select whichever candidate they prefer in the election with secret ballots.

There is a post-election stage, in which the elected candidate has the option to transfer ECUs to voters. The elected candidate can transfer the exact amounts that s/he promised, but s/he is also free to transfer more or less than that quantity. No voter is

informed about whether or how much the other four voters in their group receive. The elected candidate's payoff is: $300 - \sum_{i=1}^{5} y_{e,i}$. Thus, his payoff is maximized if he does not deliver any amounts (i.e., $\sum_{i=1}^{5} y_{e,i} = 0$). The sum of transfers must not exceed 300. The unsuccessful candidate receives a payoff of 100 and does not have the transfer opportunity. The setup where only the elected candidate can distribute favors is similar to the theoretical setup of Dekel *et al.* (2008). The payoff of voter *i* is: $\pi_i + y_{e,i}$. The PROMISE treatment will be over once the elected candidate makes his/her transfer decisions. The losing candidate is not informed of the elected candidate's transfer behaviors.

The PROMISE-R treatment has a total of ten periods, each of which has the pre-election promise stage and the post-election transfer stage. The first period is designed the same as the PROMISE treatment. After period 1, the requirement regarding the promise-making and transfer decisions differs by period. First, the promise a candidate makes to each voter in period *t* must not exceed the candidate's accumulated payoff up to that period (assuming that the candidate wins). Second, the total post-election transfer amounts cannot exceed the candidate's budget. The budget is her accumulated payoff up to the time of transfer decisions.

2.3. The Choice Treatments

In the CHOICE treatment, each candidate is able to not only offer some pre-election payment, but also promise to make a post-election transfer to each voter. Candidates can exercise both, one, or neither of the two opportunities.

The requirements of these two offers are identical to the BUYING and PROMISE treatments, except that (i) the sum of the five post-election transfers must not exceed 300 $- \sum_i b_{e,i} \cdot 1_{e,i}$, where $b_{e,i}$ is the elected candidate e's up-front payment offer, and $1_{e,i} = 1(0)$ if voter i accepted (declined) the offer from e, and (ii) the sum of vote-buying offer $b_{k,i}$ and promise $pm_{k,i}$ to voter i cannot exceed 300 ($b_{k,i} + pm_{k,i} \leq 300$ for each i). Requirement (i) reflects the fact that the elected candidate's budget is diminished by their up-front payment acts. In the election, five voters select either candidate A or B with secret ballots.

Political actors in the CHOICE-R treatment have a total of ten interactions. The first period is identical to that of the CHOICE treatment. The CHOICE-R treatment has essentially the same requirement for vote-buying and promise decisions in periods after period 1 as the BUYING-R and PROMISE-R treatments. The following two requirements are imposed for candidates' period t decision on $b_{k,i}$ and $pm_{k,i}$: (a) $b_{k,i} + pm_{k,i} \leq 300$ + candidate k's total accumulated payoff up to period $t - 1$, and (b) $\sum_i y_{e,i} \leq 300 - \sum_i b_{e,i} \cdot 1_{e,i}$ + elected candidate e's total accumulated payoff up to period $t - 1$.

2.4. Experimental Procedure

A total of 29 experimental sessions were conducted in the EXEC (the Centre for Experimental Economics) laboratory at the University of York from August 2016 through May 2017.[13] The total number of subjects was 476.[14] The experiment, except the instructions and control questions, was programmed using the z-Tree software (Fischbacher, 2007). Invitation messages were sent through *hroot* (Bock *et al.*, 2014) to all eligible subjects (students in the University of York) in the database. Subjects then

voluntarily registered for and participated in the experiments. No subjects participated in more than one session. All the instructions were neutrally framed (see Appendix B). Communication among subjects was prohibited during the experiment.

3. Discussions on Political Actor's Behaviors

If it is assumed that all political actors are selfish and believe that their peers are also selfish, a voter with the political position of x_i in the BUYING treatment would vote for candidate A if $\pi_i(x_i; 0) > \pi_i(x_i; 10)$ and candidate B if $\pi_i(x_i; 10) > \pi_i(x_i; 0)$ because the vote-buying contract is not binding (i.e., cheap talk). The condition, $\pi_i(x_i; 0) \gtreqless \pi_i(x_i; 10)$, is equivalent to $x_i \lesseqgtr 5$.[15] Anticipating this, candidates would not offer any ECUs before the election, because providing up-front payment would simply reduce their payoffs. Likewise, any promises made by candidates would not affect the behaviors of those involved in the PROMISE treatment. Specifically, elected candidates would not transfer anything to voters after the election because transfer acts are costly. Voters, anticipating the elected candidate's selfish behavior, would vote based on their political position. Candidates would thus make promise decisions randomly because promise-making would not affect voting in the election. Political actors' behaviors in the CHOICE treatment would be the same as those in the BUYING and PROMISE treatments: there would be no vote-buying behavior, candidates would randomly promise transfers, voting decisions would be made based only on voters' political positions, and there would be no post-election transfers by candidates.

The assumption of selfish actors and the common knowledge of others' selfishness predict the same behaviors of candidates and voters for each stage game in the

BUYING-R, PROMISE-R and CHOICE-R treatments by the logic of backward induction.

In short, no incomplete contracting is predicted by the standard theory if secret ballots are perfectly implemented. Considering this, as discussed in Section 1, scholars have proposed mechanisms behind the occurrence of vote buying and clientelism, including candidates' monitoring and punishment of voters and some form of incentive-compatible clientelism.

Hypothesis 1: Standard theory prediction.

(i) Candidates do not make any up-front payment or systematic promise offers to voters. (ii) Voters select candidates solely based on their political positions. That is, a voter i votes for candidate B (A) if political position $x_i > 5$ ($x_i < 5$). (iii) Candidates do not deliver on any promises after the election.

Once the assumption of the common knowledge of rationality is relaxed, however, candidates may engage in vote buying or systematic promises even in the one-shot treatments. Political actors' behaviors in the final stage would, nevertheless, remain the same as Hypothesis 1. For example, suppose that candidate A believes that candidate B would offer to pay $b^A_{B,1}$, $b^A_{B,2}$, $b^A_{B,3}$, $b^A_{B,4}$, and $b^A_{B,5}$ to the five voters in the BUYING treatment. Suppose also that candidate A believes that voter *i* would select a candidate based on the size of $\pi_i(x_i; x_k) + b_{k,i}$ by reciprocating a candidate's up-front payment. Here, $b_{k,i}$ refers to an offer by candidate *k* to voter *i*. Even with this setup, candidate A would not offer anything if the payoffs of at least three voters are already higher with $b_{A,i} = 0 \ \forall i$ when A is elected than otherwise. Suppose that this is not in that case. Then,

candidate A would weigh up (a) giving nothing to all five voters versus (b) giving $b_{A,i}$ to some three voters so that $\pi_i(x_i; 0) + b_{A,i} > \pi_i(x_i; 10) + b^A_{B,i}$. Candidate A receives 200 (= 300 – 100) ECUs more if s/he wins. Hence, if the sum of three vote-buying amounts under case (b) is less than 200, candidate A would attempt to buy the three voters. Note, however, that in the election, regardless of the size of vote-buying offers, selfish voters would not vote for candidate A if $\pi_i(x_i; 0) < \pi_i(x_i; 10)$.

Something similar could be observed in the PROMISE treatment. Candidates would transfer nothing after the election because they incur costs if they deliver some promises. Nevertheless, the candidates may strategically make promises, for example if the candidates believe that (i) voters are not selfish and (ii) voters believe that the candidates would deliver on the promises after the election. In that case, candidate A would promise $pm_{A,i}$ to voter *i* so that $\pi_i(x_i; 0) + pm_{A,i} > \pi_i(x_i; 10) + pm^A_{B,i}$, where $pm^A_{B,i}$ is the candidate A's belief on the promise candidate B makes. In the election, voter *i* may also vote for candidate A, if the voter believes that A would deliver the promise and $\pi_i(x_i; 0) + pm_{A,i} > \pi_i(x_i; 10) + pm_{B,i}$.[16]

By the same logic, candidates could offer to pay positive amounts before the election and/or promise to give after the election in the CHOICE treatment.

Hypothesis 2: Relaxing the common knowledge of political actors' self-interest.

(i) Some candidates make up-front payment offers or systematic promises even in the one-shot treatments. (ii) Some voters in the PROMISE and CHOICE treatments select a candidate if that candidate promises sufficiently larger transfers than the other candidate, even if the voters have political position x closer to the other candidate's x. (iii) Political

actors' final stage behaviors (voting [transfer] in the BUYING [PROMISE and CHOICE] treatment) are the same as described in Hypothesis 1.

The behaviors specified in Hypotheses 2(i) and (ii) are not equilibrium behaviors because the political actors' beliefs do not coincide with actual behaviors specified in Hypothesis 2(iii). If the assumption of selfishness of political actors is further relaxed, incomplete political contracts can be sustained in equilibrium under certain conditions (e.g., Fehr and Schmidt, 1999; Charness and Rabin, 2002). Unlike in Hypothesis 2(iii), some candidates may deliver on their promises in the final stage of the PROMISE and CHOICE treatments. A strongly reciprocal voter in the BUYING treatment may select a candidate k who provided a sufficiently large up-front payment, even if $\pi_i(x_i; x_k) < \pi_i(x_i; x_m)$, where m is the opposition candidate.[17]

Finan and Schechter (2012) recently documented the importance of interdependent preferences for vote buying. They argue that intrinsic reciprocity may play an important role through political operatives. Specifically, they observed in an experiment that politicians in Paraguay use middlemen who successfully select more reciprocal individuals for buying votes. However, how can reciprocity alone affect the political actors' behaviors in the absence of such a selection process? As Finan and Schechter acknowledged, their results may also stem from other factors that cannot be controlled in the field, such as voters' beliefs about monitoring.[18] Moreover, if interactions between voters and middlemen are indefinitely repeated, clientelistic relationships can be sustained without assuming reciprocal preferences. Thus, to the

knowledge of the author, whether interdependent preferences alone can lead to political actors' clientelistic relationships in secret ballots remains an empirical question.

The structure of the two candidates' competition game with incomplete political contracts is a Colonel Blotto game. Due to its notorious intractability, theoretical papers on vote buying impose additional assumptions without using other-regarding utility functions. For example, Dekel *et al.* (2008) assume that voters select a candidate based on $\pi_i(x_i; x_k) + b_{k,i}$ in vote buying setups, and that a candidate *has to* honor a promise if she is elected in setups with promises. Notice that a positive vote buying situation can be an equilibrium outcome also in the present paper if we assume that voters select a candidate based on $\pi_i + b_{k,i}$ in the vote buying setup. To show this, suppose that $b_{k,i} = 0$ for k = A, B and $\forall i$, and that, without loss of generality, candidate B would win when no vote buying takes place in the BUYING treatment. In this situation, regardless of the distribution of x, candidate A has a profitable deviation to engage in vote buying and collect three votes. As an extreme example, suppose that five voters' x values are 10, 10, 10, 10, 10. If candidate A gives 51 ECUs each to any three voters, candidate A will win the election and receive 147 ECUs (= 300 – 51×3), instead of 100 ECUs (loser's payoff).[19] This suggests that there must be some positive vote-buying activities in equilibrium. Note that there exists at least one (mixed-strategy) Nash Equilibrium in a finite game according to Nash's existence theorem.

Recall that as discussed in Section 1, a series of seminal labor market experiments (e.g., Fehr *et al.*, 1993, 1998) demonstrated the role of reciprocity in a quite different employment context. As in their studies, subjects' deviation from the standard theory

prediction (Hypothesis 1) can be clearly interpreted as violation of the common knowledge of rationality, subjects' interdependent preferences, and/or reputation building behaviors, because their interactions are not indefinitely repeated. Hence, it is possible to test whether reciprocity alone is strong enough to sustain clientelistic relationships.

Hypothesis 3: *If the political actors have interdependent preferences, such as reciprocity, then (i) some voters select candidate k if k gives sufficiently large up-front payment in the BUYING treatment even if* $\pi_i(x_i; x_k) < \pi_i(x_i; x_m)$*; and (ii) some candidates deliver on promised amounts after the election in the PROMISE and CHOICE treatments.*

It should be noted that Hypotheses 2 and 3 also apply to the three repetition treatments. Indeed, repetition in the BUYING-R, PROMISE-R and CHOICE-R treatments could make incomplete contracts between voters and candidates more likely to be agreed, because it gives the actors strategic incentives to build reputations as in the logic of Kreps *et al.* (1982).[20] For example, during all but the final period, even a selfish voter in the BUYING-R treatment may strategically pretend that he is not selfish and vote for the candidate *k* who gives him the most ($\pi_i(x_i; x_k) + b_{k,i} > \pi_i(x_i; x_m) + b_{m,i}$) in an attempt to induce candidates to keep a high level of vote-buying activity in future periods. Some voters may accept only an offer by a candidate that gives the highest $\pi_i(x_i; x_k) + b_{k,i}$ and then vote for that candidate, for instance, if the voter believes that the candidate will stop bribing once the number of votes the candidate collects in an election is not equal to the number of her accepted offers.[21] Hence, repetition may amplify clientelistic relationships between the political actors. Nevertheless, selfish

political actors' behaviors in the final stage in period 10 would be the same as summarized in Hypothesis 1.

Hypothesis 4: *Repetition magnifies candidates' clientelistic behaviors and voters' reciprocation to the candidates' approaches, thereby helping sustain clientelistic relationships between the political actors.*

This study includes the CHOICE and CHOICE-R treatments because of the interest in candidates' clientelistic behaviors when both options are available. The literature on contract design in labor markets can inform on possible candidates' choices. The closest papers to these two treatments are Fehr *et al.* (2007), Fehr and Schmidt (2000), and Karakostas *et al.* (2017), although these papers used one-worker-one-firm environments unlike the two-candidates-five-voters environments employed here. For example, Fehr *et al.* (2007) and Fehr and Schmidt (2000) showed principals' overwhelming support for a bonus contract (where a principal offers base wage in advance and a non-binding promise of bonus payment after effort provision), relative to a trust contract (where a principal offers only unconditional payment to the agent in advance), when each principal randomly interacts with an agent under a perfect stranger matching protocol. Using the Fehr-Schmidt (1999) model, Fehr *et al.* (2007) argue that the cost of trusting is lower in a bonus contract than in a trust contract and that the structure of gift exchange is important for a sustainable cooperative relationship. These studies imply that candidates have the potential to affect voters' behaviors more in the CHOICE treatment than in the BUYING and PROMISE treatments, by combining up-front payment and post-election transfers to magnify the salience of reciprocity.[22] Something similar can also be expected in the CHOICE-R

treatment. However, candidates' approaches may affect voters' behaviors similarly in all three repetition treatments. This is because each voter's political stance x remains fixed for the full set of periods and is common knowledge to all the political actors, and thus candidates can form long-term gift-exchange relationships with voters irrespective of the form of political exchange process.

