

Measuring Vote-Selling: Field Evidence from the Philippines[†]

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Many developing democracies struggle with vote-buying and other forms of clientelism in electoral politics. Clientelism has been associated with reduced accountability and trust, increased corruption, and inefficient public administration (Stokes 2005; Desposato 2007; Keefer 2007; Kitschelt et al. 2010; Hicken and Simmons 2008; Hicken 2011; Stokes et al. 2013). In response, many government agencies and nongovernmental organizations have attempted to combat vote-buying, primarily by convincing voters not to sell their votes. Anti-vote-selling campaigns generally urge voters either to not accept money from candidates in the first place, or to “vote your conscience” despite taking money (Schaffer 2005).¹ However, there is little work on whether such campaigns are effective at reducing vote-selling.

We conducted a field experiment during the 2013 Philippine elections to test the effectiveness of interventions against vote-selling. A central challenge in assessing the impact of any anti-vote-selling effort, including ours, is creating a valid measure of vote-selling. Measuring

vote-selling is clearly not a trivial endeavor, particularly in contexts (including the Philippines) where individual votes are not publicly observed.

In this paper, we describe a proxy measure that we constructed out of self-reports of Philippine voters participating in our experiment, and present empirical patterns of correlation that we argue help validate it as a measure of vote-selling. This measure is the key outcome variable in the experiment, whose results are described in full in Hicken et al. (2014).

We first describe the context, data collection, and the experimental treatments, before turning to the proxy measure of vote-selling and its correlates.

I. Context and Experimental Design²

We conducted our study in Sorsogon City, Philippines during the May 2013 municipal elections. Vote-buying is widespread in this region, as in other areas of the country. Most of the vote-buying occurs in the last few days before the election. Representatives of a candidate offer likely voters money or goods in return for their vote. The Philippines uses secret electronic balloting, so vote-buyers cannot directly verify an individual’s vote, but must instead rely on trust and reciprocity.³ The amount of money offered by candidates varied widely, both across races, and between candidates. Mayor and vice-mayor candidates typically offered between 250 and 500 pesos (US\$5.57–\$11.14), while city council candidates offered 20 to 100 pesos (US\$0.45–\$2.23). Our data collection occurred in two waves: a baseline survey in the month leading up to the election, as well as a post-election survey.

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¹A prominent example was the Archbishop of Manila exhorting voters to “take the bait but not the hook” during Marcos-era presidential elections.

²Additional details are available in Hicken et al. (2014).

³Finan and Schechter (2012) demonstrate that politicians disproportionately target reciprocal voters for vote-buying.

A. Baseline Survey

During the baseline survey, enumerators visited randomly selected households and conducted 883 surveys. Study participants were asked to rate each candidate for mayor, vice-mayor, and city council on a seven-point scale (ranging from “extremely unfavorable” to “extremely favorable”). At the end of the survey, all participants were shown a three-minute voter education video produced by a Philippine actress and activist that urged voters not to sell their vote.

B. The Experiment

Participants were randomly assigned to one of three treatment conditions. In the control group, the baseline interaction ended after the video clip. Two treatment interventions (Promise 1 and Promise 2) invited participants to promise not to sell their vote. Participants randomly assigned to the Promise 1 treatment group were asked to promise not to accept money from any candidates, while participants assigned to the Promise 2 treatment were asked to promise to “vote your conscience,” even if they took money from a candidate.

We consider outcomes in three types of municipal elections: mayor, vice-mayor, and city council. We are interested in the impact of the promise treatments on vote-selling by study participants.

C. Post-Election Survey

In the month following the election, participants were surveyed a second time. Participants reported whether they had voted, and who they had voted for in each race. Voters could vote for one candidate for mayor, one candidate for vice-mayor, and up to four candidates for city council.⁴ We experienced very little attrition between waves: 95.9 percent of baseline participants completed the post-election survey, with no significant difference in attrition between treatments.

⁴Sorsogon City is divided into three separate districts for the City Council election. Each district had between 11 and 13 candidates.

