Essays in Political Economy and Governance: Lessons from the Philippines

by

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To the Filipino Statesman

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ABSTRACT

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This dissertation addresses a central question in modern political economy: How do we improve governance in low-income democracies? In the first essay, I employ formal modeling and use natural experiment to examine how politician behavior impact governance. In the last two essays, I use randomized field experiments to evaluate policy interventions that strengthen the ability of voters to hold politicians accountable and attract a more qualified pool of candidates to public office. This collection of works, therefore, advances the frontier of modern political economy, first, by understanding how the behavior of political agents impact governance, and then, by evaluating novel policies that can improve their quality and behavior and, ultimately, governance.

CHAPTER I

Introduction

This dissertation addresses a central question in modern political economy: How do we improve governance in low-income democracies? It does so, first, by examining how politician behavior affects governance and, second, by evaluating policies that improve accountability and political selection.

Essay 1: Partisan Motives in Pork Distribution. This paper uses two sources of regression discontinuities (RD) to disclose legislators' partisan motives when distributing pork. First, using RD in close mayoral races, I identify the effect of the political alignment between legislators and mayors on pork distribution in the Philippines, and I find that aligned mayors receive twice as much pork funds obtained by their unaligned counterparts. Next, using RD in close House races, I show that pork distribution favors aligned mayors in safe seats the most, and unaligned mayors in safe seats the least. This is because narrowly winning legislators ultimately care about winning their own races, and they do so by favoring powerful mayoral allies and penalizing their opponents' powerful allies.

Essay 2: Temptation in Vote-Selling: Evidence from a Field Experiment in the Philippines. (Co-authored with Allen Hicken, Stephen Leider & Dean Yang). We report the results of a field experiment on the effects of two common anti-vote-selling strategies—having voters promise not to take money for their vote, and having

them promise to take money, but vote their conscience. The invitation to promise not to vote-sell is taken up by a majority of respondents, reduces vote-selling, and has a larger effect in electoral races with smaller vote-buying payments. This treatment reduces vote-selling in the smallest-stakes election by 10.9 percentage points. Inviting voters to accept vote-buying payments, but to nonetheless "vote your conscience", is significantly less effective. The results are consistent with voters being partially (but not fully) sophisticated about their vote-selling temptation. We demonstrate this with a behavioral model of transactional electoral politics. We model selling one's vote as a temptation good: it creates positive utility for the future self at the moment of voting, but not for past selves who anticipate the vote-sale. We also allow keeping or breaking promises regarding vote-selling to affect utility. Voters who are at least partially sophisticated about their vote-selling temptation can thus use promises not to vote-sell as a commitment device.

Essay 3: Nudging Good Politicians: Evidence from a Field Experiment in the Philippines. This paper evaluates a policy that seeks to attract a more qualified pool of individuals to public office. We implement a randomized field experiment of a leadership training workshop with incentives among youth interested in running for an elective post in the Philippines. Subjects took baseline exams designed to measure several dimensions of candidate quality: public service motivation (PSM), intellectual ability, personality, aspiration, and integrity. We then assigned subjects into three groups: no workshop (C), workshop with unconditional incentives (T1), and workshop with conditional incentives (T2). A year later, we measured subsequent political attitudes and behavior. We find evidence for political selection, in which subjects with above (below) median levels of PSM in both T1 and T2 are more (less) interested in standing in election, more (less) likely to engage in village youth programs, and more (less) likely to be nominated and designated as village youth leaders than their counterparts in C. But only in T2 do we find similar evidence

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for political selection in terms of intellectual ability, aspiration and integrity. Overall, these results imply that leadership training workshops with conditional incentives screen-out less qualified individuals and nudge the good ones to serve in public office.

CHAPTER II

Partisan Motives in Pork Distribution

2.1 Introduction

It is a central theme in the distributive politics literature that politicians favor co-partisans, and there is a lot of evidence that they often do. Presidents distribute more to legislators and governors of the same party (Berry, Burden and Howell, 2010; Larcinese, Rizzo and Testa, 2006). Central governments bias intergovernmental transfers in favor of politically aligned local governments (Arulampalam et al., 2009; Brollo and Nannicini, 2012; Sole-Olle and Sorribas-Navarro, 2008).[1] But while empirical evidence of partisan distribution is well established, it is less clear why there is significant variation in the amount of funds distributed among political allies and even among political opponents.

Fernanda Brollo and Tommaso Nannicini's (2012) insightful article, "Tying Your Enemy's Hands in Close Races: The Politics of Federal Transfers in Brazil," greatly advances scholarly research on partisan distribution by highlighting the role of competition at the local level in explaining the extent to which politicians favor political allies and penalize opponents. In their model, federal transfers generate political credit spillovers at the local level. Hence the President and his party bias transfers

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[1]Other studies that have estimated the impact of partisanships on the allocation of public funds include, in the context of the United States (Grossman, 1994; Levitt and Snyder, 1995); in Australia (Worthington and Dollery, 1998); and in Portugal (Veiga and Pinho, 2007).

in favor of aligned incumbent mayors. Among aligned mayors, those in marginal seats receive more, because it is especially to them that larger municipal revenues can make a difference in subsequent elections. By the same token, the President penalizes unaligned mayors who won by a narrow margin to tie their hands in the next elections.

Underpinning this model is the assumption that, "delivered benefits simultaneously affect two levels of electoral competition, both the reelection campaigns of local mayors and—eventually—the president's reelection campaign (Brollo and Nannicini, 2012, 742)." But following this assumption, it is not straightforward that politicians in charge of distribution would favor weak allies and penalize their weak opponents. When political authorities stand for reelection, they may have good reason to bias distribution in favor of allies in safe seats and correspondingly penalize unaligned local politicians in safe seats. They would want to distribute in favor of politically secure allies for a number of reasons. Common to them is that the greater the victory margins of local allies, the more likely they are to be effective in mobilizing political support in exchange for distributive benefits. For the same reason, they would want to discriminate against unaligned local politicians in safe seats, because they are likely to be their enemy's powerful political intermediaries. In light of these considerations, the distributing political authority faces a tradeoff: help his allies in hotly contested seats and penalize their opponents, or distribute in favor of powerful allies and penalize his enemy's powerful political intermediaries. The focus of this paper is to investigate how this tradeoff shapes pork distribution using the case of legislator-mayor partisanship in the Philippines.

In the Philippines, incumbent national legislators and mayors stand for reelection at the same time every three years. Mayors are elected at the municipal and city levels, while single-member district legislators are elected by plurality vote at the district level. Given that House and mayoral elections are contemporaneous,

legislators distribute in favor of aligned mayors not just to help the latter win reelections. Legislators also take into account the value of leveraging pork to establish and maintain—*or* curb and stem—powerful alliances at the local level and, ultimately, secure their incumbency in the House of Representatives. In other words, the legislator has the choice between distributing pork to tie enemy's hands in close mayoral races or to tie enemy's hands in close House races.

This article identifies a 'tying the enemy's hands' effect in two steps. First, following Brollo and Nannicini (2012), I employ a regression discontinuity (RD) design in close elections to identify the causal effect of partisan alignment on pork distribution. In particular, I limit the analysis to cases where the only reason why the legislator-mayor alignment became politically salient is because one of them narrowly won the race. And because there are multiple districts as well as multiple municipalities, I not only consider close mayoral elections; I also exploit close House elections as a source of identification. I perform statistical tests on the density and covariate balance in both races to show that, indeed, partisan alignments arising from close elections mimic random assignment.

Second, focusing on close House elections, I test how the partisan alignment effect varies in the degree of political competition facing mayors. This is achieved by interacting the partisan alignment variable with the vote-share margin of victory of the mayor. Because the analysis never moves away from (House) elections decided by a narrow margin, measuring heterogeneous treatment effects in the safeness of mayors' seats does not undermine the randomness of the assignment to partisan alignment.

However, even if partisan alignments due to close House races are plausibly random, the safeness of the mayor's seat is not. Its potential correlation with other observable and unobservable factors implies that any observed effect that varies in mayor's victory margin might simply be driven by other dynamics of distribution. For instance, it is possible that the mayor's victory margin is a proxy for the geo-

graphical distribution of the legislator's swing voters and core supporters. In this case, the dynamics of pork distribution might really be about favoring aligned *and* 'swing' localities – a story with theoretical basis and supported by empirics elsewhere in the literature (Arulampalam et al., 2009). Alternatively, it could well be about favoring aligned *and* core constituencies. More generally, it is plausible that mayors in safe seats tend to come from localities that systematically differ from where mayors in marginal seats hold office. I perform statistical tests and provide evidence to rule out these alternative explanations.

This article's analysis is based on a novel dataset of a unique type of pork spending: the constituency development funds (CDF) in the Philippines. Other countries that have this type of fund include India, Kenya, Pakistan and Sierra Leone. CDF are lump sum discretionary funds over which legislators exercise the "power of the purse." The dataset is highly disaggregated and details CDF releases across all of 229 single-member legislative districts encompassing all of 1,631 municipalities and cities in the Philippines for years 2001-2010.

Empirical results from the RD analyses reveal that, in close mayoral elections, municipalities where the mayor is aligned with the district legislator receive larger CDF funds by about 90%. The effect is more pronounced in close House elections. Municipalities where the mayor is aligned with a narrowly winning legislator receive twice the CDF funds received by their unaligned counterparts. These effects are economically meaningful: the lower bound of the difference corresponds to about 70% of a municipality's average annual spending on education, culture and sports or to about 40% of average annual spending on social services and welfare. These effects are also politically consequential: if all money were pocketed by either or both parties in the alliance, and given the prevalence of vote-buying in the country (Hicken et al., 2015), this differential pork allocations could tip election outcomes in favor of incumbents able to take advantage of such alliances.

Further, while I find evidence consistent with Brollo and Nannicini (2012) findings that legislators favor narrowly winning aligned mayors and penalize their opponents, I also find that mayoral allies in safe seats, in particular, those in the 4th quartile of vote-share margin of victory account for the lion's share of CDF funds, receiving two-thirds more than the funds received by those in the 1st quartile. Likewise, among unaligned mayors, those in safe seats receive the least amount of CDF funds. These results lend support to a story of 'tying the enemy's hands' in close House races, and highlight a different dynamic of partisan distribution than what has been previously documented in the literature. When distribution generates political credit spillovers and legislators' reelection prospects are at stake, they end up favoring partisan allies in safe seats and penalizing unaligned politicians in safe seats even as their allies in marginal seats could use some of the electoral benefits of distribution.

These findings also have implications extending beyond research on political competition and partisan distribution. In the political economy literature, one of the central challenges is to understand legislator incentive to "bring home the pork." Scholars in the past have mostly focused on institutional constraints such as electoral rules in search of explanations.[2] Recently, Keefer and Khemani (2009) observed that pork barrel activities vary significantly even when institutions are held constant. Examining the allocation of CDF funds across legislative districts in India, they find that legislators use pork more freely to build a "personal vote" in constituencies with weak party presence. In a similar vein, this article finds that legislators use pork more freely to favor fellow partisans in districts where they face intense political competition. Moreover, this article shows that pork barrel activities vary significantly even *within* legislative constituencies, and that legislator incentive to pass the pork to powerful political intermediaries plays an important role in explaining this variation.

The remainder of the paper is organized as follows. In the next section, I use Brollo

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[2]See for example, (Ames, 1995; Baron and Ferejohn, 1987; Carey and Shugart, 1995; Shepsle and Weingast, 1981).

and Nannicini (2012) model of partisan distribution as a takeoff point to highlight the conditions under which legislators especially favor mayoral allies in marginal seats and discriminate against narrowly winning mayoral opponents, and the conditions under which legislators end up favoring mayoral allies in safe seats and penalizing unaligned mayors in safe seats. I also outline in this section the regression discontinuity (RD) design and empirical specifications. The subsequent section describes Philippine institutions and data. I then present the results, discuss alternative explanations, and evaluate the validity of the RD's identifying assumption. The final section concludes.

2.2 Theoretical Preliminaries

This section takes Brollo and Nannicini's (henceforth B&N) (2012) model of partisan distribution as a point of departure. To enhance comparability, the specific objective is to make one basic change to their model – considering the role of mayors as political intermediaries – while closely following their assumptions. This adaptation suggests that mayors can mobilize electoral support for legislators, but that some mayors are more effective vote mobilizers than others. Whereas their model predicts that legislators will especially favor mayoral allies in marginal seats and penalize narrowly winning mayoral opponents, here, the extended version of the model predicts that under certain conditions, legislators may end up favoring mayoral allies in safe seats and penalizing mayoral opponents in safe seats.

2.2.1 Mayors as Political Intermediaries

Just like in many developing democracies, no long-standing and stable national party organizations exist in the Philippines that can serve reliably as the principal vehicles for advancing political careers (De Dios, 2007). Instead, the structure of the political system in the country beginning from the U.S. colonial government is such that local political intermediaries play an important role in securing electoral victory

for politicians running for higher office (Abinales and Amoroso, 2005; Hutchcroft and Rocamora, 2003; Lande, 1965).

At the base of the hierarchy of political intermediaries are the *barangay kapitans* and *kagawads*–local leaders with no official party affiliations–who are elected into office at the barangay level, the smallest administrative unit in the Philippines. Deeply embedded in local social networks, they rely on repeated relationships, individual reputations, and community and social norms to facilitate the exchange of distributive goods for political support. Higher-level politicians such as the municipal and city mayors, in turn, rely on kapitans and kagawads as brokers that can mobilize electoral support at the grassroots level. District legislators, too, sometimes establish direct ties with barangay leaders, but there being a few hundred barangays that comprise typically less than 10 municipalities and cities in a district, legislators instead tap mayors and their preexisting cadre of local leaders. Mayors find it in their best interest to maintain partisan ties with legislators, because in exchange for the "command votes" that they can mobilize, they are able to reap the benefits of the *quid pro quo* in the determination of the legislator's district spending priorities (De Dios, 2007).

Following this line of reasoning, legislators have good reason to be partisan and to distribute in favor of mayoral allies who are effective vote mobilizers. Among mayoral allies, those in safe seats are likely to be seasoned politicians, better rent-seekers, and adept at electoral strategies whether legal or illegal. They are also likely to have established networks of brokers at the grassroots level. Moreover, they have the more credible exit option – they can withhold electoral support to the legislator should they get neglected in pork allocations. All of these reasons are ultimately reflected by their ability to secure safe seats for themselves. At the end of the day, the legislator who is faced with the opportunity cost of allocating pork is led to favor mayoral allies in safe seats. By the same token, the legislator is led to bias pork allocations against unaligned mayors in safe seats who are likely to be the more powerful political

intermediaries of his opponents in the House race.

2.2.2   An Augmented Model of Partisan Distribution

To account for the role that mayors play as political intermediaries to legislators, I build upon B&N's model of partisan distribution and extend their theoretical framework. I consider the political maximization problem of a legislator who must choose the amount of pork funds τ_i to allocate to each municipality $i = 1, ..., N$ within his district. As in B&N's model, I assume that the legislator cares about the general goodwill of the citizens in every municipality i towards himself or his party: $U(\tau_i)$, with $U' > 0$, $U'' < 0$. Moreover, he desires to increase the likelihood that municipality i is run by a politically aligned mayor because they are valuable assets in terms of policy coordination or rent-seeking. Thus, if the future margin of victory of the mayoral candidate aligned with the legislator is positive, the legislator increases his political capital by a positive (fixed) amount R. The relative weight of these two political benefits depends on whether voters give political credit for the pork benefits to the legislator or to the mayor. Let $\theta \in [0, 1]$ be the political credit spillovers accruing to the incumbent mayor: if $\theta = 0$ the legislator can claim full credit for τ_i; if $\theta = 1$ all the credit goes to the mayor.

In this article, I incorporate the fact that the legislator faces political competition and desires to increase his own reelection prospects at the district level. A legislator is unable to enjoy the benefits of increasing his political capital, R, as well as the pecuniary and non-pecuniary rewards of maintaining incumbency – a positive (fixed) amount W – if he is unable to win his reelection bid. Given these considerations the legislator's objective function can be expressed as:

$$
\max_{\tau_i} \quad (1 - \theta) \sum_i U(\tau_i) +
$$
$$
\left(\sum_i R * \Pr[MMV_i > 0] + W \right) \Pr[CMV_d > 0] - \sum_i C(\tau_i) \tag{2.1}
$$

where $CMV_d \in [-1, 1]$ is the future margin of victory of the legislator, $MMV_i \in [-1, 1]$ is the future margin of victory of the mayor aligned with the legislator, and $C(\tau_i)$ captures the opportunity cost of allocating τ_i, with $C' > 0, C'' < 0$.

Note that when $W = 0$ and $\Pr[CMV_d > 0] = 1$, that is, when the legislator is not standing for election and cares only about the outcomes of the mayoral races, then his objective function reverts to B&N's specification. It makes sense that this is the case in B&N's specification, because the timing of elections in Brazil is such that presidential elections take place every four years, whereas mayoral elections take place every two years, and their analysis focuses on intergovernmental transfers in proximity to municipal elections when the president/ruling party is not standing for reelection.

As in B&N, I also assume the cost function for i to be independent of what happens in $j \neq i$. This is in line with the budgetary rules and procedures in pork distribution that I describe in the next section, because legislators often choose to allocate away from municipal governments to line agencies. It is therefore plausible that the legislator can meet the requests of every municipality independently of each other, but that meeting each individual request comes at the opportunity cost of time and bureaucratic red-tape.

Following B&N, I also assume that pork allocations increase the electoral prospects of the mayor. If the incumbent mayor is aligned with the legislator ($P_i = 1$), τ_i will increase the former's future margin of victory, and vice versa if the incumbent mayor is unaligned with the legislator ($P_i = 0$):

$$MMV_i = \rho MMV_i^0 + \epsilon_i + \theta(2P_i - 1)f(\tau_i) \tag{2.2}$$

where MMV_i^0 is the margin of victory of the aligned mayoral candidate in the previous election. The parameter ρ captures persistence in electoral outcomes at the municipal level and $\epsilon \sim \mathcal{N}(0, \sigma_{MMV}^2)$ is a normally distributed random shock. The function $f(\tau_i)$

translates pork allocations into votes for the mayoral incumbent, with $f' > 0, f'' < 0$. With these assumptions, the winning probability of the mayor can be expressed as:

$$\Pr[MMV_i > 0] = \Phi\left[\frac{\rho MMV_i^0 + \theta(2P_i - 1)f(\tau_i)}{\sigma_{MMV}}\right] \tag{2.3}$$

where $\Phi[\bullet]_{MMV}$ is the cumulative distribution function of the standard normal.

In a similar vein, pork funds allocated to municipality i could increase the legislator's reelection probability in two ways. First, voters directly attribute some political credit to him, which helps build a "personal vote": $\sum_i(1 - \theta)h(\tau_i)$, with $h' > 0, h'' < 0$.[3] Second, pork allocations help mayors mobilize votes for their allies in the House. Thus the fraction of pork benefits attributed by voters to mayors also get translated into votes to the legislator, weighted by the mayor's baseline vote-share margin of victory, $MMV_i^0 \in [-1, 1]$, which accounts for her effectiveness as a political intermediary: $\theta MMV_i^0 g(\tau_i)$, with $g' > 0, g'' < 0$. Thus, pork allocations increase the reelection probability of the legislator as follows:

$$CMV_d = \psi CMV_d^0 + \mu_d + \sum_i \theta MMV_i^0 g(\tau_i) + \sum_i (1 - \theta)h(\tau_i) \tag{2.4}$$

and the winning probability of the legislator can be expressed as:

$$\Pr[CMV_d > 0] = \Phi\left[\frac{\psi CMV_d^0 + \sum_i \theta MMV_i^0 g(\tau_i) + \sum_i(1 - \theta)h(\tau_i)}{\sigma_{CMV}}\right] \tag{2.5}$$

where CMV_d^0 is the margin of victory of the legislator in the previous election, the parameter ψ captures persistence in electoral outcomes at the district level, $\mu_d \sim \mathcal{N}(0, \sigma_{CMV}^2)$ is a normally distributed random shock, and $\Phi[\bullet]_{CMV}$ is the cumulative distribution function of the standard normal.

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[3] "[The] personal vote reflects a principal feature of the single-member district plurality electoral system: the distinction between the interests and fortunes of an individual representative and those of any collectivity, especially party, to which he or she may belong (Cain, Ferejohn and Fiorina, 1984, 111)."

2.2.2.1 Equilibrium Pork Allocations

The first-order condition of the legislator's maximization is:

$$(1 - \theta)U'(\tau_i) + \phi_{MMV}[\bullet] \left[\frac{\theta R(2P_i - 1)f'(\tau_i)}{\sigma_{MMV}} \right] \Pr[CMV_d > 0] +$$

$$\phi_{CMV}[\bullet] \left[\sum_i R * \Pr[MMV_i > 0] + W \right] \tag{2.6}$$

$$\left[\frac{\theta MMV_i^0 g'(\tau_i) + (1 - \theta)h'(\tau_i)}{\sigma_{CMV}} \right] = C'(\tau_i)$$

where $\phi[\bullet]$ is the density function of the standard normal. The above condition generates empirically testable predictions on the direction and size of pork allocations across municipalities within the legislative district.

If there are no political credit spillovers ($\theta = 0$), the legislator simply weighs the combined marginal benefit of citizens' goodwill and of building a "personal vote" against the marginal cost of τ_i. This yields an amount of pork allocation τ', such that $U'(\tau_i) + \phi_{CMV}[\bullet][\sum_i R * \Pr[MMV_i > 0] + W][\frac{h'(\tau_i)}{\sigma_{CMV}}] = C'(\tau_i)$.