Candidates may put greater weight on using post-election transfers by reducing up-front payments in the CHOICE-R relative to the CHOICE treatment, because of the political actors' on-going reciprocal relationships. Even if a candidate k reduces up-front payment, she can maintain the same or even increase the total pre-election offer to voter i (i.e., $b_{k,i} + pm_{k,i}$) by enlarging promise $pm_{k,i}$. If the voter attaches some positive satisfaction to the promise made by candidate k, he would enjoy larger non-material utility in total when k relies more on promise-making because then k can also deliver large transfer $y_{k,i}$ if elected. Thus, i's perceived kindness toward k in the next period ($\tilde{f}_{iki}$) increases even if the total amount received by i ($b_{k,i} + y_{k,i}$) is the same. Further, since, in the repeated setup, voters' experiences in a given period may affect their voting behaviors in future periods, another possible driver of behavior is betrayal aversion (e.g., Bohnet and Zeckhauser, 2004; Bohnet *et al.*, 2008). If voters are averse to being betrayed, candidates may be more willing to honor their promises to avoid negative reciprocal effects of non-fulfillment. This could shift the candidates' use of resources from up-front payment to post-election delivery in a repeated setup.

These considerations can be summarized as Hypothesis 5:

Hypothesis 5: *(i) Candidates combine up-front payment and post-election transfers to magnify the salience of reciprocity. (ii) Voters reciprocate candidates' approaches more strongly in the CHOICE treatment than in the BUYING and PROMISE treatments. (iii) Candidates put greater weight on using promise-making and post-election transfers rather than up-front payment in the CHOICE-R treatment than in the CHOICE treatment.*

With which voters do candidates form incomplete contracts? This question has been extensively studied in the theoretical and empirical literature, yet to date there is little consensus. On the one hand, the literature on vote buying and clientelism suggests that candidates would target swing or mildly-opposed voters the most, because the cost of mobilizing those voters is smaller (see, e.g., Dixit and Londregan [1996] and Lindbeck and Weibull [1987] for theoretical work; and Stokes [2005], Dunning and Stokes [2007] and Corstange [2010] for empirical work). Several other studies, on the other hand, suggest that candidates would instead distribute more resources to their core voters to protect these voters from being appropriated by the opposition (see, e.g., Cox and McCubbins [1986] for theoretical work; and McGillivray [2004] and Golden and Picci [2008] for empirical work).

These two strands of literature do coincide on one prediction: candidates are less likely to attempt to detach opposition voters from the opposition candidate. This prediction can apply also to the present study, because in the experimental election considered here: (a) two candidates are polarized and have the same sized endowment and (b) candidates do not need to receive all voters' support, since a majority rule is used

in the election. Hence, the cost of mobilizing opposition voters is more expensive than that of attracting the other voters (see $\pi_i(x_i; x_e)$ in Equation (1)). Further, the design setup of this study suggests that it is reasonable to expect that candidates would target voters whose political positions *x* are near the middle of the distribution more often than core voters for providing resources. Even if a voter *i* is strongly reciprocal, the cost required for the opposition candidate *l* to attract the voter is increasing with the difference in *x* between *l* and *i*. Anticipating this cost for the competitor, a candidate should spend a larger fraction of resources attempting to attract swing (or mildly-opposed) voters than to protect their core voters from the opposition. Notice that this prediction is obtained because the experimental design excludes turnout buying. A turnout buying model predicts that candidates would attempt to reward unmobilized strong supporters because doing so is less costly than attracting swing voters (e.g., Nichter, 2008).

A similar logic can be applied to the promise setup. The target of promise-making could be more concentrated on swing (or mildly-opposed) voters if candidates have a preference for keeping their words per se (e.g., Vanberg, 2008) or if they do not want to break promises to avoid feeling guilty (e.g., Charness and Dufwenberg, 2006), since post-election transfer activities are costly.

Hypothesis 6: *(i) Candidates are less likely to target opposition voters, than swing or core voters, to detach them from the opposition. (ii) Candidates spend larger resources to attract swing voters than to protect their core voters.*

4. One-Shot Incomplete Political Contracts

This section explores the emergence of incomplete political contracts when candidates interacted with voters only once (the BUYING and PROMISE treatments). Although votes were cast with secret ballots, candidates did offer to distribute large amounts in these two treatments (Table 1), contrary to the standard theory prediction (Hypothesis 1(i)). Total vote-buying offers to the five voters ($\sum_{i=1}^{5} b_{k,i}$) were on average 69.91 ECUs in the BUYING treatment.

In the PROMISE treatment, candidates on average promised 232 ECUs in total to the five voters, a significantly larger amount compared the offers with the BUYING treatment (*p*-value = .0006, two-sided Mann-Whitney test). This is not surprising considering that making a promise does not involve payment and the condition that $pm_{k,i}$ must satisfy is not restrictive (i.e., $pm_{k,i} \leq 300$). Their post-election transfer amounts ($\sum_{i=1}^{5} y_{e,i}$) were smaller, and this promise-payment differential was significant (*p*-value = .0066, two-sided Wilcoxon signed ranks test). Nevertheless, 75.08 ECUs were still paid on average, comparable to the up-front payments in the BUYING treatment (Table 1). The strong post-election transfer behaviors support Hypothesis 3, rather than Hypothesis 2: candidates are on average not purely selfish, meaning that they have interdependent preferences (e.g., Fehr *et al.*, 1993 and 1998), and/or preferences for keeping their words (e.g., Vanberg, 2008) or for avoiding possible feelings of guilt (Charness and Dufwenberg, 2006).

<<COMP: Place Table 1 about here>>

Result 1: *Even in one-shot interactions, inconsistent with Hypothesis 1, candidates offered large up-front payments in exchange for votes. Candidates promised much larger amounts*

to voters when promise-making was available, and the winners delivered sizable amounts after the election. The strong post-election transfer behaviors support Hypothesis 3.

a. Targets of Political Contracts

With whom did candidates form incomplete contracts? A regression approach was used to answer this question (Table 2.I). The dependent variable is an (up-front payment or promise) amount that candidate *k* offered to voter *i*, and the independent variables are dummies that indicate voter *i*'s political position.[23] Two interesting patterns were found. First, in the BUYING treatment, candidates offered significantly larger amounts to voters whose political positions are near the middle of the distribution (column (1)). This resonates with the idea that candidates take the cost of mobilizing voters into account when buying votes (Hypothesis 6).

Second, in the PROMISE treatment, candidates on average promised large amounts to all kinds of voters, regardless of their political position (see the constant term in column (2)). The candidates' offers to their loyalists and swing-voters were larger than to the others, in line with Hypothesis 6(i) – however the differences were insignificant. The difference in candidates' behaviors between the two treatments could be caused by the setup that promise-making is cost-free and they interact with each other only once (not exercising the full promises does not materially hurt the candidates).

<<COMP: Place Table 2 about here>>

This kind of regression analysis may lose useful information unless candidates are homogeneous. An investigation of each candidate's raw data reveals that candidates' clientelistic behaviors were heterogeneous in the experiment, implying that simplified

theoretical discussions, like Hypothesis 6, are not precise. Candidates can be classified into three categories dependent on with which voters they attempted to form incomplete contracts (Table 2.II): (a) "targeting swing voters" for candidates who offered the most to voters whose political position (x_i) differs from the candidate's by more than 2 but less than 8 [i.e., $|x_i - 5| \leq 2$], (b) "targeting loyalists" for candidates who did so to voters whose x_i differs from candidates' by less than 3 [i.e., $|x_i - x_k| \leq 2$], and (c) "targeting ideologically-opposed voters" for candidates who did so to voters whose x_i differs from the candidates' by more than 7 [i.e., $|x_i - x_m| \leq 2$, where m is the opposition candidate]. While the likely motive for (b) is to retain loyal voters, candidates who fall into category (c) more likely intend to capture opposition voters.

The classification, summarized in Table 2.II, shows that 36.4% and 33.3% of the candidates targeted swing voters in the BUYING and PROMISE treatments, respectively. At the same time, non-negligible fractions of candidates targeted loyalists or ideologically-opposed voters. It is noted that 27.3% of candidates did not make any offers in the BUYING treatment. Considering that providing up-front payment would involve a cost, this finding implies that some candidates may have doubted the impact of vote buying in one-shot interactions. The heterogeneity in the candidates' behaviors suggests that a single theory does not explain every candidate's behavior.[24]

Result 2: *(i) When up-front payments were possible, candidates on average offered to pay larger amounts to swing voters than others before the election – in support of Hypothesis 6. When promise-making was possible, candidates promised substantial amounts to all voters. (ii) Candidates' clientelistic behaviors were heterogeneous,*

whether up-front payment or promise-making mode was used. Some targeted swing voters, others targeted loyalists, and still others targeted opposition voters.

b. Voters' Reactions to Candidates' Approaches

An analysis of voting also helps determine which theory is appropriate to explain political actors' behaviors. For example, as discussed in Section 3, candidates in the BUYING treatment can influence voting only when non-selfish preferences are assumed. This subsection will discuss voters' decisions to accept offers and choose their preferred candidates.

First, the data indicate that most voters accepted any offers. This is natural because votes were cast through secret ballots. However, 10.3% and 25.5% of offers were rejected in the BUYING and PROMISE treatments, respectively. The majority of these rejections can be explained by the relative size of offers: the offer by candidate *k* tends to be rejected if $\pi_i(x_i; x_k) + z_{k,i} < \pi_i(x_i; x_m) + z_{m,i}$.[25] Here, $z_{j,i}$ is the offer from candidate *j* to voter *i*. One possible interpretation is that some voters feel guilty about conveying their willingness to vote via acceptance of an offer if they do not plan to vote for the candidate.

Second, voters were significantly affected by the candidates' approaches, despite their ability to free ride on others' voting with ballot secrecy. Voting behaviors can be examined by classifying the data into two categories: (A) when there were conflicts between offers and political positions (i.e., $\pi_i(x_i; x_k) > \pi_i(x_i; x_m)$, and $\pi_i(x_i; x_k) + z_{k,i} < \pi_i(x_i; x_m) + z_{m,i}$), and (B) when there were no such conflicts (i.e., $\pi_i(x_i; x_k) > \pi_i(x_i; x_m)$, but also $\pi_i(x_i; x_k) + z_{k,i} \geq \pi_i(x_i; x_m) + z_{m,i}$). The data show that the

percentage of voters that selected candidates based on π_i, not $\pi_i + z_{k,i}$, is much less in the conflict situations than in the no-conflict situations in both the BUYING and PROMISE treatments (Figure 4.II). This suggests that incomplete contracting has a distortionary effect on voting, inconsistent with Hypothesis 1(ii).

Intriguingly, there is a stark difference in the percentage of conflict cases between the two treatments: while the percentage is only 5.5% in the BUYING treatment, it is 36.2% in the PROMISE treatment (Figure 4.I). The difference is significant at the 1% level.[26] This resulted from the clear difference in the size of offers before the election between the two treatments (Table 1) and suggests that promise-making can be more powerful than offering up-front payment in mobilizing voters by creating conflict situations more easily through strong promises.

As shown in Table 3, a formal regression analysis was also conducted to study the impact of candidates' offers on voting. The dependent variable is a dummy that equals 1(0) if voter *i* voted for candidate B(A). Amounts offered by the two candidates ($b_{A,i}$ and $b_{B,i}$ for the BUYING treatment, and $pm_{A,i}$ and $pm_{B,i}$ for the PROMISE treatment), the differences in the offering amount ($b_{B,i} - b_{A,i}$ or $pm_{B,i} - pm_{A,i}$), or the ratios between them ($b_{B,i}/b_{A,i}$ or $pm_{B,i}/pm_{A,i}$) are included as independent variables. The estimation found that, as expected, voters were more likely to select candidates whose political positions were closer to their own (see variables xi and x). It is at the same time confirmed that in the BUYING treatment, the larger amount a candidate *k* offered to a voter than the opposition did, the greater the likelihood that the voter voted for candidate *k* (columns (1) to (3)).[27] This result supports the view on the significance of some voters' non-selfish

preferences, and is consistent with Hypothesis 3(i). Unlike the BUYING treatment, variables indicating candidates' promises do not obtain significant coefficients in the PROMISE treatment (columns (4) to (6)). This insignificant result is inconsistent with the above discussions based on the conflict versus no-conflict situations, but might be caused by the high variance in the promise data.[28] In fact, as will be explained in Section 5, a significant distortionary effect on voting was found in the PROMISE-R treatment where many more observations are available.

<<COMP: Place Table 3 about here>>

Result 3: *(i) Candidates created conflict situations, in which* $\pi_i(x_i; x_k) < \pi_i(x_i; x_m)$ *but* $\pi_i(x_i; x_k) + z_{k,i} > \pi_i(x_i; x_m) + z_{m,i}$*, more often through promise-making than up-front payment offers. (ii) Even in one-shot interactions with secret ballots, voters' behaviors were strongly influenced by the amounts offered by candidates.*

c. Post-Election Transfer Decisions

As discussed earlier, while candidates in the PROMISE treatment on average did not deliver the full amount of the promises, they nevertheless delivered amounts comparable to those in the BUYING treatment (Table 1, Result 1). A detailed look at the data reveals that 95% of voters in the PROMISE treatment were offered promises of positive post-election transfers from elected candidates. However, the candidates' actual transfer behaviors are quite heterogeneous. Panel a of Figure 2 reports the candidates' delivery rates, defined as an elected candidate *k*'s post-election transfer to voter *i* divided by his/her promise to the voter. The figure shows that in the PROMISE treatment, while only 14.0% of the voters received full delivery or more, 66.7% received some positive

amounts.[29] The cumulative distribution exhibits a linear shape, meaning that the delivery rates voters experienced were quite diverse.

<<COMP: Place Figure 2 about here>>

A regression analysis was conducted in order to study more carefully to which voters the elected candidates transferred resources (column (1) of Table 4). The dependent variable is a post-election transfer from a successful candidate *e* to voter *i*, while the independent variables includes a dummy that indicates whether *i* accepted the offer from *e* before the election, and the variables that indicate the political position of voter *i*. Two interesting patterns emerged. First, candidates transferred significantly positive amounts to voters who accepted the offers. This implies that candidates may judge how the voters would have voted based on their accept/decline decisions. Keeping the promises to some degree can be interpreted as candidates' preferences to honor the commitment per se (e.g., Vanberg, 2008) or against feeling guilty which they might experience if they deceive voters (e.g., Charness and Dufwenberg, 2006). Second, the differences in the political position did not affect the candidates' decisions to transfer. This strengthens the view that political actors are not selfish, implying that the strength of candidates' feelings of obligation mostly depend on whether voters accepted their offers.

Result 4: *(i) When candidates promised post-election transfers to voters, they delivered significantly positive amounts only to those who accepted the offers after the election. (ii) The size of post-election transfer did not differ by the difference in the political position between a candidate and a voter.*

<<COMP: Place Table 4 about here>>

d. Social Optimality and Consequence for Voters' Welfare

No Pareto improvement is possible under incomplete contracting in this study. As discussed in Section 3, given the assumption of political actors' self-interest and common knowledge that all are of selfish type, any transfer activities (either before or after the election) are pure redistributions from candidates to voters without affecting the election outcome and accordingly the total group payoff. In other words, incomplete contracting leads to another Pareto optimal allocation. If it is instead assumed that voters are not selfish and do respond to offers, a candidate may be able to turn the competition around and obtain a higher payoff by forming appropriate clientelistic relationships, but this is not Pareto improvement either because it entails a decrease in the payoff of the other candidate.