II. Proxy Measure of Vote-Selling

We cannot directly observe vote-selling, and therefore must use a proxy measure. We construct a measure of “vote-switching” as a proxy for vote-selling. In the mayoral and vice-mayoral races, we say that a voter switched if they report voting for a candidate who was not their highest rated candidate (including ties) in the baseline survey. For the city council race, we say that a voter switched if they voted for at least one candidate that was not in their top four highest rated candidates (including ties) in the baseline survey. We identify if a voter switched in any of the races. Overall, we observed 56 percent of voters switching in at least one race, with 12 percent switching for mayor, 22 percent switching for vice-mayor, and 44 percent switching for city council.

There are many innocuous and legitimate reasons why a voter may have switched their vote. For example, they could have acquired more information about candidates, or decided to evaluate that information differently. However, vote-switching for these reasons should be equally likely in each treatment condition, and, due to randomization, should be orthogonal to the treatments. If a promise treatment leads to a change in vote-switching, it should reflect a change in actual vote-selling.

A limitation of our data is that we must rely on self-reported voting. One concern is that a reduction in vote-switching may be driven by a social desirability bias in our survey responses, rather than a real change in voting behavior. In Hicken et al. (2014) we explicitly model how a social desirability bias would affect survey responses in this setting, and demonstrate that the pattern of our treatment results are not consistent with such a bias. In short, we would expect that the bias would lead to the largest reduction in reported switching in the more important mayoral and vice-mayoral elections, while the bias would be the smallest in the less important city council elections. By contrast, we find that our interventions reduce switching the most in the city council race.

A. Relationship between Voter Preferences and Vote-Switching

If the vote-switching measure accurately reflects real voting patterns, we would expect

that vote-switching would be related with voters' candidate ratings. Specifically, we would expect that a voter would be most likely to switch if the difference in favorability ratings between the voter's initially preferred candidate and the highest-rated other candidate is small. Conversely, we would expect voters to be less likely to switch when the most attractive alternative candidate is rated much worse than the preferred candidate.

For each race we calculate a "favorability gap": the difference in favorability rating between the voter's preferred candidate and the highest rated non-preferred candidate. For the city council race we use the lowest rating among the voter's top four preferred candidates and subtract the highest rating among the voter's other non-preferred candidates. When pooling across races, we take the minimum favorability gap, since this is the race where we would expect the switch to occur.

Column 1 of Table 1 reports the results of regressing the indicator for switching in any race on the favorability gap for all races.⁵ We find that switching probability is significantly decreasing in the favorability gap, with a one-category favorability gap (out of seven) leading to a 13 percentage point decrease in switching probability. We find similar results for each race separately. A one category difference is associated with a 7 percent reduction in switching in the mayoral race ($\beta = -0.070$, *s.e.* = 0.009), a 4 percent reduction for the vice-mayoral race ($\beta = -0.044$, *s.e.* = 0.013), and a 6 percent reduction in the city council race ($\beta = -0.064$, *s.e.* = 0.021). Hence, our vote-switching measure is related in an intuitive way to a voter's underlying candidate preferences.

B. Relationship between Candidates' Vote-Buying and Vote-Switching

We now examine how differences between candidates in the amount of money offered is related to vote-switching. We asked our survey enumerators to rate the vote-buying activities of each candidate on a five-point scale (where

TABLE 1—FAVORABILITY AND VOTE-BUYING GAPS

	Switching in any race	
Favorability gap	-0.133*** (0.0179)	
Vote-buying gap		0.232*** (0.0261)
Demographic controls	Yes	Yes
Constant	0.785*** (0.131)	0.319*** (0.137)
Observations	748	748

Notes: OLS with robust standard errors. The dependent variable is an indicator that equals one if the voter voted for a non-favorite candidate in at least one race. Column 1: For each race we calculate the difference in favorability rating between the voter's preferred candidate (lowest rated among the top four for the city council race) and the highest rated non-preferred candidate. Favorability gap is the minimum difference across races. Column 2: For each race we calculate the difference in vote-buying rating between the voter's preferred candidate (lowest rated among the top four for the city council race) and the highest vote-buying rating among non-preferred candidate. Vote-buying gap is the maximum difference across races. Demographic controls include age, gender, religion, number of household voters, employment, education, migrant status, and marital status.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

1 = "not at all" and 5 = "very much").⁶ As expected, mayoral candidates had the highest rating for vote-buying (avg. = 4.97), followed by vice-mayor (3.49) and city council (2.75). For the city council race, there is also a large variation in the ratings between candidates (*st. dev.* = 0.78).

