Received: 26 September 2022 | Revised: 16 August 2024 | Accepted: 16 August 2024

DOI: 10.1111/ajps.12982

AJPS AMERICAN JOURNAL of POLITICAL SCIENCE

ARTICLE

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. We ask whether voters perceive vote buying as substituting for local public goods provision in office, or whether they think that candidates who buy votes will excel at securing local public goods. Voters who place great value on future public goods may opt out of vote buying if they believe they are substitutes and punish vote-buying candidates at the polls. We explore these issues in a nationwide 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.

Political scientists theorize that clientelism and public goods provision are alternate campaign strategies—that is, *substitutes*—for candidates and parties (Stokes et al., 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 clientelist relationships (Pellicer et al., 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, 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 (Hicken et al., 2022; Muñoz, 2014), 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 and Nathan (2020, p. 288) have labeled "credibility buying." Under these circum-

Verification Materials: The materials required to verify the computational reproducibility of the results, procedures, and analyses in this article are available on the *American Journal of Political Science* Dataverse within the Harvard Dataverse Network at: https://doi.org/10.7910/DVN/BC6KYY.

The Cornell Center for Social Sciences verified that the data and replication code submitted to the AJPS Dataverse replicates the numerical results reported in the main text of this article.

stances, 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 contingent 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.

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 focuses 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 (Aidt & Jensen, 2017; Brusco et al., 2004; Camp et al., 2014; Cox, 1987; Stokes et al., 2013).

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 are multiple ways that this tradeoff 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 toward democracy, non-economic preferences, and other psychological factors affect voter beliefs and behavior (Finan & Schechter, 2012; Gonzalez Ocantos et al., 2014; Nichter & Peress, 2017). Recent work highlights the need to more explicitly 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, 2014), other research provides evidence that poor voters prefer public goods promises to vote buying (Kao et al., 2017), or that poor voters are diverse in their preferences for clientelism relative to public goods (Wantchekon,

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

[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).

[3] We assume that all politicians are able to engage in both public goods provision and clientelism, and that 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 and make policy promises in every election. We therefore leave intriguing questions of party or politician ultra-specialization and endogenous voter goods preference for future work.

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 contingent 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 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 and Wantchekon (2009) discuss a similar signaling mechanism, asserting that clientelism indicates a candidate's "…control of public allocations and resources" (Vicente and 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 used in Kramon's work do not test a direct substitution versus 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 Gonzalez Ocantos et al. (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" (Gonzalez Ocantos et al., 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:

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

[4] This paper is part of a larger project that seeks to unpack the empirical relationship between income and clientelism. A full pre-analysis plan, for the entire project, is available at https://doi.org/10.17605/OSF.IO/BEQ9J.

[5] Our research design focuses on contingent vote buying because it represents the baseline case and underpins the logic of many of our theoretical models. In other words, contingent vote buying is the case in which we most clearly should find a perception of substitution. If voters perceive even contingent vote buying and local public goods provision as complementary, then our core theories are fundamentally flawed. But it is important to stress that our findings may not generalize to unconditional clientelism. Similarly, our research does not directly address relational clientelism, which is driven by repeated interactions with particular brokers (Hicken, 2011).

[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 countries (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 Gonzalez Ocantos et al. (2014) is implied but never explicitly stated.

provision as substitutes are more likely to vote against vote-buying candidates than are poor voters.

DATA AND METHODS

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 vote-buying 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% 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-three 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 (EDI) scores Nepal .62, comparable to the regional average for Latin America, and well above sub-Saharan Africa's .45. Nepal held local and national legislative elections on November 26 and December 7, 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 et al., 2018; Pemstein et al., 2022). 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 toward clientelism and investigating how such attitudes vary with income.

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 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 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

[9] This number was similar to alcohol (63%) and food (72%) but far outstripped consumer goods, building materials, or access to loans (all under 25%).

[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 came from the Nepal Census and National Living Standards Survey, which were last completed in 2011 at the VDC level.

TABLE 1 Candidate conjoint experiment text.

Dimension	Variable Value 1	Variable Value 2	Variable Value 3
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	
Party	This candidate represents the party you most often support	This candidate does not represent the party you most often support	
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
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	
Competitive	The election will be very close		

Note: Each row represents a dimension of the conjoint experiment and cells contain the text that enumerators read to participants (translated). Each candidate profile varied randomly across the first four dimensions, but every candidate profile highlighted that the election would be competitive. Thus, for a given profile, enumerators would read text representing one randomly selected cell from each row in this table to describe the candidate. Randomization was handled by software on a tablet carried by the enumerator, to avoid human error.