It is possible, however, that political incomplete contracts lead to a sub-optimal outcome from a utilitarian perspective if incomplete contracting successfully reverses the election outcome.[30] Due to its distortionary effects on voting (Result 3(ii)), the election outcomes were in fact reversed in two out of 11 groups (18.18%) and four out of 11 groups (36.36%) in the BUYING and PROMISE treatments, respectively – see Appendix Table A4 for the detail.[31] Such reversing did not raise total group payoffs in all the cases. Among the six groups, in one group and three groups in the BUYING and PROMISE treatments, respectively, their realized total payoffs were lower than those calculated based on Hypothesis 1 (the assumption of rational selfish actors).[32]

Clientelism, nevertheless, materially hurt almost no voters, including those in the reversed groups, thanks to the redistribution of resources. Instead, clientelism helped

reduce within-group inequality. No voters were hurt by clientelism in nine (seven) out of 11 groups of the BUYING (PROMISE) treatment where incomplete contracting did not reverse the election outcomes. A monetary transfer to a voter in these groups was just additional earnings for the voter. Even in the groups where incomplete contracting did reverse the election outcomes, only two and ten voters in the BUYING and PROMISE treatments, respectively, experienced drops in own payoff. In sum, only 3.6% and 18.2% of voters in the BUYING and PROMISE treatments, respectively, were hurt by clientelism.[33] It is worth noting, however, that within-group inequality among the five voters was on average little changed with incomplete contracts because of Result 2(ii).[34]

Result 5: *Incomplete contracting occasionally reversed the election outcome. The reversing did not improve group total payoff.*

5. Sustained Clientelistic Relationships

It was uncovered in the previous section that incomplete political contracts do occur even though voters can free ride on peers' voting under secret ballots. Are such dyadic relationships sustainable? Does iteration magnify candidates' approaches, as suggested by Hypothesis 4, and further bias voters' voting and election outcomes? This section explores these questions using the BUYING-R and PROMISE-R treatments.

Repetition drastically changed the balance of power between candidates and voters (Table 1). First, the candidates' per-period vote-buying offers were on average 63.6% higher in the BUYING-R than in the BUYING treatment.[35] Second, while per-period promise amounts were similar between the PROMISE and PROMISE-R treatments, per-period post-election transfer amounts were 107.3% larger in the

PROMISE-R than in the PROMISE treatment.[36,37] These candidates' strong redistribution behaviors, whether before or after the election, are consistent with Hypothesis 4. With the increases in the redistribution of resources, voters in the BUYING-R treatment on average received higher payoffs than candidates; the average payoffs were similar between candidates and voters in the PROMISE-R treatment (again see Table 1).[38]

Figure 3 reports the dynamics of candidates' decisions in the BUYING-R and PROMISE-R treatments. Strikingly, the average up-front payment offers and post-election transfers were always higher, compared with the corresponding one-shot treatments, and the candidates' strong willingness to spend resources was maintained (panel a).[39] Note, however, that candidates learned to behave dishonestly over time in the PROMISE-R treatment: pre-election promises became much larger than post-election transfer amounts in later periods.

In the BUYING-R treatment, despite the stable size of offers made in each period, the fractions of cases in which positive amounts were offered declined over time (panel b of Figure 3). This suggests that candidates learned to target specific voters gradually. Nevertheless, the fractions of positive offers settled at fairly stable levels, around 60%, in later periods.

<<COMP: Place Figure 3 about here>>

To study the targets of candidates' offers, a regression analysis was again performed; however, no informative results were obtained (columns (5) and (6) in Table 2.I). This could be due to the heterogeneity in candidates' behaviors. It is thus instructive to look at with which voters candidates attempted to form incomplete contracts (Table

2.II). Two clear findings were obtained. First, all candidates made positive offers to some voters in the BUYING-R and PROMISE-R treatments. This is likely due to a repeated-game effect: selfish candidates may have strategically mimicked the behaviors of collusive candidates to maximize payoffs. Second, consistent with Result 2(ii), there was a substantial fraction of candidates for each of the three categories: targeting swing voters, loyalists, or ideologically-opposed voters. This implies that candidates have heterogeneous preferences regarding with whom they want to foster long-term relationships, suggesting that the cost of mobilizing voters may not be the most decisive factor of clientelism.

Further, the trends of candidates' clientelistic behaviors were explored by voter category (Appendix Figure A.1). Two interesting patterns emerged. First, candidates' average offers per swing voter were at much higher levels than those per ideologically-opposed voter, and the former high levels stayed stable in the BUYING-R treatment (panel a of Figure A.1). The average candidates' offers per loyalist were at similarly high levels to those per swing voter in the first six periods, but steadily decreased after that. These are in line with Hypothesis 6(i), but not Hypothesis 6(ii). However, the initial high levels of offers to loyalists resonate with the view that candidates try to prevent the opposition from appropriating their core voters. Such protection may have been vital since the political actors had repeated interactions. Second, in the PROMISE-R treatment, candidates delivered stable amounts after the election, regardless of the voter category (panel b2 of Figure A.1). This shows candidates' motives to retain clientelistic

relationships with voters. Nevertheless, the transfers to swing voters were on average higher than to any other type of voter, in support of Hypothesis 6.

Result 6: *(i) Whether up-front payment or promise-making was possible, candidates spent significantly larger resources to form incomplete contracts with voters in repeated interactions, compared with one-shot interactions; and the high levels of spending were well sustained. (ii) Interestingly, candidates' clientelistic behaviors were heterogeneous even with repetition.*

There was a clear distortionary effect of repetition on voting. As shown in Figure 4.I, in the BUYING-R treatment, voters were more frequently confronted with conflict situations ($\pi_i(x_i; x_k) > \pi_i(x_i; x_m)$, but $\pi_i(x_i; x_k) + z_{k,i} < \pi_i(x_i; x_m) + z_{m,i}$) in each period, relative to the BUYING treatment.[40] This parallels the candidates' quite stronger vote-buying offers in the BUYING-R than in the BUYING treatment (Table 1, Figure 3). As in the BUYING treatment, many voters selected a candidate based on $\pi + z$, not π, in such conflict situations in the BUYING-R treatment (panel II.a of Figure 4). Likewise, voting was affected by the size of promises in conflict situations in the PROMISE-R treatment (panel II.b of Figure 4). However, the impact of repeated interactions may be modest for the candidates' promise-making behavior: the likelihood that voters were faced with the conflict situations were similar between the PROMISE and PROMISE-R treatments (panel I.b of Figure 4). This resulted from candidates' already very large promise-making behaviors in the PROMISE treatment (Table 1).

<<COMP: Place Figure 4 about here>>

A regression analysis was also conducted to formally study the impact of incomplete contracting on voting (Table 5). The dependent variable is the voting decision. Similar to Table 3, $b_{A,i}$ and $b_{B,i}$ ($pm_{A,i}$ and $pm_{B,i}$), $b_{B,i} - b_{A,i}$ ($pm_{B,i} - pm_{A,i}$), or $b_{B,i}/b_{A,i}$ ($pm_{B,i}/pm_{A,i}$) are included as independent variables for the BUYING-R (PROMISE-R) treatment. The estimation reveals that, consistent with Result 3(ii), the higher the amount that candidate *k*, relative to the opposition, offered to voter *i*, the more likely *i* was to select *k* in the BUYING-R treatment (columns (1) of Table 5). It also uncovers that voters were significantly influenced by the relative size of candidates' promises in the PROMISE-R treatment (column (2) of Table 5).

<<COMP: Place Table 5 about here>>

Candidates' post-election transfers were also markedly amplified by repetition. First, as shown in Figure 5, the candidates' decisions to fulfill promises in the PROMISE-R treatment were clearly different from the PROMISE treatment. Specifically, voters received post-election payments greater than or equal to the promised amounts on 69.1% of occasions, with the mode being the full delivery of a promise (panel a of Figure 5). Second, while Result 4(i) extends to the repeated environment, voters who accepted promises received much larger post-election transfers in the PROMISE-R than in the PROMISE treatment (Table 4). The candidates' strong promise-keeping and the large transfer amounts in the PROMISE-R treatment may have made their promises more credible. This can partly explain why the distortionary effect on voting was more distinctly seen in Table 5 for the PROMISE-R treatment, than in Table 3 for the

PROMISE treatment. Hence, these behavioral patterns resonate with the idea that repetition helps deepen dyadic collusive relationships between political actors.

<<COMP: Place Figure 5 about here>>

It is worth noting here that the candidates' post-election transfer decisions are *not* affected by the difference in political position *x* between the candidates and voters in the PROMISE-R treatment (column (3) of Table 4). Although this is consistent with the finding in the PROMISE treatment (Result 4(ii)), this pattern is a surprise for the repeated environment, considering that candidates' costs of maintaining good long-term relationships would be smaller if they form incomplete contracts with voters whose *x* are closer to theirs. The patterns imply that candidates' might have sought to form incomplete contracts with more reciprocal voters (e.g., Finan and Schechter, 2012), judging that voters who accepted offers are the ones who supported them. It should also be noted that as in the corresponding one-shot treatments, some voters did not accept offers (panel c of Figure 3). The majority (60.0% and 61.2% of these rejections in the BUYING-R and PROMISE-R treatments, respectively) can be explained by insufficient size of offers by the candidate k: $\pi_{i,t}(x_i; x_k) + z_{k,i,t} < \pi_{i,t}(x_i; x_m) + z_{m,i,t}$.

The presence of incomplete contracts reversed the electoral competition outcomes for some groups (panels II.a and b of Appendix Table A4). There are nine and 11 groups in the BUYING-R and PROMISE-R treatments, respectively, for which the assumption of rational selfish voters (Hypothesis 1) sharply predicts that one of the two candidates would win. Among these groups, the predictions did not hold at least once in six groups (66.7%) and ten groups (90.9%) in the BUYING-R and PROMISE-R treatments,

respectively. Moreover, such reversed election outcomes were realized at least 50% of the time in two groups and three groups in the BUYING-R and PROMISE-R treatments, respectively. Consistent with the logic of Section 4.d, all the reversed election outcomes, except for those in one group, resulted in a decrease in group total payoff (see panels II.a and b of Appendix Table A4 for the detail). This suggests that the occurrence of such distortion was a stable phenomenon and the cause was not due to decision-making errors by the political actors.

Result 6: *(iii) Repetition magnified candidates' decisions to fulfill promises. (iv) Repetition also strengthened distortionary effects of incomplete contracting on voting. When candidates could provide up-front payment, voters were more likely to be confronted with conflict situations between offers and political positions in repeated than in one-shot interactions. (v) With repetition, election outcomes were flipped due to incomplete contracts in most groups at least once; and such flipping repeatedly happened in some groups.*

In sum, the results of the BUYING-R and PROMISE-R treatments support Hypothesis 4. While the general patterns of political actors' behaviors remain similar to the case of the one-shot treatments, repetition amplifies the clientelistic relationships between the political actors through magnified candidates' distributions of resources to voters. It was striking that the high levels of up-front payment and post-election transfers were sustained over the ten periods.

6. How did Candidates Combine Vote Buying and Promise-Making?

Lastly, the experimental data of the CHOICE and CHOICE-R treatments can be used to explore which option (up-front vote buying or promise-making) candidates put more weight on forming incomplete political contracts when both options were available.

First, in the CHOICE treatment, candidates on average offered to pay 60.05 ECUs to five voters before the election, which is similar to the amount in the BUYING treatment. Elected candidates further distributed a total of 65.00 ECUs on average after the election (Table 1). This behavior supports Hypothesis 5(i). The candidates' use of both up-front payment and promise-making options is similar to the principals' preference for bonus contracts in the context of employment relationships (e.g., Fehr *et al.*, 2007; Fehr and Schmidt, 2000), thereby making reciprocity salient. In line with the results for the PROMISE treatment, candidates delivered larger amounts to the voters who accepted the offers and the candidates' transfer behaviors were not affected much by the difference in the political position between candidates and voters (column (2) in Table 4). This implies that Result 4 extends to an environment where candidates can combine up-front payment and promise-making.

Second, in the environment where candidates repeated interactions, they relied more on the promise option, rather than the vote-buying option (Table 1), in line with Hypothesis 5(iii). Specifically, while candidates' up-front payment offers were on average 16.24% lower in the CHOICE-R than in the CHOICE treatment, their pre-election promise and post-election actual transfer amounts were 51.73% and 93.05% higher in the former than in the latter treatment. This implies that candidates consider strong promise-making and promise-keeping as a force of sustaining reciprocal relationships with voters.

Consistent with this interpretation, the candidates fulfilled their promises or delivered more 55.4% of the time (panel b of Figure 5). As in the other treatments, the strong deliveries were made to the voters who accepted their offers (column (4) in Table 4).

A closer look at period-by-period dynamics in the CHOICE-R treatment found that candidates' up-front payment offers ($\sum_{i=1}^{5} b_{k,i}$) gradually declined in earlier periods and then settled at around 40 ECUs in later periods, much lower than the level in the CHOICE treatment (Figure 6, Appendix Figure A.2). This trend is clearly different from the BUYING-R treatment seen in Figure 3. By contrast, the candidates' promises and post-election transfers exhibit similar dynamics to the PROMISE-R treatment. Especially, the elected candidates' post-election transfers ($\sum_{i=1}^{5} y_{e,i}$) were constantly higher in the CHOICE-R than in the CHOICE treatment except near the end periods (Figure 6.a).

<<COMP: Place Figure 6 about here>>

As in the other treatments, candidates' clientelistic behaviors were found to be heterogeneous (Table 2.II). Nevertheless, on average, they offered more to influence swing voters and protect loyalists from the opposition candidates (columns (4), (7) and (8) in Table 2.I), whose behaviors are again consistent with Hypothesis 6(i).

Consistent with Result 3(ii), candidates' approaches significantly affected voting (Table 3, Table 5). There are two points worthwhile discussing. First, while voters in the CHOICE treatment reciprocated candidates' up-front payment offers (columns (7), (9) and (11) of Table 3), they also responded positively to the candidates' promise offers (columns (10), (12) of Table 3). The latter aspect is different from the PROMISE treatment in which the coefficient estimate on neither $pm_{B,i} - pm_{A,i}$ nor $pm_{B,i}/pm_{A,i}$ is

significantly positive. This suggests that up-front payment provided to the voters made promise-making more credible to the voters in the CHOICE treatment, supporting the view of Hypothesis 5(ii).

Second, with repetition, the degrees of voters' reciprocation are not much different between the CHOICE-R treatment and the other two repetition treatments (Table 5). For example, the coefficient estimates on $b_{B,i} - b_{A,i}$ ($pm_{B,i} - pm_{A,i}$) and $b_{B,i}/b_{A,i}$ ($pm_{B,i}/pm_{A,i}$) are significantly positive in both the CHOICE-R and BUYING-R (PROMISE-R) treatments.

