Vote buying and Local Public Goods Provision: Substitutes or Complements? *

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Abstract

We seek to better understand the demand side of vote buying: the conditions under which voters participate in, eschew, tolerate, or punish the exchange of targeted material benefits for votes. Specifically, we ask whether voters perceive campaign vote buying as substituting for local public goods provision in office, or whether they think that candidates who buy votes will also excel at securing local public goods. Voters who place great value on future public goods may opt out of vote buying if they believe the two are substitutes and also punish vote-buying candidates at the polls. We explore these issues in a nation-wide survey in Nepal. Multiple survey experiments provide evidence that Nepali voters perceive vote buying and local public goods provision as substitutes. Voters who hold this view also express a preference for candidates who do not engage in vote buying, implying they prioritize public goods provision, although this latter result is not causally identified.

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1 Introduction

Political scientists theorize that clientelism and public goods provision are alternate campaign strategies — i.e., *substitutes* — for candidates and parties (Stokes, Dunning, Nazareno & Brusco 2013). The logic underpinning this assertion is that effort and resources expended buying votes or building client networks consume time and money that could be used to bring home pork to one's district or to craft broad policy. In turn, when one has enough clients to win an election, the incentive to produce public goods in office wanes. Only when clientelist strategies become inefficient, the prevailing theory goes, will politicians bother with broad public goods provision, or even pork-barreling. This way of thinking about the relationship between clientelism and public goods provision rests on transactional top-down stories about how clientelism dies, which contend that clientelism is an inefficient tool for securing votes in mass elections once voters get too rich to buy cheaply (Stokes et al. 2013) or once a too-expensive mass electorate can participate in politics under the secret vote (Cox 1987, Mares 2015).

A growing literature centers voters in theories of clientelistic relationships (Pellicer, Wegner, Benstead & Lust 2017) and allows for "voter agency" (Hicken & Nathan 2020, p. 290). In some contexts, wealthier voters disdain clientelism and seem willing to punish politicians for it (Weitz-Shapiro 2012, Weitz-Shapiro 2014), in which case the transition from clientelism to governance may be driven by both top-down and bottom-up forces. However, how voters perceive the relationship between candidates' clientelism and their likelihood of securing public goods for their constituents once in office, is far from clear-cut. In particular, in contexts where vote buying and clientelism are pervasive, such behavior may signal competence or viability (Muñoz 2014, Hicken, Aspinall, Weiss & Muhtadi 2022), or allow politicians to buy brokers' credibility with their client networks (Kiefer & Vlaicu 2007) in lieu of building a policy-based reputation of their own. Indeed Kramon (2016) argues that, as elections in

Africa are largely "competitions over credibility" (Kramon 2016, p. 462), voters perceive clientelism as a signal of candidates' ability and willingness to secure resources for the poor, a phenomenon Hicken & Nathan (2020, p. 288) have labeled "credibility buying." Under these circumstances, voters may see vote buying and local public goods provision as *complements*—politicians who can mobilize the resources necessary to buy votes may also have the connections, experience, and resources not only to provide downstream private goods, but to bring key public goods to their districts after election.

We study the substitutability versus complementarity of vote buying and local public goods provision in Nepal, where vote-buying is common. We embed a series of experiments into a large national survey and examine how voters conceptualize the relationship between campaign-period contingent vote buying and in-office public goods provision. We find strong evidence that Nepali voters see direct vote buying as a signal that a candidate will do a poor job of providing local public goods in office.[1] Overwhelmingly, they see contingent behaviors as substitutes for effective policymaking. Voters who perceive this substitution effect also report a lower propensity to vote for hypothetical vote-buying candidates, although we cannot prove causality. While we find some evidence that wealthier voters vote against vote-buying candidates more than poorer voters, how constituents understand the relationship between vote buying and public goods provision—whether they perceive these behaviors as substitutes or complements—seems to be unrelated to wealth. To the extent that a growing middle class is responsible for pushing candidates away from vote buying, their tendency to vote against such behavior appears not to stem from a different understanding of how politicians' electoral behavior predicts their public goods provision in office.

[1]Note, however, our tests in this paper focus on transactional offers, limiting the scope of our claims.

2 Voters' Perceptions of Clientelism

The canonical macro-level theory surrounding how mass electorates transition away from various forms of clientelism, vote buying, and gift giving focus on the cost-benefit calculations of political parties. Countries experience industrialization or some other deep structural economic transformation, and then states urbanize, income rises, and voters become richer. Wealthier voters can no longer be efficiently bought off with low-value clientelist offers, and parties begin to find clientelism inefficient, instead shifting to courting voters first through local public goods provision and eventually broadly targeted policy.[2] As economic development continues, clientelism dies out, replaced by a system in which voters are in a position to benefit from—and understand why they benefit from—local development, public infrastructure, and other public goods.In turn, urbanization makes public goods provision more efficient and expanded education and access to news make it possible for politicians to effectively communicate broad policy and local goods promises to voters, and for voters to evaluate politicians' follow-through on commitments (Cox 1987, Brusco, Nazareno & Stokes 2004, Stokes et al. 2013, Camp, Dixit & Stokes 2014, Aidt & Jensen 2017).

[2]Strictly speaking, public goods refer to nonrival, nonexcludable goods provided to a group of beneficiaries. In practice, the literature often conceptualizes public goods to mean examples that are marginally nonrival or nonexcludable. Examples of what we refer to as public goods include education, health, and sanitation. As other scholars have noted, there is also a blurry distinction between large-scale (i.e., national) "policy programs" (Schaffer & Schedler 2007, p. 15), "local public goods" (Desposato 2002, p. 10), and purely clientelistic benefits (Wantchekon 2003) in much of the literature. Our focus is on the distinction between clientelism and local public goods provision: "technically public goods but with a decidedly local scope" (Desposato 2002, p. 10).

The formal model in Stokes et al. (2013) makes an underlying assumption of much of the literature explicit: parties and candidates must make tradeoffs between providing widespread clientelist benefits during the campaign period and substantial public goods once in office, and voters understand this tradeoff. In the standard macro-story, clientelism and public goods provision are substitutable strategies. While the model assumes that politicians pay for clientelism and public goods provision out of a single fixed budget, there a number of ways that this trade-off might operate in practice. For example, politicians might spend their own money to buy votes, and then pilfer public coffers once in office to compensate themselves. Less directly, patronage jobs might reduce bureaucratic efficacy, limiting public goods provision, or candidates who expend energy to fund and maintain client networks might have insufficient time to craft parliamentary logrolls or design policies.[3]

Voters play a surprisingly passive role in the dominant party-focused theory, though numerous studies explore how voters feel about clientelism. This work considers how voters' attitudes towards democracy, non-economic preferences, and other psychological factors affect voter beliefs and behavior (Finan & Schechter 2012, González-Ocantos, Kiewiet de Jonge & Nickerson 2014, Nichter & Peress 2017). Recent work highlights the need to more explic-

[3]The literature, by and large, assumes that over a long enough time frame politicians will adopt strategies that maximize their electoral advantage, which may not be true over the short-run. A politician or party could be (partially) trapped by their capabilities. Therefore, electoral strategy and voter perceptions of vote buying are determined by context. Our field work in Nepal indicates that all major parties are associated with widespread vote buying. We take this context as given, as our experiments cannot speak to politician strategy and how it dictates voter perceptions, nor how it constrains voters and politicians. The interaction of party and politician strategy with voter preferences is fertile ground for future work.

itly model how voters' economic and non-economic considerations and social network density affect their willingness to engage in clientelist exchange (Cruz 2019, Pellicer et al. 2017). Indeed, while some studies find that rich voters react negatively to clientelist appeals when political competition is fierce (Weitz-Shapiro 2012, Weitz-Shapiro 2014), other research provides evidence that poor voters prefer public goods promises to vote buying (Kao, Lust & Rakner 2017), or that poor voters are diverse in their preferences for clientelism relative to public goods (Wantchekon 2003), directly contradicting a core assumption of the standard model.

Specifically, voters' understanding of the substitutability of vote buying and public goods provision is a largely untested assumption in the micro-level literature. In our paper,[4] we interrogate this assumption, examining whether voters view vote-buying offers and local public goods provision as substitutes or complements.[5] In other words, if a voter observes a candidate pursuing votes through vote buying, does she conclude that the candidate would be better, or worse, at local public goods provision than a counterfactual identical candidate

[4]This paper is part of a larger project that seeks to unpack the empirical relationship between income and clientelism. The portions of the registered pre-analysis plan (PAP) relevant to this paper are included in the SI.

[5]Our research design focuses on contingent vote buying and our findings may not generalize to unconditional clientelism. Nevertheless, since evidence suggests vote-buying exchanges can be unmonitored and depend on fuzzy pathways like reciprocity for contingency (Finan & Schechter 2012, Lawson & Greene 2014, Guardado & Wantchékon 2018, Gallego, Guardado & Wantchekon 2023), this distinction may not be clear cut. Similarly, our research does not directly address relational clientelism, which is driven by repeated interactions with particular brokers (Hicken 2011).

who did not provide such offers to voters? In turn, does this understanding of the relationship between the use of vote buying and effectiveness at public goods provision drive vote choice? Do certain voters who tend *not* to participate in contingent vote-buying exchanges exhibit different beliefs about the substitutability or complementarity of vote buying and public goods provision?

We argue that voters may reject vote buying because they realize that it means that future public goods provision will be lower. Vote-buying candidates produce poor public goods, while those not engaging in vote buying will be more likely to deliver public goods. The logic of this assumption leads to the following hypotheses about voters:

Hypothesis 1 (Substitutes). *Voters perceive vote buying and local public goods provision as substitutes.*

Hypothesis 2 (Substitutes-Voting). *Voters who perceive vote buying and local public goods provision as substitutes are more likely to vote against vote-buying candidates.*

We also advance an alternative possibility: when voters see politicians delivering private goods, they perceive them as more likely to deliver on their public policy promises. Vote buying has long been thought to signal a candidate's electoral resources (van de Walle 2007), and Kramon (2016) finds that voters—particularly poor voters—actually *support* clientelist offers because they perceive them as a signal of a candidate's competence, trustworthiness, and electoral viability. Vicente & Wantchekon (2009) discuss a similar signaling mechanism, asserting that clientelism indicates a candidate's "...control of public allocations and resources" (Vicente & Wantchekon 2009, p. 300).[6] With the exception of Kramon's work, however, any signaling effect of clientelism remains largely undocumented. In addition, the instruments

[6]Voter-driven explanations for clientelism's popularity (or lack thereof) may explain empirical observations that are at odds with the standard substitution story, such as the tendency for parties to mix clientelist and policy-oriented electoral strategies, even in rich coun-

used in Kramon's work do not test a direct substitution vs. complementarity argument.[7] We test the complementary of vote buying explicitly in the context of experimentally-induced contingent exchange scenarios. This logic, in turn, leads to the following hypotheses:

Hypothesis 3 (**Complements).** *Voters perceive vote buying and local public goods provision as complements.*

Hypothesis 4 (**Complements-Voting).** *Voters who perceive vote buying and local public goods provision as complements are less likely to vote against vote-buying candidates.*

The above hypotheses illuminate a possible causal mechanism explaining the relationship between voter income and propensity to engage in vote buying. As González-Ocantos, Kiewiet de Jonge & Nickerson (2014) assert, perhaps wealthier voters are better educated and therefore better able to understand the tradeoff between vote-buying transfers today and public goods tomorrow - the "system-wide problems with vote buying" (González-Ocantos, Kiewiet de Jonge & Nickerson 2014, p. 201).[8] Or perhaps wealthier voters and poor voters hold similar beliefs about the substitution or complementarity of vote buying and public goods provision, but wealthier voters are willing and able to refuse benefits to "take a stand" against vote buying at the polls. Unlike the macro-structural work, micro-empirical findings on the role of wealth and vote-buying distaste are clearly mixed, but nevertheless worth testing. These considerations lead to the following interaction hypotheses:

tries (Kitschelt 2007, Stolfi & Hallerberg 2016), and the existence of "normal" constituency service alongside clientelism (Bussell 2019).

[7]Voters perceiving politicians more likely to help "people like me" with education and food expenses, for example, could simply be expressing an expectation of additional clientelist benefits, rather than by reducing costs through local public goods provision or policy.

[8]The link between wealth and education in (González-Ocantos, Kiewiet de Jonge & Nickerson 2014) is implied but never explicitly stated.

Hypothesis 5 (Wealth-Substitutes). *Wealthy voters are more likely to view vote buying and local public goods provision as substitutes than are poor voters.*

Hypothesis 6 (Wealth-Substitutes-Voting). *Wealthy voters who view vote buying and local public goods provision as substitutes are more likely to vote against vote-buying candidates than are poor voters.*

3 Data and Methods

3.1 Context

We test our hypotheses in Nepal, a compelling case for examining how voter preferences, party linkages, and wealth interact. Nepal offers a combination of a growing middle class and declining poverty, yet middling economic growth and a population struggling with political and economic uncertainty (World Bank Group 2016). Against the backdrop of these economic conditions, vote buying in Nepal has been a constant presence in the political landscape. All of those we interviewed in the pre-experiment phase agreed vote buying is widespread, particularly in rural areas. As one party leader said, "Everyone knows that parties are buying poor votes." A community leader said, "In a high competition situation, [vote buying] can have a big impact [on the election]."