For each voter, we can compare the average rating for "dropped" candidates (i.e., candidates rated in the top four, but not voted for) and "added" candidates (i.e., candidates not in the top four, but voted for). Added candidates have a significantly higher rating than dropped candidates (3.21 versus 2.76; sign-rank test $p < 0.01$ ⁷)—indicating that voters are disproportionately switching toward candidates who are doing more vote-buying. This is what one

⁶Our enumerators were able to directly observe a number of examples of candidate's cash offerings (see the Appendix of Hicken et al. 2014 for examples), as well as learn of the amounts offered by candidates from friends and family.

⁷The within-subject comparison is significant for each district individually, as well as for each treatment separately.

⁵Regressions also include demographic controls. Regressions for individual races are reported in the online Appendix. Full regression results are available from the authors upon request.

would expect if switching behavior is being predominantly influenced by vote-buying.

For each voter, we can also calculate the “vote-buying gap”—the difference in the vote-buying rating of the voter’s preferred candidate⁸ and the highest rating among all other candidates. The larger the vote-buying gap, the greater the imbalance between the amount of money being offered by the voter’s preferred candidate and an alternative candidate. We would expect that switching would be more likely to occur when this difference is large. We also construct a measure of the vote-buying gap pooled across races, in which we take the maximum vote-buying gap, since this is what should determine whether any switching should occur in any of the races.

In column 2 of Table 1 we regress vote-switching on the vote-buying gap. We find that switching increases with the vote-buying gap: a one-point larger vote-buying gap is associated with an increase in the likelihood of switching in any race of 23 percent. The corresponding increase is 17 percent in the vice-mayor race ($\beta = 0.172$, s.e. = 0.034), and 24 percent in the city council race ($\beta = 0.238$, s.e. = 0.032).⁹

III. Discussion

In this paper we examine a proxy measure of vote-selling. We compare respondents’ pre-election ratings of candidates in three municipal electoral races with the candidates they reported actually voting for post-election. We construct an indicator of “vote-switching,” capturing whether a respondent voted for a candidate who was not his or her initially most-preferred candidate (or in the set of most-preferred candidates, for an election where one votes for multiple candidates).

We show empirical patterns in “vote-switching” that validate its use as the key dependent variable of interest in our experimental study of the impact of anti-vote-selling interventions (Hicken et al. 2014). First, we show that voters who have weaker preference differences between candidates are more likely to vote-switch. Because this pattern is consistent

with many reasons behind vote-switching, of which vote-selling is only one, this pattern simply indicates that vote-switching patterns are plausibly related with voter preference intensity.

Second, we show that voters are more likely to vote-switch when their initially-preferred candidate does less vote-buying compared to other competing candidates. Hence, differences in vote-switching are much more likely to be driven by vote-selling than by other potential causes of vote-switching.

In Hicken et al. (2014), we take “vote-switching” as our main dependent variable, and estimate the impact of two anti-vote-selling treatments. There, we argue that the variation in “vote-switching” induced by the treatments should be interpreted as variation in vote-selling. That argument is bolstered by the patterns we find in this paper, in particular the relationships shown between vote-switching and vote-buying by candidates initially not preferred by respondents.

In that paper, we find that inviting voters to promise not to accept money does substantially reduce vote-switching, with the reduction we observe coming entirely from the city council election where candidate payments are smallest. Inviting promises to vote one’s conscience do not reduce vote-switching overall. In fact, this treatment increases vote-selling in the mayoral and vice-mayoral races, where larger amounts of money are being offered. We propose a behavioral model that can explain the differences in treatment effects between promises and across races. Key to the model is that accepting money from a candidate creates a temptation to actually vote for the vote-buyer on election day, and that voters may not fully anticipate the magnitude of this temptation. A promise to “take the money but vote one’s conscience” actually increases the number of voters accepting money, since they believe that the promise will allow them to accept the money without changing their votes. However, if the temptation is greater than anticipated, the promise will actually increase vote-switching. The model generates predicted differences in results between promises and between races that are verified in our data.

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⁸For the city council race we use the minimum rating among favorite candidates.

⁹We cannot look at the mayor’s race individually, since the two candidates had essentially the same rating.

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