The impact of repeated interactions for the CHOICE-R treatment can be seen in Figure 4. Voters sometimes selected a candidate based on $\pi + b + pm$ in conflict situations ($\pi_i(x_i; x_k) > \pi_i(x_i; x_m)$, but $\pi_i(x_i; x_k) + b_{k,i} + pm_{k,i} < \pi_i(x_i; x_m) + b_{m,i} + pm_{m,i}$) in both the CHOICE and CHOICE-R treatments (panel II of Figure 4). Yet such situations occurred significantly more frequently in the CHOICE-R than in the CHOICE treatment (panel I of Figure 4).[41] This again underscores the strong impact of repetition on deepening clientelistic relationships between the political actors, as summarized in Hypothesis 4.

The remaining results found in the other four treatments hold also for the two choice treatments, meaning that the findings discussed earlier are robust to the different setups. Among others, it is worth noting that election outcomes were distorted by the presence of incomplete contracts, especially in the CHOICE-R treatment (see panel II.c of Appendix Table A4), in line with Results 5 and 6. All groups experienced the situation where the elected candidates were not the same as predicted by the assumption of selfish

voters (Hypothesis 1(ii)) at least once in the CHOICE-R treatment. Further, such reversed election outcomes were observed on more than half of occasions in three of the groups. These observations can be explained by the candidates' largest redistribution activities in the CHOICE-R treatment (Table 1).

Result 7: *(i) Candidates used both the vote-buying and promise options equally in the CHOICE treatment. However, they relied more on the promise option in the CHOICE-R than in the CHOICE treatment. (ii) In one-shot interactions, voters' reciprocation to candidates' approaches was stronger in the CHOICE than in the PROMISE treatment. However, under repeated interactions, the degrees of such reciprocation were similarly strong for the three political exchange formats, due to the strong impact of repetition on amplifying clientelistic relationships already seen in the BUYING-R and PROMISE-R treatments.*

7. Discussion and Conclusion

This paper studied how candidates form incomplete contracts with voters in elections and how voters respond to the candidates' offers when they can free ride on others' voting under secret ballots. It was found that candidates do offer to buy votes before the election and the voters' behaviors are significantly affected by such candidates' approaches, even without any reputation concerns. When a promise option was instead available, the candidates promised to make substantial post-election transfers and the voters who received large promises were more likely to vote for the candidates. When candidates were elected, they delivered non-negligible amounts to voters who accepted the offers. These findings suggest that candidates and voters have interdependent

preferences, and thus even with secret ballots and in the absence of any enforcement mechanisms, they may be able to resolve double commitment problems on their own. Further, with repetition, candidates' offers substantially increased and their clientelistic behaviors were sustained over time. This suggests the significance of political actors' strategic behaviors in deepening dyadic relationships. The rapid growth of empirical work has suggested that there are blind points of secret ballots in many democratic countries, such as monitoring (e.g., Kitschelt and Wilkinson, 2007), whereby leading to the maintenance of clientelism. The findings of this study, however, imply that clientelism can evolve due to people's interdependent preferences and strategic reputation building behaviors, even if the secrecy of ballots is strictly enforced and voters can free ride on other voters.

This study is related to a large volume of prior experimental research on relational employment contracts (e.g., Fehr *et al.*, 1993 and 1998; Fehr *et al.*, 1997; Fehr and Falk, 1999; Brown *et al.*, 2004 and 2012; Fehr *et al.*, 2007). These prior experiments suggest that a worker does reciprocate her employer's gift-giving and that the two parties can enjoy large gains from trade, especially when they foster long-term reciprocal relationships. They also showed employers' preference for bonus contracts in one-worker-one-firm environments (e.g., Fehr and Schmidt, 2000; Fehr *et al.*, 2007). The present paper considers the functioning of the three political exchange processes – vote buying, promise-making, and choice (which is similar to a bonus contract in a labor market) – in elections. This is a quite different setup from prior gift-exchange experiments in labor markets in that each candidate can selectively form incomplete

contracts with multiple voters at the same time, voters can form contracts with both candidates, voters can free ride on others' voting behaviors, and only one candidate wins. The results of the present paper clearly suggest that the political actors' possible concerns for fairness and reciprocity are indeed strong enough to resolve the commitment problem between political actors.

One may wonder more precisely what behavioral preferences motivated voters to deviate from the prediction that voters select candidates solely based on their political position (Hypothesis 1(ii)). The first possibility is that voters' inequity aversion drove some voters to select a corrupt candidate (e.g., Fehr and Schmidt, 1999). However, this possibility cannot fully explain the voters' behaviors. Recall that the voting behaviors in the experiment were severely affected by the size of $\pi_i(x_i; x_k) + z_{k,i}$. On the one hand, a voter *i* who is strongly averse to coming out ahead in a conflict situation (i.e., $(x_i; x_k) < \pi_i(x_i; x_m)$, but $\pi_i(x_i; x_k) + z_{k,i} > \pi_i(x_i; x_m) + z_{m,i}$) may vote for candidate *k* if *i* believes that she received a sufficiently large offer, i.e., she is wealthier than the other voters. The intention is to increase the likelihood that candidate *k* wins, since it would reduce the voter's own payoff while on average increase the other voters' payoffs. This story resonates with the data: many subjects did vote for the candidate *k* that gives the largest $\pi + z$ in such conflict situations. On the other hand, as discussed along with Figure 4.II, when not only $\pi_i(x_i; x_k) > \pi_i(x_i; x_m)$, but also $\pi_i(x_i; x_k) + z_{k,i} \geq \pi_i(x_i; x_m) + z_{m,i}$, almost all voters selected candidate *k*. This pattern does not fit well with the inequity aversion idea, because if some voters are strongly averse to coming out ahead, they would instead vote for candidate *m*, i.e., the candidate whose political position is far away from the voters in

no conflict situations to help reduce inequality.[42] The second possibility is that voters' reciprocity helps the political actors resolve the commitment problem, creating a contractual sense of obligation to vote for a corrupt candidate. For example, a reciprocity model, such as Rabin (1993), does suggest that a reciprocal voter *i* increases her own utility if she responds kindly to kind intentions by her matched candidate through voting. The reciprocity idea seems to be more appropriate in explaining why the size of $\pi_i(x_i; x_k)$ + $z_{k,i}$ affected voting in the experiment whether voters were faced with conflict or non-conflict situations.

While the findings obtained from the experiment is clear, it is worth emphasizing that this study is only a first step in understanding the mechanisms of incomplete political contracts through the methodology of controlled experiments. In the experimental election used here, two polarized candidates A and B competed with each other to collect votes on a one-dimensional policy space via incomplete contracting. For simplicity, the candidates' election payoffs were solely determined by the vote outcome. However, the reality is more complex: for example, candidates may want to form incomplete contracts based on ideological similarity. A candidate may enjoy a positive utility (incur a negative utility) when she forms a clientelistic relationship with voters whose ideologies are similar to (far away from) hers. In addition, while it was assumed in this experiment that candidates' policy selection coincides with their political positions, this may not always be the case. For instance, candidates in reality can select policies different from their ideologies. Also, candidates' political positions may not always be situated at two extremes, unlike the experimental setup. Further experiments will be required before the results can be

generalized regarding with which voters candidates form clientelistic relationships. For example, a new experiment could be designed by (a) letting candidates select policies after the election stage and/or by (b) setting the payoff of a candidate as being dependent on the difference in political ideology between her and the voters with whom she forms contracts. Allowing candidates to have flexibility in their political positions would be another way to uncover people's incomplete contracting behaviors.

Of course, there are a number of other directions for further research. For instance, it would be interesting to explore how the prevalence of incomplete political contracts changes when the number of candidates is more than two, or when the ability of candidates or the size of their support base is asymmetric. As another example, it would be worthwhile studying incomplete political contracts when roles (politician or voter) are non-randomly assigned. The characteristics of politicians are likely different from voters in practice. It is possible to design an experiment that allocates the role of candidate or voter, dependent on the subjects' backgrounds, views or behaviors. For example, pre-experiment data could be collected by conducting a survey or even an incentivized task from experiment participants. Alternatively, studying how candidates combine different kinds of buying, including turnout buying and abstention buying, is also an exciting direction (e.g., Nichter, 2008; Gans-Morse *et al.*, 2014). This paper used compulsory voting to study the prevalence of vote buying and promise making in a controlled environment. As discussed, however, other types of buying are also known to be important, and these are ripe for further theoretical research under the assumption that political actors are not selfish (e.g., inequity averse, reciprocal), or in the absence of common knowledge of rationality.

Care needs to be exercised in extrapolating any findings from a laboratory to the field. Until recently, almost all studies on clientelism were based on field data. For example, in his survey paper, Hicken (2011) discussed that research on clientelism was mostly conducted based on specific case studies, but explained the potential weakness of these methods as follows: "The case-study approach has produced works of scholarship that are notable for their empirical richness … are limited by the standard problem of generalizability… their very creativity can make replicability a challenge." Laboratory experiments, along with public opinion surveys and field experiments, are complementary research tools, considering their controllability and ease of replication. The usefulness of laboratory experiments to explain field behaviors were demonstrated in the context of employer-worker, gift-exchange relationship. As summarized by Fehr *et al.* (2009) and Charness and Kuhn (2011), many scholars explored the role of fairness and reciprocity by conducting clever experiments in real-life work environments (e.g., librarian work, salespeople work, production performance in manufacturing companies). The literature has shown that the behaviors of student subjects in laboratories are similar to those of real workers, such as firm employees, soldiers and fishermen, when they were brought to laboratories as human subjects. It has also revealed that gift exchange is common in one-time employment exchanges (with no possibility of further work), as well as in ongoing employment relations, in their real workplace settings. As discussed in Section 3, theoretically, the behavioral factors that could drive clientelism share similarities with those behind employment contracts. Thus, prior work into gift-exchange relationships implies that the findings obtained in this study is informative to the field behaviors on

clientelism. Having this said, more laboratory experiments in more realistic setups, such as the ones already discussed above, are definitely meaningful as a robustness check. Framed experiments, by relaxing neutral framing adopted as a standard protocol of economic experiments, would also be another step to gain greater insight into the external validity of the lab in election settings. For example, it is possible to design a new laboratory experiment while using real political structure as well as political party names in a given society (e.g., Democratic or Republican Party in the United States; Conservative, Labour, or Liberal Democrats Party in the United Kingdom), or even using real politician names. It is unquestionably worthwhile to explore clientelistic behaviors in more depth.

REFERENCE

Abeler, Johannes, Sebastian Kube, Steffen Altmann, and Matthias Wibral. 2010. "Gift Exchange and Workers' Fairness Concerns: When Equality is unfair," 8 *Journal of European Economic Association* 1299-1324.

Andreoni, James, and Rachel Croson. 2008. "Partners versus Stranger: Random Rematching in Public Goods Experiments." Chapter 82 in *Handbook of Experimental Economics Results*. Amsterdam: North Holland.

Andreoni, James, and John Miller. 1993. "Rational cooperation in the finitely repeated prisoners' dilemma: experimental evidence," 37 *Economic Journal* 291-304.

Bock, Olaf, Ingmar Baetge, and Andreas Nicklisch. 2014. "hroot: Hamburg Registration and Organization Online Tool," 71 *European Economic Review* 117-120.

Bohnet, Iris, Fiona Greig, Benedikt Herrmann, and Richard Zeckhauser. 2008. "Betrayal Aversion: Evidence from Brazil, China, Oman, Switzerland, Turkey, and the United States," 98 *American Economic Review* 294-310.

Bohnet, Iris, and Richard Zeckhauser. 2004. "Trust, risk and betrayal," 55 *Journal of Economic Behavior & Organization* 467-484.

Bolton, Gary, and Axel Ockenfels. 2000. "ERC: A Theory of Equity, Reciprocity, and Competition," 90 *American Economic Review* 166-193.

Bond, Robert, Christopher Fariss, Jason Jones, Adam Kramer, Cameron Marlow, Jaime Settle, and James Fowler. 2012. "A 61-million-person experiment in social influence and political mobilization," 489 *Nature* 295-298.

Brown, Martin, Armin Falk, and Ernst Fehr. 2004. "Relational Contracts and the Nature of Market Interactions," 72 *Econometrica* 747-780.

Brown, Martin, Armin Falk, and Ernst Fehr. 2012. "Competition and Relational Contracts: The Role of Unemployment as a Disciplinary Device," 10 *Journal of the European Economic Association* 887-907.

Brusco, Valeria, Marcelo Nazareno, and Susan Stokes. 2004. "Vote Buying in Argentina," 39 *Latin American Research Review* 66-88.

Calvo, Ernesto, and Maria Victoria Murillo. 2004. "Who Delivers? Partisan Clients in the Argentine Electoral Market," 48 *American Journal of Political Science* 742-757.

Camerer, Colin, George Loewenstein and Matthew Rabin (eds.), 2003. *Advances in Behavioral Economics*. Princeton University Press.

Casella, Alessandra, Aniol Llorente-Saguer, and Thomas Palfrey. 2012. "Competitive Equilibrium in Markets for Votes," 120 *Journal of Political Economy* 593-658.

Casella, Alessandra, Thomas Palfrey, and Sébastien Turban. 2014. "Vote trading with and without party leaders," 112 *Journal of Public Economics* 115-128.

Charness, Gary, and Martin Dufwenberg. 2006. "Promises and partnership," 74 *Econometrica* 1579-1601.

Charness, Gary, and Peter Kuhn. 2007. "Does Pay Inequality Affect Worker Effort? Experimental Evidence," 25 *Journal of Labor Economics* 693-723.

Charness, Gary, and Peter Kuhn. 2011. "Chapter 3 - Lab Labor: What Can Labor Economists Learn from the Lab?" 4 *Handbook of Labor Economics* 229-330.

Charness, Gary, and Matthew Rabin. 2002. "Understanding Social Preferences with Simple Tests," 117 *Quarterly Journal of Economics* 817-869.

Corstange, Daniel. 2010, "Vote Buying under Competition and Monopsony: Evidence from a List Experiment in Lebanon," 2010 Annual Conference of the American Political Science Association.

Cox, Gary, and Mathew D. McCubbins. 1986. "Electoral Politics as a Redistributive Game," 48 *Journal of Politics* 370-389.

Dal Bó, Ernesto. 2007. "Bribing Voters," 51 *American Journal of Political Science* 789-803.

Dal Bó, Pedro. 2005. "Cooperation under the Shadow of the Future: Experimental Evidence from Infinitely Repeated Games," 95 *American Economic Review* 1591-1604.

Dekel, Eddie, Matthew Jackson, and Asher Wolinsky. 2008. "Vote Buying: General Elections," 116 *Journal of Political Economy* 351-380.

Dellavigna, Stefano, John List, Ulrike Malmendier, Gautam Rao. 2017. "Voting to Tell Others," 84 *Review of Economic Studies* 143-181.

Dixit, Avinash, and John Londregan. 1996. "The Determinants of Success of Special Interests in Redistributive Politics," 58 *Journal of Politics* 1132-1155.

Dufwenberg, Martin, and Georg Kirchsteiger. 2004. "A theory of sequential reciprocity," 47 *Games and Economic Behavior* 268-298.

Dunning, Thad, and Susan Stokes. 2007. "Persuasion vs. Mobilization," mimeo.