Similarly, the survey we used to collect the data for this work included a list experiment that estimated around a quarter of Nepali voters were offered bribes during the 2018 general election. While voting is purportedly secret in Nepal, respondents seem to consider them contingent: the list experiment estimates about a quarter of respondents were offered a bribe, but also that a quarter exchanged their votes for money. Further, respondents believe that vote buying is even more common than the list experiment implies: respondents guessed that somewhere between 41 and 56 per cent of voters sell their votes. Politicians also routinely

offer both cash and jobs for votes in the Nepali context. Qualitative work—interviews, focus groups, and a round-table with journalists and civil society organization members—informed our choice of these two forms of vote-buying offers for the conjoint experiment. We also provided survey respondents with a menu of items that candidates potentially offered "directly to individual voters in return for their votes," to check the plausibility of the treatments. Sixty-four per cent selected cash and 60% selected jobs as among offered items.[9]

Nepal also has relatively free, fair, and competitive elections, which implies that voters, collectively, have the power to encourage or discourage various campaign tactics by candidates (e.g., vote buying) and in-office behaviors by elected officials (e.g., public goods provision). The Varieties of Democracy (V-Dem) Electoral Democracy Index scores Nepal a 0.62, comparable to the regional average for Latin America, and well above sub-Saharan Africa's 0.45. Nepal held local and national legislative elections on November 26 and December 7 of 2017. These elections were marked by sporadic violence and manipulation, but were judged largely free and fair by observers (European Union Election Observation Mission 2018), though there is more variation in elections at the local level (Coppedge, Gerring, Knutsen, Lindberg, Skaaning, Teorell, Altman, Bernhard, Fish, Cornell, Dahlum, Gjerlow, Glynn, Hicken, Krusell, Lührmann, Marquardt, McMann, Mechkova, Medzihorsky, Olin, Paxton, Pemstein, Pernes, von Römer, Seim, Sigman, Staton, Stepanova, Sundstöm, Tzelgov, Wang, Wig, Wilson & Ziblatt 2018). Turnout is also high, at nearly 70% during the 2017 elections, which were assessed to have "relatively stable and enduring political groups which compete for political influence at the national level" (Coppedge et al. 2018). In sum, the Nepali case provides fertile ground for examining attitudes towards clientelism and investigating how such attitudes vary with income.

[9]This number was similar to alcohol (62%) and food (72%), but far outstripped consumer goods, building materials, or access to loans (all under 25%).

3.2 Sample

Within Nepal, we conducted a nationwide survey of Nepali voters in a sample of 117 local government units (Village Development Committees (VDCs) and municipalities (palikas)).[10] Our sample was drawn at the local government level because of the availability of demographic data.[11]

At the time of data collection, there were 3,374 local government units in Nepal. To ensure a sufficiently large population from which to recruit participants, we restricted the sample to local government units with more than 500 people. Then, since candidates are more likely to engage in clientelism in competitive elections with heterogeneous populations unlikely to vote for a candidate solely based on shared ethnicity, we restricted the sample to local government units within first-past-the-post (FPTP) constituencies where the winning candidate in the 2017 national legislative elections won 60% or less of the vote, and where there are six or more ethnic groups (using the ethnicity census data to calculate the effective number of ethnic groups). These restrictions dropped the theoretical population of local government units to 2,264. Next, to ensure variation on industry and education, we calculated the percentage of the population working outside agriculture and the percentage of the population who completed secondary school, and then restricted the population of local government units to only those in the bottom and top quartiles of these two variables. These restrictions dropped the theoretical population of local government units to 1,179. Finally, from this restricted

[10]The VDCs were replaced by municipalities (palikas) in 2017, though the overlap between the VDCs and municipalities is significant, and we were able to map all of the old VDCs to current municipalities.

[11]Most of the observational data on the sampling variables (see below) came from the Nepal Census and National Living Standards Survey, which were last completed in 2011 at the VDC level.

population of local government units, we sampled 172, stratifying on population density (as a proxy for rural vs. urban location of the community) and the percentage of homes with electricity (as a proxy for community average wealth). We removed 54 especially remote local government units with prohibitive transportation costs from the sample, resulting in a final sample of 117 local government units.

Within each sampled local government unit, the enumeration team worked with a local facilitator (typically a well-educated community member with some research experience) to recruit 11 participants in advance of the research team's arrival. The sampling protocol the facilitators followed was designed to achieve a certain group composition, critical for a behavioral game that constitutes another aspect of this project. This protocol resulted in a sample of individuals that is more educated and less wealthy than the broader population, which may limit the generalizability of the findings to all of Nepal.[12]

In total, the projected survey sample was designed to include 1,287 individuals, maximizing N, given our available budget. Twelve participants attrited mid-survey, so the final sample size for analysis is 1,275 individuals.

3.3 Survey Experiments

Our analysis focuses primarily on a series of survey experiments, which we present sequentially here.

Candidate Conjoint Experiment In a conjoint experiment, survey participants learn about a pair of randomly assigned hypothetical "profiles" and then answer a series of questions about these two profiles. In the conjoint experiment that we describe here, each profile represented a hypothetical candidate running for election in Nepal. The profile for each

[12]See SI C for analysis comparing the sample to the population on key demographic variables.

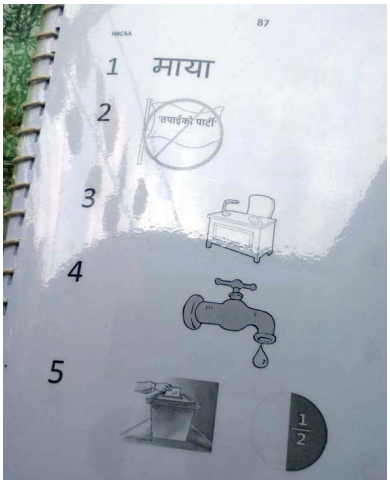
candidate was randomly generated along five dimensions from a set of possible values for each dimension (see Table 1). However, we note there was only one possible value on the "Competitive" dimension,[13] and that the "Policy Promise" dimension allowed for two types of public goods provision rather than variation in whether or not public goods are provided.

Table 1: Candidate Conjoint Experiment Text

Dimension	**Variable Value 1**	**Variable Value 2**	**Variable Value 3**
1. Gender Strength	[Name = common female or male name]	[Name = common female or male name] + Stating candidate is female or male and showing female or male stick figure	
2. Party	This candidate represents the party you most often support	This candidate does not represent the party you most often support	
3. Clientelism	This candidate is not offering money, or a job for a family member, in exchange for people's vote pledges	This candidate is offering people who pledge their votes a small amount of money in exchange for people's vote pledges	This candidate is offering people who pledge their votes a job for a family member
4. Policy Promise	This candidate is promising to increase water connections in the community	This candidate is promising to build additional school infrastructure in the community	
5. Competitive	The election will be very close		

[13]We held the level of competition in the hypothetical election constant to anchor the participants' perception of competitiveness, but we did not posit any hypotheses regarding how variation in competitiveness might affect voter support for clientelism.

Figure 1: Example Conjoint Profile Image



Following Cooperman, McLarty & Seim's (2022) approach to conjoint experiments in low-literacy areas, the enumerators simultaneously read information about the hypothetical profiles and presented visual depictions of each profile's dimensions using pictograms compiled on a page in a "profile book." Figure 1 depicts a page from the profile book. Enumerators carried two profile books so they could display a page for each hypothetical profile. Enumerators turned to the appropriate page for each candidate profile (A on the participant's left and B on the right) in the books. They set the books on a table in front of the participant before describing each candidate profile. After the verbal and visual presentation of the hypothetical candidates, each participant answered six questions.[14] Our analysis focuses on three of them, as follows:

1. Which candidate would you vote for? (A/B)
2. Which candidate do you think would be more likely to provide water connections to the community? (A/B)
3. Which candidate do you think would be more likely to build additional school infrastructure in the community? (A/B)

In this Conjoint Experiment, these outcome variables are turned into binary choice vari-

[14]See the PAP.

ables, as follows: *Water Likely (Yes/No)* takes a value of one (1) when the candidate is selected as being most likely to provide water connections to the community; *Schools Likely (Yes/No)* takes a value of one (1) when the candidate is selected as being most likely to build additional school infrastructure the community; and *Vote (Yes/No)* takes a value of one (1) when the candidate is selected as the most likely candidate to receive the respondent's vote. We address potential concerns about social desirability bias in this experiment in the Discussion.

Vignette Experiment 1 In a separate part of the survey, we approached the candidate choice question with two vignette experiments. In the first vignette experiment, the survey participant learned about two candidates' vote-buying practices, and then learned that both promise specific public goods. The participants were then asked which candidate is more likely to follow through on their public goods provision promise. The vignette allowed for two types of public goods provision rather than variation in whether or not public goods were promised.[15] The vignette text is as follows (randomized text options appear within brackets):

> "National parliament candidate A [is/is not] offering people who pledge their votes [a small amount of money/access to small loans]. Candidate B [is/is not] offering people who pledge their votes [a small amount of money/access to small loans]. Both candidates promise to [increase household water connections/build school infrastructure] in the community. In your view, which candidate is more likely to [increase household water connections/build school infrastructure] in the community, if elected to office?" *Answer Options: Candidate A is most likely/Candidate B is most*

[15]We wanted to hold constant the provision of public goods, but operationalize this with two types of public goods. Note that this experiment is identical to a conjoint experiment with the following dimensions: 1) vote-buying benefit (yes/no and money/loans); 2) public goods type (water/school infrastructure).

likely

In Vignette Experiment 1, the outcome variable of *Expected Public Goods Likely (Yes/No)* is coded as a binary variable, taking a value of one (1) when the candidate is selected as being most likely to provide public goods in office and taking a value of zero (0) when the candidate is not selected.

Vignette Experiment 2 Finally, we conducted a vignette experiment in which we provided information about one candidate and mention only the type of clientelist benefit they provided. In this vignette, we provided no information about the candidate's public goods promises, which allows us to assess whether voters use information about vote buying as a proxy to assess the likelihood of public goods provision. The vignette text is as follows (randomized text options appear within brackets):

> "A candidate for national parliament is offering people who pledge their votes [a small amount of money/access to small loans]. Compared to a candidate who is not engaging in this behavior, is this candidate more or less likely to help the community obtain things like improved roads, better access to water, educational infrastructure, health services, after being elected?" *Answer Options: Much less likely/Slightly less likely/Slightly more likely/Much more likely*

In Vignette Experiment 2, the outcome variable of *Expected Public Goods Likelihood Scale* is coded on a four-point scale, with higher values indicating a higher likelihood.

Construct Validity All survey experiments pose hypothetical scenarios and elicit hypothetical decisions. That said, we strategically included or omitted certain details to maximize realism. Specifically, our hypothetical candidates always promise public goods provision. The ubiquity of public goods promises by candidates during the campaign period was emphasized by almost all of those we interviewed in the pre-experiment period. For example, an interviewed party leader said, "People make big promises about development." A community leader in Dhading said, "Reconstruction of school buildings, health posts, irrigation

systems, and drinking water that were destroyed by the April earthquake, clean and healthy environment, roads, drinking water, building new park and park management, and one home water tap were among the common promises made by candidates during their election campaigns." As one journalist put it during a focus group discussion with civil society leaders and journalists in Kathmandu, "...if we talk about how election [takes] place, in the last election Nepali Congress has prepared a song, "Aswasanko paka diye, akhirima dhoka diye." ("Promises were made but at the end (people) were cheated."). In addition to aligning with the local context, this design feature of our conjoint experiment set up a "hard test" of substitutability: with all candidates in our survey experiments pledging public goods provision, variation in the credibility of these pledges in the eyes of voters is strong evidence of substitutability.

Similarly, each of the vote-buying offers by the hypothetical candidates is taken directly from examples of vote buying in the Nepal context given in our pre-experiment interviews and focus group discussions. Even though the vote-buying offers described in our survey experiments vary in their implied contingency, we limit our interpretation of the findings to understanding vote buying, not clientelism more generally. As discussed in Section 3.1, vote buying in Nepal is generally *de facto* contingent. Further, our experiment designs specifically refrain from asking respondents whether they would *accept* the offer, and identifying the level of contingency of vote-buying exchanges in Nepal is beyond the scope of this paper.

Note that the order of the experiments in the survey was always as we present above. While it would have been optimal to randomize the order of the experiments, this would have been costly to code.[16] Each respondent participated in each experiment only once.

[16]The conjoint experiment was separated from the two vignette experiments by batteries of demographic and political participation questions. The two vignette experiments were asked one right after the other.