But if there are political credit spillovers to mayors ($\theta > 0$), then the legislator's choice of τ_i also takes account of the additional marginal benefit or cost of influencing electoral outcomes at the municipality level and mobilizing mayors as political intermediaries. Note that, by definition, $P_i = 1$ if $(MMV_i^0 > 0)$ *or* $(CMV_d^0 > 0)$, and $P_i = 0$ if $(MMV_i^0 < 0)$ *or* $(CMV_d^0 < 0)$. Hence, from Eq. 6 it is evident that the second term in the left-hand side are either positive if $P_i = 1$, or negative if $P_i = 0$. Therefore, at the zero thresholds $(MMV_i^0 = 0)$ and $(CMV_d^0 = 0)$, there is a sharp jump in the maximization problem of the legislator, as the marginal cost of allocating pork funds to an unaligned incumbent mayor suddenly becomes the marginal benefit of allocating pork funds to an aligned mayor. Here, the discontinuity at the zero threshold is driven not only by the desire of the legislator to increase the probability of reelection of his mayoral allies, but also by the added benefit of incentivizing

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aligned mayors as vote mobilizers and penalizing unaligned mayors who are likely to be his opponent's political intermediaries. Combined, these considerations imply a strong and positive impact of the legislator-mayor alignment on distribution in close races:

Proposition 1a. *The local Average Treatment Effect (ATE) of legislator-mayor partisan alignment on pork distribution is positive at* $MMV_i^0 = 0$. *That is:* $lim_{MMV_i^0 \downarrow 0\%} \tau_i - lim_{MMV_i^0 \uparrow 0\%} \tau_i > 0.$[4]

Proposition 1b. *The local Average Treatment Effect (ATE) of legislator-mayor partisan alignment on pork distribution is positive at* $CMV_d^0 = 0$. *That is:* $lim_{CMV_d^0 \downarrow 0\%} \tau_i - lim_{CMV_d^0 \uparrow 0\%} \tau_i > 0.$

However, because the legislator's partisan motives entail two countervailing political considerations, it is no longer straightforward how pork distribution would depend on the baseline vote-share margin of victory of the aligned incumbent mayor:

Proposition 2. *The relationship between the amount of politically motivated pork allocations and* MMV_i^0 *on either side of the threshold* $MMV_i^0 = 0$ *is ambiguous.*[5]

The intuition follows from the fact that the legislator faces a tradeoff. On the one hand, he can allocate pork funds to make a difference in electoral outcomes at the municipal level by penalizing narrowly winning unaligned mayors and favoring aligned mayors in marginal seats. On the other hand, he can use the same pork funds to especially favor aligned mayors in safe seats who are his powerful political

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[4]The proofs of Proposition 1a and 1b are sketched in the supplemental Appendix.

[5]To obtain interior solutions, I assume that the FOC evaluated at $-P_i = 0$, $\tau = 0$, and MMV_i^0 – is strictly positive. The proof of Proposition 2 follows from the implicit function theorem and is sketched in the supplemental Appendix.

intermediaries, and correspondingly penalize unaligned mayors in safe seats to stunt the vote-mobilization efforts of his political opponents. In short, the legislator has the choice between distributing pork to "tie enemy's hands" at the municipality level, or to "tie enemy's hands" at the district level.

[Figure 1 about here.]

Given this tradeoff, several scenarios are possible, and the stylized case is shown in Figure 1. This figure graphically shows the optimal amount of pork allocations to municipality i expressed as a function of the aligned mayor's past vote-share margin of victory. The dashed horizontal line represents τ' and the solid lines on the two sides of zero depict the politically optimal transfers as a function of MMV_i^0. The sharp jump at zero is the local ATE of legislator-mayor partisan alignment in razor-close mayoral elections. Which scenario actually ensues depends on which political consideration bears the more weight to the legislator, and in any case, can be tested empirically.

2.2.3 Identification: Two Regression Discontinuities

To test the above propositions, the identification strategy that I adopt in this article is in the spirit of a regression discontinuity (RD) design in close elections pioneered by Lee (2008), and employed in B&N's (2012) analysis. I exploit two close elections separately to identify plausibly exogenous variations in legislator-mayor partisan alignments – the close elections of mayors at the municipal level, and the close elections of legislators at the district level.

In applications of RD, the standard approach is to compare outcomes of bare winners and bare losers in popular elections. The assumption is that close elections have a nontrivial random chance component so that candidates at the threshold of winning or losing cannot manipulatively sort into treatment. In short, the key

identifying assumption of the RD design is that the treatment assignment arising from close elections is "as good as random." However, there is no reason that this assumption automatically holds (Grimmer et al., 2011). Its applicability must be justified on the basis of context-specific theory and data (Caughey and Sekhon, 2011; Sekhon and Titiunik, 2012). Hence I proceed with caution and address the validity of the identifying assumption in the results and discussion section.

Consider the case in which the RD design is based on close mayoral elections. Taking as given the partisan identify of the winning legislator, the treatment group is composed of municipalities where the aligned mayoral candidate barely won her race, and the control group is composed of municipalities where the aligned mayoral candidate barely lost her race. I then compare pork allocations of these treatment and control groups. That is, the effect of interest is the change in pork allocations caused by the legislators' mayoral ally narrowly winning her race.

Formally, define the treatment variable of interest as $P_i \in \{0, 1\}$. As in the theoretical framework in the preceding section, P_i takes on a value of 1 if the incumbent mayor in municipality i is from the same party as the incumbent district legislator, and 0 otherwise. Consider an observed continuous variable MMV_i (known in the literature as the "forcing", "assignment", or "running" variable), defined as the vote-share margin of victory of the mayoral ally against her highest-ranking opponent in municipality i. As such, MMV_i ranges from -100% if the mayoral ally obtained 0 votes and all votes counted towards her winning opponent, to 100%, if the opposite were true. At the threshold value of $MMV_i = 0\%$ is where the mayoral ally and her highest-ranking opponent have equal vote-shares, so that immediately to the left of this threshold is where the mayoral ally just narrowly lost the race and immediately after this threshold is where she just narrowly won. Therefore, $MMV_i = 0\%$ is the "cut-point" where the causal variable of interest, P_i "jumps discontinuously" from 0 to 1. Following Hahn, Todd and Van der Klaauw (2001) and Lee (2008), the average

treatment effect of treatment P_i on unit i at the cut-point $MMV_i = 0\%$ is as follows:

$$\tau_{RD}^M \equiv E[Y_i(1) - Y_i(0)|MMV_i = 0\%] =$$
$$lim_{MMV_i \downarrow 0\%} E[Y_i(1)|MMV_i = 0\%] - \qquad (2.7)$$
$$lim_{MMV_i \uparrow 0\%} E[Y_i(0)|MMV_i = 0\%]$$

where $Y_i(1)$ denotes the pork allocations of municipality i under treatment, and $Y_i(0)$ under control. Intuitively, for as long as razor-close mayoral elections are as good as random, if municipalities in which the legislator's mayoral ally won receive significantly higher pork allocations than municipalities in which his mayoral ally lost, then the difference can be attributed to the effect of the legislator-mayor partisan alignment.

Equation 7 delivers a direct test of Proposition 1a. Moreover, this average treatment effect is local and interpreted as the causal effect of legislator-mayor partisan alignment on pork distribution in municipalities in which the incumbent mayor narrowly won.

Consider this time the case in which the RD design is based on close House elections. Taking as given the party affiliation of the incumbent mayor, define CMV_d as the vote-share margin of victory of the aligned legislator at the district level. As such, CMV_d ranges from -100% if the aligned legislator obtained 0 votes and all votes counted towards his winning opponent, to 100% if the opposite were true. At the threshold value of $CMV_d = 0\%$ is where the aligned legislator and his highest-ranking opponent have equal vote-shares, so that immediately to the left of this threshold is where the aligned legislator just narrowly lost the House race and immediately after this threshold is where he just narrowly won. Therefore, $CMV_d = 0\%$ is also a "cut-point" where the causal variable of interest, P_i "jumps discontinuously" from 0 to 1. As in Eq. 7 above, the average treatment effect of P_i on unit i at the cut-point

$CMV_i = 0\%$ is as follows:

$$\tau_{RD}^C \equiv E[Y_i(1) - Y_i(0)|CMV_d = 0\%] =$$
$$lim_{CMV_d \downarrow 0\%} E[Y_i(1)|CMV_d = 0\%] - lim_{CMV_d \uparrow 0\%} E[Y_i(0)|CMV_d = 0\%] \quad (2.8)$$

In this case, taking as given the party affiliation of the incumbent mayor, the treatment group is composed of municipalities in districts where the aligned legislator barely won his race, and the control group is composed of municipalities in districts where the aligned legislator barely lost hist race. Eq. 8 directly tests Proposition 1b. Similar to Eq. 7, this ATE is local and is interpreted as the causal effect of partisan alignment on pork distribution in districts in which the incumbent legislator narrowly won. As will be elucidated in the next subsection, this second identification strategy is useful in empirically testing Proposition 2, that is, in testing how the amount of politically motivated pork allocations varies with the vote-share margin of victory of the aligned mayor.

2.2.4 Estimation

To estimate the local ATE expressed in Eq. 7, I begin with a spline polynomial approximation of order p in MMV_{it} as follows:

$$\tau_{it} = \alpha + \sum_{k=0}^{p} (\rho_k MMV_{it}^k) + P_{it} \sum_{k=0}^{p} (\pi_k MMV_{it}^k) + \delta_t + \nu_d + \eta_{it} \quad (2.9)$$

where δ_t are year fixed effects and ν_d are district fixed effects. The estimated coefficient $\hat{\pi}_0$ identifies the local ATE at 0, and I expect $\hat{\pi}_0 > 0$ to validate Proposition 1a.

The above specification is useful because the shape of the polynomial to the left of zero (captured by the estimated $\hat{\rho}_0$) and to the right of zero (captured by the estimated $\hat{\pi}_0 > 0$) can indirectly test Proposition 2, shedding light on the relationship between partisan pork allocations and the level of political competition facing mayors.

However, the assignment to political alignment is endogenous as one moves away from the zero threshold, hence any evidence from this specification ought to be interpreted as merely suggestive.

As an alternative estimation method, I apply local linear regression, which restricts the sample to municipalities in the interval $MMV_{it} \in [-h; +h]$ and estimates the model:

$$\tau_{it} = \rho_0 + \rho_1 MMV_{it} + \pi_0 P_{it} + \pi_1 P_{it} MMV_{it} + \delta_t + \nu_d + \eta_{it} \qquad (2.10)$$

where the optimal bandwidth h is selected as in Imbens and Kalyanaraman (2012). In this case, close mayoral races need not be defined because the ATE is identified as the difference between the boundary points of two regression functions on either side of zero. As a result, close mayoral races are those with a margin of victory approaching zero at the limit.

Finally, I also show results for two alternative estimators, namely OLS in close intervals around zero: $[-5; +5]$ and $[-2.5; +2.5]$. Here, close races are defined as elections for which the margin of victory is lower than 5% or 2.5% in absolute terms, respectively.

To estimate the local ATE expressed in Eq. 8, I apply the same estimation methods expressed in Eqs. 9 and 10, but replacing mayor's vote-share margin of victory at the municipality level (MMV_{it}) with legislator's vote-share margin of victory at the district level (CMV_{it}). In particular, I estimate the following:

$$\tau_{it} = \alpha + \sum_{k=0}^{p}(\rho_k CMV_{dt}^k) + P_{it}\sum_{k=0}^{p}(\pi_k CMV_{dt}^k) + \delta_t + \nu_d + \eta_{dt} \qquad (2.11a)$$

$$\tau_{it} = \rho_0 + \rho_1 CMV_{dt} + \pi_0 P_{it} + \pi_1 P_{it} CMV_{dt} + \delta_t + \nu_d + \eta_{dt} \qquad (2.11b)$$

In Eq. 11a, the estimated coefficient $\hat{\pi}_0$ identifies the local ATE expressed in Eq. 8, and I expect $\hat{\pi}_0 > 0$ to validate Proposition 1b. In Eq. 11b, the sample is restricted

to municipalities in the interval $CMV_{dt} \in [-h; +h]$, where the optimal bandwidth h is also selected as in Imbens and Kalyanaraman (2012).

Testing Heterogeneous Treatment Effects in Mayor's Margin of Victory. Focusing on Eq. 11b, which exploits close House elections to identify partisan alignments, I then interact the alignment variable P_{it} with the quartiles of vote-share margin of victory of the mayor to test Proposition 2 as follows:

$$
\begin{aligned}
\tau_{it} = \rho_0 &+ \rho_1 CMV_{dt} + \pi_0 P_{it} + \pi_1 P_{it} CMV_{dt} + \\
&P_{it} \sum_{q=1}^{4} \lambda_q MQ_{it,q} + \sum_{q=1}^{4} \kappa_q MQ_{it,q} + \delta_t + \nu_d + \eta_{dt}
\end{aligned}
\tag{2.12}
$$

where $MQ_{it,q}$ is a dummy variable for the qth quartile of the mayor's vote-share margin of victory in absolute terms.

Equation 12 can shed light on the relationship between political competition facing mayors and partisan pork distribution. If the story is about "tying *your* enemy's hands" as in B&N, that is, legislators especially favor aligned mayors in marginal seats and penalize unaligned mayors in marginal seats, then $max\{\lambda_q\}_{q=1}^{4} = \lambda_1$ and $max\{\kappa_q\}_{q=1}^{4} = \kappa_1$. On the other hand, if the story is about "tying *my* enemy's hands," that is, legislators especially favor aligned mayors in safe seats and penalize unaligned mayors in safe seats, then $max\{\lambda_q\}_{q=1}^{4} = \lambda_4$ and $max\{\kappa_q\}_{q=1}^{4} = \kappa_4$. Before proceeding to the empirical results, I discuss the Philippine political institutions and the construction of the data used in the analysis.

2.3   Data and Institutions

Two sets of data are used in this paper. The first set is the electoral data for the House and mayoral races, sourced from the Philippine Commission on Elections (COMELEC). The second set is the fund release of the Constituency Development

Funds (CDF) sourced from the Department of Budget and Management (DBM).[6]

2.3.1 Philippine Political System

The Philippines is a unitary presidential democracy and has a multiparty system. It is currently divided into 81 provinces, which are further subdivided into cities and municipalities that are each headed by a Mayor. There are 135 cities and 1,496 municipalities in the country. Within a province, cities and municipalities are grouped into single-member congressional districts that elect a Legislator to the House of Representatives (lower chamber of Congress). There are 229 legislative districts in the Philippines, each composed of an average of 250,000 inhabitants. Legislators and mayors serve 3-year terms and are elected at the same time every three years. Legislators are directly elected by plurality rule at the district level. Mayors are also elected by plurality rule at the municipal level. In this paper, the relevant election years are 2001, 2004, 2007, and 2010.

2.3.2 Parties and Partisanships

The Philippines has a weakly institutionalized party system. Since democratization in 1986, the party system has been unstable, characterized by increased variability in the number of parties and frequent changes in party labels from one election to another (Kasuya, 2009).[7] In the 2013 midterm elections alone, over 125 registered political parties contested seats in the local and House races.

Despite the fact that parties are weak in that they speak little of alignments in ideology or policy preferences among party members and between politicians and

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[6]DBM makes available online the data for years 2003-2006, 2009, and 2010. To augment the DBM data, I use data on CDF releases for 2001 that was collected and archived by the Philippine Center for Investigative Journalism (PCIJ). I also use the data for 2007-2008 that are sourced from the website of the Congressional Budget Planning Office (CBPO) of the House of Representatives. Disaggregated data for 2002 is unavailable.

[7]See Table A3 in the Appendix for the list of parties/coalitions of legislators and mayors across election years from 2001-2010.

voters, party affiliations are nonetheless informative of alliances among politicians across levels of elective offices. This is because absent adherence to party ideology or policy platforms, the desire of politicians across levels of government to establish and maintain the political exchange of distributive benefits for votes becomes the weightier consideration in choosing party affiliations (De Dios, 2007; Hicken, 2011). Precisely because party affiliations are revealed preference for alliances that bind that partisanships between legislators and mayors matter in the conduct of pork distribution. Ultimately, parties do not have to be particularly strong for legislators to make distributive decisions that favor their mayoral allies and, obversely, for mayors to mobilize political support for their House benefactor.

The independent variable of interest that captures the legislator-mayor partisan alignment is the variable $Aligned_{it}$, which takes on a value of 1 if the incumbent mayor's party affiliation in municipality i is the same as the party affiliation of the incumbent district legislator at year t, and 0 otherwise.

2.3.3 Pork Distribution

The Constituency Development Fund (CDF) in the Philippines refers to two categories of discretionary lump sum appropriations: (1) Priority Development Assistance Funds (PDAF), and (2) Department of Public Works and Highways-Congressional Allocations (DPWH-CA).

PDAF is used to accommodate both infrastructure and non-infrastructure projects of legislators in their respective districts. Legislators are entitled to direct PDAF to local government units (LGUs) including the offices of municipal and city mayors within their district. The bulk of PDAF is in the form of "special financial assistance to LGUs" channeled directly to municipalities and cities (an average of a little above 45% of annual PDAF allocations).[8] These are reported in the official release orders

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[8]Table A1 in the Appendix details the distribution of PDAF by implementing agency across the years 2001-2010.

as funds for the "specific programs and projects to address the pro-poor programs of the LGUs." The rest of PDAF are channeled through national government agencies (NGAs).

DPWH-CA, on the other hand, is used solely for various local infrastructure projects in the legislative district. Unlike PDAF, DPWH-CA has to be released via, and implemented by, the Department of Public Works and Highways.

Across the years in the dataset, on average, municipalities receive CDF allocations that amount to about 7.5% of the average internal revenue allotment (IRA). CDF therefore constitutes an important supplementary source of income for municipalities. This amount, for example, is well above the combined average expenditure of a municipality on education, culture and sports, and social services and welfare (about 6.4% of IRA).

Even though the release of CDF follows a set budgetary procedure, legislators have the authority to specify the projects to be funded. In fact, jurisprudence supports legislators' "power of the purse" over CDF. In the case of Philippine Constitution Association vs. Enriquez, et al. (G.R. No. 113105, August 19, 1994), the Supreme Court ruled that the power of the purse vested in Congress "includes the power to specify the project or activity to be funded under the appropriation law."[9]

Although I perform the RD analyses on both PDAF and DPWH-CA, the rest of this article focuses on the results from the analyses of PDAF. As it turns out, even though the benchmark OLS and difference-in-differences estimates provide evidence of the effect of legislator-mayor political alignment on the distribution of DPWH-CA, this effect gets attenuated when subjected to RD analyses (see Appendix Table 2A).[10] Hence, I focus on the dependent variable, *Per Capita Priority Development*

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[9]This ruling has been reaffirmed in Sarmiento, et al. vs. The Treasurer of the Philippines, et al. (G.R. No. 125680 & 126313, September 4, 2001). However, in November 2013, the Supreme Court declared this "power of the purse" unconstitutional (G.R. No. 208566, 208493, & 209251), and the release of CDF has been suspended since.

[10]An avenue for future research is testing the theory that bureaucratic authority moderate politicians' partisan incentives as has been shown in other contexts (c.f. Gerber and Gibson, 2009; Gerber

Assistance Funds distributed by a legislator to municipality i in year t.

2.3.4 Descriptive Statistics

Descriptive statistics provide preliminary evidence in support of this article's hypotheses. In every year from 2001-2010, per capita Priority Development Assistance Funds (PDAF) is substantially and statistically higher in municipalities whose mayor is the district legislator's partisan ally (Figure 2.1). This pattern holds across all regions of the Philippines (Figure 2.2), suggesting that partisan pork distribution is widely pervasive in the country. These figures also indicate substantial variation in pork allocations across aligned municipalities as well as across unaligned municipalities.

[Figures 2.1 and 2.2 about here.]

Descriptive statistics presented in Table 1 confirm this high variance. On average, aligned municipalities receive PHP35 per capita PDAF allocations, but the standard deviation is 140. Likewise, unaligned municipalities receive PHP8 per capita PDAF allocations, but the standard deviation is 50.

[Table 1 about here.]

Table 1 also shows that municipalities aligned with the district legislator receive larger per capita PDAF by PHP27.46, and this is statistically significant at the 1% level. However, descriptive statistics also reveal that municipalities aligned with the district legislator also tend to have significantly larger per capita Internal Revenue Allotment (IRA) by PHP91.50. Moreover, there tends to be more municipalities in which the mayor is aligned with the district legislator. These statistics are telltale

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and Hopkins, 2011; Gordon, 2011; Khemani, 2003). Because the Department of Public Works and Highways implements the projects funded under the DPWH-CA, it is possible that legislators' partisan motives are tempered by bureaucrats' different set of agenda.

signs of "selection on observables" in the sample, in that municipalities controlled by legislators' mayoral allies tend to differ in important ways from their unaligned counterparts, apart from legislator-mayor partisan alignments.

The identification strategy presented in the previous section accommodates for these selection on observables as well as selection on unobservables. In the next section, I present estimates of the causal effect of the legislator-mayor partisan alignment on per capita PDAF distribution, and heterogeneous treatment effects in mayor's victory margins using regression discontinuities in close mayoral races and in close house races.

2.4   Empirical Results

Table 2 presents results on the impact of legislator-mayor partisan alignment on pork distribution. As a benchmark, I show results from OLS and difference-in-differences (DD) estimates in columns (1) and (2), respectively. Then I present results using different regression discontinuity (RD) estimators based on aligned mayors' vote-share margin of victory (in columns (3) - (6)), and based on aligned legislators' vote-share margin of victory (in columns (7) - (10)).