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.

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 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 promised.

Following Cooperman et al.'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)

[12] See Supporting Information C, p. 13, for analysis comparing the sample to the population on key demographic variables.

[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.

[14] See the PAP.

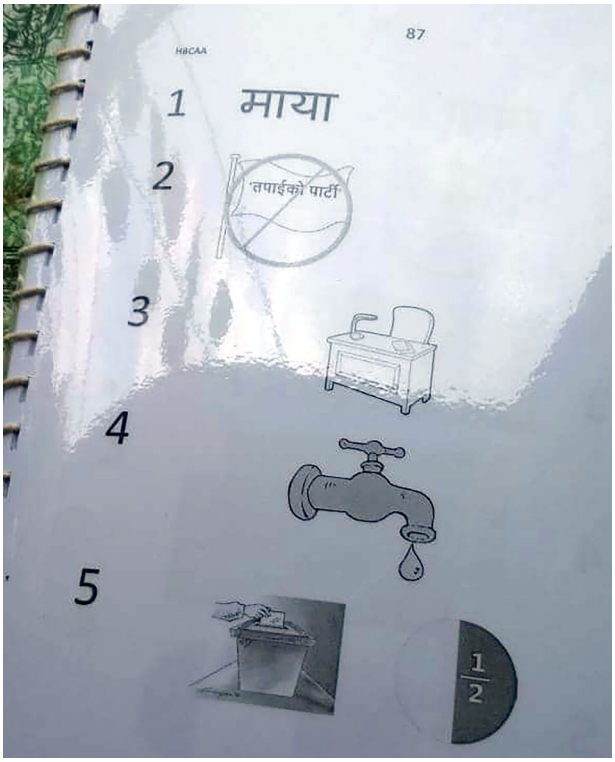

FIGURE 1 Example conjoint profile image. *Note*: Enumerators presented two candidate profiles to participants on every iteration of the conjoint experiment. They read the profiles and showed graphic depictions of each profile. This is an example of one such graphic depiction.

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 variables 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 promised specific public goods. The participants were then asked which candidate was 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 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 mentioned only the type of clientelist benefit they provided. In this vignette, we provided no information about the candidate's public goods promises, which allowed 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

[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).

> 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 4-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 Nepali 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 above, 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.

MEASURING WEALTH

To test Hypotheses 5 and 6, we construct a *Wealth Index* using a battery of proxy indicators collected from all of the 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, Chap. 6).[17]

ANALYSIS METHODS

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 (Hypotheses 1 and 3), the interactions between the vote-buying dimension and vote intention, as measured in response to Vignette Experiment 2 (Hypotheses 2 and 4), and the interaction between the vote-buying dimension and the *Wealth Index* (Hypotheses 5 and 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

$$\boldsymbol{y}_{ijk} \sim N_3\left(\boldsymbol{x}_{ijk}\boldsymbol{B}, \boldsymbol{\Sigma}\right), \tag{1}$$

where $\boldsymbol{y}_{ijk} = [y_{ijk}^{\text{water}} y_{ijk}^{\text{school}} y_{ijk}^{\text{vote}}]$, i indexes respondent, j indexes candidate, k indexes conjoint task, $\boldsymbol{B}$ is an $m \times 3$ matrix of unknown coefficients, and $\boldsymbol{\Sigma}$ 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 otherwise follow the method described by Hainmueller et al. (2014). In particular, we use a block bootstrap procedure to estimate standard errors.

[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.

[17] See Supporting Information B, p. 7, for details on the indicators and the IRT model.

In Equation (1), $\boldsymbol{x}_{ijk}$ 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]:

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

We test Hypotheses 1 and 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 Hypotheses 2 and 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 would 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.

Vignette Experiment 1 analysis

Vignette Experiment 1 provides another test of Hypotheses 1 and 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 the main effects. We expect the coefficients for the vote-buying offers to be negative if Hypothesis 1 (Substitutes) holds.

Vignette Experiment 2 analysis

Vignette Experiment 2 provides a third test of Hypotheses 1 and 3 via a straightforward ordinary least squares (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.

RESULTS

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

[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 Supporting Information. Results are consistent across all model specifications.