Fehr, Ernst, and Simon Gächter. 2000. "Cooperation and Punishment in Public Goods Experiments," 90 *American Economic Review* 980-994.

Fehr, Ernst, and Simon Gächter. 2000. "Altruistic punishment in humans," 415 *Nature* 137-140.

Fehr, Ernst, Simon Gächter and Georg Kirchsteiger. 1997. "Reciprocity as a Contract Enforcement Device: Experimental Evidence," 65 *Econometrica* 833-860.

Fehr, Ernst, Lorenz Goette, and Christian Zehnder. 2009. "A Behavioral Account of the Labor Market: The Role ofFairness Concerns," 1 *Annual Review of Economics* 355-384.

Fehr, Ernst, Erich Kirchler, Andreas Weichbold, and Simon Gächter. 1998. "When Social Norms Overpower Competition: Gift Exchanges in Experimental Labor Markets," 16 *Journal of Labor Economics* 324-351.

Fehr, Ernst, Georg Kirchsteiger, and Arno Riedl. 1993. “Does Fairness Prevent Market Clearing? An Experimental Investigation,” 108 *Quarterly Journal of Economics* 437-459.

Fehr, Ernst, Georg Kirchsteiger, and Arno Riedl. 1998. “Gift Exchange and Reciprocity in Competitive Experimental Markets,” 42 *European Economic Review* 1-34.

Fehr, Ernst, Alexander Klein, and Klaus Schmidt. 2007. “Fairness and Contract Design,” 75 *Econometrica* 121-154.

Fehr, Ernst, and Klaus Schmidt. 1999. “A Theory of Fairness. Competition, and Cooperation,” 114 *Quarterly Journal of Economics* 817-868.

Fehr, Ernst, and Klaus Schmidt. 2000. “Fairness, Incentives, and Contractual Choices,” 44 *European Economic Review* 1057-1068

Fehr, Ernst, and Klaus Schmidt. 2006. “The Economics of Fairness, Reciprocity and Altruism— Experimental Evidence and New Theories,” In *Handbook of the Economics of Giving, Altruism and Reciprocity* by S.-G. Kolm and J. M. Ythier (eds.), 615-91. North Holland.

Finan, Frederico, and Laura Schechter. 2012. “Vote-Buying and Reciprocity,” 80 *Econometrica* 863-881.

Fischbacher, Urs. 2007. “z-Tree: Zurich toolbox for ready-made economic experiments,” 10 *Experimental Economics* 171-178.

Fox, Jonathan. 1994. “The Difficult Transition from Clientelism to Citizenship: Lessons from Mexico,” 46 *World Politics* 151-184.

Gächter, Simon, Daniele Nosenzo, and Martin Sefton. 2012. "The Impact of Social Comparisons on Reciprocity," 114 *Scandinavian Journal of Economics* 1346-1367.

Gans-Morse, Jordan, Sebastián Mazzuca, and Simeon Nichter. 2014. "Varieties of Clientelism: Machine Politics during Elections," 58 *American Journal of Political Science* 415-432.

Gerber, Alan, Gregory Huber, David Doherty, and Conor Dowling. 2012. "Is There a Secret Ballot? Ballot Secrecy Perceptions and Their Implications for Voting Behaviour," 43 *British Journal of Political Science* 77-102.

Golden, Miriam, and Lucio Picci. 2008. "Pork-Barrel Politics in Postwar Italy, 1953–94," 52 *American Journal of Political Science* 268-289.

Gonzalez-Ocantos, Ezequiel, Chad Kiewiet de Jonge, Carlos Meléndez, Javier Osorio, and David W. Nickerson. 2012. "Vote Buying and Social Desirability Bias: Experimental Evidence from Nicaragua," 56 *American Journal of Political Science* 202-217.

Groseclose, Tim, and James Snyder. 1996. "Buying Supermajorities," 90 *American political Science Review* 303-315.

Hertel-Fernandez, Alexander. 2017. "American Employers as Political Machines," 79 *Journal of Politics* 105-117.

Hicken, Allen. 2011. "Clientelism," 14 *Annual Review of Political Science* 289-310.

Hidalgo, Daniel, and Simeon Nichter. 2016. "Voter Buying: Shaping the Electorate through Clientelism," 60 *American Journal of Political Science* 436-455.

Kamei, Kenju. 2019. “The power of joint decision-making in a finitely-repeated dilemma,” 71 *Oxford Economic Papers* 600-622.

Kamei, Kenju. 2020. “Voluntary disclosure of information and cooperation in simultaneous-move economic interactions,” 171 *Journal of Economic Behavior & Organization* 234-246.

Karakostas, Alexandros, Axel Sonntag, and Daniel Zizzo. 2017. “Contract Choice: Efficiency and Fairness in Revenue-Sharing Contracts,” 119 *Scandinavian Journal of Economics* 962-986.

Kitschelt, Herbert, and Steven Wilkinson (eds.). 2007. *Patrons, Clients, and Policies – Patterns of Democratic Accountability and Political Competition*. Cambridge University Press.

Kreps, David, Paul Milgrom, John Roberts and Robert Wilson. 1982. “Rational Cooperation in the Finitely Repeated Prisoners’ Dilemma,” 27 *Journal of Economic Theory* 245-252.

Lindbeck, Assar, and Jörgen W. Weibull. 1987. “Balanced-Budget Redistribution as the Outcome of Political Competition,” 52 *Public Choice* 273-297.

Lizzeri, Alessandro, and Nicola Persico. 2001. “The Provision of Public Goods under Alternative Electoral Incentives,” 91 *American Economic Review* 225-239.

Maximiano, Sandra, Randolph Sloof, and Joep Sonnemans. 2007. “Gift Exchange in a Multi-Worker Firm,” 117 *Economic Journal* 1025-1050.

Morgan, John, and Felix Várdy. 2006. “Corruption, Competition, and Contracts: A Model of Vote Buying,” IMF Working Paper.

Myerson, Roger. 1993. “Incentives to Cultivate Favored Minorities under Alternative Electoral Systems,” 87 *American Political Science Review* 856-869.

Nichter, Simeon. 2008. “Vote Buying or Turnout Buying? Machine Policies and the Secret Ballot,” 102 *American Political Science Review* 19-31.

Ostrom, Elinor. 1998. “A Behavioral Approach to the Rational Choice Theory of Collective Action: Presidential Address, American Political Science Association, 1997,” 92 *American Political Science Review* 1-22.

Pande Rohini. 2011. “Can Informed Voters Enforce Better Governance? Experiments in Low-Income Democracies,” 3 *Annual Review of Economics* 215-237.

Rabin, Matthew. 1993. “Incorporating Fairness into Game Theory and Economics,” 83 *American Economic Review* 1281-1302.

Robinson, James, and Thierry Verdier. 2013. “The Political Economy of Clientelism,” 115 *Scandinavian Journal of Economics* 260-291.

Rueda, Miguel. 2017. “Small Aggregates, Big Manipulation: Vote Buying Enforcement and Collective Monitoring,” 61 *American Journal of Political Science* 163-177.

Scheiner, Ethan. 2007. “Clientelism in Japan: the importance and limits of institutional explanations.” In H. Kitschelt and S. Wilkinson (eds.), *Patrons, Clients, and Policies – Patterns of Democratic Accountability and Political Competition* pp.276-297, Cambridge University Press.

Scott, James. 1972. “Patron-Client Politics and Political Change in Southeast Asia,” 66 *American Political Science Review* 91-113.

Snyder, James. 1991. “On Buying Legislatures,” 3 *Economics and Politics* 93-109.

Stokes, Susan. 2005. “Perverse Accountability: A Formal Model of Machine Politics with Evidence from Argentina,” 99 *American Political Science Review* 315-525.

Stokes, Susan, Thad Dunning, Marcelo Nazareno, and Valeria Brusco. 2013. *Brokers, Voters, and Clientelism: The Puzzle of Distributive Politics*. Cambridge University Press.

Tonguc, Ozlem, and Erkut Ozbay. 2018. Vote Buying with and without Binding Promises, Working paper.

Vanberg, Christoph. 2008. “Why Do People Keep Their Promises? An Experimental Test of Two Explanations,” 76 *Econometrica* 1467-1480.

Vicente, Pedro. 2012. “Is Vote Buying Effective? Evidence from a Field Experiment in West Africa,” 124 *Economic Journal* F356-F387.

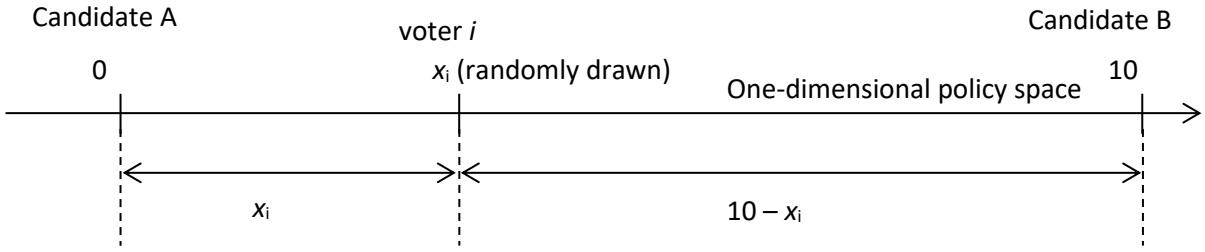
Wantchekon, Leonard. 2003. “Clientelism and Voting Behavior: Evidence from a Field Experiment in Benin,” 55 *World Politics* 399-422.

Zhou, Wenting, and John Hey. 2018. “Context Matters,” 21 *Experimental Economics* 723-756.

Figure 1: *Structure of Experimental Electoral Competition*

Payoff of candidates

	candidate A	candidate B
(a) A wins:	300 [ECUs]	100 [ECUs]
(b) B wins:	100 [ECUs]	300 [ECUs]

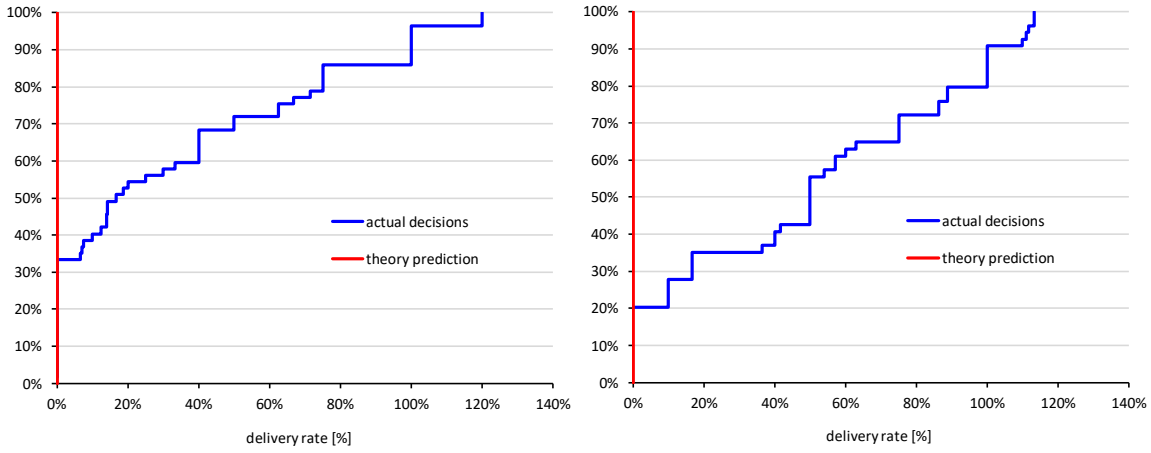


Payoff of voter *i*

$100 - \frac{1}{2}x_i^2$ [ECUs]
if candidate A is elected

$100 - \frac{1}{2}(10 - x_i)^2$ [ECUs]
if candidate B is elected

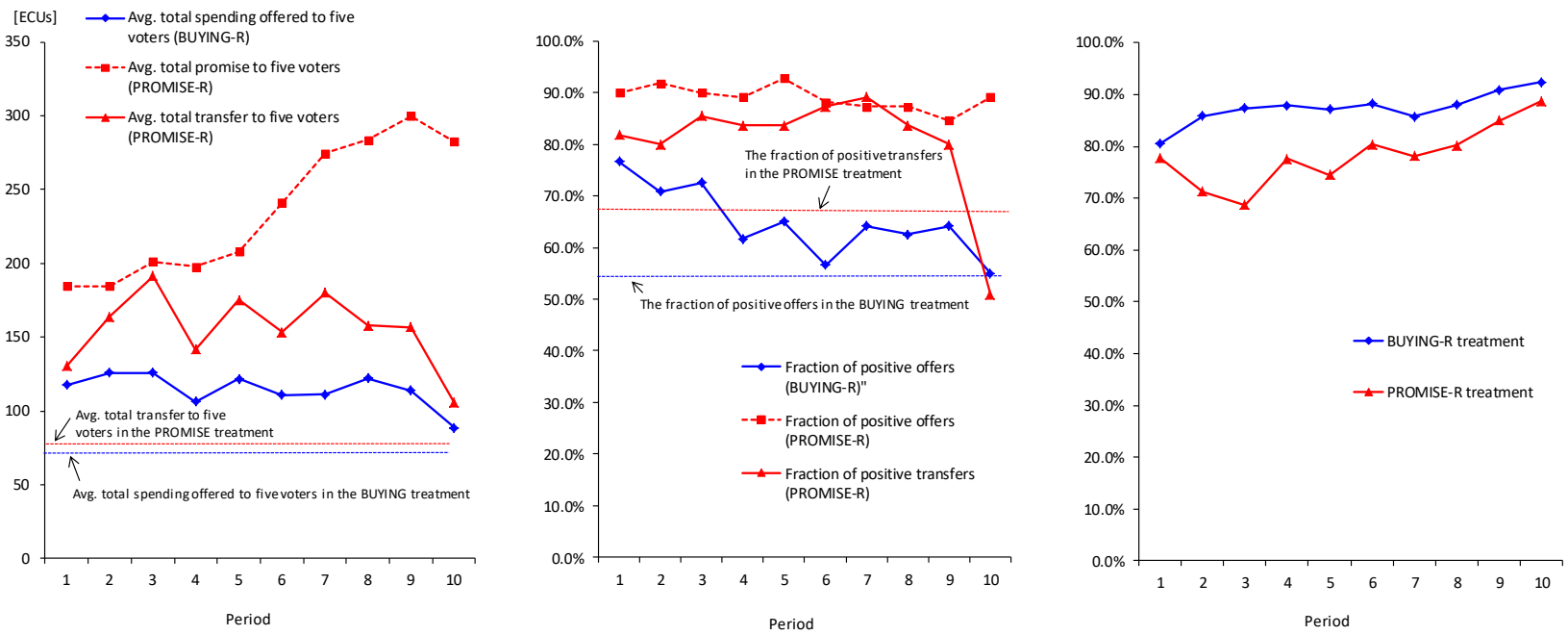
Figure 2: *Candidates' Delivery Rates in One-Shot Interactions*



(a) PROMISE treatment (b) CHOICE treatment

Notes: Cumulative distributions. Data used in the graphs are the cases in which candidates made promises of positive post-election transfers. 33.3% (20.4%) of the voters in the PROMISE (CHOICE) treatment did not receive anything after the election. 14.0% (20.4%) of the voters in the PROMISE (CHOICE) treatment received exactly the same amounts promised or larger amounts.