[Table 2 about here.]

2.4.1   The Effect of Legislator-Mayor Alignment on Distribution

The cross-sectional and panel evidence in Table 2 show that municipalities in which the mayor is politically aligned with the district legislator receive more per capita PDAF allocations. OLS estimates imply that, with respect to the average level (PHP26.00), per capita pork allocations to a municipality more than doubles when the mayor is politically aligned with the district legislator. On the other hand, DD estimates imply a smaller increase of about 60% of the average level. While this

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discrepancy might be driven by the upward bias in OLS estimates, the DD results might also suffer from time-varying omitted bias.

Columns (3) - (10) of Table 2 report results of RD estimators based on close mayoral races and close house races. As discussed in the previous section, I use four specifications: spline polynomial approximation with full bandwidth as in equations (9) and (11a), local linear regression with optimal bandwidth as in equations (10) and (11b), and two OLS estimations in close intervals around the zero threshold, [-5;+5], and [-2.5;+2.5], which test for the difference of means in close elections defined by a margin of victory lower than 5% and 2.5% in absolute value, respectively.

According to the estimation with spline polynomial in mayor's vote-share margin of victory (column (3)), being politically aligned with the district legislator increases per capita PDAF allocations to the municipality by PHP23 or approximately 90% of the average level. Local linear regression with optimal bandwidth gives the same estimate (column (4)). To gauge its economic significance, PHP23 is about 70% of a municipality's average annual per capita spending on education, culture and sports. It is similarly about 40% of spending on social services and welfare. The same amount could be used by the average-sized rural municipality to buy 4 more farm tractors, or employ 10 additional minimum wage workers full time for 365 days, or build one multipurpose drying pavement/solar dryer each year.[11]

The OLS specifications in close intervals (columns (5) and (6)) report estimates that are more or less similar to the aforementioned estimates in terms of both size and statistical significance. All in all, these RD results are a direct confirmation of Proposition 1 in the model presented earlier.

As previously discussed, the causal effect of partisan alignment on pork distribution can also be identified using RD estimations in close House races. Columns

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[11]This economically meaningful effect on fiscal policy might be weakened if the legislator-mayor partisanship also causes differential effects on other sources of municipal income. In Table A4 in the Appendix, I show that there are no differential treatment effects on all other sources of income.

(7) - (10) of Table 2 report such estimates, where robust standard errors shown are clustered at the district level, taking account of the fact that legislators' vote-share margin of victory only varies at the district level and not at the municipal level. The estimation with spline polynomial in legislator's vote-share margin of victory (column (7)) implies that the legislator-mayor political alignment increases per capita PDAF allocations to the municipality by about 123% of the average level. This estimate drops down to about 100% based on local linear regression with optimal bandwidth (column (8)). However, the estimate jumps up to 173% (column (9)) and to as high as 185% (column (10)) in OLS specifications in the intervals [-5;5] and [-2.5;2.5] around the zero threshold, respectively. On the whole, this latter set of RD estimates affirms the robustness of the effect of legislator-mayor partisanship on pork distribution to alternative identification strategies.

The fact that the magnitudes are higher when the RD estimations are in close House races compared to when the RD estimations are in close mayoral races provides insight on the dynamics of partisan pork distribution. Given that RD effects are only identified locally, these results imply that partisanships matter more in cases where the aligned legislator narrowly lost his race than in cases where it is the aligned mayor who narrowly lost her race. While not a direct test, this is consistent with the claim that legislators favor pork distribution to politically aligned mayors not only to help the latter win reelections but also to rely on them as political intermediaries who can mobilize electoral support. These findings also suggest the notion that legislators in marginal seats use state resources more freely to secure election than those in safe seats. This is fairly consistent with Keefer and Khemani (2009) article on India, in which they find that legislators tend to "pass on pork" in partisan strongholds. Before directly testing the claim that legislators particularly favor aligned mayors in safe seats and penalize unaligned mayors in safe seats, I first check the claim that close races result in as-if-random assignment in partisan alignments in the next subsection.

2.4.2 Do Close Races Mimic Random Assignment?

In order for the RD design in close races to be internally valid, winning mayors should not be able to systematically sort into being politically aligned with the winning district legislator and vice-versa. This means that, taking as given the partisan identity of the winning legislator, neither aligned nor unaligned mayors should be able to systematically win close races. Likewise, taking as given the partisan identities of the winning mayors, neither aligned nor unaligned legislators should be able to systematically win close races at the district level.

A priori, there are two reasons why the above assumptions are plausibly valid in the case of local elections in the Philippines. First, given the plurality voting system with multiple parties/candidates contesting the congressional and mayoral seats, the threshold of narrowly winning or losing is more difficult to predict than, for instance, elections in the U.S. Second, House races and mayoral races in the Philippines are simultaneously held and hence mayoral candidates could not determine with certainty the partisan identity of the winning legislators – and vice-versa – until after the elections are over. For as long as there is a nontrivial element of randomness of both narrowly winning *and* being politically aligned, bare winners assigned to treatment group should be comparable with bare winners assigned to the control group in all other ways except in their partisan alignments.

Notwithstanding, "the burden is on the researcher to provide affirmative evidence for the validity of RD's assumptions...; [to] identify and collect accurate data on the observable covariates most likely to reveal sorting at the cut-point. A good rule of thumb is to always check lagged values of the treatment and response variables (Caughey and Sekhon, 2011, 405)." Table 3 presents such checks. It shows estimates of discontinuities at the 0 threshold in the lagged value of the response variable, as well as in lagged values of treatment and forcing variables. In addition, the table also shows discontinuities at the 0 threshold in the formula-based municipal income and in

other important municipal characteristics. In particular, I run tests of the difference in means between aligned versus unaligned municipalities in close intervals near zero, namely [-5;+5] and [-2.5;+2.5]. Because most of these variables are predetermined with respect to the treatment (political alignment), there should be no discontinuity as long as no manipulative sorting occurs around zero. This is indeed the case, as all of the variables are balanced around the threshold, except for one exception in the log of land area.

[Table 3 about here.]

To further check for the absence of manipulative sorting, in the Appendix, I perform: (i) visual inspection of the histograms of mayor's vote-share margin of victory in bins of 2.5% (Figure A3.1) and 0.5% (Figure A3.2) and histograms of legislator's vote-share margin of victory in bins of 2.5% (Figure A3.3) and 0.5% (Figure A3.4), respectively; (ii) formal tests of the continuity of the density at the threshold in the spirit of McCrary (2008), in Figures A4.1 and A4.2; and (iii) placebo tests (following Imbens and Lemieux 2008) by estimating treatment effects at fake thresholds, where there should be no effect, in Figures A5.1 and A5.2.

2.4.3 Competition and Partisan Pork Distribution

Figure 3 shows the estimated spline polynomial in mayor's vote-share margin of victory and highlights the jump in per capita PDAF allocations at the zero-threshold.[12] Looking at the figure and judging only by the shape of the relationship between per capita PDAF allocations and political competition for aligned munici-palities (on the right of zero) and unaligned municipalities (on the left of zero), it might be tempting to conclude that legislators particularly favor aligned mayors in

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[12]In the Appendix, I also show the graphical representation of the RD estimates using local linear regressions with optimal bandwidths both in mayor's vote-share margin of victory and legislator's vote-share margin of victory.

safe seats and, to some extent, penalize unaligned mayors in safe seats. However, such evidence should only be interpreted as suggestive at best, because the randomness of assignment to partisan alignments is undermined by omitted factors as one moves away from the zero-threshold.

[Figure 3 about here.]

To directly test the interactive effects of partisanship and political competition facing mayors on pork allocations, I exploit close House races as a source of as-if random variations in partisan alignments. I then interact the political alignment variable with the quartiles of the vote-share margin of victory of the aligned mayor as specified in Equation 10.

Figure 4.1 summarizes the relationship between per capita PDAF allocations and political competition for aligned municipalities in quartiles of mayor's vote-share margin of victory. It is remarkably similar in shape to the stylized figure shown in Figure 1. According to this figure, legislators, in fact, particularly favor aligned mayors in safe seats and especially penalize unaligned mayors in safe seats. Figure 4.2 confirms this result. The figure shows how aligned mayors in the 4th quartile of vote-share margin of victory receive the lion's share of per capita PDAF allocations, amounting to twice as much differential funds received by those in the 1st quartile and this difference is statistically significant.[13]

[Figures 4.1 and 4.2 about here.]

But what does it mean when legislators favor aligned mayors in safe seats and penalize unaligned mayors in safe seats? My claim is that mayors in safe seats are powerful political intermediaries that help legislators win reelections hence legislators bias pork distribution in their favor when they are aligned, and against them when

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[13]In the Appendix, I use quintiles of mayor's vote-share margin of victory instead of quartiles. The shape of the figure remains similar to the stylized figure in Figure 1.

they are unaligned. However, there are other plausible reasons that could be driving these results. For instance, municipalities in which aligned mayors have high vote-share margin of victory also tend to be the legislator's political strongholds. In which case the story is actually more in line with Cox and McCubbins (1986) story of politicians using pork to reward core supporters and not about rewarding powerful political intermediaries. Alternatively, the mayor's victory margins could be proxying for the presence of swing voters. If true, then the story is really about rewarding aligned and 'swing' localities, consistent with the findings of Arulampalam et al. (2009) in India. More generally, it is possible that mayors in safe seats tend to come from municipalities that systematically differ from others in ways that also impact pork allocations (e.g. they tend to be richer municipalities, have larger population, etc.)

Figures 5.1 - 5.6 and Figure 6 show results of regressions that rule-out the afore-mentioned plausible alternative stories. To generate the first six figures, I use Eq. 10 but I replace the outcome variable with other potential outcomes. Figures 5.1 - 5.6 present results on whether municipalities with mayors in safe seats (i.e. in the 4th quartile of vote-share margin of victory in absolute terms) systematically differ from other municipalities in terms of the municipalities' main source of income, land area, number of registered voters, rural/urban classification, lag of forcing variable, and lag of treatment variable, respectively. Results indicate no clear pattern of systematic differences across these grouped municipalities. There are no statistically significant differences in important characteristics of municipalities that vary with the safeness of the mayor's seat.

[Figures 5.1 - 5.6 about here.]

In Figure 6, I test whether municipalities with aligned mayors in safe seats tend to be the same municipalities that are the district legislator's political strongholds, and municipalities with unaligned mayors in safe seats tend to be his opponent's

political strongholds. I use the ratio of the number of votes the legislator received in a given municipality to the total number of votes he received in the district to proxy for the extent to which the same municipality is a political stronghold. If this ratio is increasing in the aligned mayor's vote-share margin of victory or decreasing in the unaligned mayor's vote-share margin of victory or both, then I cannot rule out the possibility that what might be driving the pattern observed in Figures 4.1 and 4.2 is simply Cox and McCubbins (1986) core supporter theory of pork distribution. However, Figure 6 indicates that while there is some qualitative evidence in support of a 'partisan *and* core theory', this evidence is not statistically significant. A F-test of the joint equality of means across quartiles by partisan alignment gives a p-value of 0.6740 and 0.7183 for aligned and unaligned municipalities, respectively.

[Figure 6 about here.]

The lack of a clear relationship between the municipal-level vote-shares of the legislator and mayor's vote-share margin victory also implies that the story of a 'partisan *and* swing' dynamic of distribution (as in Arulampalam et al. (2009) is also unlikely to be driving the results. If it were the case, then we should expect a concave function that peaks and discontinuously jumps at the zero threshold. Figure 6 does not support this alternative story.

2.5   Conclusion

This article takes off from the theoretical point emphasized by Brollo and Nannicini (2012) in the context of discretionary transfers in a federal system, but which more broadly applies to partisan incentives in distribution: "(i) many polities feature multiple levels of electoral competition layered on top of one another and (ii) whoever allocates benefits may care about electoral outcomes at all levels (760)." Building on these premises, I uncover the tradeoff that political authorities face when allocating

discretionary funds: on the one hand, they can help allies in hotly contested seats win reelections by punishing unaligned narrowly winning local politicians. On the other hand, they can distribute in favor of allies in safe seats and against their enemy's powerful political intermediaries to increase their own reelection prospects.

I put this theoretical insight to the litmus test of empirics by analyzing how Philippine legislators' partisan motives impact pork distribution across municipalities in their respective districts. Exploiting mayoral and House races as sources of as-if-random variations in legislator-mayor political alignments, I produce RD estimates that show that mayors politically aligned with their respective district legislator receive larger pork allocations by about 90% in razor-close mayoral elections and about 100% in razor-close House elections.

Testing how the partisan alignment effect varies in the degree of political competition facing mayors, I find that mayoral allies in safe seats receive the lion's share of pork. Similarly, among unaligned mayors, those in safe seats receive the least amount of pork. Hence, unlike in the case of Brazil in which politically motivated federal transfers are mainly driven by the desire to punish (weak) local enemies, politically motivated pork allocations in the Philippines are, by and large, driven by the incentive to reward powerful aligned political intermediaries and penalize their unaligned counterparts.

In light of these findings and in view of the fact that political competition occurs at multiple levels of government, an avenue for future research is in modeling and empirically testing how institutional constraints (e.g. timing of elections, modes of political mobilization, bureaucratic interference, etc.) as well as external shocks to the political system (e.g. natural disasters, coup d'etat, etc.,) temper the tradeoff that political authorities face when making distributive decisions. Ultimately, understanding these factors that enter the distributing politician's objective function can inform how policies can limit deviations from the efficient provision of public goods.

Tables

<div align="center">

Table 2.1: Descriptive Statistics.

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Variable (PHP)	All (1)	Aligned (2)	Unaligned (3)	Difference [(2) - (3)]
Congressional Allocations to Municipalities & Cities				
Per capita PDAF	25.90	35.46	8.00	27.46
	(117.76)	(140.28)	(49.98)	*(2.16)*
Min	0	0	0	
Max	5827.51	5827.51	1818.40	
Municipal/City Income				
Per capita IRA	1426.88	1458.92	1367.42	91.50
	(1252.97)	(1288.98)	(1181.18)	*(29.30)*
Number of Observations	7,973	5,198	2,775	

Notes: *Aligned* municipalities are those where the mayor has the same party affiliation as the district legislator's. *Unaligned* municipalities are those where the mayor has a different party affiliation as the district legislator's. Numbers in parentheses are standard deviations, except when the numbers are italicized, in which case they are standard errors.

Table 2.2: Effect of Legislator-Mayor Alignment on Distribution, OLS, DD, and RD Estimates.

Dependent Variable: Per Capita PDAF (PHP)	OLS	DD	RD in Mayor's Vote-Share Margin of Victroy				RD in Legislator's Vote-Share Margin of Victroy			
			Spline Polynomial	Local Linear Regression	OLS in an interval [-5;+5]	[-2.5;+2.5]	Spline Polynomial	Local Linear Regression	OLS in an interval [-5;+5]	[-2.5;+2.5]
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Aligned = 1	27.74***	15.55***	22.56***	23.21***	16.87***	23.86***	31.47***	26.23***	44.88**	47.81*
	(2.92)	(3.34)	(5.81)	(6.15)	(4.30)	(7.47)	(10.58)	(12.25)	(18.83)	(25.77)
Constant	5.90*	14.19***	5.43	10.30**	0.56	3.26	32.25***	25.61**	31.48*	-16.26
	(3.06)	(3.59)	(3.90)	(4.12)	(3.87)	(7.09)	(8.64)	(11.86)	(16.90)	(13.84)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bandwidth h	.	.	.	10	.	.	.	23	.	.
R^2	0.07	0.39	0.07	0.16	0.22	0.30	0.07	0.07	0.14	0.23
Clusters	1,578	1,578	1,578	961	562	291	176	142	60	38
Observations	7,973	7,973	7,973	2,688	1,279	590	7,179	3,282	703	395

Notes: Dependent variable: Per Capita Priority Development Assistance Funds distributed by a legislator to a municipality within his district. Independent variable: 'Aligned', which is a binary variable that takes on a value of 1 if the mayor is a partisan ally of the legislator, and 0 otherwise. Estimation methods: (1) OLS; (2) Difference-in-differences; (3) RD specification with 3rd-order spline polynomial approximation as in equation 7; (4) local linear regression with optimal bandwidth as in equation 8; (5)-(6) OLS in restricted intervals around the threshold of zero vote-share margin of victory, i.e., [-5;+5] and [-2.5;+2.5]; (7) RD specification with 3rd-order spline polynomial approximation as in equation 9a; (8) local linear regression with optimal bandwidth as in equation 9b; and (9)-(10) OLS in restricted intervals around the threshold of zero vote-share margin of victory, i.e., [-5;+5] and [-2.5;+2.5]. Robust standard errors are clustered at the municipality level for columns (1) - (6) and clustered at the district level for columns (7) - (10), and are in parentheses below the coefficient estimates. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table 2.3: Covariate Balance Between Treated (Aligned=1) and Control (Un-aligned=0) in Close Races.

| | | Mayor's Vote-Share | | Legislator's Vote-Share | |
| | All Sample | Margin of Victory | | Margin of Victory | |
OLS Interval	[-100;+100]	[-5;+5]	[-2.5;+2.5]	[-5;+5]	[-2.5;+2.5]
Lagged value of response variable					
Per Capita PDAF (t-3)	13.62***	2.60	-4.79	17.55	9.91
	(2.79)	(6.14)	(7.45)	(26.00)	(36.95)
Lagged values of treatment and forcing variables					
Aligned (t-3)	0.16***	-0.04	-0.09	0.04	0.06
	(0.02)	(0.05)	(0.08)	(0.14)	(0.19)
|MMV| (t-3)	0.12***	0.02	0.02	.	.
	(0.01)	(0.01)	(0.01)	.	.
|CMV| (t-3)	0.04***	.	.	-0.05	0.09
	(0.01)	.	.	(0.11)	(0.14)
Formula-based municipal income					
Per capita IRA	121.4**	36.09	127.9	232.6	491.8
	(53.27)	(88.35)	(159.4)	(364.5)	(582.6)
Municipal characteristics					
Ln population	-0.08***	0.03	-0.13	-0.07	-0.15
	(0.03)	(0.07)	(0.09)	(0.15)	(0.20)
Ln voters	-0.09***	0.02	-0.13	-0.07	-0.18
	(0.03)	(0.07)	(0.09)	(0.14)	(0.19)
Ln land area	0.03	-0.10	-0.21**	-0.09	-0.20
	(0.03)	(0.07)	(0.10)	(0.14)	(0.14)
Rural	0.01	-0.03	0.01	0.00	-0.07
	(0.01)	(0.02)	(0.03)	(0.04)	(0.05)

Notes: Estimated discontinuities of municipality characteristics at the threshold of zero vote-share margin of victory. OLS specifications shown are for the whole sample, and restricted to the close intervals [-5;+5] and [-2.5;+2.5], respectively. IRA or Internal Revenue Allotment is an automatic, formula-based transfer from the national government to the municipality. Population is the number of inhabitants in the municipality in 2007. Voters are the number of registered voters in the municipality in 2010. Land area is in thousand hectares. Rural = 1 for municipalities classified as rural. Per capita PDAF (t-3) is the per capita allocation of PDAF to the municipality in the previous term. Aligned (t-3) is the partisan alignment dummy in the previous term. |MMV| (t-3) is the mayor's and |CMV| (t-3) legislator's vote-share margin of victory in the previous term. Robust standard errors clustered at the municipality level for the first three columns, and at the district level for the last two columns. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Figures