3.4 Measuring Wealth

To test Hypothesis 5 and Hypothesis 6, we construct a *Wealth Index* using a battery of proxy indicators collected from all of participants during the survey, and one question asked during the associated lab-in-the-field study. We converted these questions into binary indicators and then used Bayesian item response theory (IRT) to construct a latent index (Johnson & Albert 1999, ch. 6).[17]

3.5 Analysis Methods

3.5.1 Conjoint Experiment Analysis

For the conjoint experiment, we are interested in the effects of the vote buying dimension on the perception of potential public goods provision (Hypothesis 1 and Hypothesis 3), the interactions between the vote-buying dimension and vote intention, as measured in response to Vignette Experiment 2 (Hypothesis 2 and Hypothesis 4), and the interaction between the vote-buying dimension and the *Wealth Index* (Hypothesis 5 and Hypothesis 6). Our tests are complicated somewhat because we have three outcome variables: one for water connections, one for school infrastructure, and one for vote choice. Because these outcomes are correlated, we use a multivariate regression framework, where we assume that

$$\mathbf{y}_{ijk} \sim \mathcal{N}_3(\mathbf{x}_{ijk}\mathbf{B}, \mathbf{\Sigma}) \tag{1}$$

where $\mathbf{y}_{ijk} = \left[y_{ijk}^{\text{water}} y_{ijk}^{\text{school}} y_{ijk}^{\text{vote}}\right]$, i indexes respondent, j indexes candidate, k indexes conjoint task, $\mathbf{B}$ is a $m \times 3$ matrix of unknown coefficients, and $\mathbf{\Sigma}$ is an unknown variance-covariance matrix. As discussed above, y_{ijk}^{o} is a binary choice variable that equals one (1) if respondent i selects candidate j for outcome o in task k. While we use a multivariate framework, we

[17] See SI B for details on the indicators and the IRT model.

otherwise follow the described by (Hainmueller, Hopkins & Yamamoto 2014). In particular, we use a block bootstrap procedure to estimate standard errors.

In Equation 1, $\mathbf{x}_{ijk}$ is an m-vector containing a dummy variable indicating whether candidate j is offering money, a dummy variable indicating whether candidate j is offering jobs, a dummy variable indicating whether respondent i selected much/slightly less likely in Vignette Experiment 2 (*Expected Public Goods Likely (Yes/No)*), the *Wealth Index* for respondent i, two-way interactions between each clientelist offer dummy and both *Expected Public Goods Likely (Yes/No)* and the *Wealth Index*, and three-way interactions between each clientelist offer dummy, the Vignette Experiment 2 response dummy, and the *Wealth Index*. It also includes main effects for the conjoint dimensions of candidate party, gender, gender treatment strength, and public goods promise, which are not of substantive interest in this paper, but which we include to account for known variance. Dropping participant, candidate, and task indices for readability, we therefore estimate the following regression equation simultaneously across all three outcomes $o \in \{\text{water, school, vote}\}$:[18]

[18]We also fit relevant sub-models: main effects only, main effects and interactions between clientelism and vignette response, main effects and interactions between clientelism and wealth. We present the main effects only and fully interactive models in the main text and place the partial interaction models in the SI. Results are consistent across all model specifications.

$$
\begin{aligned}
y^o = &\beta_0^o + \beta_1^o \text{money} + \beta_2^o \text{jobs} + \beta_3^o \text{vignette} + \beta_4^o \text{wealth} \\
&+ \beta_5^o (\text{money} \times \text{vignette}) + \beta_6^o (\text{jobs} \times \text{vignette}) + \beta_7^o (\text{money} \times \text{wealth}) + \beta_8^o (\text{jobs} \times \text{wealth}) \\
&+ \beta_9^o (\text{vignette} \times \text{wealth}) \\
&+ \beta_{10}^o (\text{money} \times \text{vignette} \times \text{wealth}) + \beta_{11}^o (\text{jobs} \times \text{vignette} \times \text{wealth}) \\
&+ \beta_{12}^o \text{supported party} + \beta_{13}^o \text{woman} + \beta_{14}^o \text{gender strong} + \beta_{15}^o \text{school},
\end{aligned}
\tag{2}
$$

We test Hypothesis 1 and Hypothesis 3 by examining the coefficients for the vote-buying offer dummies for the outcomes *Water Likely (Yes/No)* and *Schools Likely (Yes/No)*: if the substitutes story holds, these four coefficients should be **negative**, while complementarity would imply **positive** coefficients. Specifically, from equation 2, we expect that, $\beta_1^{\text{water}} < 0$, $\beta_1^{\text{school}} < 0$, $\beta_2^{\text{water}} < 0$, and $\beta_2^{\text{school}} < 0$, if Hypothesis 1 (Substitutes) holds. We do not expect to find distinct relationships across outcomes, nor do we expect to find that vote-buying offers of money reveal different results than vote-buying offers of jobs.

We test Hypothesis 2 and Hypothesis 4 by examining the interactions between the vote-buying offer dummies and the dummy for Vignette Experiment 2, for the outcome *Vote (Yes/No)*. We expect these interaction terms to be **negative** ($\beta_5^{\text{vote}} < 0$ and $\beta_6^{\text{vote}} < 0$) if Hypothesis 2 (Substitutes-Voting) holds, and **positive** if Hypothesis 4 (Complements-Voting) does.

We test Hypothesis 5 by examining the interactions between the vote-buying offer dummies and the *Wealth Index*, for the public goods provision outcomes *Water Likely (Yes/No* and *Schools Likely (Yes/No)*. If Hypothesis 5 (Wealth-Substitutes) holds, then we'd expect these interaction terms to be **negative** ($\beta_7^{\text{water}} < 0$, $\beta_7^{\text{school}} < 0$, $\beta_8^{\text{water}} < 0$, and $\beta_8^{\text{school}} < 0$).

Similarly, we test Hypothesis 6 by examining the three-way interactions between the

vote-buying offer dummies, the dummy for Vignette Experiment 2, and the *Wealth Index*, for the outcome *Vote (Yes/No)*. We expect these coefficients to be negative ($\beta_{10}^{\text{vote}} < 0$ and $\beta_{11}^{\text{vote}} < 0$) if Hypothesis 6 (Wealth-Substitutes-Voting) holds.

3.5.2 Vignette Experiment 1 Analysis

Vignette Experiment 1 provides another test of Hypothesis 1 and Hypothesis 3. In analyzing this experiment, we treat it as a conjoint experiment with one paired comparison per respondent, fewer dimensions per profile, and one outcome question per experiment: *Expected Public Goods Likely (Yes/No)*. We also greatly simplify the right hand side of the model because these hypotheses imply no interactions, so we need only include main effects. We expect the coefficients for the vote-buying offers to be negative if Hypothesis 1 (Substitutes) holds.

3.5.3 Vignette Experiment 2 Analysis

Vignette Experiment 2 provides a third test of Hypothesis 1 and Hypothesis 3 via a straightforward OLS regression. We regress the outcome *Expected Public Goods Likelihood Scale* on a dummy variable for money versus loan vote-buying offers.[19] If Hypothesis 1 (Substitutes) holds, the intercept should be negative, as should the sum of the intercept and the coefficient for offering money. We do not have a prior expectation about whether the coefficient for money will be positive or negative.

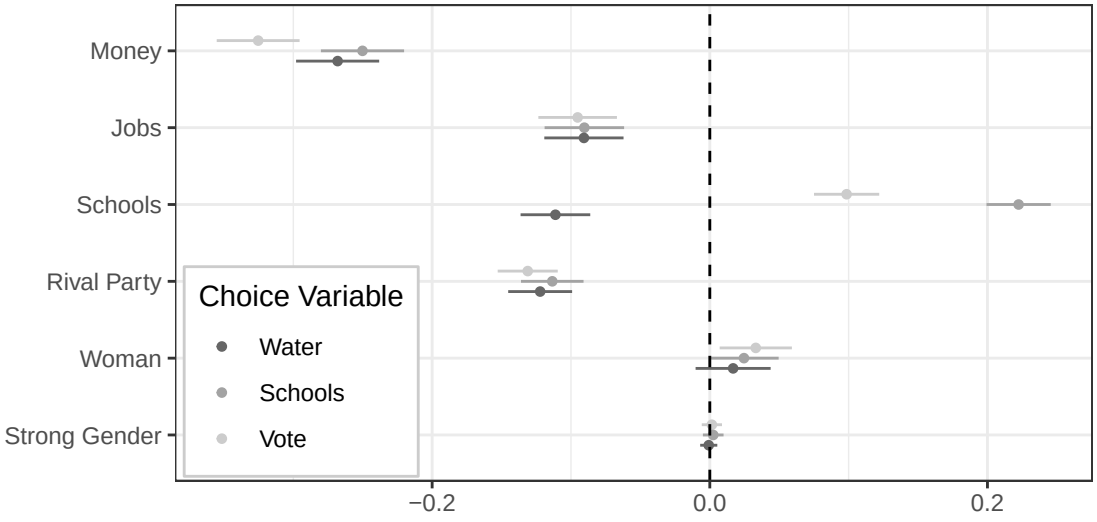
4 Results

Our results strongly support the contention that Nepali voters see vote buying and public goods provision as substitutes. Figure 2 displays average marginal component effects

[19]The results are robust to instead running an ordered probit model.

(AMCEs) for the conjoint experiment, looking only at the main effects of the dimensions on the three choice variables: *Water Likely (Yes/No)* (light gray); *Schools Likely (Yes/No)* (medium gray); and *Vote (Yes/No)* (dark gray). In support of Hypothesis 1 and in refutation of Hypothesis 3, we find large, negative, and statistically significant coefficients on the money and jobs dimensions when predicting public goods (either *Schools Likely (Yes/No)* or *Water Likely (Yes/No)*). Respondents expect candidates who offer money as payments for votes to be approximately 25 per cent **less** likely to provide public goods once in office—either water or school infrastructure—than candidates who do not buy votes. The effect for offering jobs is smaller—just under 10 per cent reductions in expectations of both school and water infrastructure provision—but otherwise mirrors the result for cash payments. SI Figure 3 shows that this effect is robust to a potential priming issue inherent in our study. Specifically, many respondents participated in a lab-in-the-field vote-choice experiment that potentially primed participants to think about vote buying and public goods provision as substitutes. While we originally planned to counter-balance the ordering of this experiment with the survey, practical issues precluded this option, and the lab experiment always ran first. Nonetheless, a sub-sample of 159 respondents participated in the conjoint experiment without first completing the vote-choice experiment. We find essentially identical results in this sub-sample.

Figure 2: Conjoint Experiment, Effect of Vote-buying Offers on Anticipated Public Goods Provision and Vote Choice



Note: Excluded categories are no money offer, no jobs offer, water promise, man, and weak gender (name only), across all conjoint results figures.

We find similar effects in the two vignette experiments, buttressing the robustness of Hypothesis 1. In Vignette Experiment 1, we find that respondents expect candidates who offer cash or loans in return for votes to be less likely to provide public goods than candidates who do not (Figure 3).[20] In Vignette Experiment 2, respondents anticipate a hypothetical candidate will be less likely to provide public goods when the candidate offers either cash or loans, although the substitution effect of cash payments is larger than that for small

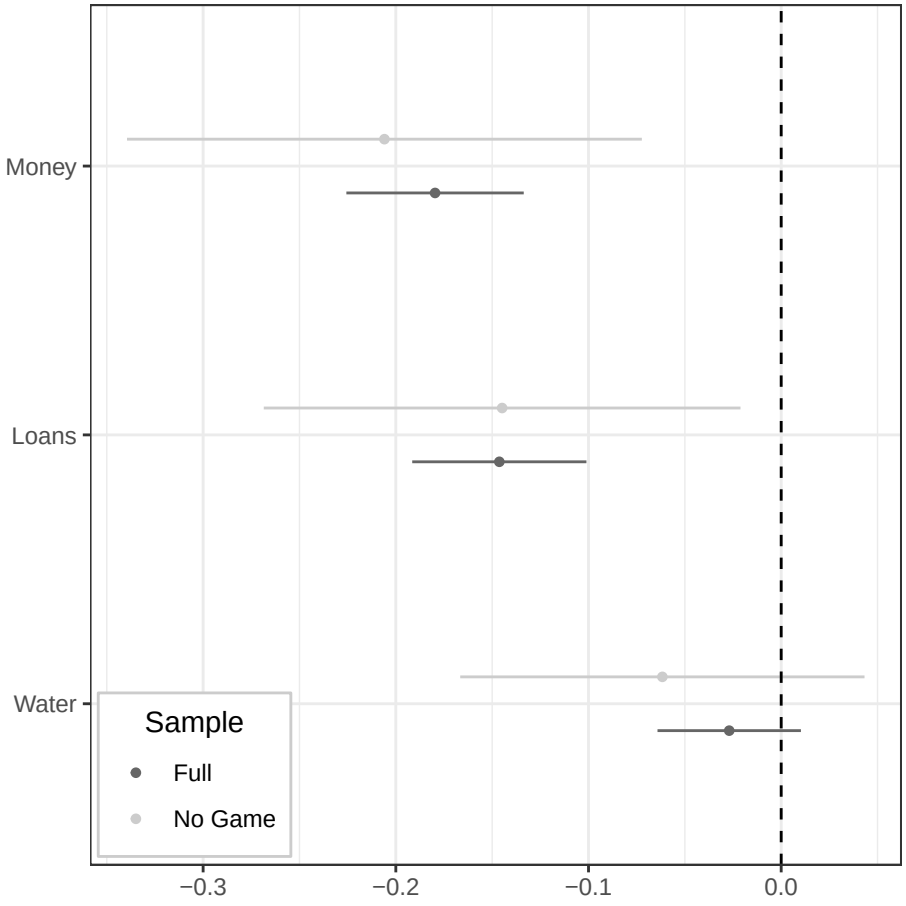
[20]The main effect for water versus school infrastructure is statistically insignificant, as expected, because we ask respondents their expectations about the provision of the promised public good. Further, SI Figure 4 also shows no interaction between vote buying and public good types.

loans.[21] Figure 4 shows that both the intercept and the dummy coefficient for cash payments are negative, and statistically significant, in the full sample.[22] In sum, our experiments unequivocally support the contention that, despite vote buying's ubiquity within the country, and the perception among many of our interview respondents that it represents a standard aspect of campaigning, Nepali voters believe that candidates who trade cash or favors for votes during the campaign period will be less likely to provide public goods once in office.