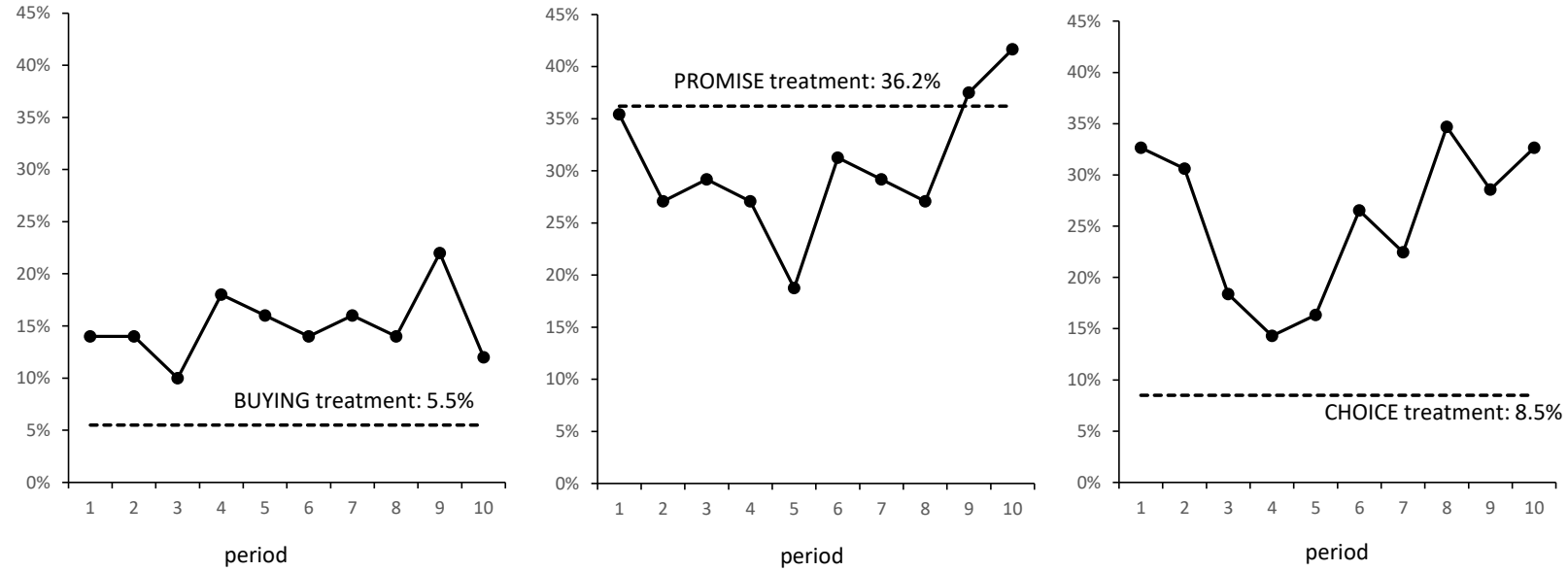
Figure 3: *Trends of Candidates' Average Offers and Post-Election Transfers in the BUYING-R and PROMISE-R Treatments*



(a) Offers and transfers (b) Fractions of positive offers (c) Fraction of acceptance

Notes: The avg. total spending offered in the BUYING-R treatment is the average of $\sum_{i=1}^{5} b_{k,i}$ across all candidates. The avg. total promise (transfer) in the PROMISE-R treatment is the average of $\sum_{i=1}^{5} pm_{k,i}$ ($\sum_{i=1}^{5} y_{e,i}$) across all candidates (all elected candidates).

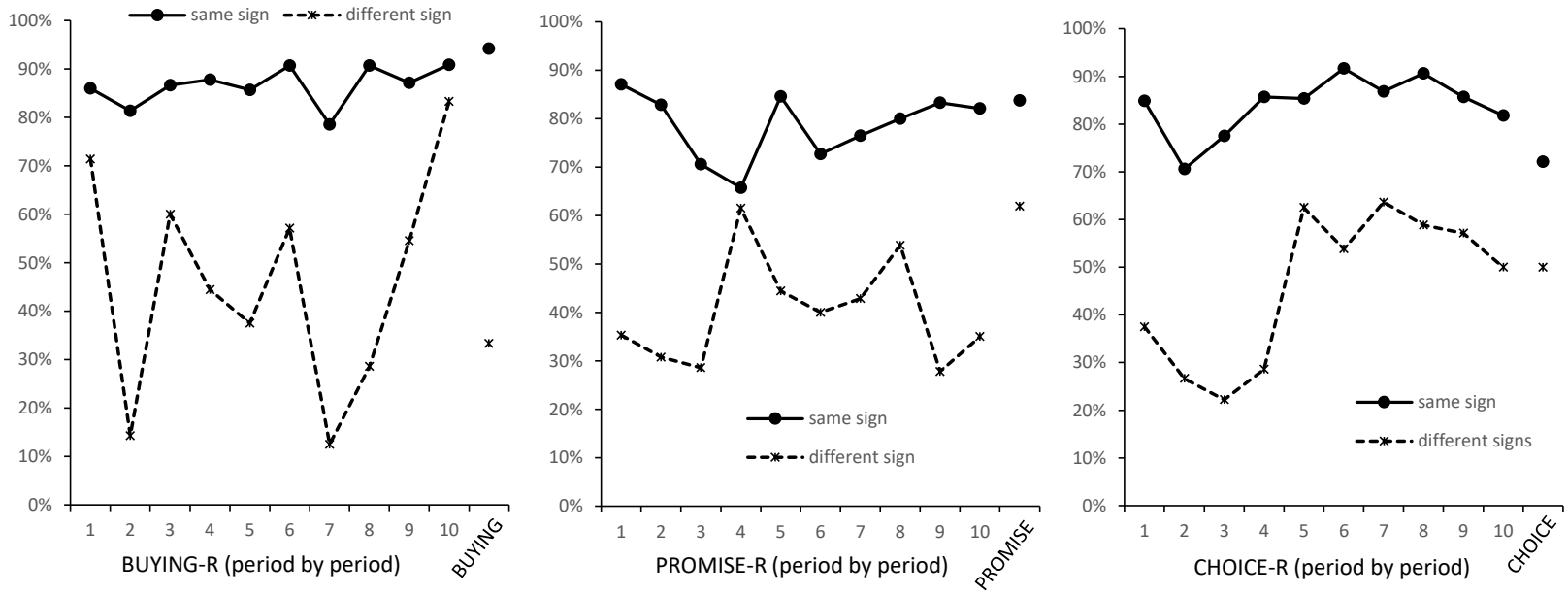
Figure 4: *The Size of Candidates' Offers and Voting Behaviors*



(a) BUYING and BUYING-R treatments (b) PROMISE and PROMISE-R treatments (c) CHOICE and CHOICE-R treatments

(I) The Percentage of the Cases in which the Sum of $\pi_i(x_i; x_k)$ and the Offer from Candidate k is Greater than the Sum calculated for the other candidates when $\pi_i(x_i; x_k) < \pi_i(x_i; x_m)$

Notes: $\pi_i(x_i; x_k)$ is given by the first term of Equation (1) assuming that candidate k wins the election. The sums of $\pi_i(x_i; x_k)$ and the offer from candidate k, $z_{k,i}$, are $\pi_i(x_i; x_k) + b_{k,i}$, $\pi_i(x_i; x_k) + pm_{k,i}$, and $\pi_i(x_i; x_k) + b_{k,i} + pm_{k,i}$ in panel a, panel b, and panel c, respectively. The solid connected lines are the period-by-period percentages for the repetition treatments, while the dashed lines are the percentages for the corresponding one-shot treatments.

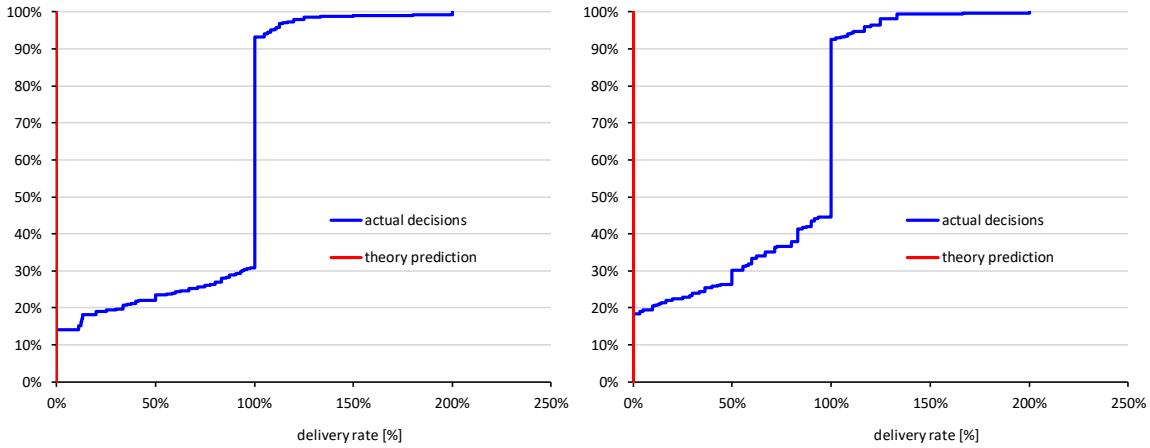


(a) BUYING and BUYING-R treatments (b) PROMISE and PROMISE-R treatments (c) CHOICE and CHOICE-R treatments

(II) The Percentage of the Cases in which Voter *i* Selected a Candidate based on the Size of $\pi_i(x_i; x_k)$, not $\pi_i(x_i; x_k) + z_{k,i}$

Notes: The "same sign" in each panel includes cases in which $\pi_i(x_i; x_k) > \pi_i(x_i; x_s)$ and $\pi_i(x_i; x_k) + z_{k,i} \geq \pi_i(x_i; x_m) + z_{m,i}$, where $z_{k,i}$ indicates the offer from candidate k to voter i. The "different sign" in each panel includes cases in which $\pi_i(x_i; x_k) > \pi_i(x_i; x_m)$ but $\pi_i(x_i; x_k) + z_{k,i} < \pi_i(x_i; x_m) + z_{m,i}$.

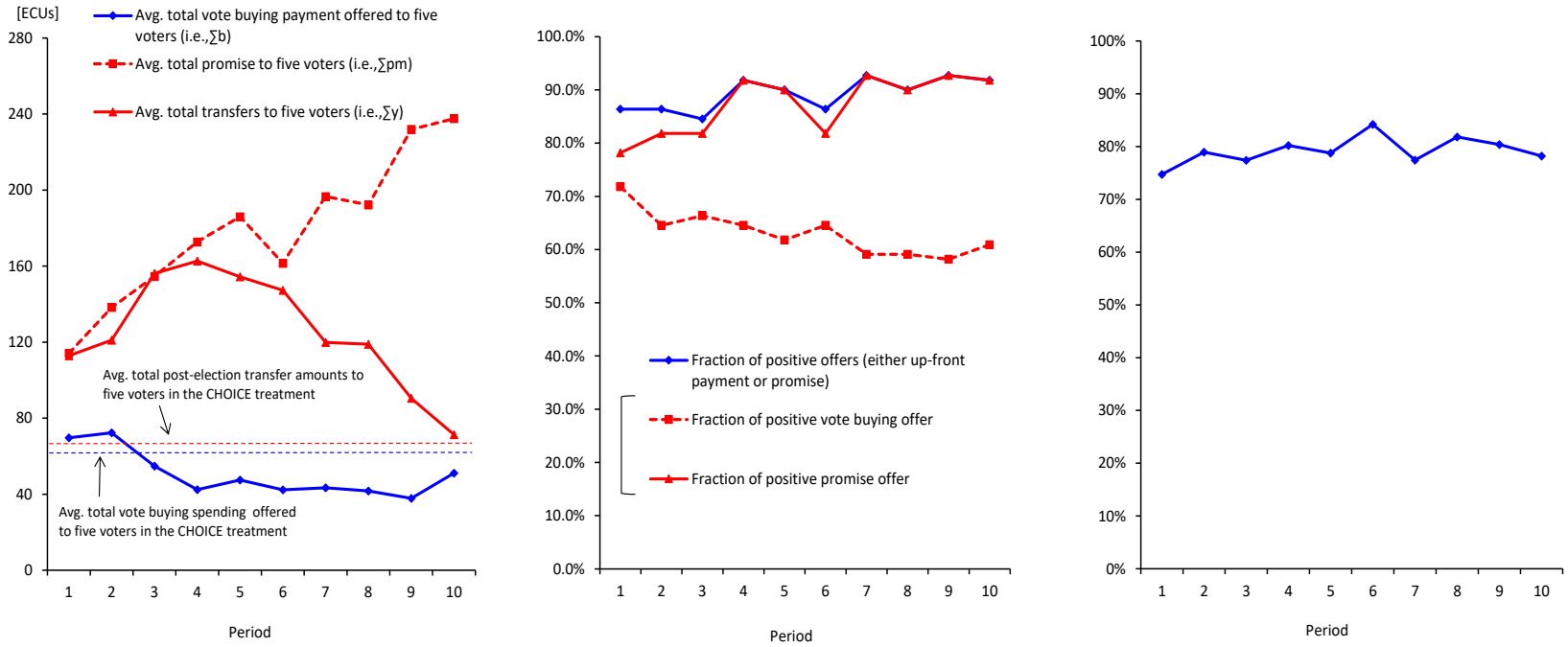
Figure 5: *Candidates' Delivery Rates in the Repeated Interactions*



(a) PROMISE-R treatment (b) CHOICE-R treatment

Notes: Cumulative distributions. Data used in the graphs are the cases in which candidates made promises of positive post-election transfers. The voters in the PROMISE-R (CHOICE-R) treatment did not receive anything after the election 14.1% (18.5%) of the time. The voters in the PROMISE-R (CHOICE-R) treatment received greater than or equal to the promised amount 69.1% (55.4%) of the time.

Figure 6: *Trends of Candidates' Average Offers and Post-Election Transfers in the CHOICE-R Treatment*



(a) Offers and transfers (b) Fractions of positive offers (c) Fraction of acceptance

Notes: The avg. total vote-buying payment offered is the average of $\sum_{i=1}^{5} b_{k,i}$ across all candidates. The avg. total promise (transfer) is the average of $\sum_{i=1}^{5} pm_{k,i}$ ($\sum_{i=1}^{5} y_{e,i}$) across all candidates (all elected candidates).