Figure 2.1: Pork Allocation as a Function of Mayor's Past Vote-Share Margin of Victory
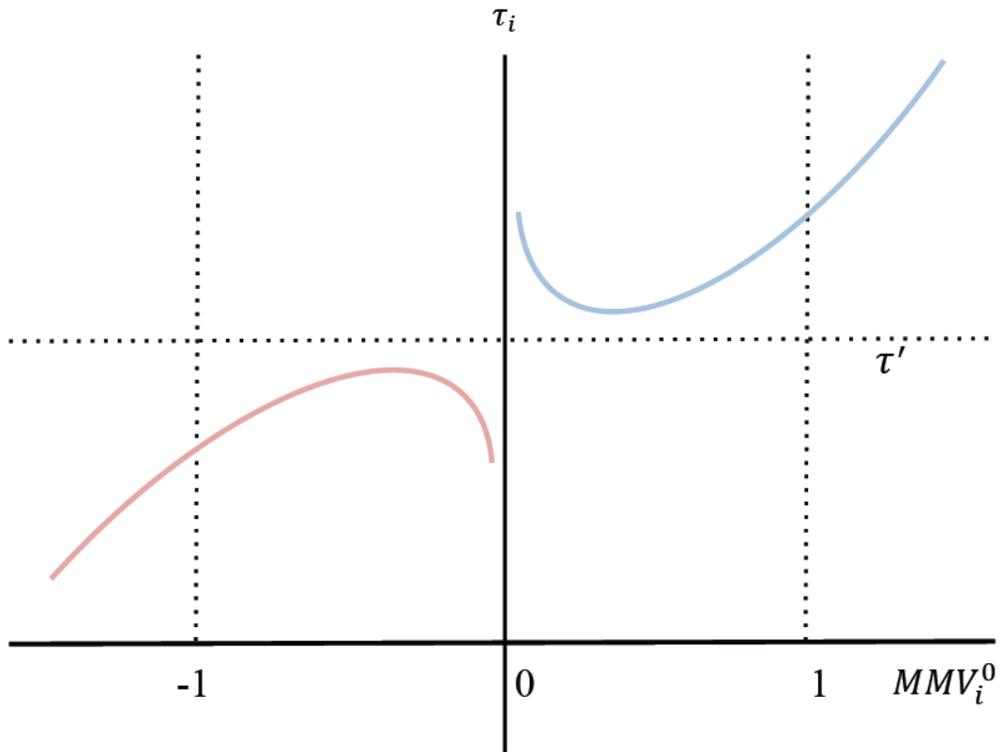
Figure 2.2: Per Capita Priority Development Assistance Funds by Partisan Alignment
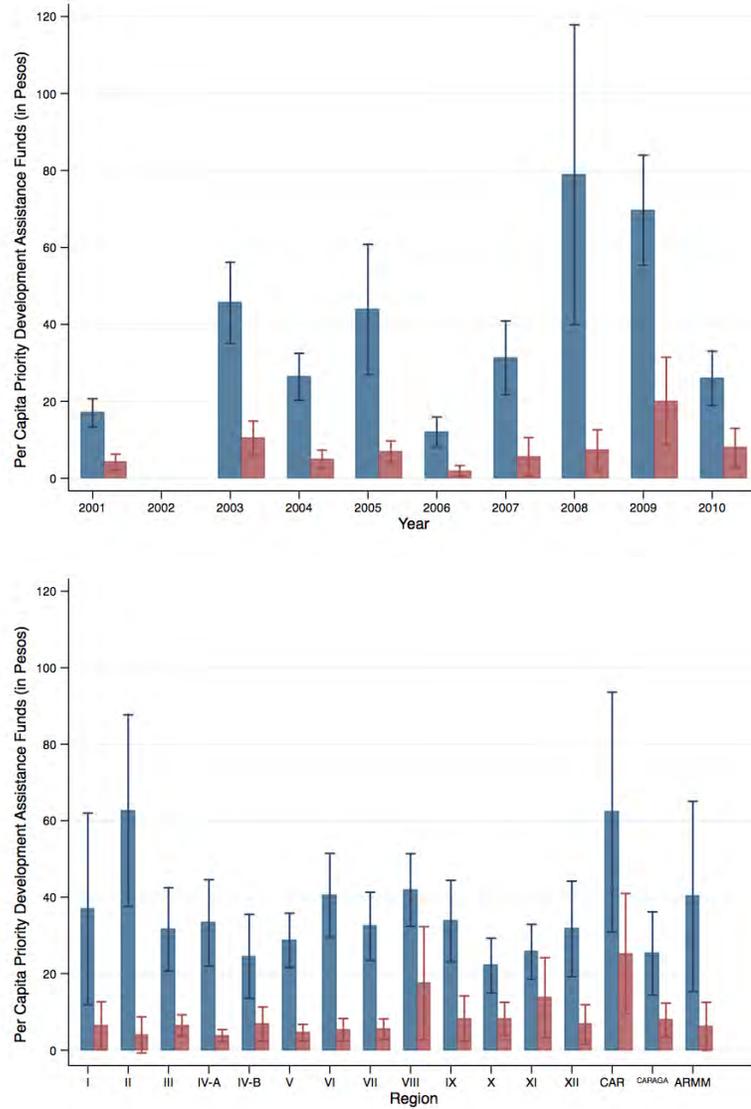
Notes: Blue bars represent Per Capita Priority Development Assistance Funds of municipalities in which the mayor is a partisan ally of the district legislator, and red bars represent that of municipalities in which the mayor is not an ally of the district legislator. Vertical lines represent 95% confidence intervals. Data is not available for year 2002.

Figure 2.3: Graphical Representation of the Regression Discontinuity
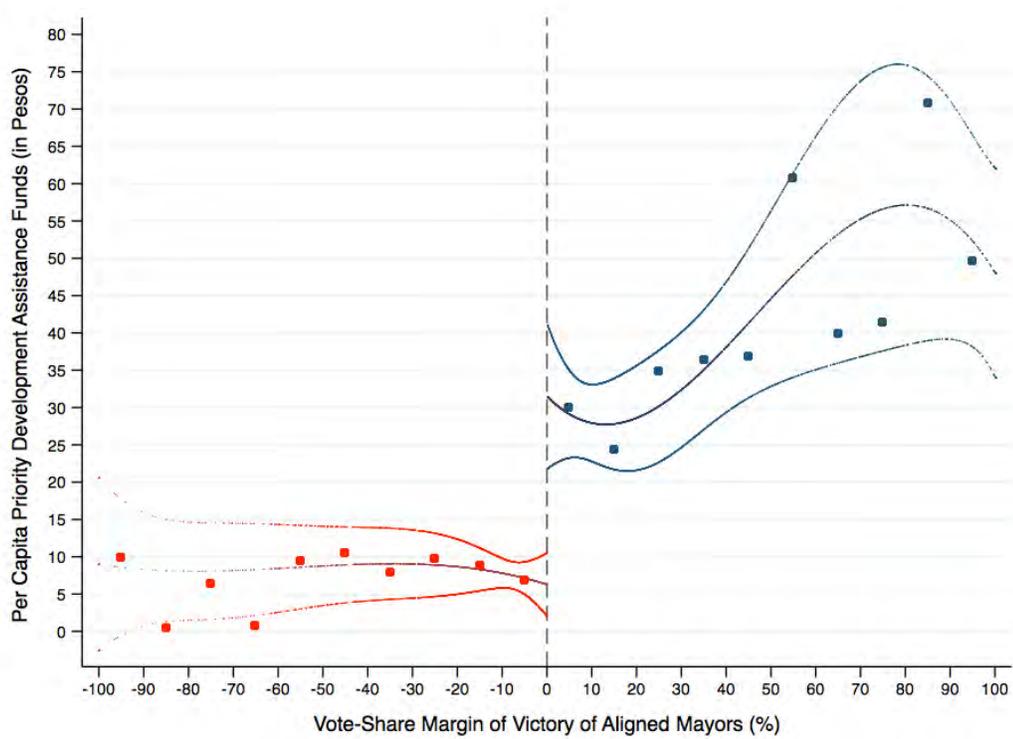
Notes: The central line is a spline 3rd-order polynomial fitted over the full interval [-100%, +100%], in the vote-share margin of victory of aligned mayors. The lateral lines represent the 95% confidence interval. Scatter points are averages over 10-unit intervals.

Figure 2.4: How Competition Facing Mayors Shapes Partisan Pork Distribution
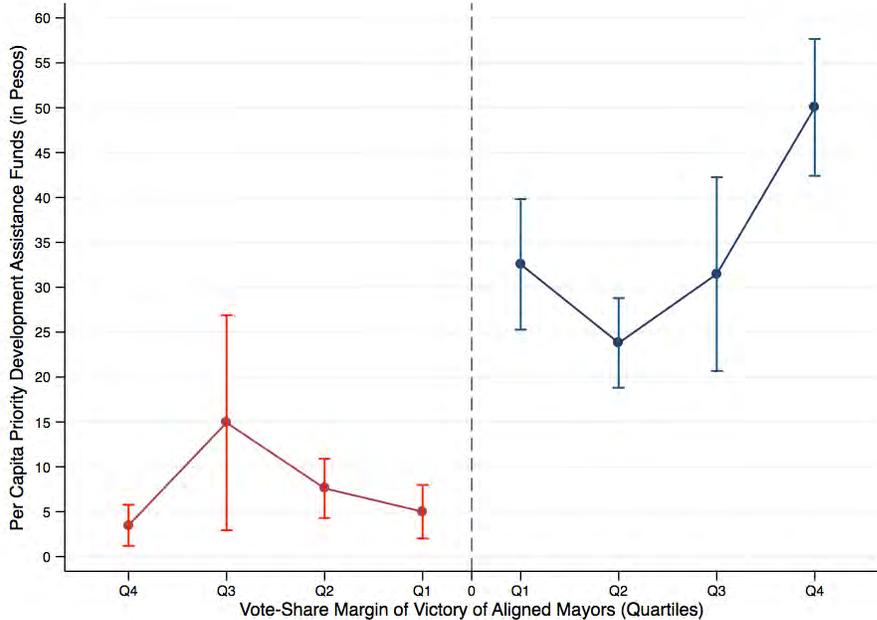
Figure 4.1
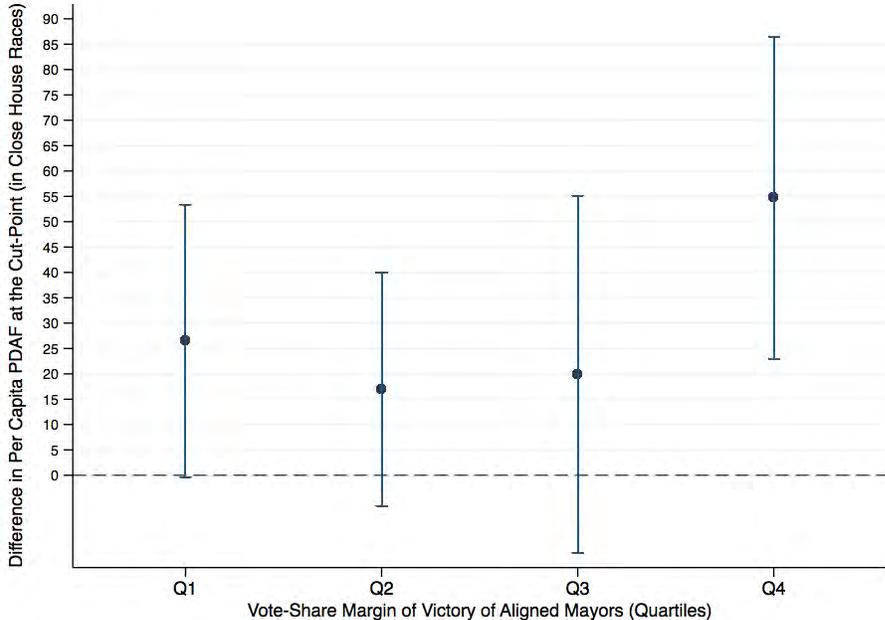
Figure 4.2

Notes: In Figure 4.1, points shown are the means of per capita PDAF, by partisan alignment and by quartiles of mayor's vote-share margin of victory. In Figure 4.2 point estimates shown are based on the regression results using the specification in Equation 10 with optimal bandwidths as in Imbens and Kalyaranaman (2012). Vertical lines represent 95% confidence intervals. Q1 represents vote-share margin in the 1st quartile, Q2 in the 2nd quartile, Q3 in the 3rd quartile and Q4 in the 4th quartile.

Figure 2.5: Covariate Balance Across Quartiles of Mayor's Vote-Share Margin of Victory
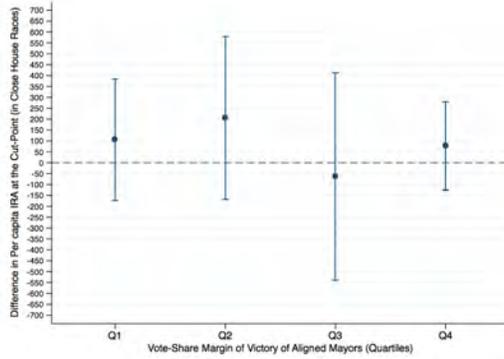
Figure 5.1
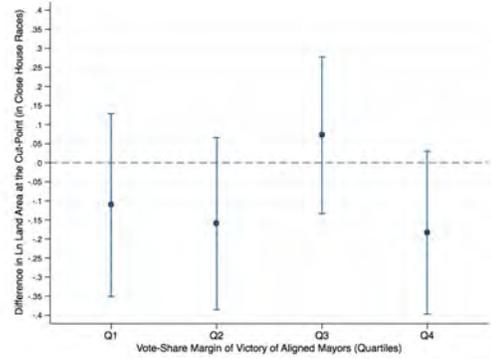
Figure 5.2
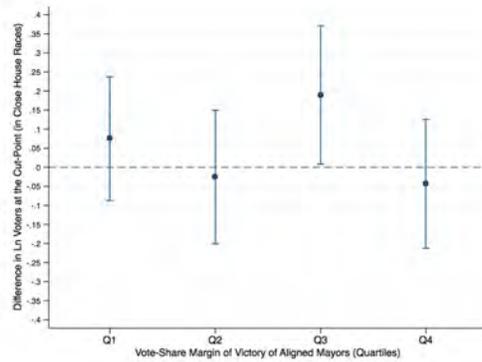
Figure 5.3
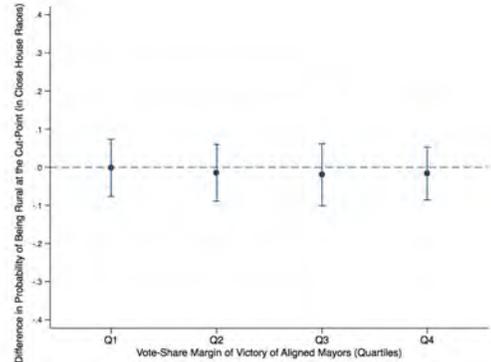
Figure 5.4
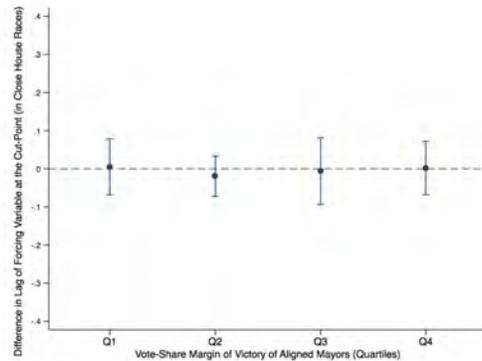
Figure 5.5
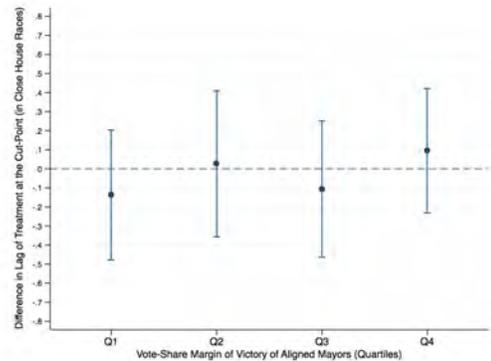
Figure 5.6

Notes: Point estimates shown are based on the regression results using the specification in Equation 10 with optimal bandwidths as in Imbens and Kalyaranaman (2012). Vertical lines represent 95% confidence intervals. Q1 represents vote-share margin in the 1st quartile, Q2 in the 2nd quartile, Q3 in the 3rd quartile and Q4 in the 4th quartile. The dependent variables are Per capita IRA (5.1), Ln land area (5.2), Ln number of voters (5.3), binary variable for urban classification (5.4), lag of the forcing variable (5.5), and lag of the treatment variable (5.6).

Figure 2.6: Legislator Municipal-Level Vote-Shares Across Quartiles of Mayor's Vote-Share Margin of Victory
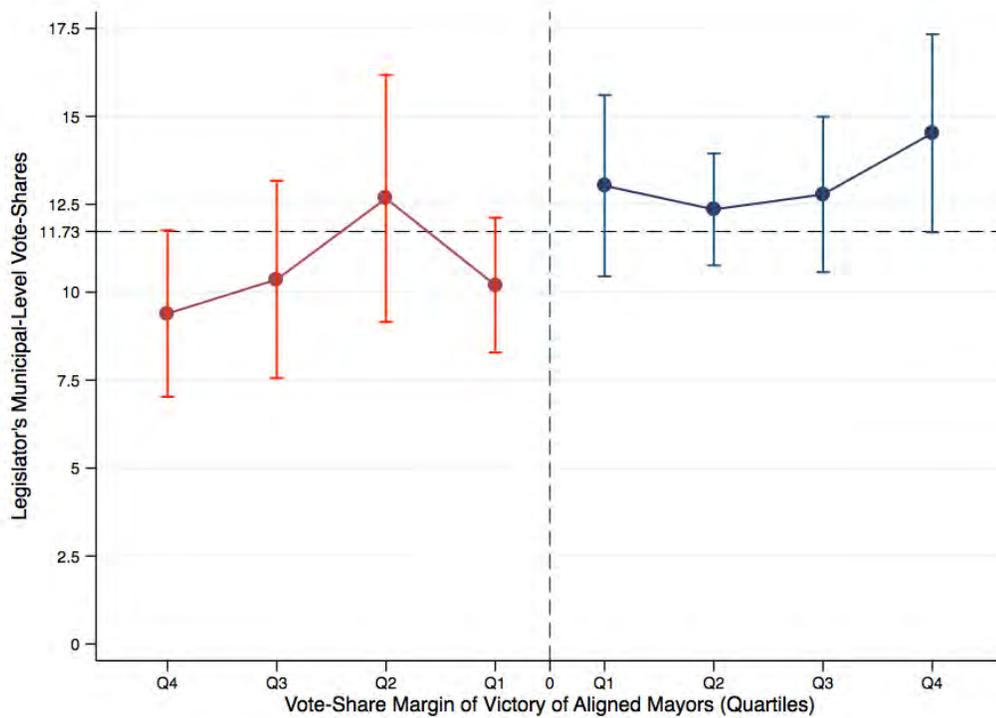
Notes: Graph shows mean of legislator's municipal-level vote-shares by partisan alignment and quartiles of mayor's vote-share margin of victory. Vertical lines represent 95% confidence intervals. Q1 represents vote-share margin in the 1st quartile, Q2 in the 2nd quartile, Q3 in the 3rd quartile and Q4 in the 4th quartile. Dashed horizontal reference line is for the mean of legislator's municipal-level vote-shares. Data is based on election years 2007 & 2010 for which municipal-level electoral returns for legislators are available.

CHAPTER III

Temptation in Vote-Selling: Evidence from a Field Experiment in the Philippines

3.1 Introduction

Vote-buying and vote-selling are pervasive phenomena in many developing democracies. While there is some debate about the consequences of the buying and selling votes, there is a consensus that transactional electoral politics brings with it a host of costs. For example, vote-buying and other forms of clientelism can undermine or even reverse the standard accountability relationship that is central to democracy (Hicken, 2011; Kitschelt et al., 2010; Lyne, 2007; Stokes, 2005; Stokes et al., 2013). Vote-buying also hampers the development of and trust in the political institutions necessary for democratic development and consolidation (Desposato, 2007; Graziano, 1973; Kitschelt et al., 2010; Lyne, 2007; Stokes, 2005). Finally, vote-buying and other forms of clientelism are associated with larger public deficits and public sector inefficiencies (Hicken and Simmons, 2008; Keefer, 2006, 2007), and higher levels of corruption (Kitschelt, 2007; Kitschelt et al., 2010; Keefer, 2007).

Because of these potential inimical effects, governments, NGOs and international donors have directed significant attention and resources towards combating vote-buying and vote-selling. Some strategies focus on the demand side of the equation—

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making it more difficult for politicians (or vote-*buyers*) to offer money in exchange for a vote. However, such strategies often fall victim to poor implementation and enforcement. As a result, a major focus of anti-vote-buying efforts has been on vote-*sellers*. Whether organized by governmental election commissions, or by concerned NGOs, campaigns to reduce the supply of votes available for purchase are common worldwide. Voter-focused campaigns against vote-selling tend to fall into two categories. The first type of campaign urges voters to avoid taking vote-buying payments at all. Voters may be asked to make promises or sign pledges to simply eschew taking money from politicians or their agents prior to elections. A second common approach seeks to subvert vote-buying by encouraging voters to take the money being offered, but nonetheless "vote their conscience." For example, Cardinal Sin, Archbishop of Manila, famously advised voters to "take the bait not the hook" (Schaffer, 2005).[1]

Motivated by both the negative consequences of transactional electoral politics, and by the prevalence of anti-vote-selling efforts, in this paper, we seek to deepen our understanding of the economics and psychology of individual vote-selling decisions. A number of questions are of general interest. What is the efficacy of anti-vote-selling campaigns? Can simple promises—such as the ones elicited from voters in anti-vote-selling campaigns—affect vote-selling behavior? If so, why might voters make such promises? Does the impact of promises differ by type of promise (e.g. "I won't take money" vs. "I'll take money, but vote my conscience")? Might some types of promises actually *increase* the incidence of vote-selling?

We test the model's predictions in the context of a randomized controlled trial of an anti-vote-selling intervention in Sorsogon City, Philippines. We randomly assigned voters to a control group or to one of two treatment groups. In the Promise 1 treatment, we invited voters to promise not to take vote-buying payments at all. In the Promise 2 treatment, we invited voters to promise that if they did take vote-

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[1]For examples of both types of campaigns, see Callahan (2000); Guiang (2013); Geronimo (2013); Schaffer (2005).

buying payments, they would nevertheless "vote their conscience." The two kinds of promises were designed to mirror the types of promises elicited in anti-vote-selling campaigns.

We estimate the impacts of promise treatments on a proxy for vote-selling: vote-switching, which we define as voting for a candidate who was not rated one?s favorite in a pre-election survey some weeks before.[2] While examining vote-switching is an indirect way of getting at vote-selling, vote-switching is self-reported, which raises concerns about social desirability bias: respondents could respond to the promise treatments by falsely maintaining consistency between their pre-election ratings and their post-election voting reports. Such biased reporting could lead us to spuriously find that the promise treatments reduce vote-switching.