[21]Again, we check the robustness of the sample to participation in the lab-in-the-field experiment, and find no evidence that support of hypothesis 1 is driven by priming.

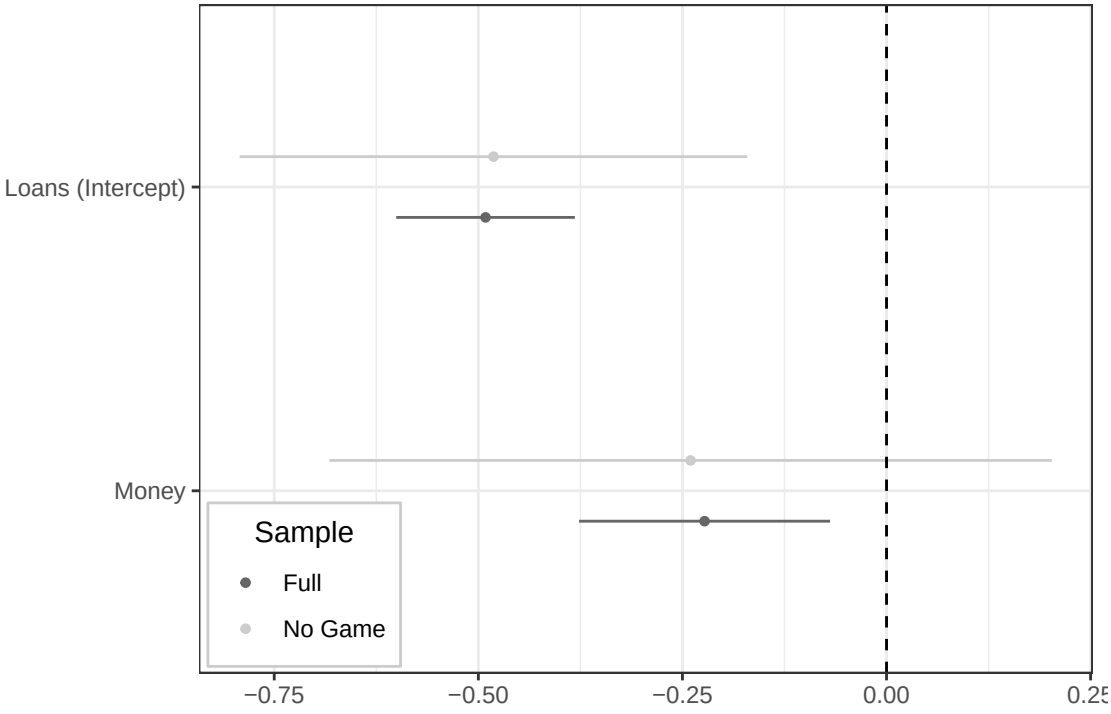
[22]The coefficient for the cash payment dummy in the non-game-playing subsample fails to reach statistical significance, but the estimated effect is essentially identical to that in the full sample, implying that this is a power—rather than a sample bias—issue.

Figure 3: Vignette Experiment 1, Effect of Vote-buying Offers on Anticipated Public Goods Provision



Note: Depicts ACMEs for full sample and sub-sample that excludes lab-in-the-field participants.

Figure 4: Vignette Experiment 2, Effect of Vote-buying Offers on Anticipated Public Goods Provision



Note: Depicts OLS coefficients for full sample and excluding lab-in-the-field participants.

Nepali voters in our sample think that candidates who buy votes do a worse job of providing public goods than candidates who eschew such behavior. Do they prefer to vote for non-vote-buying candidates as a result? Figure 2 shows that, in the vote-choice decision, participants select candidates who pay cash for votes about 32 per cent less often than those who do not. Again, for candidates who buy votes with jobs instead of cash, the penalty is smaller, at about 10 per cent. So, in hypothetical elections, Nepali voters - at least those in our study - punish vote buying (or reward clean campaigning, as these are observationally equivalent behaviors).[23]

[23]After the experimental portions of the survey we directly asked respondents their attitude

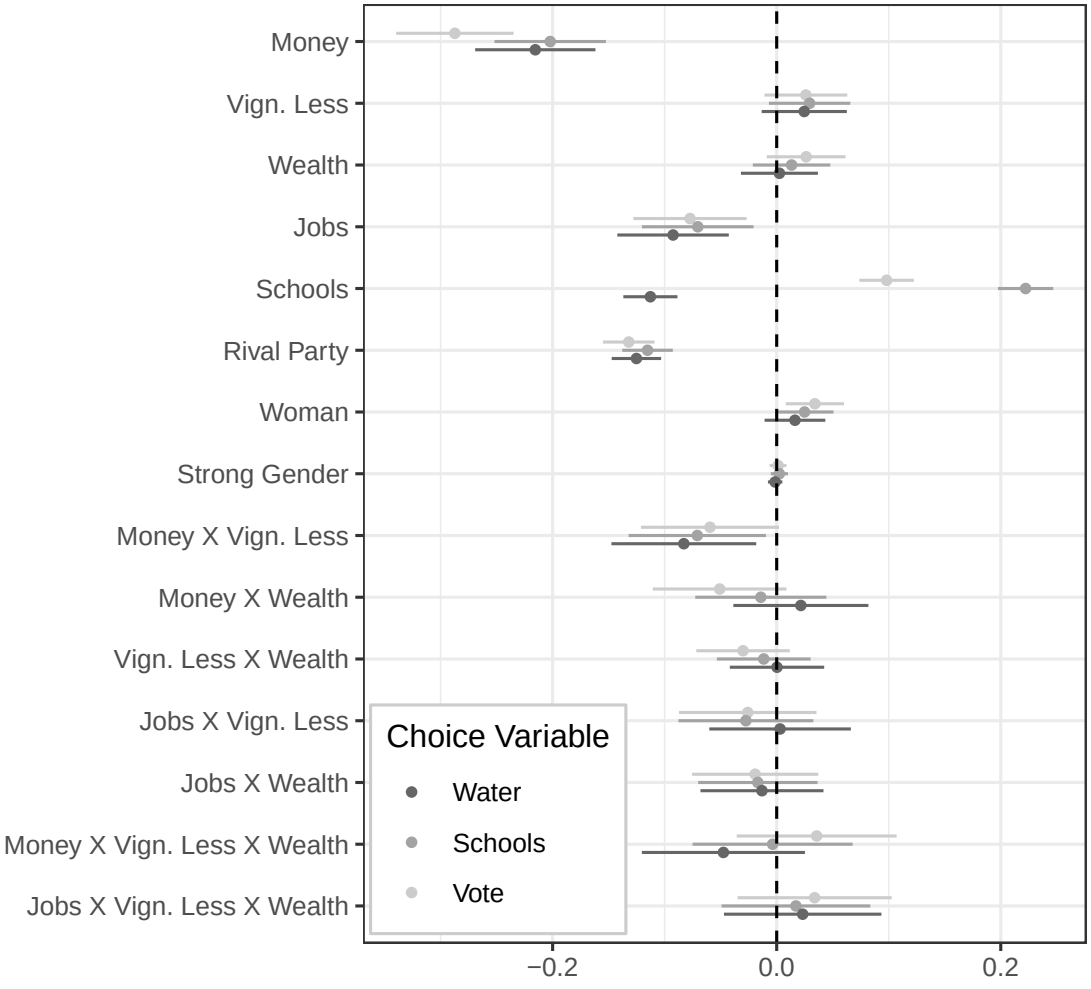
We next explore whether this penalty is driven specifically by voters' understanding that there is a trade-off between vote buying and public goods. Hypothesis 2 contends that voters who see vote buying and public goods provision as substitutes should vote against candidates who buy votes. Figure 5 presents the fully interactive analysis of the conjoint experiment as specified in Equation 2. Specifically, to test Hypothesis 2 and Hypothesis 4, this analysis interacts participants' responses to Vignette Experiment 2—using a dummy variable separating the slightly and much *less likely* responses from their *more likely* counterparts, as specified in the PAP (*Vign. Less*)—with the vote-buying offer dimensions in the conjoint experiment.[24] First, note that once again, we see that our findings relevant to Hypothesis 1 and Hypothesis 3 are robust to the inclusion of interactions capturing heterogeneous effects. Then, consistent with Hypothesis 2 and contradicting Hypothesis 4, voters who perceive a substitution effect are even **less likely** to vote for candidates who offer cash payments than those who see the two behaviors as complementary. However, this effect is modest — about six percentage points — and while the effect is statistically significant at the $p < .05$ level in Figure 5, it dissipates after implementing a Holm correction for multiple comparisons (see SI Table 1). The voting coefficient for the interaction between offering jobs and substitution perception, as measured by the vignette experiment, is also negative, consistent with Hypothesis 2, but this result is not statistically significant. Overall, our interactive results

towards vote buying. Seventy-nine per cent consider it "very immoral" and another 12 per cent "slightly immoral."

[24]Note that Vignette Experiment 2 was conducted after the conjoint experiment, making it a post-treatment moderator. Our tests using this variable should be treated as suggestive. We find a similar result looking only at the conjoint: reported beliefs about candidates' relative public goods provision correlated at 0.61 (water) and 0.65 (schools) with expressed vote choice.

are weak and should be treated as preliminary evidence. Still, they are nonetheless more consistent with Hypothesis 2 than with Hypothesis 4. Voters who perceive vote buying and public goods provision as substitutes are less likely to vote for vote buying candidates than are voters who do not hold this perception.

Figure 5: Conjoint Experiment, Interaction Effects of Clientelist Offers, Substitution Belief, and Wealth on Anticipated Public Goods Provision and Vote Choice



Finally, we explore whether wealthier voters are both more likely to perceive vote buying and public goods provision as substitutes (Hypothesis 5), and more likely to vote against

vote-buying candidates as a result (Hypothesis 6). Specifically, to test Hypothesis 5, the model in Figure 5 interacts participant *Wealth Index* and the vote-buying offer dummies from the conjoint experiment (*Money* and *Jobs*) to assess the effects of these interactions on perceptions of public goods provision (*Water* and *Schools*). An additional test of this hypothesis is included by interacting the binary *Vign. Less* from Vignette Experiment 2 with the *Wealth Index*. Practically speaking, we are assessing whether the light and medium gray lines of rows 10, 11, and 13 in Figure 5 are statistically significant.[25] None of these lines is statistically significant, meaning we find no evidence to support Hypothesis 5, and wealthier voters are no more likely to perceive vote buying and public goods provision as substitutes.

Similarly, Hypothesis 6 predicts that the three-way interactions between the vote-buying offer dummies from the conjoint experiment, participant belief in substitution from Vignette Experiment 2, and participant wealth should be negative for the *Vote* outcome variable. Practically speaking, this means assessing whether the dark gray lines of row 14 and 15 in Figure 5 are statistically significant. Once again, these interactions are not statistically significant predictors of *Vote*, meaning we find no support for Hypothesis 6. Wealthy voters in our sample in Nepal who hold stronger beliefs about substitution are not more likely to vote against vote-buying candidates. However, as we discuss more in Section 5, wealthy voters do vote against hypothetical candidates who offer cash payments more than poorer voters: the interaction between wealth and cash offers is negative and approaches statistically significance ($p < 0.1$) for the *Vote* outcome variable (dark gray line of row 10 in Figure 5).

[25]The additional pre-specified test of this hypothesis based on Vignette Experiment 1 appears in SI Figure 5.

5 Discussion

This paper considers voters' perceptions of the substitutability versus complementarity of vote buying and public goods provision in Nepal, a context in which both vote buying and local public goods promises are common. Based on three survey experiments embedded in a large national survey, we find evidence that Nepali voters perceive campaign-period vote buying and in-office public goods provision as substitutes. Voters who perceive this substitution effect also report a lower propensity to vote for hypothetical vote-buying candidates. This evidence is particularly striking because our study set up a "hard test" of substitutability: in our experiment (and in Nepal more generally), all candidates pledged public goods provision. Still, not all Nepali voters perceived these pledges as credible, and lower credibility was strongly associated with vote-buying offers.

While none of our experiments speak to precisely why voters perceive vote buying and public goods provision as substitutes, our qualitative data sheds some light. Voters in Nepal interviewed during the scoping phase for our study intuitively sense the tradeoffs between the campaign period and the in-office period. Focus group discussion participants in Dhading argued that, "those who win the election with [vote buying] never devote themselves [to the] developmental work of the village and their commitment for social services and welfare cannot be expected to be stable." Similarly, a civil society leader in Pokhara said, "Who buys votes with money never develops [the] constituency." A few of those interviewed explicitly stated the monetary opportunity cost of spending money on vote buying. One community leader in Dhading said that candidates who engage in vote buying during the campaign period need to "recoup" their money once they are in office, implying they siphon funds from the public purse to do so. In the same vein, two (separately) interviewed voters in Dhading discussed a local example of a politician going bankrupt when he lost an election.