Table 1: *Summary of Treatments*

Treatment name:	Political exchange	Number of periods	Number of political actors (groups)	Avg. total offers to five voters: Vote Buying[1]	Avg. total offers to five voters: Promise Amount[2]	Avg. total transfer amounts to five voters[3]	% of reversed election outcomes[4]	Avg. per-period payoff: (a) Candidate	Avg. per-period payoff: (b) Voter	(a)/(b)	Avg. Gini coefficient
I. Vote Buying											
BUYING	Vote buying	1	77 (11)	69.91 [52.73%]	---	---	18.18%	146.77	113.23	1.30	.217
BUYING-R		10	84 (12)	114.34 [64.92%]	---	---	25.56%	98.64	127.09	0.78	.159
II. Promise-Making											
PROMISE	Promise	1	84 (12)	---	232.00 [91.67%]	75.08 [65.00%]	36.36%	162.46	102.25	1.59	.190
PROMISE-R		10	77 (11)	---	235.80 [89.00%]	155.65 [80.55%]	29.09%	122.17	117.59	1.04	.099
III. Choice											
CHOICE	Vote buying & promise	1	77 (11)	60.05 [82.73%]	117.68 [90.91%]	65.00 [78.18%]	12.5%	121.36	120.34	1.01	.177
CHOICE-R		10	77 (11)	50.30 [63.09%]	178.55 [87.27%]	125.48 [74.56%]	42.22%	92.65	126.37	0.73	.113
Total		---	476 (68)								

Notes: The numbers in squared bracket are the fractions of positive offers (or transfers).

[1] $(1/N)\sum_{k,i} b_{k,i}$, where $b_{k,i}$ is the up-front payment offer from candidate k to voter i and N is the number of candidates.

[2] $(1/N)\sum_{k,i} pm_{k,i}$, where $pm_{k,i}$ is the amount that candidate k promised to voter i.

[3] $(1/N_e)\sum_{e,i} y_{e,i}$, where $y_{e,i}$ is the elected candidate e's transfer amount to voter i and N_e is the number of elected candidates.

[4] The percentage of the cases in which incomplete contracting reversed the electoral competition (i.e., the election outcomes that were not the same as the one predicted under Hypothesis 1). The detail of the election outcomes, group by group, can be found in Appendix Table A4.

Table 2: *Candidates' Willingness to Form Incomplete Contracts*

(I) Regression Analysis

Dependent variable: Amount that candidate *k* offered as up-front payment or promised to voter *i*.

Treatment:	BUYING	PROMISE	CHOICE		BUYING-R	PROMISE-R	CHOICE-R	
			Vote-buying offer	Promise offer			Vote-buying offer	Promise offer
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(a) Loyalist dummy {= 1 if $\|x_k - x_i\| < 3$; 0 otherwise}	2.57	8.29	3.89	11.33***	6.73	1.15	3.09***	.83
	(4.63)	(15.69)	(3.29)	(4.06)	(5.78)	(7.98)	(.92)	(2.26)
(b) Swing-voter dummy {= 1 if $3 \leq \|x_k - x_i\| \leq 7$; 0 otherwise}	18.03***	9.14	3.67	10.87***	9.80[#1]	5.48	2.11**	6.79***
	(5.61)	(6.82)	(2.72)	(3.61)	(6.07)	(6.73)	(.93)	(1.98)
Constant	7.22**	39.84***	9.19***	15.22***	16.50***	44.44***	-.70	29.49***
	(2.91)	(8.01)	(2.04)	(2.19)	(3.99)	(7.52)	(1.33)	(4.90)
# of observations	110	120	110	110	1200	1100	1100	1100
Prob > F (Wald Chi-squared)	.0051	.4005	.4121	.0363**	.2303	.4084	.0000***	.0000***
[two-sided *p*-value for testing] H_0: (a) = (b)	.0103**	.9497	.9446	.9188	.6371	.2964	.2052	.0015***

Notes: Linear regressions with robust bootstrapped standard errors, based on 1,000 replications. The standard errors were further clustered by candidate ID in columns (1), (2), (5) and (6); and multivariate linear regressions in columns (3) and (4), and (7) and (8). To control for the panel structure, candidate fixed effects were also included in columns (5)-(8). The reference group is the observation where x_i is remote enough from candidate *k* that $8 \leq |x_k - x_i|$. While the significance levels (*p*-values) in the table were calculated based on normal approximation, the levels were also checked using the percentile bootstrap method, i.e., a method with asymptotic refinement, as a robustness check – all the significance levels did not change except for the estimate in (#1). The coefficient estimate (#1) was significant at the 10% level if the percentile bootstrap method was used.
*, **, and *** indicate significance at the .10 level, at the .05 level and at the .01 level, respectively.

(II) Classification of Candidates

	Percentages of category				
	Targeting swing voters	Targeting loyalists	Targeting ideologically-opposed voters	Giving uniformly	Selfish
One-Shot Treatments					
BUYING	36.4%[#1]	13.6%[#1]	18.2%	9.1%	27.3%
PROMISE	33.3%	20.8%[#2]	45.8%[#2]	4.2%	0.0%
CHOICE	36.4%[#4]	27.3%[#3,#4]	18.2%[#3]	22.7%	4.5%
Repetition Treatments					
BUYING-R	58.3%	20.8%	20.8%	0.0%	0.0%
PROMISE-R	36.4%	36.4%	27.3%	0.0%	0.0%
CHOICE-R	40.9%	27.3%[#5]	31.8%[#5]	4.5%	0.0%

Notes: "Selfish" refers to candidates who did not make any offer to voters. "Giving uniformly" refers to candidates who offered the same amounts to each of the five voters. When classifying a candidate in the CHOICE and CHOICE-R treatments, the sum of vote-buying and promise amounts were used. In the BUYING-R, PROMISE-R and CHOICE-R treatments, the average offers to each voter across the ten interactions were used.

There were five exceptions when classifying candidates ([#1], [#2], [#3], [#4], and [#5]). These five candidates were each classified into two categories. Thus, the sums of percentages in the "BUYING," "PROMISE," "CHOICE," and "CHOICE-R" rows exceed 100%. See online Appendix for the details.

Table 3: *The Impact of Candidates' Approaches on Voting Behavior (One-Shot Treatments)*

Dependent variable: A dummy which equals 1(0) if voter *i* voted for candidate B(A).

Treatment:	BUYING			PROMISE			CHOICE[†1]					
Independent Variable:	(1)	(2)	(3)[†4]	(4)	(5)	(6)[†4]	(7)	(8)	(9)	(10)	(11)[†4]	(12)[†4]
(i) $b_{A,i}$	-.0024 (.0053)	---	---	---	---	---	-.015*** (.006)	---	---	---	---	---
(ii) $b_{B,i}$	0041** (.0020)	---	---	---	---	---	.002 (.004)	---	---	---	---	---
(iii) $b_{B,i} - b_{A,i}$	---	.0037** (.0019)	---	---	---	---	---	---	.006* (.003)	---	---	---
(iv) $b_{B,i}/b_{A,i}$	---	---	.078[†2] (.085)	---	---	---	---	---	---	---	.047** (.023)	---
(v) $pm_{A,i}$	---	---	---	-.0009 (.0009)	---	---	---	-.007 (.004)	---	---	---	---
(vi) $pm_{B,i}$	---	---	---	-.0002 (.002)	---	---	---	.003 (.003)	---	---	---	---
(vii) $pm_{B,i} - pm_{A,i}$	---	---	---	---	.0006 (.0007)	---	---	---	---	.004* (.002)	---	---
(viii) $pm_{B,i}/pm_{A,i}$	---	---	---	---	---	.03 (.03)	---	---	---	---	---	.14** (.06)
(ix) Dummy that indicates whether *i* is loyal to candidate A {= 1 if $x_i < 3$; 0 otherwise}[†3]	-.87*** (.10)	-.86*** (.094)	-.72***,[#1] (.23)	-.65*** (.13)	-.65*** (.13)	-.70*** (.13)	-.80*** (.12)	-.74*** (.13)	-.80*** (.11)	-.75*** (.13)	-.77*** (.13)	-.71*** (.14)
(x) Dummy that indicates whether *i* is swing voter {= 1 if $2 < x_i < 8$; 0 otherwise}[†3]	-.50*** (.15)	-.48*** (.12)	-.54*** (.20)	-.29** (.14)	-.30** (.13)	-.35** (.14)	-.30** (.13)	-.28**,[#2] (.14)	-.31** (.13)	-.30** (.14)	-.31*,[#3] (.16)	-.36** (.14)
Constant	.96*** (.03)	.97*** (.02)	.84*** (.21)	.96*** (.12)	.92*** (.09)	.87*** (.10)	.95*** (.11)	.88*** (.15)	.81** (.10)	.80*** (.11)	.68*** (.15)	.60*** (.16)
# of observations	55	55	20	60	60	60	55	55	55	55	47	49
Prob > Wald χ^2	.0000***	.0000***	.0000***	.0000***	.0000***	.0000***	.0000***	.0000***	.0000***	.0000***	.0000***	.0000***
Two-sided *p*-value for Wald χ^2 test for H_0: (ix) = (x)	.0439**	.0126**	.4471	.0130**	.0091***	.0112**	.0000***	.0000***	.0000***	.0000***	.0000***	.0024***

Notes: Linear regressions with robust bootstrapped standard errors, based on 1,000 replications. $b_{A,i}$($b_{B,i}$) indicates up-front payment offers by candidate A(B) to voter *i*. $pm_{A,i}$($pm_{B,i}$) indicates promises made by candidate A(B) to voter *i*. While the significance levels (*p*-values) in the table were calculated based on normal approximation, the levels were also checked using the percentile bootstrap method as a robustness check – all the significance levels did not change except for the estimates in (#1), (#2) and (#3). The coefficient estimates (#1), (#2), and (#3) were significant at the 5%, 10% and 5% levels, respectively if the percentile bootstrap method was used.

[†1] Only one of the variables that indicate vote-buying or promise offers were included to avoid a collinearity issue. [†2] Significant at the 5% level if bootstrapping is not used. [†3] The reference group is the voters who are loyal to candidate B (i.e., $x_i > 7$). [†4] Only observations with $b_{A,i}$ ($pm_{A,i}$) > 0 are used as data.

*, **, and *** indicate significance at the .10 level, at the .05 level and at the .01 level, respectively.

Table 4: *Elected Candidates' Post-Election Transfer Behaviors*

Dependent variable: Amount that elected candidate *e* transferred to voter *i* ($y_{e,i}$) in a given period

Treatment:	One-shot Treatments		Repetition Treatments	
Independent variables:	PROMISE (1)	CHOICE (2)	PROMISE-R (3)	CHOICE-R (4)
(a) Accept dummy {= 1 if voter *i* accepted the offer from candidate *e*; and 0 otherwise}	14.42*,#1 (7.43)	13.55*** (2.32)	28.23*** (4.46)	19.48*** (3.29)
(b) Loyalist dummy {= 1 if $\|x_e - x_i\| <$ 3; 0 otherwise}	9.32 (7.87)	-4.80 (3.58)	-1.54 (7.44)	.69 (3.25)
(c) Swing-voter dummy {= 1 if $3 \leq \|x_e - x_i\| \leq 7$; 0 otherwise}	5.61 (3.87)	-1.51 (3.99)	5.40 (5.52)	4.00#2 (2.61)
Constant	-4.49 (7.15)	3.89 (3.03)	7.59 (7.19)	7.99* (4.69)
# of observations	57	54	502	499
Prob > Wald Chi-squared	.0649*	.0000***	.0000***	.0000***
Two-sided *p*-value for Wald χ^2 test for H_0: (b) = (c)	.5349	.3264	.0310**	.2420

Notes: Linear regressions with robust bootstrapped standard errors, based on 1,000 replications. The standard errors in parenthesis were further clustered by candidate ID as each elected candidate had five transfer decisions in a given group. In order to control for the panel structure, candidate fixed effects were also included in columns (3) and (4). Only observations where positive offers were made were used in the regressions. The reference group is the opposition voters (i.e., $|x_e - x_i| > 7$). While the significance levels (*p*-values) in the table were calculated based on normal approximation, the levels were also checked using the percentile bootstrap method as a robustness check – all the significance levels did not change except for the estimates in (#1) and (#2). The coefficient estimates (#1) and (#2) were significant at the 1% level and the 10% level, respectively, if the percentile bootstrap method was used. As another robustness check, the regressions were performed only having the Accept dummy as an independent variable since the Accept dummy and the voters' political position variables may be correlated with each other (Panel II of Appendix Table A1), finding that the Accept dummy obtains a significantly positive coefficient for each of the four treatments at the 1% level.

*, **, and *** indicate significance at the .10 level, at the .05 level and at the .01 level, respectively.

Table 5: *The Impact of Candidates' Approaches on Voting Behavior (Repetition Treatments)*

Dependent variable: A dummy which equals 1(0) if voter *i* voted for candidate B(A) in a given period.

Treatment:	(1) BUYING-R			(2) PROMISE-R			(3) CHOICE-R[†1]					
Independent Variable:	(a)	(b)	(c)[†3]	(a)	(b)	(c)[†3]	(a)	(b)	(c)	(d)	(e)[†3]	(f)[†3]
(i) $b_{A,i}$	-.004*** (.001)	---	---	---	---	---	-.003*,#2 (.002)	---	---	---	---	---
(ii) $b_{B,i}$	.003*** (.001)	---	---	---	---	---	.006*** (.002)	---	---	---	---	---
(iii) $b_{B,i} - b_{A,i}$	---	.004*** (.001)	---	---	---	---	---	---	.005*** (.001)	---	---	---
(iv) $b_{B,i}/b_{A,i}$	---	---	.0055*** (.0016)	---	---	---	---	---	---	---	.016**,#3 (.008)	---
(v) $pm_{A,i}$	---	---	---	-.0005 (.0004)	---	---	---	-.005*** (.002)	---	---	---	---
(vi) $pm_{B,i}$	---	---	---	.006*** (.0008)	---	---	---	.003*** (.001)	---	---	---	---
(vii) $pm_{B,i} - pm_{A,i}$	---	---	---	---	.001**,#1 (.0005)	---	---	---	---	.004*** (.001)	---	---
(viii) $pm_{B,i}/pm_{A,i}$	---	---	---	---	---	.049*** (.013)	---	---	---	---	---	.042*** (.013)
(ix) Dummy that indicates whether *i* is loyal to candidate A {= 1 if $x_i < 3$; 0 otherwise}[†2]	-.62*** (.06)	-.61*** (.06)	-.64*** (.07)	-.48*** (.06)	-.47*** (.07)	-.46*** (.07)	-.54*** (.05)	-.52*** (.06)	-.53*** (.05)	-.54*** (.06)	-.54*** (.06)	-.51*** (.06)
(x) Dummy that indicates whether *i* is swing voter {= 1 if $2 < x_i < 8$; 0 otherwise}[†2]	-.32*** (.06)	-.33*** (.06)	-.34*** (.08)	-.32*** (.07)	-.28*** (.08)	-.26*** (.07)	-.36*** (.06)	-.32*** (.07)	-.36*** (.06)	-.34*** (.06)	-.32*** (.06)	-.35*** (.06)
Constant	.83*** (.05)	.81*** (.04)	.79*** (.05)	.54*** (.07)	.75*** (.06)	.64*** (.06)	.77*** (.04)	.83*** (.06)	.79*** (.03)	.77*** (.04)	.76*** (.05)	.70*** (.05)
# of observations	600	600	412	550	550	499	550	550	550	550	378	488
Prob > Wald χ^2	.0000***	.0000***	.0000***	.0000***	.0000***	.0000***	.0000***	.0000***	.0000***	.0000***	.0000***	.0000***
Two-sided *p*-value for Wald χ^2 test for H_0: (ix) = (x)	.0000***	.0000***	.0000***	.0039***	.0016***	.0018***	.0015***	.0026***	.0033***	.0017***	.0009***	.0085***

Notes: Linear regressions with robust bootstrapped standard errors, based on 1,000 replications. The standard errors in parenthesis were further clustered by voter ID as each voter had ten voting decisions. Subject random effects are also included to control for the panel structure since the model includes political position dummies as an independent variable (the results for variables (i) to (viii) little change even if fixed effects are instead used). $b_{A,i}$($b_{B,i}$) indicates up-front payment offers by candidate A(B) to voter *i*. $pm_{A,i}$($pm_{B,i}$) indicates promises made by candidate A(B) to voter *i*. While the significance levels (*p*-values) in the table were calculated based on normal approximation, the levels were also checked using the percentile bootstrap method as a robustness check – all the significance levels did not change except for the estimates in (#1), (#2) and (#3). The coefficient estimates (#1), (#2) and (#3) were significant at the 1%, 5% and 1% levels, respectively, if the percentile bootstrap method was used. [†1] Only one of the variables that indicate vote-buying offers or promises in stage 1 were included to avoid a collinearity issue. [†2] The reference group is the voters who are loyal to candidate B (i.e., $x_i > 7$). [†3] Only observations with $b_{A,i}$ ($pm_{A,i}$) > 0 are used as data. *, **, and *** indicate significance at the .10 level, at the .05 level and at the .01 level, respectively.