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

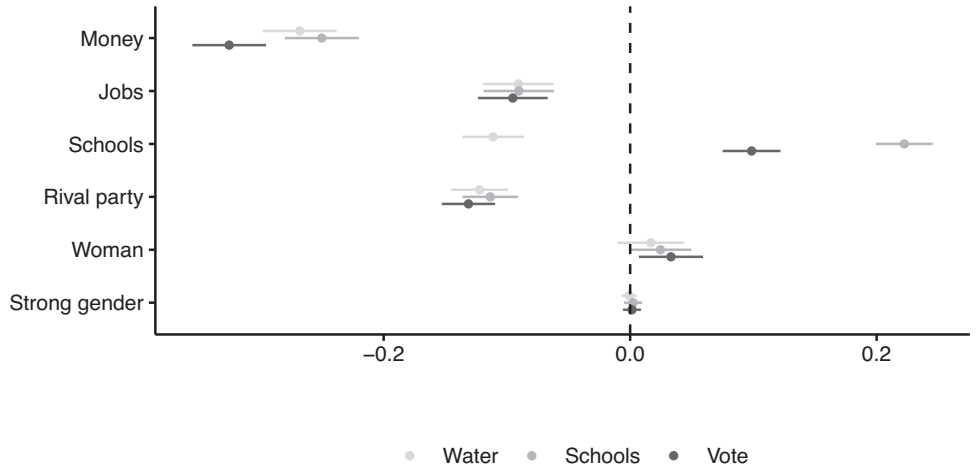

FIGURE 2 Effect of vote-buying offers on anticipated public goods provision and vote choice. *Note*: Plotted values are average marginal component effects (AMCEs) for all three dependent variables (expected water provision, expected school provision, and vote choice) included in the conjoint experiment. Excluded categories are no money offer, no jobs offer, water promise, man, and weak gender (name only).

component effects 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% 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% reductions in expectations of both school and water infrastructure provision—but otherwise mirrors the result for cash payments. Supporting Information Figure S4 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 counterbalance the ordering of this experiment with the survey, practical issues precluded this option, and the lab experiment always ran first. Nonetheless, a subsample of 159 respondents participated in the conjoint experiment without first completing the vote-choice experiment. We find essentially identical results in this sub-sample.

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

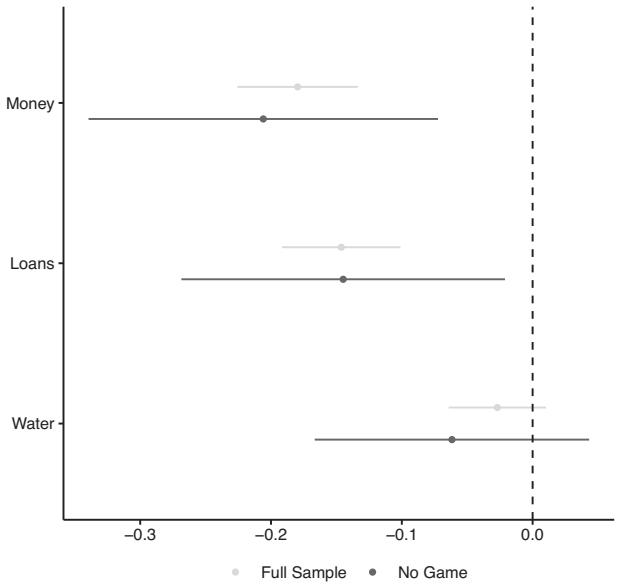

FIGURE 3 Effect of vote-buying offers on anticipated public goods provision. *Note*: Plotted values are AMCEs from Vignette Experiment 1 and depict ACMEs for the full sample and a sub-sample that excludes lab-in-the-field participants.

[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, Supporting Information Figure S4 also shows no interaction between vote buying and public good types.