Our results provide a reasonably strong indication that social desirability bias is not a significant concern in our setting. Support for this claim comes from comparisons of the treatment effects of Promise 1 ("Don?t take the money") on vote-switching across electoral races.[3] One would expect social desirability bias to be constant across electoral races, or increasing in the importance of the race. In our setting, if there were only social desirability bias and no "true" treatment effects, this would mean that we should find larger (negative) treatment effects for the mayor and vice-mayor races, compared to the city council race. As it turns out, we find the opposite to be true: the Promise 1 treatment effects on vote-switching, while negative, are very close to zero in the two most important electoral races that we examine (the elections for mayor and vice-mayor.) By contrast, we find much larger negative effects on vote-switching in the city council election, the least important of the races. We conclude from this comparison that our treatment effect estimates are minimally biased (if at

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[2]Individuals can be "vote-switchers" for many reasons aside from vote-selling (such as learning new information about candidates), but, given random assignment, the promise treatments should only affect vote-switching via changes in vote-selling.

[3]As we discuss further below, analogous comparisons across races for Promise 2 treatment effects are not as revealing of the extent of social desirability bias because Promise 2, in principle, can actually raise vote-switching.

all) by intentional misreporting.

We estimate that the Promise 1 treatment reduced vote-switching (and therefore vote-selling) in the race involving smaller vote buying payments (the city council race) by 10.9 percentage points. Compared to the city-council vote-switching rate of 47.1 percent in the control group, this is a large effect, given that vote-switching can occur for reasons other than vote-selling. As mentioned previously, the impacts of the Promise 1 treatment on vote-switching in the more important races (mayor and vice-mayor) are close to zero and are not statistically significant.

We also conduct statistical tests of pairwise differences in treatment effects across promise types (within electoral races), and across races (within promise types). We find that the Promise 1 treatment has a more negative effect on vote-switching than does the Promise 2 treatment. We also find that the promise treatments reduce vote-switching more for races with lower vote-buying payments (the city council race) than in the higher-money races (the mayor and vice-mayor races).

To help explain this pattern of heterogeneity in impacts across promises and electoral races, we developed a behavioral model of transactional electoral politics. First of all, we model selling one's vote as a temptation good: it creates positive utility for the future self at the moment of voting, but not for past selves who anticipate the sale of the vote. In addition, voters can make promises in advance of elections regarding whether or not they will sell their votes, and gain (lose) utility when they keep (break) such promises. We also allow for the possibility that voters may not be fully sophisticated about their vote-selling temptation. Specifically, when deciding whether to accept a gift from a candidate, they may underestimate how much utility the future self will gain from voting for the candidate who provided the gift (said another way, they underestimate the impact of accepting vote-buying payments today on their propensity to vote for the vote-buying candidate in the future.) The model also implies that voters who are at least partially sophisticated about their

vote-selling temptation can use promises not to take money from candidates at all as a commitment device.

The pattern of our empirical results is consistent the case of the model in which voters are partially aware of their vote-selling temptation (neither fully aware nor fully nave of it). In the model, the worse performance of Promise 2 comes from respondents who would not have accepted money if they had been in the control group, but who (incorrectly) believe they can accept money without changing their vote due to making the promise. By contrast, a fully sophisticated voter correctly anticipates his temptation, so would not make this mistake. Fully nave voters would not increase their uptake of money offers due to the promise treatments, since they would accept money in the control treatment as well.

Our research is related to work on electoral malpractices more generally. Existing research has established, via natural experiments in a variety of contexts, that electoral malpractices have material influence on election outcomes (Golden and Tiwari, 2009; Acemoglu, Robinson and Santos, 2009; Baland and Robinson, 2008; Golden, Kramon and Ofosu, 2014). On the specific topic of vote-selling, research has shown it to be more prevalent among poor voters (Scott, 1969; Stokes, 2005; Blaydes, 2006; Bratton, 2008), and that parties, candidates and brokers are often strategic regarding which populations they target for vote-buying (Stokes et al., 2013). Khemani (2013) finds that the extent of vote-buying is negatively correlated with public health service delivery across municipalities in one Philippine province. Banerjee et al. (2011) find, in the context of a randomized controlled trial in urban India, that provision of "report cards" comparing electoral candidates reduces vote buying and leads to higher vote shares for higher-quality candidates. Finan and Schechter (2012) find that vote-buying payments in rural Paraguay are targeted to "reciprocal" individuals (as measured in an artefactual field experiment), suggesting that vote-buying exploits informal norms of reciprocity. Vicente (2014) conducted a randomized controlled trial

of an anti-vote-selling intervention, finding that it raised the vote share of incumbents, consistent with challengers' use of vote-buying to overcome incumbency advantages. Cruz, Keefer and Labonne (2015) find in the Philippines that provision of information to voters on candidates' spending priorities led those voters to be targeted for vote-buying.[4]

In its focus on the real-world impact of promises, this paper is also related to recent work from behavioral psychology and economics that shows that promises and other informal agreements can substantially change behavior and lead to more socially efficient outcomes by changing social norms (Charness and Dufwenberg, 2006; Vanberg, 2008; Kessler and Leider, 2012; Krupka, Leider and Jiang, 2013). Shu et al. (2012) show that the form of promise elicitation affects honesty in reporting of information in auto insurance applications. We also have a clear connection to research on temptation goods (Banerjee and Mullainathan, 2010; Fudenberg and Levine, 2006; Gul and Pesendorfer, 2001) and on self-control problems (Laibson, 1997; Ashraf, Karlan and Yin, 2006; Duflo, Kremer and Robinson, 2011; Kaur, Kremer and Mullainathan, Forthcoming).

In the next section we describe the experimental context and vote-buying practices in the Philippines. In Section 3 we describe the data collection and the experimental design. Section 4 discusses the proxy measure of vote-selling and the regression specification. Section 5 presents the empirical results. In Section 6 we present a behavioral model of vote-selling, which we use to demonstrate how the outcomes of the experiment are consistent with voters being partially sophisticated about the temptation to sell their vote in the future. Section 7 concludes.

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[4]There is of course a larger related literature on voter decision-making, separately from vote-selling or -buying. Olken and Pande (2011) survey recent research (using experimental and observational methods) demonstrating that voter behavior is highly malleable, and information provision in the context of elections can improve electoral accountability in developing country democracies. Recent studies of note include Wantchekon (2003); Ferraz and Finan (2008); Banerjee, Green and Pande (2012); Chong et al. (2011); Gine and Mansuri (2012); Beaman et al. (2009).

3.2   Context and Overview of Vote-Buying

The experiment was conducted in Sorsogon City, Sorsogon Province, Philippines. Sorsogon Province is located at the southern tip of Luzon island, roughly 12 hours by road from the national capital, Manila. Sorsogon City, with a population of roughly 150,000, is the provincial capital, and is slightly below the median across Philippine municipalities in terms of economic development. With a municipal poverty rate of 35%, it is slightly worse than the median (the 45th percentile, to be exact) poverty rate among Philippine municipalities.[5]

We study voting behaviors in the 2013 elections for Sorsogon City municipal positions (mayor, vice-mayor, and city council). The mayoral and vice-mayoral elections are the more important races at the local level. The mayor is the chief executive of the city government, and among its many powers (see Local Government Code of the Philippines 1991) is to direct the formulation of the city government plan, issue executive orders, and represent the city in all its business transactions and sign on its behalf all bonds, contracts, and obligations. The vice-mayor is the presiding officer of the city council and signs all warrants drawn on the municipal treasury for all expenditures appropriated for the operation of the council. The vice-mayor also appoints all officers and employees of the council. The city council has the legislative power, including the power to approve ordinances and pass resolutions necessary for an efficient and effective city government, as well as the power to approve or veto the annual and supplemental budgets of the city government.

Mayors and vice-mayors do not run in pairs, and winners sometimes come from different parties (often yielding a divided executive). City council members are elected from a single (district) constituency, using block voting: voters may vote for up to four councilors, with the top four vote-getters in a district being awarded council

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[5]Poverty rates are from 2003. The Philippines? overall poverty incidence is 29% (National Statistical Coordination Board 2009).

seats. Both the split-ticket mayoral and vice-mayoral race and the block vote system for city council seats tend to undermine the value of party affiliation (or running in a single ticket) and encourage individual candidates to develop personalized networks of support (Hicken, 2009).

As in many other parts of the Philippines, vote-buying is widespread in our study location. We define vote-buying as the offer of resources by political campaigns to individuals or households in order persuade them to vote for a particular candidate. This definition is consistent with the definitions elsewhere in the literature (e.g. Stokes et al., 2013; Vicente, 2014).

Most vote buying in Sorsogon City occurs in the week leading up to election day.[6] Using voter lists each campaign has developed, candidate representatives approach households directly, offering money or goods in exchange for their vote. Based on observations of our project field staff, vote-buying payments differed substantially across races. In the mayor and vice-mayor races, payments typically amounted to 250 to 500 Philippine pesos, while those for city council were in the range of 20 to 100 pesos.[7]

Vote buying is done systematically and strategically. Typically, each voter in a household will be offered a packet with their name on it, and campaigns track who accepted and who did not. Candidates may also engage in a second round of vote buying if they learn that a challenger is offering more money than they are. Campaigns seek to ensure that voters clearly associate the gift with their candidate. For example, the candidate's flyer may be stapled to packages of food handed out to voters or cash may be attached to flyer or letter from the candidate. Most commonly, candidates distribute money attached to a sample ballot, and encourage voters to

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[6]For some candidates vote buying may be the culmination of long-term efforts at cultivating voter loyalty via constituency service or other strategies.

[7]According to the Commission on Elections (COMELEC), they received reports of vote buying from all over the country during the May 2013 elections, with reported amounts ranging from P200 to P5000 (Flores, Jaymalin and Crisostomo, 2013). See also (Quijano, 2013).

take the ballots with them to the polls as a guide. The sample ballot includes not just the candidate's name, but also allied candidates from other races up and down the ticket. For further background, including images of sample ballots, please see Online Appendix A.

3.3 Experimental Design and Data Collection

We implemented a randomized controlled trial of treatments encouraging individual voters not to sell their votes. Study participants were registered voters in Sorsogon City. Participants were selected from the Certified List of Voters that we obtained from the Commission on Elections (COMELEC). The list included the name, address, date of birth, gender, and the assigned polling precinct of each of Sorsogon City's 84,284 registered voters.[8] From this list, we randomly selected 900 primary targeted respondents and 900 alternates.

Prior to fielding the baseline survey and intervention, primary respondents and alternates were randomly assigned to the control or treatment groups. One-third of individuals were randomly assigned to the control group, one-third to the Promise 1 treatment, and one-third to the Promise 2 treatment.

3.3.1 Baseline Survey and Voter Educational Video

The baseline survey and treatments were administered prior to the May 13, 2013 elections for Sorsogon City mayor, vice-mayor, and city council. A local team of enumerators administered the baseline survey, treatment interventions, and the endline survey. Surveys were administered on a hand-held device (an iPad) using an offline survey app (iSurvey). The baseline survey was fielded from April 17 to May 8, 2013 (5 to 26 days prior to the election).

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[8]The registration deadline for the May 2013 elections was October 2012, so this list was the complete list of registered voters for our election of interest.

Enumerators located primary respondents at their residential addresses, invited them to participate in the research study using a recruitment script (see Online Appendix B), and obtained consent to participate in the study. When a primary respondent could not be interviewed due to out-migration, refusal, or being deceased, the enumerator sought to interview an alternate respondent with the same treatment assignment.[9] Following this procedure, we generated a sample of 883 respondents, just slightly below the target sample of 900.[10]

The baseline survey was administered immediately before the experimental treatments, and asked questions about participants' demographics, past experience with vote-selling, expectations about monetary offers, and preference ratings for the candidates for mayor, vice-mayor, and city council. We also asked participants to rate each candidate for mayor, vice-mayor, and city council according to how favorable they felt towards each candidate on a 7-point Likert scale (-3 =extremely unfavorable, 0 =neutral, 3 =extremely favorable).

After completing the baseline survey, all participants were shown a three-minute video clip on the hand-held device. The video clip was part of a humorous voter education campaign encouraging viewers to turn out to vote, vote for honest and competent candidates, and avoid vote-selling.[11] The video clip was shown to all respondents to ensure that those in control and treatment groups received similar appeals not to sell their votes. This is important because the promise treatments, by themselves, might be construed as including an implicit suggestion not to sell one's vote. Our interest is in evaluating the effectiveness of the promise treatments

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[9]The list of alternates was sorted according to a randomly assigned number. When replacing primary respondents who could not be interviewed, enumerators picked alternates in the prescribed randomized order.

[10]In total, enumerators sought to locate 1,496 voters. Reasons for unsuccessful baseline surveys were as follows: failed to contact after repeated visits (170 voters), out of town (154), migrated out of Sorsogon City (92), refused (65), moved to unknown location (65), deceased (21), and other (27). This led to 902 voters being administered the baseline survey. Of these, 19 provided incomplete baseline responses, yielding our baseline sample of 883.

[11]The video clip features Mae Paner, a political activist and actress, as the fictional character "Juana Change." The video can be viewed here: http://www.youtube.com/watch?v=10Jh8Nzu7Zs

themselves, over and above appeals to eschew vote-selling. Making an explicit appeal to all respondents not to sell their votes (by showing the video) helps sharpen the interpretation of the treatments as being due to the promises elicited, and not due to any appeal not to sell one's vote that might be perceived as bundled with the promise elicitation.

3.3.2   Treatments

At the end of the voter educational video clip, respondents in the two treatment groups (Promise 1 and Promise 2) were invited to make promises not to sell their votes in the upcoming election, in ways that differed across the treatments.[12] Individuals in the Promise 1 treatment were asked to make a promise not to accept money from any candidate, while those in the Promise 2 treatment were asked to promise to vote according to their conscience even if they accepted money.

Elicitation of the promises was implemented by showing respondents a screen on the hand-held device. For Promise 1, the screen is reproduced as Figure 3.1a. The text on the screen reads "Would you promise not to take the money from any candidate or local leader before the elections?" For Promise 2, the screen image can be seen as Figure 3.1b, and the corresponding text is "If any candidate or local leader gives you money before the elections and you decide to keep it, would you promise to vote according to your conscience?"

On both screens, participants were asked to tap on either of the images shown in the figures to register their response. Tapping the left image (of a handshake, above the words "Yes, I promise.") would signify agreement to promise, and tapping the right image (of an open hand in a "halt" signal, above the words "No, I can?t make that promise.") would indicate refusal to promise.

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[12]The majority of vote-buying in Sorsogon happens in the few days in advance of the election, so the treatments were administered roughly two weeks prior to the period when vote-buying payments were made.

A participant who agreed to make the promise by tapping on the image of the handshake was then asked, on the next screen, to write the words "I promise" on a blank space using their finger (see Figure 3.1c.[13] After the signature, participants were asked two additional questions on politics and vote-buying, and the survey ended.[14]

3.3.3 Post-Election Survey

We fielded an endline survey of the same study participants from May 17 to June 8, 2013, a period spanning 4 to 26 days after the May 13, 2013 midterm election. The endline survey collected data on whether respondents voted (turnout), as well as which candidates they voted for in each race (mayor, vice-mayor, and city council). We achieved a high (95.9%) endline survey success rate, and this rate is not differential by treatment status (as discussed further below).

3.3.4 Initial Hypotheses

Based on previous research on promises and informal agreements, we anticipated that both promises would be effective in reducing vote-selling (Charness and Dufwenberg, 2006; Vanberg, 2008; Kessler and Leider, 2012; Krupka, Leider and Jiang, 2013). If voters have made the promise not to sell their vote, we expect that the social norm that it is important to keep one's promises will cause many voters to follow their promise ? and either turn down offers of money, or vote for their preferred candidate even if they receive money from other candidates. We expected that the primary difference between treatments would be in the uptake of the promise, with more voters predicted to make Promise 2 — since those voters could still accept money without breaking their promise.

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[13]On that screen, the text read "Thank you for your promise. As a symbolic act of your solemn promise, please write the phrase 'I promise' on the space below."

[14]These last two questions were also asked of participants who refused to promise as well as of those in the control group (who were not asked to make any promises).

3.4 Empirical Strategy

3.4.1 Proxy for Vote-Selling

The Philippines has a secret ballot, so measurement of vote-selling behavior is a first-order challenge. We did not ask participants directly whether they sold their votes, due to concerns about experimenter demand or social desirability bias. If individuals in the treatment groups underreported the extent to which they sold their votes, this would lead to spurious findings that the treatments reduced vote-selling.

Our approach instead is to simply ask participants in the endline who they voted for in the individual races (for mayor, vice-mayor, and city council), and to compare their reported votes in the endline survey with the candidate favorability ratings they reported in the baseline survey. Our key outcome variable is vote switching: an indicator equal to one for a particular election race if the respondent reported in the endline survey that they voted for a candidate who they did not rank highest in the baseline survey for that position (in the Likert-scale elicitation), and zero if they did say in the endline that they voted for their highest-rated candidate. We construct vote-switching indicators for each race separately, as well as indicators of whether the voter switched in any race.[15]

There are a number of reasons other than vote-selling why a voter may have voted for a candidate other than his or her top-rated candidate (e.g., learning new information.) We expect that such "legitimate" reasons should be unaffected by the promise interventions. Therefore, *differences* in vote-switching across treatment conditions should represent differences in vote-selling.[16]

A few comments are in order regarding the use of vote switching as a proxy for

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[15]For the city council race, in which each voter casts votes for four candidates, vote switching is defined as voting for at least one candidate who was not among their top four rated candidates.

[16]Another possibility is that voter preferences and actual voting could be misaligned due to strategic voting (Alvarez and Nagler, 2000). However, we find no obvious reason to believe that the promise treatments would affect strategic voting, so we simply consider strategic voting as another determinant of vote-switching that should be orthogonal to our treatments.

vote-selling. First of all, it is important that candidate favorability ratings in the baseline must provide an unbiased indication of participants' true preferences for candidates. This is likely to be satisfied: our survey staff presented themselves as neutral and unaffiliated with any candidate or political party, and favorability ratings were elicited before respondents were exposed to any of our promise treatments.

Second, it is important that our vote switching measure take into account bias that might be due to social desirability. It might be the case that voters are reluctant to appear to have broken a promise, and so they may be less willing to report voting for candidates that gave then money. In Section 6, we formalize social desirability bias within our model, and discuss empirical tests (comparisons of treatment effects across promises and across races) that are robust to the presence of social desirability bias. However, there is also an a priori case to be made that social desirability bias may not be large to begin with. In the endline survey we did not remind respondents that we had data on their candidate ratings from the baseline survey. It also requires a fair degree of sophistication for a respondent to recall that our project was about vote-selling, to recall their candidate preferences from the baseline, and to intentionally misreport their votes to be consistent with their original preferences. What's more, it should be far from clear to respondents that reporting in the endline that one voted for someone who was not their highest-rated choice in the baseline would be viewed by enumerators as ethically questionable, because vote-switching could occur for legitimate reasons (as mentioned above). Respondents, in essence, have "cover" to report at endline that they voted for someone other than their initially-preferred candidate, since such switches can occur for many reasons other than vote-selling.

In other work we analyze the plausibility of the vote switching measure by assessing whether it corresponds to relationships outside of the scope of our theory (Hicken et al., 2015). For example, as we would expect, we find that switching rates are higher when more money is offered, and voters are more likely to switch the narrower the gap

in preference between their most preferred candidate and the next best alternative.

If one believes that vote-switching is an acceptable proxy for vote-selling, and if our interventions are effective at reducing vote-selling, respondents in the treatment groups should do less vote switching (as defined above). If, on the other hand, any of our treatments led to more vote-selling, we should see increases in the rate of vote switching.

3.4.2 Regression Specification

We assess the effect of the promises on vote-switching by estimating the following ordinary-least-squares regression equation (a linear probability model):

$$y_{ij} = \alpha + \beta_{1j} Promise1_i + \beta_{2j} Promise2_i + X_i'\gamma + \epsilon_{ij} \tag{3.1}$$

y_{ij} is an indicator variable equal to 1 if the respondent switched his or her vote in race j, and 0 otherwise. $Promise1_i$ and $Promise2_i$ are indicator variables equal to 1 if the respondent was randomized into (respectively) the Promise 1 or Promise 2 treatment, and 0 otherwise. X_i' is a vector of baseline (pre-treatment) control variables. ϵ_{ij} is a mean-zero error term. We report robust (Huber/White) standard errors.

The coefficients of interest are β_{1j} and β_{2j} on the treatment indicators, which measure (respectively) the impact of treatment on the probability of vote-switching. To be clear, because making the promise is endogenous, we focus here on the effect of being in the promise treatment (being invited to make a promise), and not on whether the respondent actually made the promise. Our estimates are therefore intent-to-treat (ITT) effects.

3.5 Results

3.5.1 Summary Statistics, Baseline Balance, and Promise Take-Up

Panel A of Table 3.1 reports summary statistics for key baseline variables, in the full sample (column 1) and in the subsamples by treatment condition (columns 2–4). The columns to the right report, for each baseline variable, the p-values of F-tests of the joint equality of means across treatment conditions as well as for pairwise combinations of the treatment conditions. There is no indication of substantial imbalance in baseline characteristics across treatment conditions. Out of 100 p-values shown in Panel A, 10 are below 0.10, which is exactly the proportion that would be expected to occur by chance. To account for any biases generated by these chance imbalances, these baseline variables will be included as control variables in the regressions.[17]

Panel B of Table 3.1 reports similar summary statistics for promise-making and the key dependent variables of interest. The first row of this panel reports the fraction of respondents making the elicited promises in each treatment group. In each treatment group, slightly more than half of respondents make the promise—51% for Promise 1 ("Don't take the money") and 56% for Promise 2 ("Take money, vote conscience")—and these proportions are not different from one another at conventional levels of statistical significance.