Critically, poor and wealthy voters in our sample in Nepal do *not* differ in their belief that

campaign-period vote buying and in-office public goods provision are substitutes. They also do not differentially incorporate this belief in their voting choices. However, separate from their beliefs regarding the substitutability of vote buying and public goods provision, wealthy voters *are* marginally more likely to vote against vote-buying candidates, particularly those who offer cash payments. Collectively, these findings provide preliminary evidence that it is *means*, not *preferences*, that enables wealthy voters to reject vote-buying offers and vote for candidates they perceive as likely to provide public goods in office. Interventions that enable poor voters to do the same - to exercise their right to support candidates they think will be the best representatives - have the potential to improve the quality of elections and the democratic process.

There are several important caveats to our findings. First, we are achieving variation by manipulating hypothetical profiles (in this case, of candidates), a more artificial experimental treatment than in other experiments on vote buying and clientelism (e.g., the field experiment varying real political party campaign messages executed by Wantchekon (2003)). We argue that our extensive piloting to identify both vote-buying benefits and public goods promises that were prevalent and believable in Nepal made the survey experimental treatments realistic, if not real. Still, the experimental treatments cannot deliver the usual contextual factors that are present when a vote-buying offer or public goods promise is made. The context would undoubtedly affect whether or not a given voter accepts or rejects a vote-buying offer and then subsequently votes for or does not vote for the candidate. That said, we have no reason to believe that these contextual factors would affect the voters' beliefs about substitutability between vote buying and public goods provision. Further, even if these contextual factors do affect beliefs about substitutability, it seems unlikely they would do so in a way that would render our findings systematically biased.

Questions about the nature of our instrument and vote buying in the field dovetail nicely with possible mechanism concerns raised by more recent findings about the efficacy

of vote buying. Some recent studies observe that vote buying has only, at best, limited marginal effects on aggregate turnout (Guardado & Wantchékon 2018, Gallego, Guardado & Wantchekon 2023). As discussed in Section 3.1, voters in our sample in Nepal appear to treat vote buying as contingent and vote accordingly. Still, it is possible that individuals en masse "take the money and run." If, in practice, individuals almost never honor their vote pledges/exchanges, then there would be an aggregate effect on the budget for public goods with no accompanying electoral benefit. That would raise the possibility that the people we surveyed view hypothetical politicians who vote-buy as politicians taking the sucker's payoff, signaling low candidate quality and therefore low levels of public good provision—a mechanism that would imply substitution, but for different reasons than prevailing theory posits. Ultimately, we cannot completely dismiss this explanation for our findings, however, our qualitative and quantitative evidence does not support a completely ineffective view of transactional clientelism—as we discussed in 3.1, other survey questions, focus groups, and interviews with citizens, community representatives, journalists, and political leaders painted a picture of extensive vote buying where voters accept private transfers from candidates whom they subsequently support.

Second, our experimental treatments focus on a vote-buying offer. Our treatments do not explore explicitly unconditional offers of private goods, so speak only indirectly to this form of clientelism. Yet even exchange-based promises like our experimental "pledge" are often found to be unmonitored and may have elements of credibility buying or signaling, whether they are effective in garnering votes or not (Finan & Schechter 2012, Lawson & Greene 2014, Guardado & Wantchékon 2018, Gallego, Guardado & Wantchekon 2023). Ultimately, further work is required to see if voters regard unconditional private good offers as a different sort of signal about potential public goods provision.

A third note of caution pertains to the validity of our outcome variables, particularly their vulnerability to social desirability bias. One strength of our design is that we focus on

voter *perceptions* of anticipated public goods provision: after all, voter evaluations of candidates' potential for in-office action is the core mechanism underpinning elections. However, if these outcomes are biased by social desirability pressures, we may be seeing inflated estimates of substitutability. Further, our self-reported, prospective measure of vote choice is arguably inferior to a real-world behavioral measure, and may also be plagued by social desirability bias. Fortunately, perceptions captured as part of other conjoint experiments have correlated with real-world behavior (Hainmueller, Hangartner & Yamamoto 2015, Auerbach & Thachil 2018). Another refutation of these concerns comes from our qualitative work, in which voters and politicians alike were open and frank about clientelistic practices and in-office performance with us, giving no indication of hesitation due to social pressures or (perceived) judgement from the research team. Finally, conjoint experiments seem less vulnerable to social desirability bias than other survey perception questions (Horiuchi, Markovich & Yamamoto 2021).

Overall, however, we provide evidence that voters perceive campaign-period vote buying and in-office public goods provision as substitutes, a largely untested but vital assumption underpinning the prevailing theory of vote buying and clientelism's decline. This finding may run contrary to recent evidence from Kenya, where voters plausibly view vote buying and public goods provision as complements,and somewhat against recent evidence from Peru,[26] where the understanding of the tradeoffs of vote buying is conditioned by voters' level of education (Kramon 2016, Auerbach, Bussell, Chauchard, Jensenius, Nellis, Schneider, Sircar, Suryanarayan, Thachil, Vaishnav, Vermaand & Ziegfeld 2021, González-Ocantos, Kiewiet de Jonge & Nickerson 2014). Collectively, this work suggests that voters differ across

[26]The González-Ocantos, Kiewiet de Jonge & Nickerson (2014) finding regarding education was not robust in Nicaragua, the other country for which they had data to test this hypothesis.

contexts and that one promising avenue of future research is to continue documenting their preferences under different circumstances and interrogating existing assumptions regarding their homogeneity.

In the meantime, we propose the following scope conditions on the generalizability of our findings, based on Nepal's conditions in 2018, when our data collection occurred. Nepal is a consolidating democracy,[27] a lower-middle income country,and a state where both corruption and vote buying are prevalent but not extreme.[28,29] In addition, Nepal's voters are generally well educated and politically empowered,[30] and vote at relatively high rates.[31] In addition, our sampling strategy resulted in a particularly highly educated, yet poor, sample. We might expect our findings to generalize to contexts with these same traits, but not to contexts with key differences, or even to less educated areas of Nepal. Differences between Nepal and Kenya (lower level of education), India (lower level of corruption), and Peru (higher level of democracy) may explain the inconsistent findings across these contexts. More research—focused on voter, rather than elite, decision-making—is needed to map the circumstances under which voters support vote buying and document their perceptions of its costs and benefits.

[27]2018 V-Dem Electoral Democracy Index (EDI) score of 0.62 on a scale of 0 to 1 (Coppedge et al. 2018).

[28]2017 V-Dem Political Corruption Index (PCI) of 0.65 on a scale of 0 to 1 and a 2017 V-Dem vote buying score of 1.77 on a scale of 0 to 4 (Coppedge et al. 2018).

[29]These are the four factors considered when evaluating the generalizability of findings in González-Ocantos, Kiewiet de Jonge & Nickerson (2014).

[30]2019 secondary school enrollment rate of 80% and 2018 women's political empowerment index score of 0.76 on a scale of 0 to 1 (Coppedge et al. 2018).

[31]Turnout was 70% in the 2017 election (Coppedge et al. 2018).

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Supporting Information

A Robustness Checks

A.1 Conjoint Experiment

Here we present a few additional model specifications for analyzing the conjoint experiment. Figures 1 and 2 provide one-way interaction models, as promised in the PAP. They provide results that are consistent with the fully interactive model presented in Figure 5 and we include them only for completeness.

Figure 1: Expected public goods provision and vote choice in a conjoint experiment varying candidate clientelist offers, public goods promises, and candidate characteristics. Partial interactive model with heterogeneous effects of substitution belief.

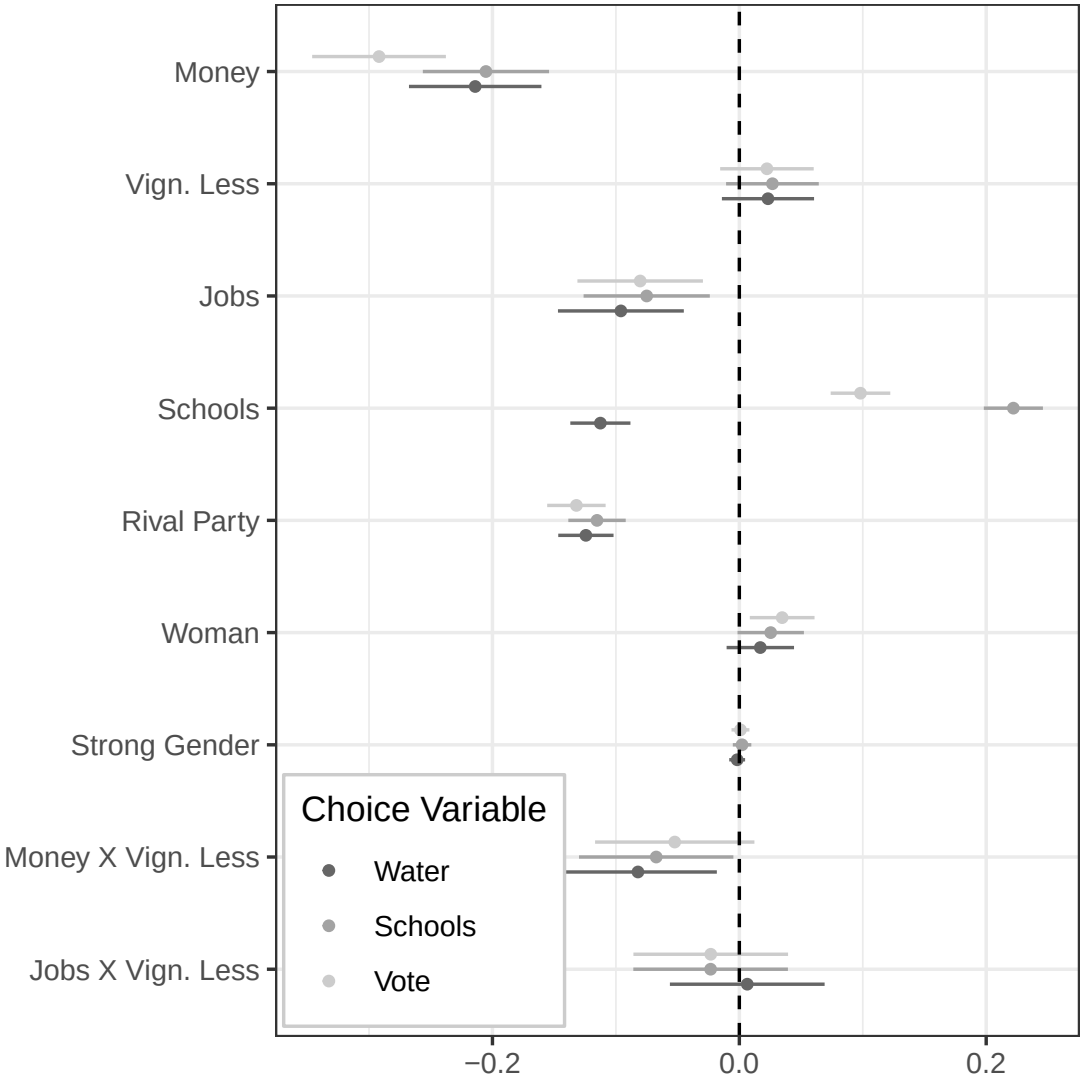


Figure 2: Expected public goods provision and vote choice in a conjoint experiment varying candidate clientelist offers, public goods promises, and candidate characteristics. Partial interactive model with heterogeneous effects of wealth.