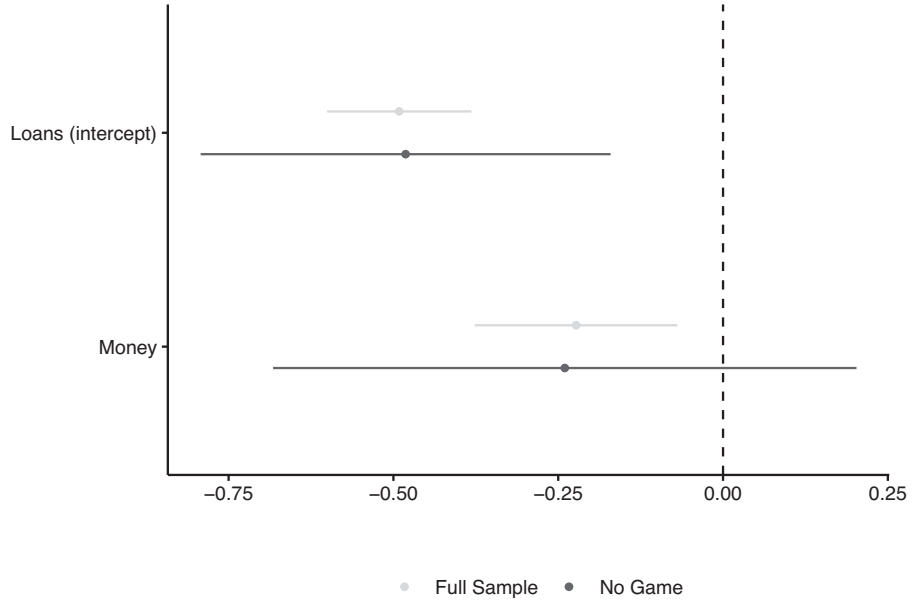

FIGURE 4 Effect of vote-buying offers on anticipated public goods provision. *Note*: Depicts OLS coefficients from Vignette Experiment 2 for the full sample excluding lab-in-the-field participants.

larger than that for small 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.

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% 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%. 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]

We next explore whether this penalty is driven specifically by voters' understanding that there is a tradeoff 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 Hypotheses 2 and 4, this analysis interacts participants' responses to Vignette Experiment 2—using a dummy variable (*Vignette less*) separating the slightly and much *less likely* responses from their *more likely* counterparts as specified in the pre-analysis plan (PAP)—with the vote-buying offer dimensions in the conjoint experiment.[24] First, note that once again, we see that our findings relevant to Hypotheses 1 and 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 Supporting Information Table S1). 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 are weak and should be treated as preliminary evidence. Still,

[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.

[23] After the experimental portions of the survey we directly asked respondents their attitude toward vote buying. Seventy-nine per cent consider it "very immoral" and another 12% "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 .61 (water) and .65 (schools) with expressed vote choice.

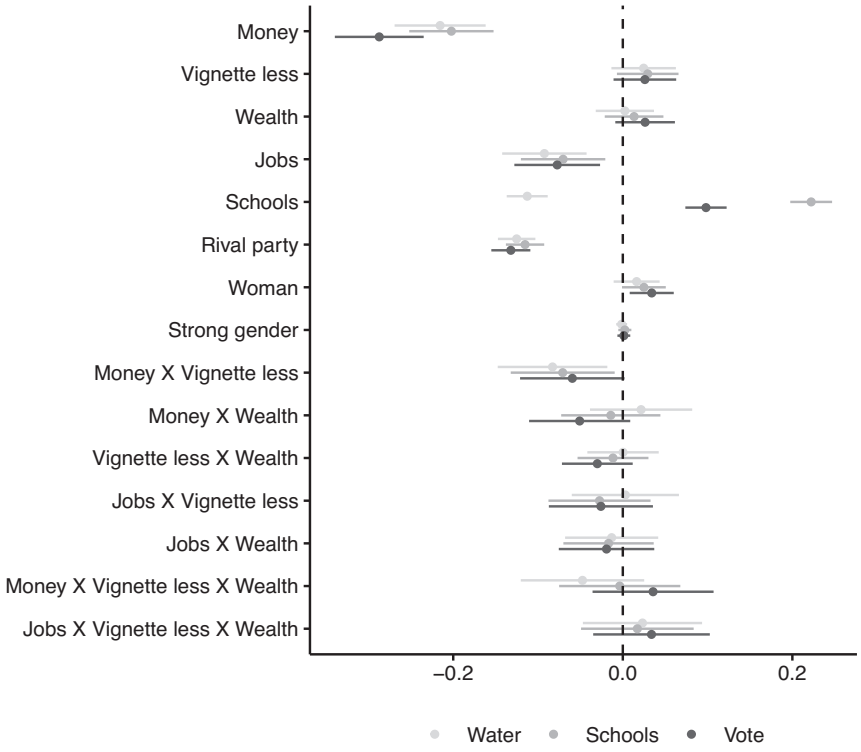

FIGURE 5 Interaction effects of clientelist offers, substitution belief, and wealth on anticipated public goods provision and vote choice. *Note*: Plotted values are AMCEs for all three dependent variables (expected water provision, expected school provision, and vote choice) included in the conjoint experiment. Excluded categories are no money offer, no jobs offer, water promise, man, and weak gender (name only).

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.