Footnotes

[1] There is much theoretical research in this area across a number of contexts. Examples include Myerson (1993), Lizzeri and Persico (2001), Dekel *et al.* (2008), and Dixit and Londregan (1996) for electoral competition with up-front payment or (campaign) promises, and Snyder (1991), Groseclose and Snyder (1996) and Morgan and Várdy (2008) for vote buying of legislators in deciding on a policy. Also see Dal Bó (2007) for a principal-agent analysis with one principal who can bribe and multiple agents.

[2] The Australian ballot – a system in which people vote in secret on uniform ballots (listing all candidates) prepared and administrated by the state – was first used in Australia in 1856.

[3] While most studies that uncover the significance of such incomplete contracts were conducted in developing countries, for example, in Argentina (Stokes, 2005), Benin (Wantchekon, 2003), Brazil (Hidalgo and Nichter, 2016), Colombia (Rueda, 2017), Mexico (Fox, 1994), Nicaragua (Gonzalez-Ocantos *et al.*, 2012), Paraguay (Finan and Schechter, 2012), and São Tomé and Príncipe (Vicente, 2014), clientelism is widespread also in developed countries, for example, in the United States (Hertel-Fernandez, 2017), Italy (Golden and Picci, 2008), and Japan (e.g., Scheiner, 2007).

[4] Stokes (2005), for instance, argues that clientelistic parties "insert themselves into the social networks of constituents" and threaten to punish them if they attempt to support the opposition.

[5] There is a related literature on decentralized vote trading in committees (e.g., Casella *et al.*, 2012; Casella *et al.*, 2014). The present paper differs from this strand of literature in

two important ways: (a) vote trading is between candidates and voters, not within committees, and (b) the unique equilibrium under the assumption of self-interest and common knowledge of rationality is no political exchanges between the actors.

[6] Gerber *et al.* (2012) call these two forms of secrecy the "psychological secrecy" and the "social secrecy." They reported that 25% of their USA sample did not believe in the secrecy of ballots, and more than 70% of the sample responded that they share vote choices with friends or family members most or all of the time.

[7] As an anonymous referee pointed out, some subjects may not believe in vote secrecy also in a laboratory experiment if they do not trust the instructions given to subjects. The percentage of such suspicious voters should be less in the present study, because subjects were explained the no-deception rule as a general rule of laboratory experiments when registering in the recruiting database.

[8] Ostrom (1998) explained that: "Careful experimental research designs frequently help sort out competing hypotheses more effectively than does trying to find the precise combination of variables in the field."

[9] It would be more difficult for the political actors to sustain clientelism in finitely repeated than in infinitely repeated setups (e.g., Dal Bó, 2005).

[10] As discussed in Section 1, all treatments in Tonguc and Ozbay (2018) were built on a finitely repeated community game where each candidate is randomly paired with a voter in each period (election). This feature makes it difficult to disentangle the impact of repetition and that of non-binding contracts from the data. It is known that if a given community game

repeats, actors' reputation building behaviors within a given community may also be large even under random matching (e.g., Kamei, 2020; see also Andreoni and Croson [2008]).

[11] It is acknowledged that there are multiple categories of policies in real elections. The policy dimension was simplified to be one dimension as this paper's focus is on clientelistic relationships between the political actors. In addition, to simplify the design further, two candidates' policies are set to be polarized. Nevertheless, even with this simplification theoretical analyses become complex as discussed in Section 3 once the assumption of the political actors' self-interest and/or common knowledge of agents' rationality is relaxed.

[12] A candidate receives only 100 ECUs if she loses the election. Thus, she may interpret 100 as her budget constraint when deciding on any up-front payment offer. In the PROMISE treatment (Section 2.2), by contrast, the successful candidate has a budget of 300 with certainty. This feature makes the comparison between the BUYING and PROMISE treatments not fully comparable. However, as will be explained in Section 4.1, the average up-front payment offer in the BUYING treatment and the average post-election transfer in the PROMISE treatment were both far less than 100 ECUs, and they were not significantly different, implying that the offering decisions of candidates were on average not constrained by the budget.

[13] In addition to the 29 sessions, there were two sessions for which the data are not usable. The first one was conducted as the very first session, but it contained erroneous feedback on subjects' screens due to a programming error. The second one was conducted as the CHOICE-R treatment, but a computer crash happened during the

session and thus the session had to be canceled. The data of these two failed sessions were dropped.

[14] The numbers of subjects per session were 28 for three sessions, 21 for seven sessions, 14 for 16 sessions and 7 for three sessions. The average number of subjects per session was 16.4.

[15] The voter would vote randomly between the two candidates if $x_i = 5$.

[16] As an extreme case, a candidate can promise to each voter that she would transfer the maximum points (300 ECUs). This is unlikely to happen, however, because voters would not consider such a promise credible.

[17] As an illustration, suppose that a voter *i* in the BUYING treatment has a reciprocal preference by Rabin (1993). Then, *i* has the following class of utility function: $\pi_i + \rho \cdot \tilde{f}_{iki} \cdot [1 + f_{ik}]$, where π_i is *i*'s payoff in the election stage (the first term of Equation (1)), $\tilde{f}_{iki}$ is *i*'s belief on candidate *k*'s kindness toward *i*, and f_{ik} is *i*'s kindness toward *k*. ρ is the utility weight that voter *i* places on reciprocity. Suppose that $x_i = 4$, the four other voters' *x* values are 0, 2, 8, and 10, and candidate A did not offer up-front payment to any voter. Finally suppose that B distributed sufficiently large positive up-front payment to voter *i* whose act *i* perceived to be kind. Since $\pi_i(4; 0) - \pi_i(4; 10) = 10$, *i* would vote for candidate B if the non-material utility term from reciprocating B's kindness is more than ten ECUs greater when *i* votes for B and then B wins than otherwise. See Dufwenberg and Kirchsteiger (2004) for an extension of Rabin (1993) to sequential reciprocity. A similar implication can also be obtained if an inequity-averse preference, such as Fehr and Schmidt (1999), is instead assumed.

[18] 19% of the voters in Finan and Schechter (2012) stated that they did not believe in the secrecy of the ballots.

[19] There exist no symmetric pure-strategy Nash Equilibria with positive vote buying in the BUYING treatment due to the structure of Colonel Blotto game.

[20] Subjects repeat interactions under fixed matching. Such reputation building behaviors are known to be stronger under fixed matching, rather than random matching, in other contexts such as prisoner's dilemma interactions (e.g., Andreoni and Miller, 1993; Kamei, 2019).

[21] This is one example of a grim-trigger-like strategy which could sustain clientelism between the political actors. It is noted, however, that such strict behavior was rarely observed in the experiment (see Sections 4 to 6).

[22] Tonguc and Ozbay (2018) studied incomplete contracting using a political exchange process in which a candidate decides two offering amounts: (a) up-front payment, and (b) promise, and the paired voter selects (a), (b) or neither. The voter cannot select both of the offers. Their design setup makes it difficult to study how candidates combine the two payment forms as a package unlike the present study.

[23] Throughout the paper, the coefficient estimates of all independent variables used in regressions are included in a given table (Table 2.I, Table 3, Table 4 and Table 5).

[24] Cox and McCubbins (1986) propose that risk-averse candidates would target loyalists the most, while risk-loving candidates would target swing or even ideologically-opposed voters. Their theory alone is not enough to explain the data, because more than 50% of

candidates targeted swing or ideologically-opposed voters (Table 2.II), despite that students in York are known to be on average risk averse (Zhou and Hey, 2018).

[25] 5.2% and 20.0% of offers were rejected in the BUYING and PROMISE treatments, respectively, for this reason.

[26] Two-sided *p*-value = .0093. The significance level was calculated with two steps. First, the percentages of conflict cases were calculated by the interaction unit for each treatment. 11 (12) percentages were calculated since there are 11 (12) groups in the BUYING (PROMISE) treatment. After that, a Mann-Whitney test was performed.

[27] Unlike columns (1) and (2), the ratio of up-front payment offers in column (3) fails to obtain a significant coefficient. However, this is merely due to the inclusion of bootstrapping despite its small sample size. The ratio is significantly positive at the 5% level if bootstrapping is not used.

[28] The candidates' average per voter vote-buying offer was 13.98 ECUs with standard errors of 3.33 ECUs in the BUYING treatment, whereas their average per voter promise amount was 46.4 ECUs with standard errors of 8.33 ECUs in the PROMISE treatment (see also Table 1). Here, the standard errors were clustered by candidate ID.

[29] In the one-candidate-one-voter environment of Tonguc and Ozbay (2018), a much larger percentage (50%) of candidates delivered fully on their promises. Due to the various differences in experimental design, it is difficult to specify the causes for the difference in the transfer behavior between this study and their study. However, it might be due to the difference in the number of voters (five voters in this study, versus only one voter in Tonguc and Ozbay [2018]), since a candidate's reciprocation can be stronger if

there is only one voter in a community and the voter can unilaterally influence the election outcome. Alternatively, the difference might be due to the difference in the matching protocol: perfectly one-shot interactions in this study, versus repeated interactions under random matching in their study (see footnote 10). This view is equally plausible, because, as discussed in Section 5, the percentage that candidates delivered fully on their promises was far more than 50% in PROMISE-R treatment. The candidates' promise-keeping behaviors in Tonguc and Ozbay (2018) fall between the PROMISE and PROMISE-R treatments.

[30] It is theoretically possible that the total group payoff increases due to the reversing, because rational voting may not result in the highest group payoff. Notice that the total group payoff is larger when candidate B [A] wins if the sum of voters' payoffs from the election, $\sum_{i=1}^{5} \pi_i(x_i; 10)$, is larger [smaller] than $\sum_{i=1}^{5} \pi_i(x_i; 0)$. Here, the condition $\sum_{i=1}^{5} \pi_i(x_i; 10) \lesseqgtr \sum_{i=1}^{5} \pi_i(x_i; 0)$ is equivalent to $\sum_{i=1}^{5} x_i \lesseqgtr 25$. According to the realized distributions of *x* in the experiment, there was only one group in which rational voting predicts candidate B (A) wins while $\sum_{i=1}^{5} x_i < 25$ ($\sum_{i=1}^{5} x_i > 25$). See Appendix Table A4 for the detail.

[31] One group out of 12 groups in the PROMISE treatment was not included in the analysis because $x = 5$ for two voters, with which the assumption of selfish voters (Hypothesis 1(ii)) does not predict the winning of a single candidate.

[32] For the remaining two groups, $\sum_{i=1}^{5} x_i$ was exactly equal to 25 (see footnote 30). In this comparison, it was assumed that selfish voters would support a candidate solely based on $\pi_{\mathrm{i}}(x_i; x_e)$ in Equation (1).

[33] The impact of clientelism on within-group inequality can be examined by calculating (a) Gini coefficients of realized payoffs by group and (b) Gini coefficients of hypothetical payoffs under the assumption of rational selfish actors. The calculation of part (b) can be performed with two steps. First, it is calculated which candidate would win based on Hypothesis 1(ii). In case that a single candidate is not predicted to win (because some voters have $x = 5$), it is assumed that each candidate would win with a 50% probability for simplicity. Each political actor's payoff is then calculated, also assuming that no vote buying or post-election transfer occurs. The average Gini coefficients based on (b) are calculated as .248, and .242 in the BUYING and PROMISE treatments, respectively (Appendix Table A2). The realized average Gini coefficients (Table 1) are 12.5% and 17.8% lower than the above hypothetical Gini coefficients in the BUYING and PROMISE treatments, respectively. The differences are significant at two-sided p =.0997 and .0163 in the BUYING and PROMISE treatments, respectively.

[34] The average of the realized (hypothetical) Gini coefficients among five voters are .099(.071), .077(.060), .069(.063), .084(.055), .058(.059) and .056(.076) in the BUYING, PROMISE, CHOICE, BUYING-R, PROMISE-R and CHOICE-R treatments, respectively. See footnote 33 for the method to calculate hypothetical Gini coefficients based on the standard theory.

[35] A significantly larger fraction of up-front payment offers (76.7%) were non-zero in period 1 of the BUYING-R treatment, compared with the BUYING treatment (52.7%), according to a two-sided Fisher's exact test (p = .0002).

[36] The difference is significant at p = .0053 (two-sided group-level Mann-Whitney test).

[37] The ratios of the average transferred amount to the promised amount are 55.2% and 70.3% in the PROMISE and PROMISE-R treatments, respectively.

[38] The within-group Gini coefficients were significantly smaller in the BUYING-R and PROMISE-R than in the BUYING and PROMISE treatments at the two-sided *p*-value (Mann-Whitney test) of .0792 and .0028, respectively – see Appendix Table A2 for the detail.

[39] See Appendix Table A3 for a regression analysis.

[40] The difference is significant at the 10% level according to a Mann-Whitney test (two-sided $p = .0790$). The test was performed using the same method discussed in footnote 26.

[41] The difference is significant at the 5% level according to a Mann-Whitney test (two-sided $p = .0181$). The test was performed using the same method discussed in footnote 26.

[42] Bolton and Ockenfels (2000) propose another outcome-based preference model in that people are concerned about getting a fair share, by assuming that their utility depends on the relative payoff standing. This model is also not perfect. For example, reversed election outcomes were observed for some groups due to incomplete contracts in the experiment (Results 5 and 6). A calculation finds that in 71% (five out of seven cases) and 83% (77 out of 93 cases) of the reversed groups in the three one-shot and three repetition treatments, respectively, the realized Gini coefficients among voters were larger than the Gini coefficients calculated under the alternative scenarios where the election outcomes had not been reversed. Hence, reversing was usually harmful for

within-voter inequality if the election stage is considered in isolation. Some voters still selected corrupt candidates, nevertheless.