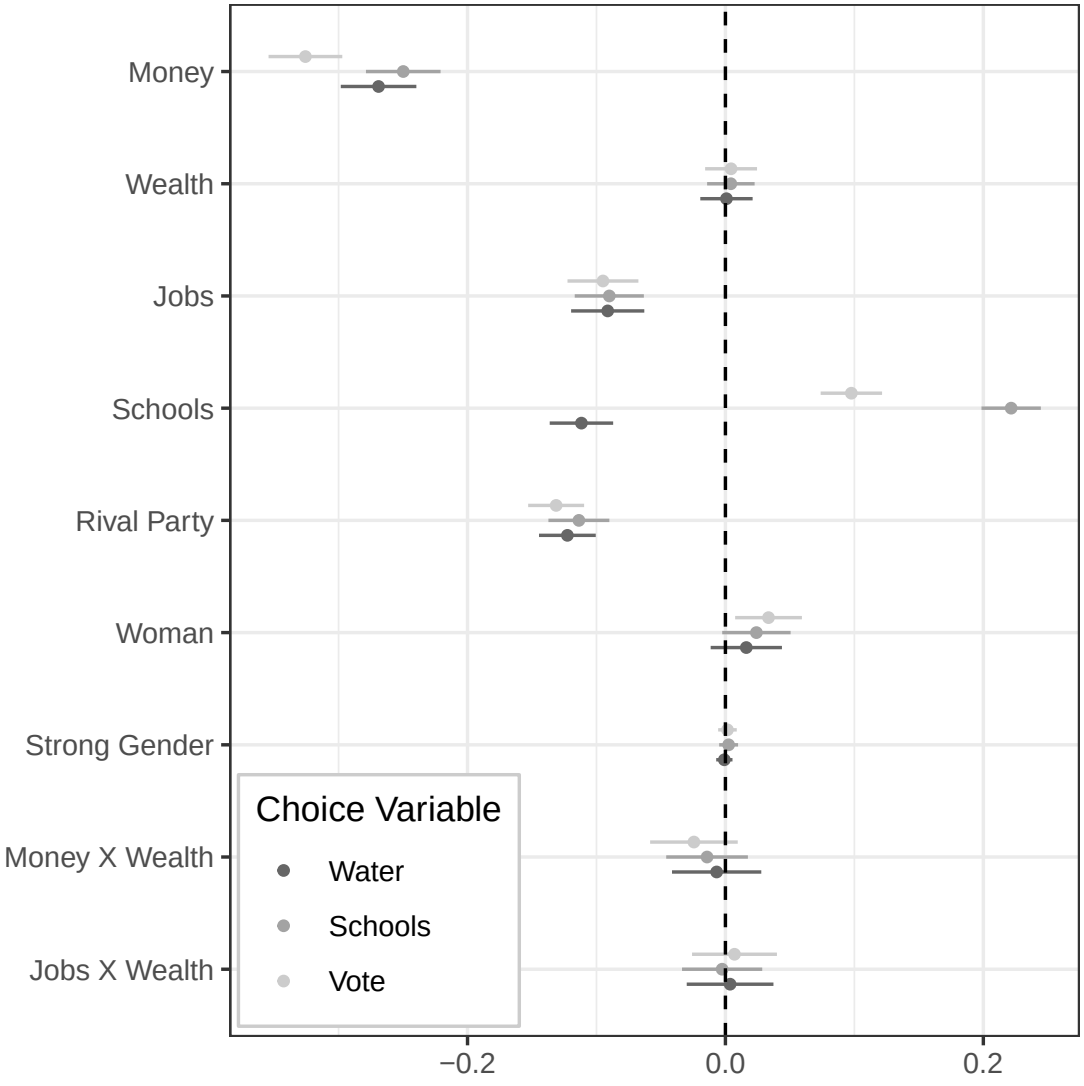
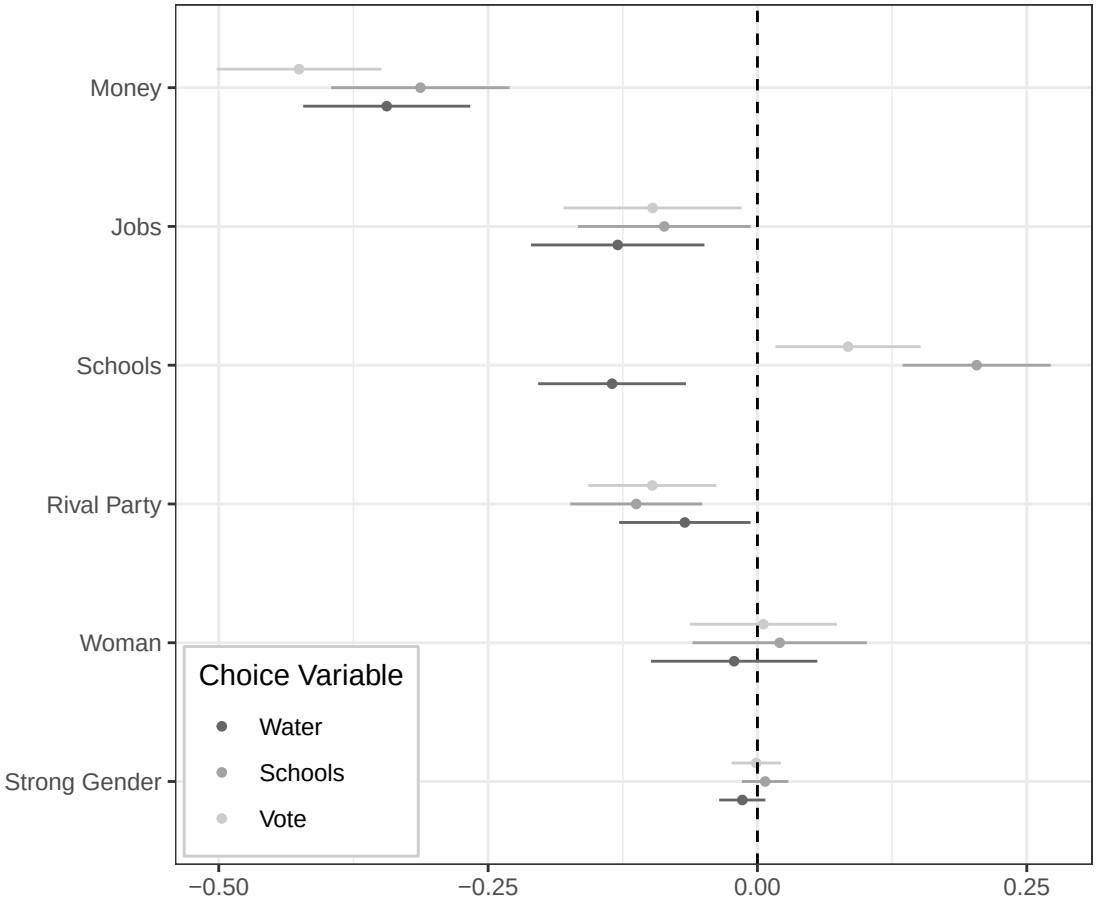


Figure 3 replicates the main effects findings for the subsample containing only those respondents who did not participate in the lab-in-the-field voting game that preceded the survey. Enumerators often recruited more participants than necessary for the game and randomly selected a subset of participants to play the game. Remaining participants completed

the survey. Figure 3 shows that both types of participants behaved similarly in the conjoint experiment, making it unlikely that our findings in support of a substitution effect are a result of the game format—which adopted a substitution model—priming the participants.

Figure 3: Expected public goods provision and vote choice in a conjoint experiment varying candidate clientelist offers, public goods promises, and candidate characteristics. Main effects only. Subset of the sample who did not participate in the lab-in-the-field voting game.



In Table 1, we delineate which coefficients in the main conjoint model are statistically significant at the $p < .05$ level before and after a Holm correction for multiple comparisons. Coefficients that lost statistical significance after the Holm correction are noted with red text. Generally, the results discussed in the main text are robust to this correction, with the

exception of the interaction term between the dummy for the cash clientelist offer treatment in the conjoint experiment and the dummy variable separating the slightly and much *less likely* responses from their *more likely* counterparts from Vignette Experiment 2.

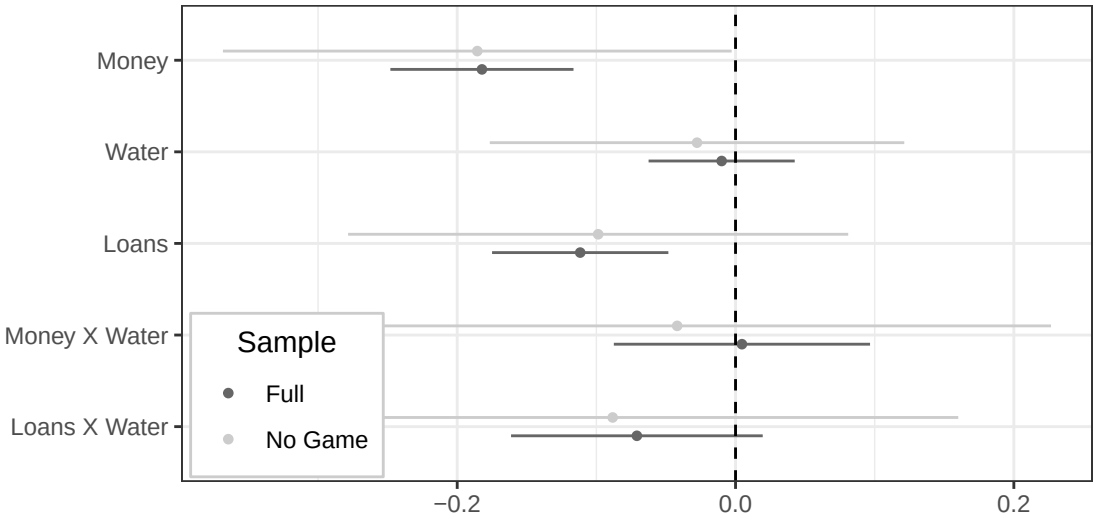
Table 1: Coefficient Significance at $p < .05$ Level Before and After Holm Correction

	DV: School		**DV: Water**		**DV: Vote**	
Independent Variable Name	**Before**	**After**	**Before**	**After**	**Before**	**After**
Intercept	TRUE	TRUE	TRUE	TRUE	TRUE	TRUE
Money	TRUE	TRUE	TRUE	TRUE	TRUE	TRUE
Vign. Less	FALSE	FALSE	FALSE	FALSE	FALSE	FALSE
Wealth	FALSE	FALSE	FALSE	FALSE	FALSE	FALSE
Jobs	TRUE	TRUE	TRUE	TRUE	TRUE	TRUE
Schools	TRUE	TRUE	TRUE	TRUE	TRUE	TRUE
Rival Party	TRUE	TRUE	TRUE	TRUE	TRUE	TRUE
Woman	FALSE	FALSE	TRUE	FALSE	TRUE	TRUE
Strong Gender	FALSE	FALSE	FALSE	FALSE	FALSE	FALSE
Money X Vign. Less	TRUE	FALSE	TRUE	FALSE	TRUE	FALSE
Money X Wealth	FALSE	FALSE	FALSE	FALSE	FALSE	FALSE
Vign. Less X Wealth	FALSE	FALSE	FALSE	FALSE	FALSE	FALSE
Jobs X Vign. Less	FALSE	FALSE	FALSE	FALSE	FALSE	FALSE
Jobs X Wealth	FALSE	FALSE	FALSE	FALSE	FALSE	FALSE
Money X Vign. Less X Wealth	FALSE	FALSE	FALSE	FALSE	FALSE	FALSE
Jobs X Vign. Less X Wealth	FALSE	FALSE	FALSE	FALSE	FALSE	FALSE

A.2 Two Candidate Vignette Experiment

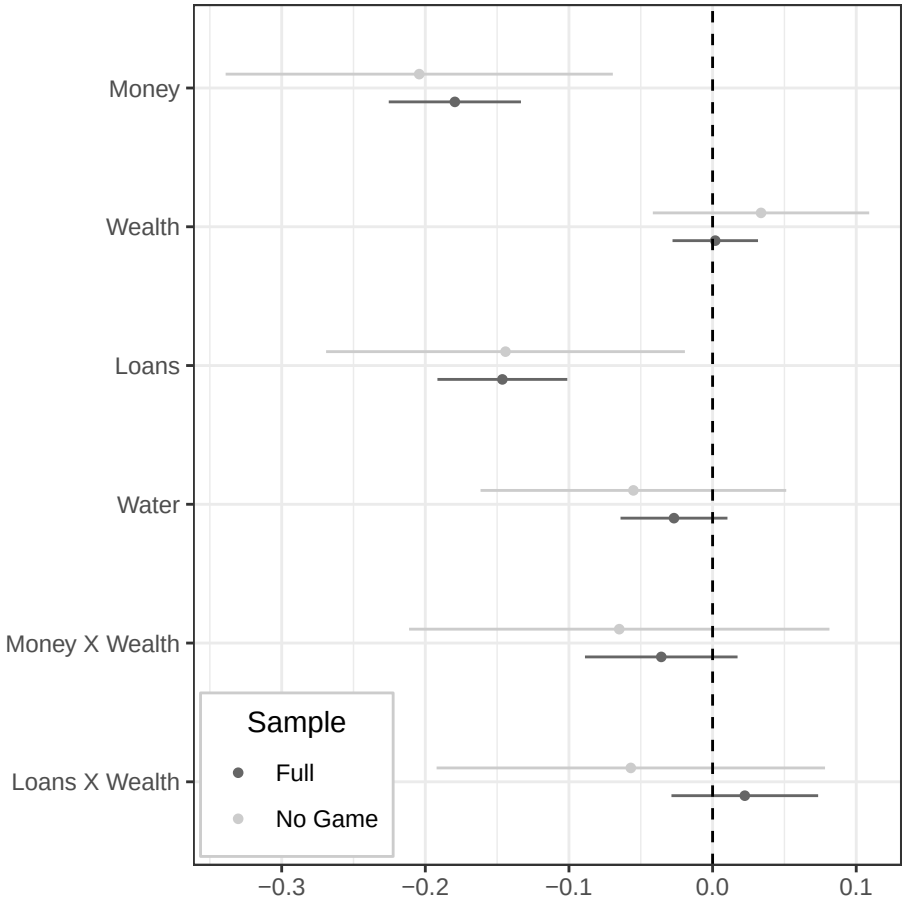
This section presents additional models for the two-candidate vignette experiment (Vignette Experiment 1). Figure 4 includes an interaction effect between clientelist offer (nothing/money/loan) and public goods promise (water/schools infrastructure) treatment conditions. As we would expect, participants' response to the private goods offer is independent of the public goods promise.

Figure 4: Who do respondents think will better provide school or water infrastructure, given clientelist offer, in a simple vignette/conjoint experiment? This model adds interactions between clientelist offers and public goods promise.



In Figure 5, we provide an additional pre-specified test of Hypothesis 5 based on Vignette Experiment 1, interacting the *Wealth Index* with the clientelist offer dummies based on the experiment conditions. The results align with those reported in the main text: wealthier voters are no more likely to perceive clientelism and public goods provision as substitutes.

Figure 5: Who do respondents think will better provide school or water infrastructure, given clientelist offer, in a simple vignette/conjoint experiment? Heterogeneous effects by wealth.