Finally, we ask 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 effects represented by the light and medium gray lines in Rows 10, 11, and 13 in Figure 5 are statistically significant.[25] None of these effects 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. This means assessing whether the dark gray lines of Rows 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 below, 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 statistical significance ($p < .1$) for the *Vote* outcome variable (dark gray line of Row 10 in Figure 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

[25] The additional pre-specified test of this hypothesis based on Vignette Experiment 1 appears in Supporting Information Figure S5.

generally), all candidates pledged public goods provision. Still, not all Nepali voters perceived these pledges as credible with respect to fulfillment, and being more skeptical of their fulfillment was strongly associated with vote-buying offers.

While none of our experiments speaks to precisely why voters perceive vote buying and public goods provision as substitutes, our qualitative data shed 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), which is 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. Voters who perceive an anonymous candidate to be engaging in substitution between vote-buying and public goods provision might, in principle, interpret things differently for a real-life candidate whom they know. The robustness of our findings to candidate familiarity is, therefore, an important avenue for future research.

Second, questions about the nature of our instrument and vote buying in the field dovetail nicely with possible mechanism concerns raised by more recent findings surrounding the efficacy of vote buying. Some recent studies observe that vote buying has only, at best, limited marginal effects on aggregate turnout (Gallego et al., 2023; Guardado & Wantchékon, 2018). As discussed in the Data and Methods section, 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; 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. In addition, recent research from India and Mexico demonstrates that what could be perceived as ineffective (or at least inefficient) vote buying is often simply a rampant—but persuasive—campaign strategy (Cantú, 2019; Chauchard, 2018).

Third, 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; Gallego et al., 2023; Guardado & Wantchékon, 2018; Lawson & Greene, 2014). 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 fourth 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 (Auerbach & Thachil, 2018; Hainmueller et al., 2015). Another refutation of these concerns comes from our qualitative work, in which voters and politicians alike were open and frank about clientelist practices and in-office performance, giving no indication of hesitation because of social pressures or (perceived) judgment from the research team. Finally, conjoint experiments seem less vulnerable to social desirability bias than other survey techniques (Horiuchi et al., 2022).

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 (Gonzalez Ocantos et al., 2014; Kramon, 2016). Collectively, this work suggests that voters differ across 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 possible scope conditions to explain inconsistencies in findings across contexts. In 2018, when our data collection occurred, Nepal was a consolidating democracy,[27] a lower-middle income country, and a state where both corruption and vote buying were prevalent but not extreme.[28,29] In addition, Nepal's voters were generally well-educated and politically empowered[30] and voted at relatively high rates.[31] Further, our sampling strategy resulted in a particularly highly educated, yet poor, sample. We might expect consistent findings in similar contexts. More research—focused on voter, rather than elite, decision-making—is needed to fully map the conditions under which voters support vote buying and perceive higher or lower costs and benefits.

[26] The Gonzalez Ocantos et al. (2014) finding regarding education was not robust in Nicaragua, the other country for which they had data to test this hypothesis.
[27] 2018 V-Dem EDI score of .62 on a scale of 0 to 1 (Coppedge et al., 2018).
[28] 2017 V-Dem Political Corruption Index of .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 Gonzalez Ocantos et al. (2014).
[30] 2019 secondary school enrollment rate of 80% and 2018 women's political empowerment index score of .76 on a scale of 0 to 1 (Coppedge et al., 2018).
[31] Turnout was 70% in the 2017 election (Coppedge et al., 2018).

ACKNOWLEDGMENTS

The authors would like to thank Allen Hicken, Kristen Kao, Ellen Lust, Miquel Pellicer, Matt Winters, and seminar participants at the Instituto Tecnológico Autónomo de México, University of Gothenburg, and European Political Science Association annual meeting for feedback on this project. At NDSU, Anuj Shrestha programmed our survey instruments, handled data management, and provided all-around fantastic research assistance, while Privthi Awasthi and Prakash Pokhrel helped with translation. In Nepal, Keshav Bashyal, Dipak Basnet, Gruarav Chetri, Amit Gautam, Himalaya Kharel, and Manoj Suji expertly managed data collection teams, improved translations, and provided invaluable and extensive feedback on study and instrument design. This research was supported in part by a Google Faculty Research Award and by the V-Dem Institute through a sub-contract from the University of Gothenburg on Knut and Alice Wallenberg Foundation Grant 2013.0166 (PI: Staffan Lindberg).

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SUPPORTING INFORMATION

Additional supporting information can be found online in the Supporting Information section at the end of this article.

How to cite this article: Baniya, Jeevan, Stephen A. Meserve, Daniel Pemstein, and Brigitte Seim. 2025. "Vote buying and local public goods provision: Substitutes or complements?" *American Journal of Political Science* 1–14. https://doi.org/10.1111/ajps.12982