3.5.2 Vote-Shares and Candidate Favorability Ratings

Table 3.2 provides relevant data for each candidate and electoral race. Candidates in bold are winners of their respective races, and starred candidates are incumbents. Reported vote shares in our sample (from the endline survey) correctly predict the actual winners in each race. The correlation coefficient between actual and sample-

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[17]Our results are robust to exclusion of the baseline control variables. Regression results and tests of theoretical predictions when control variables are not included in the regressions are presented in Appendix Tables B.2 and B.3, and should be compared with Tables 3.3 and 3.4 of the main text.

reported vote shares (columns 1 and 2) is 0.957.

Average favorability ratings across candidates from our endline survey are also highly correlated with vote shares. The correlation coefficient between the average favorability rating (column 3) and reported vote share in the sample (column 2) is 0.838 (and the corresponding correlation with the actual vote share in column 1 is 0.839). The remaining columns of the table display the distribution of discrete candidate favorability ratings, across the integers ranging from -3 to 3. There is considerable variation in candidate favorability ratings across our survey respondents across the range of possible responses.

3.5.3 Attrition from Baseline to Endline Surveys

To be included in the endline sample for analysis of a particular electoral outcome, a baseline respondent had to have: 1) completed the endline survey, 2) actually turned out to vote in the election, and 3) reported who they voted for in a given electoral race. If either treatment affected attrition (on any of these margins), one might worry that any observed treatment effects on vote-switching could be simply due to compositional changes in the sample. Out of the 883 baseline respondents, the share who completed the endline survey, voted, and reported their mayoral vote was 86.0%. The corresponding shares for vice-mayor and city council are 85.0% and 90.0%.

Differences in these measures of attrition across treatment conditions are very small, and none are statistically significantly different from zero, so attrition bias is of little concern in this context.[18] Please refer to the Appendix B.1 for further details.

3.5.4 Impact of Treatments on Vote-switching

We first present our results in graphical form. Figure 3.2 displays the bar graphs of the fraction vote switching, by treatment condition, with 95% confidence intervals.

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[18]In results available upon request, we also find that, among respondents completing the end line survey, there is no effect of either promise treatment on turning out to vote.

Figure 3.2a presents the share of respondents who switched votes in at least one of the races. In the control group, 57.4% of subjects switched their vote at least once, compared to 50.4% in the Promise 1 treatment, and 61.8% in the Promise 2 treatment. This provides a first indication that the promise treatments had opposite effects, with asking subjects not take money from candidates reducing the amount of vote switching, while asking subjects to vote their conscience even if they take money increases the amount of vote switching.

Figures 3.2b and 3.2c examine vote switching separately in, respectively, the mayor/vice-mayor races and the city council race. In the mayor/vice-mayor races, vote switching rates are very similar in the control and Promise 1 groups (26.4% and 27.1%, respectively), but higher in the Promise 2 group (33.7%). By contrast, for the city council race, the control and Promise 2 groups have similar vote switching rates (47.1% and 47.7%, respectively), while the rate for the Promise 1 group is much lower, at 38.4%.

To confirm these visual impressions, we now turn to estimation of regression equation (1) for vote switching in different races. Results are presented in Table **??**.

In column 1, the dependent variable is vote switching in any race. As in Figure 3.2a, the coefficient for the Promise 1 treatment is negative, while the coefficient for Promise 2 is positive. The negative coefficient on Promise 1 is statistically significantly different from zero at the 5% level.

In columns 2-4 we examine treatment effects in specific races (mayor, vice-mayor, and city council, respectively). These results reveal that the estimated effect on overall vote switching estimated in column 1 obscures heterogeneity of treatment effects across races.

In regressions for vote-switching in the mayor or vice-mayor races, the coefficient on Promise 1 is always relatively small in magnitude and negative in sign. The coefficient on Promise 2, on the other hand, is larger in magnitude, and positive in

sign in both cases. None of these promise treatment effects for the mayor or vice-mayor races are statistically significantly different from zero at conventional levels.

The pattern is quite different in analysis of vote switching in the city council race (column 4). The Promise 1 treatment has a large, negative and statistically significant (at the 5% level) effect on vote switching, amounting to a 10.9 percentage point reduction. The corresponding coefficient for Promise 2 is also negative but, by contrast, is very small in magnitude and is not statistically significantly different from zero.

3.5.5 Estimating the Amount of Social Desirability Bias

Examination of individual treatment coefficients may be misleading, because social desirability bias may lead estimated promise treatment effects to be biased in a negative direction (i.e., for treatments to appear to reduce vote switching). As discussed in Section 3.6, however, we formalize what effect social desirability should have, and present tests of differences in treatment effects across promises and across races that are robust to the presence of social desirability bias.

In addition, examining the impact of the Promise 1 treatment in the more important races can provide an indication of the likely magnitude of social desirability bias. If social desirability bias is constant across electoral races (e.g., a constant multiplicative factor p applied to vote-switching rates in all races, as discussed in Section 3.6), and if true Promise 1 treatment effects are zero for the higher-money races (mayor and vice-mayor), then the Promise 1 treatment effect in the higher-money races provides an estimate of the magnitude of social desirability bias. If the Promise 1 treatment does have some true effect on reducing vote-switching in the higher-money races, then this estimate will be an upper bound of the true magnitude of social desirability bias. Also, if social desirability bias is not constant across electoral races but is larger in the more important races (mayor and vice-mayor), this estimate will also be an upper

bound.

The pattern of Promise 1 treatment effects across races reported in Table 3.3 indeed suggests that social desirability bias is not a significant concern in this setting. Promise 1 treatment effects in the mayor and vice-mayor races, while both negative, are quite close to zero, and neither are statistically significantly different from zero. The coefficient in the mayor's race is worth particular attention, since the mayor's race is the most important race of the three. The coefficient (-0.003) indicates a reduction in vote-switching of three-tenths of a percentage point, which is very small relative to the control group mayoral election vote-switching rate of 10.6 percent. Assuming no "true" Promise 1 treatment effect on vote-selling in the mayor's race, taking this coefficient seriously would imply p = 0.97 (social desirability bias leads the self-reported vote-switching rate to be 97% of the true vote-switching rate). Given this, we conclude that our estimated treatment effects (the β_{ij} estimates) are likely to be minimally affected by reporting bias.[19]

3.6   Theoretical Model

We describe here a simple model of vote selling and the impact of promises not to sell one's vote. We focus here on intuition and results, and discuss additional details in the Appendix. We present here one potential mechanism that is consistent with the results we observe. We discuss which features of the model are important to explaining our results, and examine other potential models that cannot generate our results.

We take as a starting point the findings of Finan and Schechter (2012) that vote-buying operates through a reciprocity channel. This is an appropriate assumption in our setting, since as described previously the Philippines uses electronic balloting and

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[19]An analogous assessment of the magnitude of social desirability bias is not possible for the Promise 2 treatment, because it is theoretically possible for that treatment to have a true positive effect on vote-switching, alongside any negative reporting bias due to social desirability effects.

therefore candidates cannot verify the vote of an individual voter. However, we model a voter's inclination to reciprocate a candidate's gift through voting as a temptation, rather than an intrinsic part of the voter's utility. If the reciprocity of vote-selling was a preference of the voter, then the promises we study can only have a beneficial effect, and the promise to "take money but vote your conscience" (Promise 2) would dominate the promise not to accept money (Promise 1), both in terms of uptake and in effect on voting. By modeling vote-selling as a temptation problem, we allow for Promise 2 to have either a positive or negative effect, and to be more or less effective than Promise 1. We then identify cases where Promise 2 can have a negative effect, and be less effective than Promise 1.

There are three time periods in the model: $t = 0$, 1 and 2. At time $t = 0$, the voter is asked to make either Promise 1 or 2, and decides whether or not to make the promise. At time $t = 1$ electoral candidates offer the voter money, and the voter decides to accept or reject the offers. At time $t = 2$ the voter votes for one of the candidates. Voters will receive utility at time $t = 2$ based on their choices, however because vote-selling is modeled as temptation, at times $t = 0$ and 1 voters will project forward a different utility function than the $t = 2$-self will use. We will work through the model by backwards induction. We will first identify what vote a voter will cast at $t = 2$ (based on previous decisions), then identify what gifts a voter will accept at $t = 1$ (anticipating the behavior at $t = 2$), and finally show whether voter would make a promise at $t = 0$ (given his anticipated behavior at $t = 1$ and 2).

There are two candidates (Candidate 1 and Candidate 2).[20] The voter has an intrinsic value v_i for voting for Candidate i. Without loss of generality, let $v_1 > v_2$. At time $t = 1$, each Candidate i offers the voter a "gift" of value g_i, which he may accept or reject. We model the influence of these gifts on the voter's voting decision at

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[20]We focus on two candidates for ease of exposition. With multiple candidates there will be one alternative candidate that is the most tempting to switch to (from a combination of large gift and/or high intrinsic value), so the analysis will be similar to the two candidate case.

time $t = 2$ as a self-control problem. At the time of voting, the voter receives psychic utility $\phi * g_i$ for voting for Candidate i (ϕ measures intrinsic reciprocal motivation).[21] Hence the $t = 2$ self has utility from voting for Candidate i after having received gifts g_1 and g_2 of:

$$U_i^2 = v_i + \phi * g_i + (g_1 + g_2) \tag{3.2}$$

Given these preferences, if Candidate 2 gives the Voter (and he accepts) a gift that is sufficiently larger than the gift from Candidate 1, he can "buy" the voter's vote. A voter will 'switch" his vote to Candidate 2 (i.e. vote against his intrinsic preferences) if his intrinsic reciprocity is sufficiently strong:

$$\phi(g_2 - g_1) - (v_1 - v_2) > 0 \tag{3.3}$$

Hence vote-selling is more likely when there is a larger difference in the gifts given, when the voter feels more reciprocal towards gift-giving candidates, and when the underlying difference in intrinsic preferences is smaller.

Since we are modeling reciprocity-based vote switching as a temptation, at the time of accepting money from candidates the $t = 1$ self does not feel reciprocal - he values only the intrinsic value of voting and the consumption value of receiving money:

$$U_i^1 = v_i + (g_1 + g_2) \tag{3.4}$$

Hence preferences at $t = 1$ reflect the "cold" state without temptation, while preferences at $t = 2$ reflect the "hot" state with temptation.[22] We assume that the voter may be (partially) sophisticated about his temptation - at time $t = 1$ the voter in the

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[21]Note that we assume that this reciprocal desire to vote for a gift-giver does *not* change the underlying voting preferences of the voter (otherwise asking voters to "vote your conscience" wouldn't affect their behavior).

[22]We also considered a simpler model where the voter does not have a self-control problem (i.e. he has stable preferences over time). In this case, promises (of either kind) either reduce vote-selling, or have no effect. We consider reciprocity as a self-control problem to allow the promise to vote your conscience to actually increase vote-selling.

cold state believes his intrinsic reciprocity at $t = 2$ will be $\phi' \leq \phi$.

3.6.1 Promises

With our experimental intervention, the voter is asked at time $t = 0$ to promise not to sell his vote.[23] Specifically, we ask some voters to promise not to accept money from candidates, and other voters to promise to "vote your conscience" even if they accept money. We describe here how these promises not to vote-sell affect utility.[24]

At $t = 0$ the voter has the same "cold" state preferences as at $t = 1$. Additionally, we assume the voter receives psychic utility γ for taking an action consistent with a promise, and receives *dis*utility γ for breaking a promise.[25] This could reduce vote-selling in two ways, depending on the promise. An effective promise not to accept money would make $g_1 = g_2 = 0$, causing (3.3) to fail. A promise to vote one's conscience would lead to vote-switching if

$$\phi(g_2 - g_1) - (v_1 - v_2) > 2\gamma \tag{3.5}$$

Hence if a voter was only marginally willing to switch his vote, a promise might cause him not to switch.

We next consider more formally the conditions under which each promise will have an effect on vote-selling. We will first discuss conceptually the fully sophisticated

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[23]For simplicity we assume that at time $t = 0$ the voter knows how much he will be offered. The model would also work if instead the voter had a belief distribution over payment offers. The predicted uptake would depend on the relative mass that the beliefs assign to the cases described below.

[24]We note that both promises are consistent with existing anti-vote-selling messages and broader moral sentiments and norms. Our model is not intended to describe arbitrary promises.

[25]This is consistent with the results of Krupka, Leider and Jiang (2013) who show that making a promise to take a particular action increases the normative appropriateness of taking the agreed upon action, and decreases the appropriateness of all other actions. Many previous models of promise-keeping include only the negative penalty for violating a promise (Chen, Kartik and Sobel, 2008; Ellingsen and Johannesson, 2004; Ozer, Zheng and Chen, 2011). For our purposes, the key assumption is the positive utility for following a promise, otherwise no voter would make a promise. We include the negative utility penalty for violating the promise to maintain consistency with the previous literature, but we would obtain similar results without it. We make the two utility impacts of equal magnitude for simplicity.

$(\phi' \neq \phi)$ and fully naïve $(\phi' = 0)$ cases to build intuition, and then formally consider the general model which includes these as special cases.

3.6.2 Fully Sophisticated Voters

As previously mentioned, fully sophisticated voters correctly anticipate the magnitude of their temptation to vote based on receiving gifts. Such a voter will always accept gifts from Candidate 1, and small gifts from Candidate 2 that will not affect his vote. For large gifts the value of the gift must outweigh the cost of changing his vote. Hence when there is no promise request, we expect vote-switching in cases where candidates offer a large gift, and where voters have small preference differences between candidates compared to their reciprocity.

A fully sophisticated voter also correctly anticipates the effect of making a promise. Suppose a voter was going to accept both gifts and change his vote. For him to be willing to promise not to accept money, he must be willing to turn down both gifts in order to keep his promise, and must place higher utility on keeping his promise plus voting for his preferred candidate more than the value of the gifts. Similarly, to make the promise to vote his conscience, the utility of keeping his promise plus voting for his favorite candidate must exceed the value of the gift from 2. Hence both promises can reduce vote selling for sophisticated voters if the costs of violating a promise are sufficiently large.

3.6.3 Naïve Voters

Naïve voters do not expect to be tempted at all. Hence they will always accept any gift, and will change their vote if the gift from Candidate 2 is sufficiently large. Since the naïve voter does not expect to switch, and therefore does not expect the promise to affect his vote, he will make Promise 1 if the utility of the promise exceeds the value of both gifts alone (without voting utility.) However, once the promise is

made it will be effective. A naïve voter will always make Promise 2, since he thinks he will always be able to vote his conscience. He will then accept both gifts. He will ultimately keep his promise if the utility of keeping his promise is sufficiently strong, i.e. if (2) holds. Hence both promises can also be effective for naïve voters.

3.6.4 Partially Sophisticated Voters

We now consider the intermediate case of a partially sophisticated voter[26]—i.e. a voter who recognizes the reciprocal temptation he feels during voting, but underestimates its strength. Specifically, at times $t = 0$ and $t = 1$, the voter believes that his intrinsic reciprocity at $t = 2$ will be ϕ', with $0 \leq \phi' \leq \phi$. As ϕ' approaches zero the voter will act more like a naïve voter, while as ϕ' approaches ϕ the voter will act more like a fully sophisticated voter.

3.6.4.1 No Promise

A partially sophisticated voter will always accept a gift from Candidate 1, since that will only reinforce his candidate preferences. Whether he will accept a gift from Candidate 2 depends first on whether he thinks it will affect his vote. The voter will believe that accepting the gift from Candidate 2 will change his vote if:

$$\phi'(g_2 - g_1) - (v_1 - v_2) > 0 \tag{3.6}$$

A partially sophisticated voter will accept any gift that he does not expect to affect his vote, i.e. if (3.6) does not hold.[27] Like a fully sophisticated voter, he will accept a gift that he does expect to change his vote if it is sufficiently large: $g_2 > v_1 - v_2$. Hence as with the other cases discussed above, we expect more vote-switching when

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[26]See the Appendix for additional details.

[27]Note that since a partially sophisticated voter always underestimates the impact of a gift, he will always accept a gift from Candidate 2 that will, in reality, not affect his vote, i.e. (3.3) does not hold.

candidate gifts are larger.

3.6.4.2 Promise 1

We now consider what impact a promise not to accept money would have on a voter's behavior. We want to identify cases where the promise reduces vote-selling, so we focus on the case where absent a promise the voter will accept the gift from Candidate 2 and switch his vote. As discussed above, there are two cases where this occurs: relatively naïve voters who accept money because they do not expect to be affected, and relatively sophisticated voters who accept money because the value of the gift exceeds the difference in their candidate preferences.

Case1: Relatively Naïve Voter - (3.6) Does Not Hold   In this case the voter acts like the naïve voter discussed above. The voter anticipates accepting both gifts and still voting for Candidate 1 if he does not make the promise, so he will only make the promise if the utility from following the promise outweighs the value of the gifts:

$$\gamma \geq g_1 + g_2 \tag{3.7}$$

Case2: Relatively Sophisticated Voter - (3.6) Holds and $g_2 > v_1 - v_2$ Here the voter acts like the sophisticated voter discussed above. When considering whether to make the promise, the voter will make the promise if the utility from making and keeping the promise not to accept money exceeds the utility he expects to receive for not making the promise, which will be true if:

$$\gamma \geq g_1 + g_2 - (v_1 - v_2) \tag{3.8}$$

As before this condition is also sufficient for the promise to be effective once made.

3.6.4.3 Promise 2

At $t = 1$ a partially sophisticated voter who has promised to vote his conscience will anticipate that a gift from Candidate 2 will change his vote if

$$\phi'(g_2 - g_1) - (v_1 - v_2) > 2\gamma \tag{3.9}$$

Again, we focus on the case where the gift from Candidate 2 is large enough to change his vote if there is no promise. As above we consider relatively naïve voters and relatively sophisticated voters.

Case1: Relatively Sophisticated Voter - (3.9) Does Not Hold In this case, having made the promise to vote his conscience the voter does not expect to switch even if he accepts the gift from Candidate 2. Therefore, he will accept both gifts. Additionally, the voter will make the promise, since he expects to keep it. The question, then, is when will the promise *reduce* vote-selling compared to the base case? For the promise to make a difference we need the voter to switch his vote in the base case, that is (3.3) holds and either (3.6) doesn't hold or g_2 is large enough. Additionally, we need the promise to prevent the vote-switching, i.e., (3.5) does not hold. Therefore, we need the promise-keeping utility to be sufficiently strong and/or the reciprocal preference to be in an intermediate range.

Case1: Relatively Sophisticated Voter - (3.9) Holds In this case the voter expects to switch his vote if he accepts both gifts, and since $\phi' leq \phi$ he must be correct, i.e. (3.5) holds as well. In that case, when offered the gift by Candidate 2 he is choosing between accepting it and voting for 2 ($U = v_2 + g_1 + g_2 - \gamma$) or only accepting the gift from Candidate 1 and voting for 1 ($U = v_1 + g_1 + \gamma$). He will accept

the gift if $g_2 - (v_1 - v_2) > 2\gamma$, so for the promise to prevent vote-switching we need:

$$\gamma \geq g_2 - (v_1 - v_2) \qquad (3.10)$$

So as in the previous case the promise to vote your conscience can be effective if the utility from keeping a promise is sufficiently strong.

3.6.5   Harmful Promise to Vote Your Conscience

Unlike the promise not to accept money, the promise to vote your conscience can actually have a negative effect and *increase* the amount of vote-switching. This can happen if the voter is sophisticated enough to turn down the gift from Candidate 2 absent a promise, but naïve enough to mistakenly believe that the promise will prevent him from switching. Specifically, this outcome can happen if (3.6) holds and g_2 is not too large ($g_2 < v_1 - v_2$), so without the promise the voter correctly recognizes that accepting the gift will change his vote, and he is willing to turn down the money. However, if (3.9) does not hold, then the voter thinks that after making the promise the gift will no longer affect his vote. We saw previously that in this case the voter will always accept the money, and also will always be willing to make the promise. If (3.5) does hold, then the voter is wrong—after accepting the money he will in fact change his vote. If these conditions hold then the promise actually *increased* the amount of vote-switching–the voter did not switch his vote in the base case, but does switch his vote when asked to make the promise. Furthermore, this negative effect for the promise to vote your conscience can occur simultaneously with more positive effects for other promises and races.[28]

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[28]For example, consider two elections—one with low gift sizes and one with larger gifts. Suppose that $(v_1, v_2) = (10, 1)$ for both races, $(g_1, g_2) = (2, 4)$ for the low gift race, $(g_1, g_2) = (4, 8)$ for the high gift race, $\phi' = 2.5$, $\phi = 12$, and $\gamma = 7$. Then it is straightforward to show that in the low stakes race the promise not to accept money decreases vote-selling compared to the base case, while the promise to vote your conscience has no effect. Similarly, in the high stakes race the promise not to accept money has no effect, while the promise to vote your conscience increases vote-selling.

3.6.6 Reported Voting

So far we have considered how the promises affect actual voting behavior. However, in our experiment we only observe subjects' self-reports of their votes. One might imagine that subjects may distort their reported votes due to social desirability bias. We discuss here how such biases could change reported voting, and whether they could generate our anticipated results.

We anticipate that subjects' initial candidate ratings will be an accurate reflection of their underlying preferences. The candidate ratings occur before subjects know they will be asked to promise not to sell their vote. Hence voters cannot rate a candidate as their favorite in advance so that they can appear to keep their promises by appearing not to switch their votes.