B Measuring Wealth

We create a Wealth Index from a battery of proxy indicators. First, all respondents to this survey participated in a lab-in-the-field study for which they were placed in groups of nine. We asked all participants to rate their perceived wealth, relative to their groups. We also asked all participants an array of questions about their life circumstances. Note that we deviate slightly from our pre-analysis plan when measuring wealth. Specifically,

we mistakenly registered a measurement strategy based on Bayesian factor analysis, even though the binary nature of the indicators lends itself to item response theory (BFA has the wrong functional form). Thus we adjusted the model to correct this mistake. The IRT analysis is conceptually identical to the registered approach. We use all of the pre-registered indicators, but add self-evaluated relative wealth (item 1 below), because it is a logical indicator to include and we overlooked it at pre-registration time. We did not fit a Bayesian factor analysis (given that it is difficult to fit a BFA to a set of binary indicators) and self-evaluated relative wealth has little impact on the wealth scale. Therefore, we do not consider this a substantively meaningful deviation from the registered plan.

1. Do you think you are more or less wealthy than the average participant in your group?

    (a) More wealthy

    (b) Less wealthy

2. What is your largest source of income? Do not read options aloud. Select one.

    (a) Subsistence farming/livestock rearing/fishing

    (b) Commercial farming/livestock rearing/fishing

    (c) Remittances

    (d) Business income

    (e) Wages in cash (agricultural)

    (f) Wages in cash (non-agricultural)

    (g) Salaries (Government, private, or NGO)

    (h) Pension

    (i) Allowance

    (j) Rent/lease income

(k) Other (specify)

3. Where do your family members usually go for a health check-up/treatment when someone in your family is sick? Mark up to three.

   (a) Health post

   (b) Local medical store

   (c) Government hospital

   (d) Private hospital/clinic

   (e) Traditional healer

   (f) Health institutions in India

   (g) Other (specify)

4. What type of school do your children (if applicable) attend for education?

   (a) Public/Government

   (b) Private/Boarding

   (c) Religious/Non-formal institutions

5. What is the primary construction material of your housing unit's exterior walls?

   (a) Grass/thatch/bamboo

   (b) Plastic/Polythene

   (c) Mud/unburnt brick

   (d) Wood

   (e) Stone not packed with mortar

   (f) Stone packed with mortar

   (g) GI/Metal/Asbestos sheets

(h) Concrete

(i) Burnt brick

(j) Other (specify)

6. What is the primary construction material of your housing unit's roof?

(a) Grass/thatch/bamboo/wood/mud

(b) Plastic/polythene

(c) Handmade tiles

(d) Machine made tiles

(e) Burnt brick

(f) Stone

(g) Slate

(h) CGI/Metal/Asbestos sheets

(i) Concrete

(j) Other (specify)

7. What is the primary fuel source your household uses for cooking?

(a) Wood

(b) Sawdust

(c) LPG or similar

(d) Both LPG and wood

(e) Other natural material

We converted these questions into binary indicators, collapsing categories when they proved particularly rare.[1] We then used Bayesian item response theory modeling to construct

[1]See the replication package for details.

a latent index from these binary indicators (Johnson & Albert 1999, ch. 6), fitting a model that assumes

$$p(y_{i,j} = 1|\theta) \sim \Phi(\beta_j \theta_i - \alpha_j), \tag{1}$$

where θ_i is the latent wealth of participant i, α_j and β_j are parameters describing the "difficulty" and "discrimination" ability of item j, and y_{ij} is the binary response for participant i on item j.

We use vaguely informative conjugate priors:

$$\theta_j \sim \mathcal{N}(0, 1), \text{and}, \tag{2}$$

$$[\alpha_j, \beta_j] \sim \mathcal{N}_2(0, 4 \cdot \mathbf{I}_2). \tag{3}$$

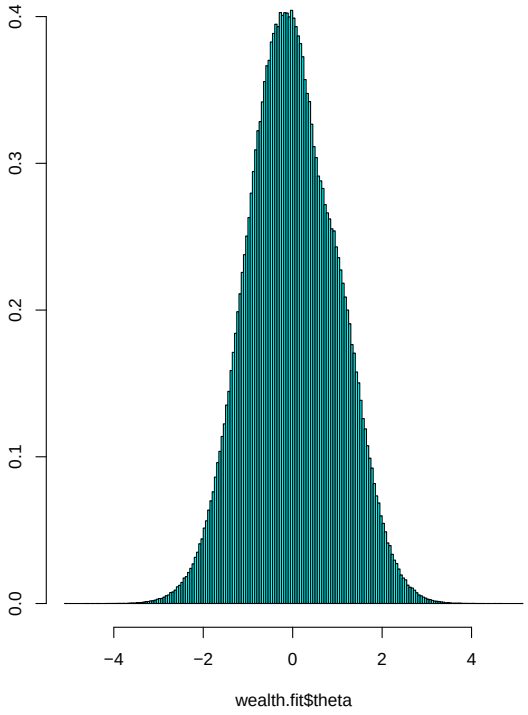
The standard normal prior on each θ_i establishes the arbitrary scale of the latent trait while the priors on the difficulty and discrimination parameters are vague. We restrict the θ_i score for a particular participant (specifically, $i = 337$) with a response pattern highly consistent with wealth (self identifies as wealthy, has a concrete house, sends kids to private school, uses private and Indian hospitals, etc) to be positive to identify the model.[2] We used the MCMCpack R package (Martin, Quinn & Park 2011) to fit the model, running a single chain for 100,000 iterations, after a 20,000 iteration burnin, and keeping every 100th draw from the chain, simulating a sample of 1000 posterior draws. Geweke and Heidelberg diagnostic tests produced results consistent with convergence, as did visual inspection of traceplots.

Figure 6 plots estimated item parameters (Figure 6a, Figure 6b) and the distribution of latent trait point estimates (Figure 6c). Note that negative discrimination parameters mean

[2]Cardinality—which way is up—is not identified by the data.

(a) Difficulty Parameters

(b) Discrimination Parameters



(c) Latent Scores

Figure 6: IRT Estimates

that a participant exhibiting that trait tends to be less (latently) wealthy, while a positive discrimination parameter indicates that participants with that trait tent to be more wealthy. Higher difficulty parameters indicate that exhibiting a given trait tends to correlate with a latent wealth closer to the top of the scale. Overall, the item parameters are consistent with what we would expect: strong signals of wealth or poverty tend to be difficult and discriminating items. Inspecting the item parameters, we see that the most discriminating indicators show that wealthy individuals tend to have concrete or brick homes, use LPG to cook, have business provide their largest source of income, use private or Indian hospitals, and send their children to private schools. Less wealthy people tend to have unmortared stone homes, metal roofs, cook with wood, use health posts, and send their children to public schools. Self-reported relative wealth is not an especially discriminating nor difficult indicator, but does load positively with other indicators of wealth. The distribution of latent wealth within our sample resembles a normal density.

C Comparing Study Sample to Population

As discussed in the main manuscript, our sampling strategy ensured a random sample of local government units across Nepal, subject to several theoretically or logistically important constraints. In sampling individuals from each locality, we relied on a local facilitator. This facilitator received the following instructions to recruit 11 participants in their local government unit:

1. Please recruit at least 9 and up to 11 participants within local government unit [NAME].

2. Only those over 18 years of age should be recruited.

3. Attempt to recruit an even number of male and female participants, as much as pos-

sible.

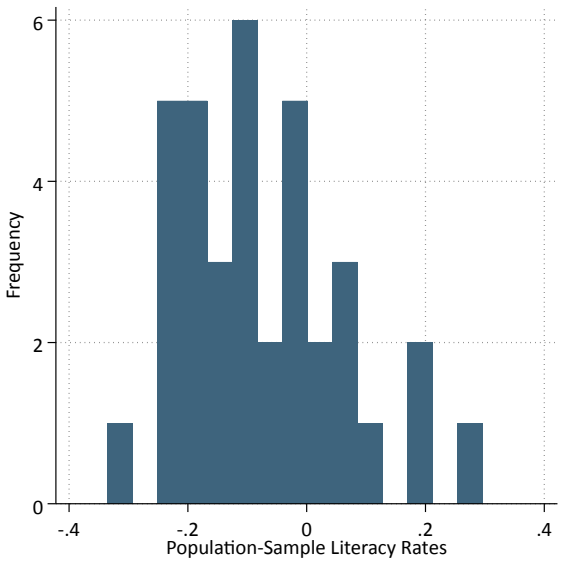
4. A certain number of these participants should be wealthier and a certain number of these participants should be poorer. See the sampling list for the exact numbers for your local government unit [NAME].

To assess similarities and differences between our sample and the population, we aggregate the survey data to the district level. In Table 2, we present one-tailed, one-sample difference in proportions tests on key demographic variables, using the Nepal census data aggregated to the district level for the reference proportions. At the district level, our sample is significantly more educated and less wealthy than the population. While these differences are striking, we note that the census includes anyone 10 years or older in their literacy and education statistics, whereas we (by construction) include only those 18 or over. The two caste variables are also based on 2011 census data, as 2021 data for ethnicity and caste variables are not yet available. Finally, note that the last variable - whether the participant's housing is made out of concrete walls - is a crude proxy for wealth, especially in the absence of more details about other materials used in housing.

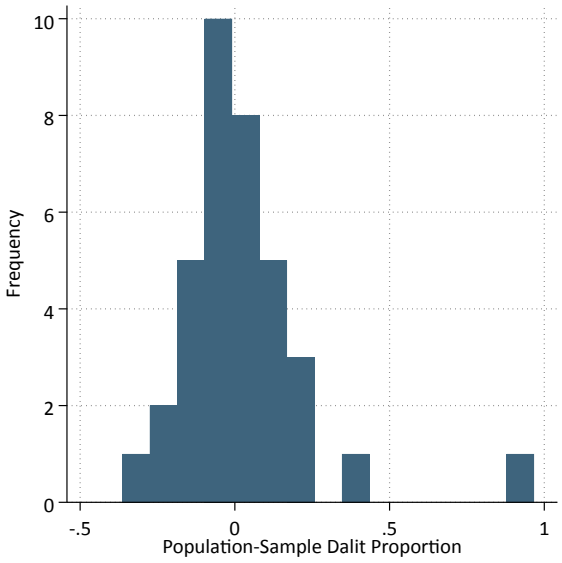
In Figure 7, we show a histogram of these differences at the district level for the proportion literate and the proportion Dalit. We see that the distribution of differences in the proportion literate (Figure 7a) is relatively wide ranging from an approximately -0.30 difference to an approximately 0.30 difference. In contrast, at the district level, the sample and population often have no difference in their level of Dalit population, as demonstrated by the large number of districts with differences between approximately -0.2 to 0.2 (Figure 7b). However, there are a few districts with a lot more Dalit in the population than are reflected in the sample.

Table 2: Sample vs. Population Difference in Proportions Tests

Variable	**Sample Proportion**	**Population Proportion (year of census data)**	$p-$**value**
Written Literacy	0.833	0.764 (2021)	0.000
At Least Secondary School Education	0.690	0.375 (2021)	0.000
Dalit Caste	0.104	0.216 (2011)	0.000
Janajati Caste	0.349	0.002 (2011)	0.000
Concrete Housing Walls	0.202	0.369 (2021)	0.000



(a) Comparing Sample to Population on Proportion Literate



(b) Comparing Sample to Population on Proportion Dalit

Figure 7: District-Level Differences in Proportion Literate and Proportion Dalit

D Pre-Analysis Plan

Here we provide an anonymized version of our PAP for this paper. This is part of a multi-paper plan which we registered after data collection, but before viewing any collected data. The larger plan includes a section on sampling, and a section on the conjoint and vignette experiments, which we describe in the main text of this paper, and therefore have not reproduced here, given the journal's 20 page-limit for appendices. The full document also includes analysis plans for additional papers analyzing other data collected at the same time as the data that we analyze in this paper (i.e., a behavioral game, list experiment, randomized voter experiment, and other aspects of the survey). Again, for space reasons, we do not include these sections here. Here we replicate only the section of the plan dedicated to the analysis for this planned paper. This is largely redundant of the main text, but we include it nonetheless, to document our adherence to the plan. We copied the following text verbatim from the registered plan, although counters (e.g., section and equation numbers) changed when we recompiled the LaTeXto create this appendix. References to excised portions of the PAP are also broken. We have opted to leave the "**??**" indicators in to denote such references. While reviewers cannot access the complete signed PAP, readers can find it at [LINK REDACTED].

This paper ask if voters view clientelism and public goods provision as substitutes or complements. In other words, if a voter observes a candidate pursuing votes through clientelism, does she conclude that the candidate would be better, or worse, at public goods provision than a counterfactual identical candidate that did not provide private goods for votes? Specifically, we consider whether voters perceive clientelist exchanges during the campaign period as substituting for public goods provision in-office, as much theory would suggest, or whether, instead, they perceive clientelist exchanges during the campaign period as a complement to future public goods provision, perhaps because candidates with the

connections to produce public goods for their constituents also have the resources to buy votes. In turn, does this understanding of the relationship between the use of clientelism, and effectiveness at public goods provision, drive vote choice?

Such perceptions may provide a mechanism for well-known income and education effects, or may stand on their own as a driver of clientelism. If voters perceive clientelism and public goods provision as substitutes, voters who place great value on future public goods may opt out of clientelism. If, however, voters perceive clientelism and public goods provision as complements, voters will view clientelism as a signal of competency and feel no need to punish clientelist candidates at the polls.

D.1 Hypotheses

The formal model in Stokes et al. (2013) makes an underlying assumption of much of the literature explicit: parties and candidates cannot provide widespread clientelist benefits during the campaign period and substantial public goods once in office, and voters understand this tradeoff. In the standard story, clientelism and public goods provision are substitutable strategies. While the model assumes a budget that can be expended on vote buying or public goods provision, there a number of ways that this trade-off might operate in practice. For example, politicians might spend their own money to buy votes, and then pilfer public coffers once in office to compensate themselves. Less directly, patronage jobs might reduce bureaucratic efficacy, limiting public goods provision, or candidates who expend energy to maintain client networks might have insufficient time to effect broad policy changes.

However, voters' understanding of the substitutability of clientelism and public goods provision is an untested assumption in the literature. In line with this assumption, voters turn away from clientelism by punishing parties promising private goods because they realize that any money spent on clientelism means that public goods provision in the future will be lower. Clientelist parties produce poor public goods, while parties not engaging in clientelism

will be more likely to deliver public goods. This logic leads to the following hypotheses:

Hypothesis D.1 (Substitutes). *Voters perceive clientelism and public goods provision as substitutes.*

Hypothesis D.2 (Substitutes-Voting). *Voters who perceive clientelism and public goods provision as substitutes are more likely to vote against clientelist candidates.*

While most of the models in the literature assume a substitution motivation for voter preferences about clientelism, we advance an alternative possibility. Voters may not punish clientelist parties because they perceive money spent on clientelism reduces their chance of receiving important public goods like roads, power, or clean water. We argue that, instead, voters may perceive clientelist promises and future public goods provision as complementary. When voters see parties delivering on clientelism, they are more likely to believe their public policy promises as well. The idea that clientelism serves an informational role, rather than a purely instrumental one, is not new; vote-buying has long been thought to signal a candidate's electoral resources (van de Walle 2007) and Kramon (2016, 2018) argues that politicians engage in clientelism to signal their ability, and commitment, to provide downstream benefits.[3] Nonetheless, the idea that clientelism signals proficiency in public goods provision remains largely unexplored.[4] The competing hypotheses to the Substitutes Hypotheses (D.1, D.2) are therefore:

[3]The logic of complementarity is also related to that advanced by González-Ocantos, Kiewiet de Jonge & Nickerson (2014), who in one hypothesis show that voters perception of clientelism and its ills is conditional on partisanship of the vote buyer. In contrast to their partisan hypotheses of giving copartisans a pass on clientelism, though, our logic is general among all voters' attitudes toward clientelism and public goods.

[4]Notably, see Baldwin (2013) who does explore the possibility of sophisticated voter inferences with respect to potential local public goods provision in the presence of even

Hypothesis D.3 (**Complements**). *Voters perceive clientelism and public goods provision as complements.*

Hypothesis D.4 (**Compliments-Voting**). *Voters who perceive clientelism and public goods provision as complements are less likely to vote against clientelist candidates.*

It is possible that the above hypotheses illuminate a causal mechanism explaining the relationship between voter income and propensity to engage in clientelism. Perhaps wealthier voters are better educated and able to understand the tradeoff between private goods today and public goods tomorrow. Or perhaps wealthier voters and poor voters hold similar beliefs about the substitution or complementarity of clientelism and public goods provision, but wealthier voters are willing and able to refuse clientelist benefits to "take a stand" against clientelism at the polls. These considerations lead to the following interaction hypotheses:

Hypothesis D.5 (**Wealth-Substitutes**). *Wealthy voters are more likely to view clientelism and public goods provision as substitutes than are poor voters.*

Hypothesis D.6 (**Wealth-Substitutes-Voting**). *Wealthy voters who view clientelism and public goods provision as substitutes are more likely to vote against clientelist candidates than are poor voters.*

D.2 Data Collection

In order to assess whether citizens perceive clientelist practices to substitute for or complement public goods provision in office, we embed three experiments in the survey of voters in Nepal. The survey is designed to address several research questions, but in this write-up (and eventual paper), we focus on assessing substitutability vs. complementarity perceptions and testing the effects of these perceptions on voting behavior.

higher level patron networks.

We use the candidate conjoint experiment and the two vignette experiments, which we describe in section **??**, to examines the compliments-substitutes question. Here, we are most interested in how the "Vote-Buying" dimension of the conjoint affects questions 1, 4, and 5. We also use vignette experiment 1 to provide a less complicated test of the dueling theories, and use vignette experiment 2 to ask: do voters use a substitutes or complements assumption to infer public good provision from clientelist behavior? We do not examine the randomized voter experiment in this paper.

We also use a series of covariates, collected as part of the survey—and described in more detail below—to examine heterogenous effects and test the Wealth-Substitutes and Wealth-Substitutes-Voting hypotheses.

D.3 Data Analysis Plan

D.3.1 Predictions

Using these data, we will first assess whether citizens perceive clientelism and public goods data are substitutes or complements by examining the evidence across the three experiments. Specifically, in line with hypothesis D.1 (Substitutes), we expect that:

- In the conjoint experiment, voters will be less likely to anticipate candidates providing water connections when the candidate offers money to voters.
- In the conjoint experiment, voters will be less likely to anticipate candidates building school infrastructure when the candidate offers money to voters.
- In the conjoint experiment, voters will be less likely to anticipate candidates providing water connections when the candidate offers jobs to voters.
- In the conjoint experiment, voters will be less likely to anticipate candidates building school infrastructure when the candidate offers jobs to voters.

- In vignette experiment 1, voters will be less likely to anticipate candidates providing water connections when the candidate offers money to voters.
- In vignette experiment 1, voters will be less likely to anticipate candidates building school infrastructure when the candidate offers money to voters.
- In vignette experiment 1, voters will be less likely to anticipate candidates providing water connections when the candidate offers access to loans to voters.
- In vignette experiment 1, voters will be less likely to anticipate candidates building school infrastructure when the candidate offers access to loans to voters.
- In vignette experiment 2, voters will perceive future public goods for the community as less likely when the candidate offers money to voters.
- In vignette experiment 2, voters will perceive future public goods for the community as less likely when the candidate offers access to loans to voters.

In line with the hypothesis D.2 (Substitutes-Voting), we expect that:

- Voters who answer either "Much less likely" or "Slightly less likely" in response to vignette experiment 2 are less likely to anticipate voting for candidates who offer money to voters in the conjoint experiment.
- Voters who answer either "Much less likely" or "Slightly less likely" in response to vignette experiment 2 are less likely to anticipate voting for candidates who offer jobs to voters in the conjoint experiment.

Hypotheses D.3 (Complements) and D.4 (Complements-Voting) produce predictions that are the converse of those above.

Finally, in order to test hypotheses D.5 (Wealth-Substitutes) and D.6 (Wealth-Substitutes-Voting), we will consider whether the above expectations find more support in the population of wealthy voters. We describe our wealth measurement strategy in section **??**.

D.3.2 Conjoint Experiment Analysis

For the conjoint experiment we are interested in the vote-buying component (hypotheses D.1 and D.3), the interactions between vote-buying component and responses to vignette 2 (hypotheses D.2 and D.4), and the interaction between the vote-buying component and wealth (hypotheses D.5 and D.6). Our tests are complicated somewhat because we have three outcome variables: one for water connections, one for school infrastructure, and one for vote choice. Because these outcomes will be correlated, and to facilitate accounting for multiple testing, we will use a multivariate regression framework, where we assume that

$$\mathbf{y}_{ijk} \sim \mathcal{N}_3(\mathbf{x}_{ijk}\mathbf{B}, \mathbf{\Sigma}), \tag{4}$$

where $\mathbf{y}_{ijk} = \left[y_{ijk}^{\text{water}} y_{ijk}^{\text{school}} y_{ijk}^{\text{vote}}\right]$, i indexes respondent, j indexes candidate, k indexes conjoint task, $\mathbf{B}$ is a $m \times 3$ matrix of unknown coefficients, and $\mathbf{\Sigma}$ is an unknown variance-covariance matrix. Note that y_{ijk}^{o} is a dummy variable that equals 1 if respondent i selects candidate j for outcome o, in task k. While we will be working in a multivariate framework, we will otherwise follow the approach to conjoint analysis described by Hainmueller, Hopkins & Yamamoto (2014). In particular, we will use a block bootstrap procedure to estimate standard errors. We will also use standard adjustments for multiple testing.[5]

[5]We may use a Bayesian framework for our analysis, to deal with multiple corrections, easily compute tests on functions of coefficients, and to avoid null hypothesis tests. We are still working out the details of how to correctly translate the Hainmueller, Hopkins & Yamamoto (2014) approach to a Bayesian setting. We will report frequentist results in the

Here, $\mathbf{x}_{ijk}$ is a m-vector containing two dummy variables indicating whether candidate j is offering money or jobs,[6] a dummy variable indicating whether respondent i selected much/slightly less likely in vignette experiment two, our wealth indicator for respondent i, two-way interactions between each private goods offer dummy and both the vignette 2 response dummy and the wealth indicator, and three-way interactions between each private goods offer dummy, the vignette 2 response dummy, and the wealth indicator. It also includes main effects for candidate party, gender, gender strength, and public goods promise conditions that are not of substantive interest in this paper, but which we include to account for known variance. Dropping participant, candidate, and task indices for readability, we therefore estimate the regression equation

$$\begin{aligned} y^o =& \beta_0^o + \beta_1^o \text{money} + \beta_2^o \text{jobs} + \beta_3^o \text{vignette} + \beta_4^o \text{wealth} \\ &+ \beta_5^o (\text{money} \times \text{vignette}) + \beta_6^o (\text{jobs} \times \text{vignette}) + \beta_7^o (\text{money} \times \text{wealth}) + \beta_8^o (\text{jobs} \times \text{wealth}) \\ &+ \beta_9^o (\text{vignette} \times \text{wealth}) \\ &+ \beta_{10}^o (\text{money} \times \text{vignette} \times \text{wealth}) + \beta_{11}^o (\text{jobs} \times \text{vignette} \times \text{wealth}) \\ &+ \beta_{12}^o \text{supported party} + \beta_{13}^o \text{woman} + \beta_{14}^o \text{gender strong} + \beta_{15}^o \text{school}, \end{aligned} \tag{5}$$

simultaneously across all three outcomes $o \in \{\text{water, school, vote}\}$.[7]

We can test the predictions for hypotheses D.1 and D.3 by examining the coefficients for the dummies that link the vote-buying component to the public goods provision outcomes: if

appendix if we end up using the Bayesian approach in the paper.

[6]No private goods offer is the omitted level.

[7]Because interactions eat up degrees of freedom, we will also fit the relevant sub-models: main effects only, main effects and interactions between vote-buying and vignette response, main effects and interactions between vote-buying and wealth.

the substitutes story holds these four coefficients should be negative, while complementarity would imply positive coefficients. Specifically, from equation 5, we expect that, $\beta_1^{\text{water}} < 0$, $\beta_1^{\text{school}} < 0$, $\beta_2^{\text{water}} < 0$, and $\beta_2^{\text{school}} < 0$, if hypothesis 1 holds. We do not expect to find distinct relationships across outcome, nor do we expect to find that offers of money behave differently than offers of jobs.

We can test the predictions for hypotheses D.2 and D.4 by examining the interactions between the vote-buying component terms and the dummy for vignette 2 response, for the vote choice outcome. We expect these interaction terms to be negative ($\beta_5^{\text{vote}} < 0$ and $\beta_6^{\text{vote}} < 0$) if the Substitutes-Voting hypothesis holds, and positive if Complements-Voting does.

We can test the predictions for hypothesis D.5 by examining the interactions between the vote-buying component terms and the wealth index, for the public goods provision outcomes. If the Wealth-Substitutes hypothesis holds then we'd expect these interaction terms to be negative ($\beta_7^{\text{water}} < 0$, $\beta_7^{\text{school}} < 0$, $\beta_8^{\text{water}} < 0$, and $\beta_8^{\text{school}} < 0$).

Similarly, the three-way interactions between the vote-buying component terms, the dummy for vignette 2 response, and the wealth index, for the vote choice outcome, speak to hypothesis D.6. We expect these coefficients to be negative ($\beta_{10}^{\text{vote}} < 0$ and $\beta_{11}^{\text{vote}} < 0$) if the Wealth-Substitutes-Voting hypothesis holds.

D.3.3 Vignette Experiment 1 Analysis

Testing the predictions for vignette experiment 1, stemming from hypotheses D.1 and D.3, uses an analogous approach to the conjoint analysis, except now respondents see only a single vignette ($k = 1$, always) and answer a single questions, as opposed to multiple conjoint comparisons with multiple responses, each. We also can greatly simplify the right hand side of the model because these hypotheses imply no interactions, so we need only include main effects when implementing the Hainmueller, Hopkins & Yamamoto (2014) approach.

We expect the coefficients for private goods provision to be negative. If the substitution hypothesis holds, then the coefficients for vote-buying and small loans will be negative, otherwise they will be positive.

D.3.4 Vignette Experiment 2 Analysis

The analysis for vignette experiment two will involve an OLS regression of the outcome variable on a dummy variable for loans.[8] We expect a negative intercept in this model, and that the sum of the intercept and the coefficient for offering loans will be less than zero, if the Substitution hypothesis holds. Clearly the Complements hypothesis implies the opposite. We do not have a prior expectation about whether the coefficient for loans will be positive or negative. That is, we do not have an expectation about how participants will differentiate between cash and loans.

[8] We will also estimate an ordinal probit to check robustness.