If our model is correct and subjects report their voting truthfully, then the conditions described above should identify which voters will switch their vote. With truthful reporting this will coincide with the observed switching. How might social desirability bias change these reports? One natural form of social desirability bias would be for any voter to be reluctant to report switching their vote, due to the general norm against vote selling.[29] A straightforward way of representing this would be to assume a voter who actually switched his vote will only report switching with probability $p < 1$. In this case the observed switching rates would be biased towards zero. However, there will only be a difference in observed switching rates if there is a difference in true switching rates. Therefore this form of bias cannot create the treatment differences our model predicts.

If instead the bias affects any voter asked to make a promise, then the observed switching rates in both promise treatments might be affected. However, comparisons between races can demonstrate that the promise must be having an effect. If the bias affects both races equally, then any difference between races in a promise treatment

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[29]In the appendix we include a simple formalization of this intuition.

can only be generated by differences in true switching rates. If voters exhibit different biases depending on the race, we would generally expect larger biases in the more important races than in the less important races.[30] In this case we would expect larger biases for the mayoral and vice-mayoral election compared to the city council election, and hence the apparent effect of the promise would be most beneficial in the higher stakes elections. In order to observe that the promise was more effective in the city council race, voters would have to feel more uncomfortable appearing to break their promise in the city council election than in the mayoral election, which seems unlikely. Furthermore, as before this kind of bias can only make the promises look beneficial, and would never make them look harmful.

3.6.7 Predictions: Differentiating Between Theoretical Cases

Empirical evidence on the impact of Promises 1 and 2 on vote-switching behavior can distinguish among the model's theoretical cases. Our empirical analysis will estimate the impact of each promise on vote-switching in three different electoral races. Let β_{ij} be the impact of promise $i \in [1, 2]$ on vote-switching in electoral race $j \in [m, v, c]$ (mayor, vice-mayor, and city council).

As discussed above, there is a concern that social desirability effects could bias the treatment effects β_{ij} in the negative direction, raising concerns about spurious findings for individual β_{ij} estimates. If, however, social desirability bias is similar across the promise treatments (which seems reasonable), focusing on the *difference* between impacts of Promises 1 and 2 ($\beta_{1j} - \beta_{2j}$) should net out social desirability bias. We therefore highlight model predictions regarding the difference in impacts across promise treatments, $\beta_{1j} - \beta_{2j}$.

Both the fully sophisticated and fully naïve cases make the predictions that (1)

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[30]This assumption is consistent with models of promises and experimental evidence on lying. For example, Erat and Gneezy (2011) find that lying rates for "white lies" depend on the payoff consequences of the lie. Similarly, Miettinen (2013) models the guilt from breaking a promise as increasing in the payoff consequence of the promise violation.

both promise treatments should reduce vote-switching ($\beta_{ij} < 0, \forall i, j$), and (2) the impact of the Promise 2 treatment will be larger in magnitude ($\beta_{1j} - \beta_{2j} > 0, \forall j$).

In the partially sophisticated case, on the other hand, the predictions are different. The Promise 1 treatment should reduce vote-switching ($\beta_{1j} < 0, \forall j$), but the impact of the Promise 2 treatment on vote-switching can be either negative ($\beta_{2j} < 0, \forall j$) or positive ($\beta_{2j} > 0, \forall j$). The difference in the impact of Promise 1 from Promise 2, $\beta_{1j} - \beta_{2j}$, can therefore be positive or negative.

The following table summarizes the model's predictions in each case:

	Partially Sophisticated	Fully Sophisticated	Fully Naïve
β_{1j}	< 0	< 0	< 0
β_{2j}	> 0 or < 0	< 0	< 0
$\beta_{1j} - \beta_{2j}$	> 0 or < 0	> 0	> 0

A finding that Promise 1 reduces vote-switching more than does Promise 2 ($\beta_{1j} - \beta_{2j} < 0$) can only be generated by the partially sophisticated case, not the fully sophisticated or fully naïve cases.

3.6.8 Predictions: Differential Effects Across Races

The model also makes predictions regarding the relative effects of the promises across electoral races that involve different sizes of vote-buying payments (gifts). In our context, the mayor and vice-mayor races involve larger vote-buying payments, compared to the city council races.

For Promise 1, which can only reduce vote-switching, the model predicts that the treatment will have more negative effects for races that involve smaller vote-buying payments (in other words, the city council race, compared to either the mayor or vice-mayor race): $\beta_{1c} - \beta_{1m} < 0$ and $\beta_{1c} - \beta_{1v} < 0$.

Promise 2 can either have a positive or negative effect on vote-switching, and there is no unambiguous prediction as to the relationship between the Promise 2 treatment effect magnitude and the size of vote-buying payments. The fully naïve, fully sophisticated and partially sophisticated cases are all potentially consistent with finding a more negative effect of Promise 2 in the city council election. However, if the Promise 2 treatment leads to an *increase* in vote-switching in races with larger vote-buying payments (the mayor and vice-mayor races) and either no effect or a decrease in vote-switching in the city council race, then this pattern is informative because it only occurs in the partial sophistication case of the model (not the fully sophisticated or fully naïve cases).

Again, these comparisons across races are robust to the presence of social desirability bias, if this bias is constant across races. If social desirability bias is different across races it is more likely that it would be larger (the bias would be more negative) in the more important races (mayor and vice-mayor), in which case social desirability bias makes it more difficult to reject the null in these tests for differential treatment effects across races. Finding that Promise 1 has a more negative effect in the city council race and/or that Promise 2 has a more positive effect in the mayor and vice mayor races is therefore evidence that our results are not entirely driven by social desirability bias.

3.6.9 Test of Theoretical Predictions

We turn our attention to tests of theoretical predictions. These involve pairwise comparisons of treatment coefficients across promises and races, and are summarized at the end of Section 3.

3.6.9.1 Effects More Negative for Promise 1 than Promise 2 Treatment

Part A of the table tests predictions of the partially sophisticated theoretical case, which unlike the other cases (fully sophisticated and fully naïve), is the only case that predicts a particular pattern found in Table 3.3's regression results: that Promise 1 has a more negative impact on vote-switching (reduces vote-switching more) than does Promise 2. We first conduct this test across treatment effects in the vote-switching regression pooled across races (coefficients in column 1 of Table 3.3). The difference in coefficients is negative and statistically significant at the 5% level. When conducting this test separately for each race, we find that for each race Promise 1 has a more negative impact than Promise 2: $\beta_{1j} - \beta_{2j} < 0$ in each race j. For the city council race, the difference is statistically significantly different from zero at the 5% level.

To test whether the theoretical prediction that Promise 1's impact is more negative than Promise 2's holds across all races considered simultaneously, we conduct an F-test of the joint hypothesis that $\beta_{1m} - \beta_{2m} = 0$ and $\beta_{1v} - \beta_{2v} = 0$ and $\beta_{1c} - \beta_{2c} = 0$. We reject this hypothesis at the 1% level (the p-value, reported in the bottom row of Part A of Table 3.4, is 0.005). This result provides statistical confirmation that the full set of empirical results is consistent with the partially sophisticated case, and not the fully sophisticated or fully naïve cases of the model.

3.6.9.2 Effects More Negative for City Council than in either Mayor or Vice-Mayor Races

The theoretical model predicts that the impact of the Promise 1 treatment on vote-switching will be more negative for the race with the lower vote-buying payments (the city council race) than for those with higher vote-buying payments (the mayor and vice-mayor races). The prediction regarding differentials in Promise 2's effects across races is ambiguous; Promise 2's effect could be either higher or lower in the city council race compared to the other races.

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We conduct pairwise tests of the differential effects of the treatments across electoral races in Part B of Table 3.4. The results reveal that, within each promise treatment, pairwise differences in treatment effects between the city council regression, on the one hand, and either the mayor or vice-mayor regression, on the other, are all negative in sign. As discussed previously, these differences are statistically significant at the 10% level or better for the Promise 1 comparisons. While the Promise 2 cross-race tests are not statistically significantly different from zero, the negative point estimates for the differences and the positive point estimates on the Promise 2 treatment coefficient (Table 3.3, columns 2 and 3) can only occur in the partial sophistication case of the model (not the fully sophisticated or fully naïve cases.)

As an overall test whether the prediction that each promise treatment is more negative for the city council race than in the other races, we conduct an F-test of the joint hypothesis that $\beta_{1c} - \beta_{1m} = 0$ and $\beta_{1c} - \beta_{1v} = 0$ and $\beta_{2c} - \beta_{2m} = 0$ and $\beta_{2c} - \beta_{2v} = 0$. This hypothesis is rejected at the 10% level (the p-value, reported in the bottom row of Part B of Table 3.4, is 0.086).

3.6.9.3 Test of Joint Significance of All Pairwise Treatment Effect Differences

Finally, we conduct an F-test of the joint significance of all the pairwise tests examined in Parts A and B. We reject at the 1% level the hypothesis that the pairwise treatment effect differences examined in Parts A and B are jointly zero (the p-value is 0.008, reported in Part C of Table 3.4).

3.6.10 Alternative Mechanisms

The above model is presented as one potential mechanism that is consistent with our results, and as a way of being concrete about the potential impact of social desirability bias. Alternative mechanisms are certainly possible, but in order to explain

our results an alternative mechanism would need to have two features. First, any alternate mechanism needs to explain why switching is more likely, and the effects of the promises are less positive (more negative), for races and candidates that offered more money to voters. Second, the alternative mechanism needs to explain why Promise 2 would be less effective than Promise 1, and increase the amount of vote-switching relative to the Control group. Therefore, an alternative mechanism needs to predict that (a) some voters will turn down money in the Control group, and (b) the mechanism by which money from candidates affects votes increases with the amount of money offered. The first feature explains how Promise 2 increases vote-switching, and the second feature explains the cross-race and cross-candidate differences.

Some potential mechanisms would have these features. For example, while we think that vote-buying primarily operates through reciprocity, to the extent that political brokers can exert coercive pressure on voters (c.f. Cruz, 2013), we would expect this to have similar effects. Voters would want to avoid such pressure—and hence may turn down money. Additionally, we would expect that brokers would exert more pressure on voters for more important races. Finally, one can imagine that voters may underestimate the amount of pressure they will face.

However, other mechanisms would not have the required features. For example, the sample ballots changing votes primarily through providing voters information would not explain our data. Sample ballots are often provided to voters along with money and it is possible that those ballots could act as useful cues to voters when casting their votes. This could help explain why those who agree to Promise 2 (and thus receive a sample ballot) are more likely to switch than those who agree to Promise 1. However, the sample ballot story cannot account for other behaviors that are successfully predicted by our model. For example, voters would have no reason to turn down money to avoid such information, and having received the information there is no particular reason to expect different effects across races.

Another alternate mechanism would be to assume that vote-selling operated through regular reciprocity (i.e. not temptation), but that voters' initial favorability reports incorporate both their true underlying preference and the anticipated monetary offers from each candidate. In this case Promise 2 would actually lead more voters to vote for their true preferred candidate, hence the apparent increase in switching would be an improvement in voting fidelity. However, in this model there would never be any apparent switching in the Control group (since voters have already factored in their vote-selling into their initial reports), and Promise 1 would also cause the same increase in apparent switching as Promise 2 (since eliminating the monetary payments also leads voters to vote for their true favorite).

3.7   Conclusion

We report the results of a randomized controlled trial of an anti-vote-selling intervention in the Philippines. We randomly assigned individual voters to treatments that invited them to make particular promises intended to reduce vote-selling. Across promises and across electoral races, we found unexpected patterns of impacts on a proxy measure of vote-selling. We outline a behavioral model of transactional electoral politics that makes sense of the results. In the model, selling one's vote is a temptation good, generating utility for the future self upon the vote-sale, but not for the present self who anticipates later selling his or her vote. We allow keeping or breaking promises to have utility consequences, so voters can use promises related to vote-selling as a commitment device. The model predicts that a promise not to take money from candidates can reduce vote-selling, but a different type of promise (to take vote-buying payments, but to nonetheless vote according to one's underlying candidate preferences) can have a smaller effect, and even possibly increase vote-selling, if voters are partially naive about (underestimate) their vote-selling temptation. Our empirical results are consistent with the case wherein voters are partially nave about

their vote-selling temptation. The results rule out full sophistication as well as full navet about one's vote-selling temptation.

From a policy standpoint, our results reveal that exceedingly simple interventions — such as eliciting promises not to sell votes — can help reduce vote-selling. We estimate that a promise not to take money from candidates leads to a reduction in vote-switching (our proxy for vote-selling) of 10.9 percentage points (compared to a rate of 47.1 percent in the control group) in the electoral race that involved the smallest vote-buying payments (the city council race). Patterns in the results for other races indicate that this treatment effect estimate is likely to be minimally biased by social desirability effects. We find no evidence that promises help reduce vote-selling in the races (for mayor and vice-mayor) in which vote-buying payments are larger.

These results reveal that approaches from behavioral economics or psychology can help us understand important phenomena in political economy, such as vote-selling transactions. Future research would do well to incorporate the behavioral factors we have highlighted into theoretical and empirical analyses of transactional electoral politics, and of vote-selling in particular.

Tables

Table 3.1: Baseline survey summary statistics and balance tests

| | | | Treatment groups | | | p−values | | |
	Full sample	Control group	Promise 1 ("Don't take money")	Promise 2 ("Take money, vote conscience")	C = P1 = P2	C = P1	C = P2	P1 = P2
Number of observations	883	291	298	294				
Panel A: Baseline variables								
Male (indicator)	0.450	0.471	0.426	0.452	0.550	0.277	0.656	0.521
Years of age	42.02	43.56	41.61	40.90	0.132	0.159	0.049	0.587
	(16.29)	(17.15)	(16.35)	(15.25)				
Religion is Catholic (indicator)	0.922	0.911	0.926	0.929	0.697	0.492	0.426	0.911
Number of voting household members	3.55	3.62	3.62	3.42	0.319	0.994	0.218	0.177
	(1.93)	(2.14)	(1.81)	(1.84)				
Single	0.258	0.251	0.269	0.255	0.879	0.627	0.906	0.712
Married	0.526	0.526	0.517	0.534	0.916	0.827	0.842	0.675
Widowed	0.075	0.083	0.071	0.071	0.836	0.584	0.617	0.964
Domestic partnership	0.123	0.117	0.138	0.116	0.673	0.451	0.964	0.423
Separated	0.018	0.024	0.007	0.024	0.094	0.088	0.985	0.091
Choose not to work	0.239	0.227	0.285	0.204	0.064	0.104	0.505	0.022
Retired	0.046	0.065	0.03	0.044	0.135	0.046	0.263	0.369
Student	0.045	0.048	0.044	0.044	0.962	0.795	0.823	0.972
Unemployed, looking	0.099	0.089	0.107	0.099	0.763	0.463	0.701	0.727
Working full-time	0.324	0.357	0.269	0.347	0.035	0.02	0.792	0.039
Working part-time	0.247	0.213	0.265	0.262	0.246	0.139	0.165	0.93
Some elementary to no schooling	0.12	0.127	0.114	0.119	0.888	0.627	0.766	0.756
Elementary	0.176	0.151	0.201	0.174	0.279	0.11	0.466	0.386
Some highschool	0.193	0.21	0.198	0.17	0.448	0.727	0.223	0.381
Highschool	0.168	0.165	0.161	0.177	0.869	0.899	0.702	0.609
Some college	0.131	0.131	0.111	0.153	0.314	0.461	0.437	0.129
College up	0.039	0.028	0.044	0.044	0.708	0.573	0.415	0.798
Vocational	0.174	0.189	0.171	0.163	0.438	0.291	0.277	0.972
Born here	0.727	0.715	0.745	0.721	0.682	0.41	0.866	0.512
Migrated as a child	0.107	0.107	0.104	0.109	0.982	0.921	0.928	0.85
Migrated as an adult	0.167	0.179	0.151	0.17	0.646	0.366	0.784	0.528
Panel B: Promise-making outcome variables								
Made promise (indicator)	.	.	0.514	0.557	.	.	.	0.295
Switched Vote in Any Race (indicator)	0.565	0.574	0.504	0.618	0.026	0.104	0.298	0.007
Switched Vote for Mayor (indicator)	0.123	0.106	0.115	0.146	0.373	0.729	0.172	0.301
Switched Vote for Vice-Mayor (indicator)	0.22	0.206	0.198	0.256	0.25	0.823	0.183	0.118
Switched Vote for City Council (indicator)	0.444	0.471	0.384	0.477	0.052	0.043	0.891	0.03

Notes: Values in the first four columns are means (standard deviations). Variables in Panel A collected in baseline survey, administered from April 17 to May 8, 2013 (prior to May 13, 2013 municipal elections). Promises (first variable in Panel B) were elicited at end of baseline survey. Remaining variables in Panel B are dependent variables in the analysis, and were constructed on the basis of reported voting in endline survey (May 17 to June 8, 2013). Respondents randomized with equal (1/3) probability into the control group, Promise 1 treatment group, or Promise 2 treatment group. P-values are for F-tests that mean of variable is equal across the specified treatment conditions.

Table 3.2: Vote shares and candidate favorability ratings, by electoral race

Candidate	Actual vote share in election	Reported vote share (endline survey)	Sample average favorability rating	% of surveyed respondents rating candidates as...						
				Extremely unfavorable (-3)	Quite unfavorable (-2)	Slightly unfavorable (-1)	Neutral (0)	Slightly favorable (1)	Quite favorable (2)	Extremely favorable (3)
Mayor race										
A*	46.3	44.8	0.5	5.9	10.7	7.4	29.2	13.9	21.3	11.7
B	48	55.2	0.6	3.2	8	6.5	30.7	21.7	21.1	8.8
Vice-mayor race										
C	30	30.2	0.3	2.5	17	7.8	27.5	19.6	20.3	5.3
D	24.6	23.4	0	3.7	19.8	8.7	33.6	16.1	13.6	4.4
E	32.9	46.5	0.4	3.1	13.7	6.3	29.8	18.2	21.9	7
City council race, Bacon District										
F	30.4	31	0.1	2.1	21.6	7	29.6	18.5	17.8	3.5
G	24.5	24.5	-0.2	4.9	25.1	7	33.1	12.5	13.9	3.5
H*	37.8	46	1	1.1	8.4	2.4	24	19.5	35.5	9.1
I	10.2	11.1	-0.3	3.8	27.9	8	35.2	8.7	14.3	2.1
J	32.7	44.4	0.6	1.1	13.2	5.9	30.7	15.3	25.1	8.7
K	32.5	39.9	0.4	2.4	15.7	4.9	27.5	19.9	22	7.7
L*	37.3	54	0.7	1.7	13.2	7	21.6	16.4	32.1	8
M	15.8	17.6	-0.1	2.8	23.3	8.4	32.4	15	16	2.1
N	17.1	18.8	0	2.8	23.3	5.6	30.7	15.3	17.4	4.9
O*	25.5	24.9	0.2	2.4	21.3	5.2	30.3	15.3	16.7	8.7
P	20.1	14.6	-0.2	4.2	26.8	6.6	32.4	12.2	14.6	3.1
City council race, East District										
Q*	31.3	28.7	0.3	1.8	18.7	7.8	26.5	15.9	26.5	2.8
R*	20.4	23	0.6	1.1	13.8	6	24	21.2	28.3	5.7
S	6.6	4.6	-0.6	4.6	33.9	8.5	35	10.3	7.1	0.7
T*	29.7	33	0.8	0.4	12	3.2	23	23.3	31.1	7.1
U	3.2	1.5	-0.8	2.8	36.8	11	35.7	10.6	3.2	0
V	5.5	1.2	-0.6	2.1	32.5	11.3	39.9	10.3	3.5	0.4
W	40.5	60.2	0.6	0.4	13.1	4.6	25.8	21.2	30	5
X	45.4	60.2	0.8	0.7	12	6.4	24.4	14.8	28.6	13.1
Y	44.4	54	0.3	0.4	20.1	6.4	29.3	15.9	23.7	4.2
Z	34.5	39.5	0.1	1.4	21.6	9.9	27.9	19.1	16.3	3.9
AA	15.1	13.8	0	0.7	20.9	7.8	35.7	17.7	14.8	2.5
AB	9.2	4.2	-0.5	2.5	31.1	9.5	35.7	12.4	8.5	0.4
AC	18.2	11.9	0.5	1.4	17.7	2.8	24.7	22.3	25.4	5.7
City council race, West District										
AD	5	4.2	-0.1	2.6	19.8	11.2	37.7	11.8	14.7	2.2
AE	20.1	13.4	0.2	2.9	14.4	7.4	35.5	19.8	18.5	1.6
AF*	49.8	60.8	1.1	0	8	1.9	26.8	14.7	36.4	12.1
AG	32.7	43.8	0.4	0.3	13.4	7.7	34.8	16.9	24	2.9
AH	18.2	19.1	0.3	1.6	17.3	5.4	38.3	11.8	17.3	8.3
AI	37.3	50.9	0.9	1.9	10.9	2.9	23.6	12.8	36.4	11.5
AJ*	38.4	59	0.7	1.3	10.2	4.8	29.1	19.2	29.7	5.8
AK	27.4	23.7	0.5	1	12.1	5.1	34.2	17.3	21.1	9.3
AL*	34.6	50.9	0.7	1	9.6	4.5	29.4	18.2	31.6	5.8
AM	3.1	4.2	-0.5	1.9	28.1	11.2	43.5	9	5.8	0.6
AN	9.7	7.4	-0.3	1.9	23.6	9.6	43.1	13.1	7.4	1.3
AO	8.3	4.6	-0.3	2.9	24.3	8.3	45.7	7	9.6	2.2

Notes: Data on actual vote share in election are from Philippine Commission on Elections (COMELEC). Reported vote share is from our endline survey. Favorability ratings are from our baseline survey. Starred (*) candidates are incumbents (but not all incumbents ran again in this election). Bold candidates are winners of their respective races. In city council races, top four candidates are elected.

Table 3.3: Impact of treatments on vote-switching (ordinary least-squares regressions)

Dependent variable:		Switched Vote in Any Race (1)		Switched Vote for Mayor (2)		Switched Vote for Vice-Mayor (3)		Switched Vote for City Council (4)
Promise 1 treatment	β_1	-0.0953**	β_{1m}	-0.00329	β_{1v}	-0.0221	β_{1c}	-0.109**
("Don't take money")		(0.0429)		(0.0278)		(0.0365)		(0.0430)
Promise 2 treatment	β_2	0.0309	β_{2m}	0.0288	β_{2v}	0.0391	β_{2c}	-0.00945
("Take money, vote conscience")		(0.0427)		(0.0299)		(0.0383)		(0.0439)
Control variables		Y		Y		Y		Y
Observations		806		759		751		793
R-squared		0.046		0.037		0.041		0.042

***$p < 0.01$,** $p < 0.05$,* $p < 0.1$

Notes: Robust (Huber/White) standard errors in parentheses. Dependent variable in columns 1-10 equal to 1 if respondent switched his/her vote in the given race or set of races, 0 otherwise. Vote switching in mayor and vice-mayor races defined as voting for a candidate not receiving respondent's highest favorability rating in baseline (pre-election) survey. Vote switching in city council race defined as voting for a candidate not among the respondent's top-four highest-favored candidates in baseline survey. Respondents randomized with equal (1/3) probability into the control group, Promise 1 treatment group, or Promise 2 treatment group. Control variables are listed in Panel A of Table 1 and were reported in baseline survey prior to treatment.

Table 3.4: Tests of theoretical predictions

	Races pooled	Mayor race	Vice-mayor race	City council race

A. Testing predictions of partially sophisticated theoretical case
(within race, effects more negative for Promise 1 than Promise 2)

	$\beta_1 - \beta_2$	-0.126**	$\beta_{1m} - \beta_{2m}$	-0.032	$\beta_{1v} - \beta_{2v}$	-0.061	$\beta_{1c} - \beta_{2c}$	-0.100**
		(0.043)		(0.030)		(0.037)		(0.043)

P-value of F-test: $(\beta_{1m} - \beta_{2m} = 0)$ & $(\beta_{1v} - \beta_{2v} = 0)$ & $(\beta_{1c} - \beta_{2c} = 0)$ 0.005

B. Testing prediction of differential effects across races
(within promise, effects more negative for city council than in either mayor or vice-mayor races)

	$\beta_{1c} - \beta_{1m}$	-0.106**	$\beta_{1c} - \beta_{1v}$	-0.087*
Comparing across races, for Promise 1:		(0.049)		(0.053)
Comparing across races, for Promise 2:	$\beta_{2c} - \beta_{2m}$	-0.038	$\beta_{2c} - \beta_{2v}$	-0.049
		(0.051)		(0.055)

P-value of F-test: $(\beta_{1c} - \beta_{1m} = 0)$ & $(\beta_{1c} - \beta_{1v} = 0)$ & $(\beta_{2c} - \beta_{2m} = 0)$ & $(\beta_{2c} - \beta_{2v} = 0)$ 0.086

C. All theoretical predictions in A. and B. combined

P-value of F-test: $(\beta_{1m} - \beta_{2m} = 0)$ & $(\beta_{1v} - \beta_{2v} = 0)$ & $(\beta_{1c} - \beta_{2c} = 0)$
$(\beta_{1c} - \beta_{1m} = 0)$ & $(\beta_{1c} - \beta_{1v} = 0)$ & $(\beta_{2c} - \beta_{2m} = 0)$ & $(\beta_{2c} - \beta_{2v} = 0)$ 0.008

***$p < 0.01$,** $p < 0.05$,* $p < 0.1$
Notes: Table reports tests of linear combinations of coefficients suggested by theory. Robust (Huber/White) standard errors in parentheses. β_{ij} is impact of promise i on vote-switching in race j in regressions reported in Table 3.

Figures

Figure 3.1: Promise Treatments as Viewed by Participants

PROMISE INTERVENTION 1: Would you promise to not take the money from any candidate or local leader before the elections?

Yes, I promise.   No, I can't make that promise.

Promise 1

PROMISE INTERVENTION 2: If any candidate or local leader gives you money before the elections and you decide to keep it, would you promise to vote according to your conscience?

Yes, I promise.   No, I can't make that promise.

Promise 2

Thank you for your promise. As a symbolic act of this solemn promise, please write the phrase "I promise" on the space below.

x

Signature

Figure 3.2: Vote-Switching by Treatment Condition

Any Race

Mayor or Vice-Mayor

City Council
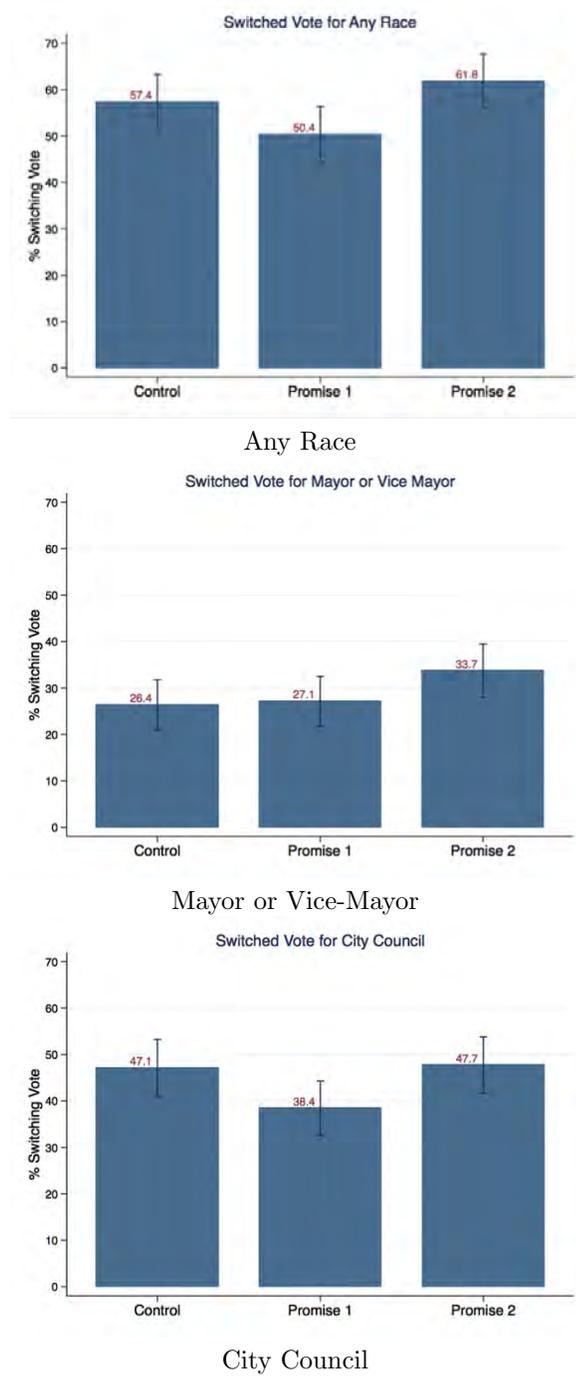
Notes: Figures show fraction of respondents switching their vote (voting for a candidate other than their top-rated candidate as reported in baseline survey), by treatment condition, along with % confidence intervals. Figure 2(a) shows fraction vote switching in any of the three races. Figure 2(b) shows fraction vote switching in either of the mayor or vice-mayor races. Figure 2(c) shows fraction vote switching in the city council race. (In city council race, voters can vote for up to four candidates. Vote switching in this race is defined as voting for at least one city council candidate who was not among the respondent?s top four rated candidates in baseline survey.)

Nudging Good Politicians: Evidence from a Field Experiment in the Philippines

"The nature of the workings of government depends ultimately on the men who run it. Let there be emphasis on those we elect to office (V.O. Key 1956)."

4.1 Introduction

Incompetent and dishonest politicians are resident features of governments in developing democracies. Yet among scholars seeking to address the ubiquity of bad policies and corruption in government, few look to the quality of politicians for answers. Those who do take as given bad politicians and offer theoretical explanations for their pervasion (Bernheim and Kartik, 2014; Besley and Coate, 1997, 1998; Caselli and Morelli, 2004) or provide empirical evidence for their adverse consequences (Chemin, 2012; Eggers and Hainmueller, 2009; Gehlbach and Sonin, 2010).[1] So far, none have sought to evaluate the efficacy of incentivizing and inducing selection of competent and virtuous citizens to public service—of nudging good politicians.

Policies that come closest to the notion of nudging good politicians and are at the frontier of research in modern political economy come in the form of reforms

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[1] Other scholars, instead, provide empirical evidence of the favorable effects of good politicians (Besley, Montalvo and Reynal-Querol, 2011; Congleton and Zhang, 2013; Dreher, Lamlac and Somogyic, 2009; Jones and Olken, 2005).

in political institutions (Chattopadhyay and Duflo, 2004; Pande, 2003), or improvements in wage and remuneration schemes (Ferraz and Finan, 2009; Gagliarducci and Nannicini, 2013)—policies that attract a more qualified pool of elected officials and motivate them to perform better. However, such policies are difficult to implement, especially in developing democracies. Reforms in political institutions face opposition from existing power holders who see their economic or political rents threatened, or create perverse incentives for those who see new ways to increase such rents (Acemoglu, 2010; Acemoglu and Robinson, 2012). At the same time, incentives that attract good politicians also attract bad ones. Without mechanisms that can screen-out bad politicians, incentives might only worsen adverse selection in politics.

If we could design a policy that induces self-selection of, or screens-in and incentivizes, competent and honest citizens to serve in public office, would it play a catalytic role in improving the quality of the political class? Inspired by the game theoretic notion of schooling as a screening and sorting device (Arrow, 1973; Johnson, 1978; Layard and Psacharopoulos, 1974; Spence, 1973; Taubman and Wales, 1973), we set out to evaluate whether a leadership training workshop—much like schooling—can work as a screening mechanism to selectively incentivize, and to induce self-selection of, good politicians. We implemented such a policy intervention where it had immediate application—among individuals considering running for the *Sangguniang Kabataan* (SK), a governing body comprised of elected youth leaders in the Philippines.

Partnering with the Angara Centre for Law and Economics (ACLE), a local nonprofit research organization in the Philippines, and Innovations for Poverty Action (IPA), we implemented a randomized field experiment of a leadership training workshop with incentives among youth interested in running for SK. All *barangays* (villages) in the country are mandated by law to establish a SK. The SK has the mandate to appropriate 10% of internal revenue allotment for youth programs and it often serves as a jump-off point for a career in politics for young Filipinos. In recent years,

SK has been censured by policymakers and civil society for being a breeding ground for corruption.[2]

We implemented the experiment in the Province of Sorsogon, Philippines. Recruitment, baseline survey and workshop interventions occurred in three months leading up to the originally scheduled October 2013 SK Elections. 720 qualified applicants expressed interest and were then invited to attend a pre-workshop session. The pre-workshop session involved baseline exams designed to measure several dimensions of candidate quality: public service motivation (Perry, 1996), intellectual ability based on Wechsler's test of memory for digit span (Wechsler, 1987), personality using the Big Five Inventory (John, 1990), aspiration (Kasser and Ryan, 1996)), and survey-based measure of integrity. 569 applicants attended and completed the pre-workshop session and were then enrolled as study subjects.

We randomly assigned study subjects into three groups: no workshop (C), workshop with unconditional incentives (T1), and workshop with conditional incentives (T2). The leadership training workshop had two goals: Provide basic leadership skills and serve as a screening mechanism by which participants can signal quality as they performed in the various workshop tasks. To prevent differential take-up across treatment arms, participants invited to the workshop were not informed of the incentives until after the workshop was over. Moreover, unbeknownst to participants, performance in the workshop was monitored, evaluated, and assigned scores. Participants in T1 received the incentives regardless of workshop performance. On the other hand, participants in T2 received the incentives if their workshop performance scores were above a pre-determined cutoff, which was known only to the Principal Investigator (PI). The incentives were a combination of two things: (1) a plaque of merit awarded at the end of the workshop, (2) and our promise to donate a few standard-sized campaign posters should they decide to file an official certificate of candidacy for the

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[2]See for example this article that appeared in a national newspaper on how, "SK, hope of motherland, but 'breeding ground for political dynasties'."

SK.

Shortly after the workshop interventions, the Philippine House of Representatives decided to defer the October 2013 SK Elections to February 2015, and with a subsequent legislation, to October 2016. In lieu of an elected youth council, all barangays were required to form an appointive body of youth leaders called the Task Force on Youth Development, which took on the roles and responsibilities of the youth council in the interim. Given these unforeseen events, we could not measure the original outcomes of interest—standing in election and subsequent behavior in office—until the SK elections are next held in 2016. However, a year after the workshop interventions, we collected administrative data and conducted a follow-up survey to find out if the workshop interventions had any effect on political attitudes and behavior among study subjects, in particular, how their interest in standing in the SK elections changed since baseline, whether they engaged in village youth programs since the workshop interventions, and whether they were nominated and designated to the Task Force on Youth Development.

Our empirical analysis is governed by publicly registered pre-analysis plans. Results based on a sample of 559 individuals (so there is 2% end-line attrition) provide evidence for political selection, in which subjects with above (below) median levels of public service motivation (PSM) in both T1 and T2 are more (less) interested in standing in election, more (less) likely to engage in village youth programs, and more (less) likely to be nominated and designated as village youth leaders than their counterparts in C.

It is possible that screening out those with low PSM comes at a cost of losing high aptitude individuals (a concern for many scholars such as Delfgaauw and Dur (2007); Francois (2000); Handy and Katz (1998); Prendergast (2007)). However, our results also show that those in T2 and are above (below) the median of aptitude score are also more (less) likely to be nominated and designated to the Task Force, are more

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(less) interested in running for SK elections in the future, and are more (less) likely to participate in village youth programs.

We also find that those in T2 and are above (below) the median of aspiration index, and integrity index, are more (less) likely to be nominated and designated to the Task Force, are more (less) interested in running for SK elections in the future, and are more (less) likely to participate in village youth programs. We find little evidence for heterogeneity in workshop treatment effects in the dimension of personality.

These results are remarkable given that the only incentive at work was the plaque of merit (since the campaign posters were not handed out given the election defer-ment), moreover, the nomination and designation of members of the Task Force were held as late as June 2014, several months after the workshop interventions, and the follow-up survey, a year after the workshop interventions. Yet subjects with above–(below-) median measures of candidate quality are more (less) likely to select into politics after attending a leadership training workshop with conditional incentives (T2), highlighting its efficacy in screening-out less qualified individuals and nudging the good ones to public office.

The paper proceeds as follows: Section 2 provides background on local politics in the Philippines. Section 3 explains the experimental design. Section 4 presents a simple model that frames the empirical exercise and specifies the hypotheses to be tested. Section 5 describes the candidate quality measures. Section 6 presents the results and discussion, and Section 7 concludes with broad implications for political economy and public policy.

4.2   Background

To date, the Philippines is the only country in the world that popularly elects youth representatives. All 42,028 barangays (smallest political unit; a village) in the country are mandated by law to establish a *Sangguniang Kabataan* (SK), a governing

body comprised of 8 elected youth leaders.

4.2.1 Brief history

The SK is an offshoot of the *Kabataang Barangay* (Village Youth), which was established in 1975, during the authoritarian rule of President Ferdinand Marcos. In writing, KB was intended to afford the youth opportunity for expression and democratic representation[3], however, in practice, it was an instrument to pacify both in-school and out-of-school youth and to limit the recurrence of student demonstrations against the dictatorship (Wurfel, 1977).

In 1991, KB was formally abolished and replaced by the *Katipunan ng Kabataan* (League of Youth) under Republic Act 7160. KK includes all 15–17 years old, Filipino citizens, who are registered residents of a barangay for at least 6 months. The Sangguniang Kabataan (SK) is its elected governing body. Since 1992, five SK elections have been held nationwide.[4]

In October 2013, the House of Representatives passed a law (Republic Act No. 10632) that defered the SK Elections from October 2013 to February 2015 (and with a subsequent law, to October 2016 while reforms in SK are underway).[5] Shortly after, the Commission on Elections (COMELEC) released the Implementing Rules and Regulation of R.A. 10632, which created the Task Force on Youth Development in lieu of the SK in the interim.

The Task Force is composed of a chairperson, and 8 members nominated by the Katipunan ng Kabataan and other youth organizations operating within the barangay.

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[3]Presidential Decree No. 684, April 15, 1975

[4]Elections were held in 1992, 1996, 2002, 2007, and 2010. The term limit for SK offficials has changed over the years and ranged between 3 and 5 years.

[5]This law had four other provisions: (1) No holdover - all incumbent SK officials shall remain in office until their end of term on November 30, 2013; (2) No appointment - the SK positions shall remain vacant until the elections of a new set of officials; (3) Use of 10% SK fund - until the election of new officers, the Sangguniang Barangay (Village Council) shall use the funds solely for youth development programs; and (4) Implementing Rules and Regulations - COMELEC and the Department of Interior and Local Government shall implement the rules and regulations of the law.

The 8 members are designated through a resolution of the Sangguniang Barangay, and have the following qualifications: (1) 15–17 years old at the date of designation as member of the Task Force; (2) of good moral character; (3) a resident of the barangay for at least 6 months before appointment; and (4) should not be related to the officials of the Sangguniang Barangay (Village Council), up to the 4th degree of consanguinity and affinity.

4.2.2 Powers and privileges of SK

The Sangguniang Kabataan (SK) has several powers. They have the mandate to appropriate 10% of the barangay's Internal Revenue Allotment for youth development programs. The SK Chairman automatically sits on the Barangay Council, and is automatically designated as Chairman of the Committee on Youth and Sports. Barangay level SKs form municipal and city federations, which then form provincial federations. Elected presidents of these federations sit on the Municipal and City Councils and Provincial Board, alongside elected Council and Board Members. Local federations then form a national federation, the president of which sits as a Commissioner of the National Youth Commission. The SK also has privileges. As incumbents, SK Officials are exempt from payment of tuition while enrolled in state colleges and public universities nearest their jurisdiction.

4.2.3 A breeding ground for bad governance?

Allegations of corruption and poor governance beleaguer SK (UNICEF, 2007). Anecdotes of SK's lack of transparency, vote-buying, bribery, corruption and nepotism abound, so much so that the main author of Republic Act 7160 which created the SK, former Senator Aquilino Pimentel, called for SK's abolition.

Instead of abolition, however, lawmakers have decided to defer the 2013 SK Elections to make way for reforms. In his sponsorship speech for the passage of the SK

Reform Bill, Senator Bam Aquino noted that, "it is urgent and important that we reform the Sangguniang Kabataan, as a platform for engaging the youth in the grassroots level, and where the youth will be honed to become better and more effective public servants in the future."

4.3 Experiment

The latest round of SK Elections was originally scheduled in October 28, 2013 so recruitment and the workshop interventions were implemented from August to early October that year. It involved three stages. In the first stage, calls for application were made to the leadership training workshop. Eligible applicants were then invited to attend a pre-workshop session in which the study team took measures of candidate quality (to be described in the next section). Finally, applicants who successfully completed the pre-workshop session were selected at random to be invited to attend the workshop. Invitees were also selected at random to receive either conditional or unconditional incentives (more on this below), however none of them were informed of any incentives at the time of invitation. Figure 4.1 presents the study timeline and intervention flowchart.

[Figure 4.1 about here.]

4.3.1 Call for applications for the leadership training workshop

Calls for application to the workshop took place in the months of August and September 2013, in the 8 largest municipalities (out of 15 total) of the Province of Sorsogon, Philippines. Sorsogon Province is located at the southern tip of Luzon island, roughly 12 hours by road from the national capital, Manila. Sorsogon City, with a population of roughly 150,000, is the provincial capital, and is slightly below the median across Philippine municipalities in terms of economic development. With

a municipal poverty rate of 35%, it is slightly worse than the median (the 45th percentile, to be exact) poverty rate among Philippine municipalities.[6]

In each of the 8 municipalities, the team visited barangays that are approximately 3 kilometers away from the main highway and handed out posters and invitation letters to schools and offices of barangay officials, to capture as many applicants as possible.

The calls for application is for an all-expense-paid, three-day workshop entitled, "Foundational Training for Aspiring Young Politicians." Posters and letters of invitation provided a general description of the workshop, application guidelines, as well as directions on how to submit applications (see sample poster in Figure C.1 in the Appendix).

Applicants were required to be 15–17 years old, Filipino citizen, residing in the Province of Sorsogon, and a registered member or plan to register as member of the *Katipunan ng Kabataan* (League of Youth). These are the same eligibility requirements to stand for election for the SK (Youth Council).

Along with the posters and invitation letters were paper copies of the application form. The application included consent and parental permission forms, which applicants were required to sign and have signed by their parent. It also indicated that successful applicants were required to attend a pre-workshop session. Finally, all applicants were informed that, because spots are limited, workshop participants will be chosen randomly. In the end, we received 720 valid applications with signed consent and parental permission forms.

4.3.2 Pre-workshop session

The pre-workshop session was conducted so the study team could administer exams designed to measure personal characteristics of applicants before any random

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[6]Poverty rates are from 2003. The Philippines? overall poverty incidence is 29% (National Statistical Coordination Board 2009).

assignment to treatment groups occurred. The study team conducted 7 sessions in several sites that were convenient for applicants to reach.[7]

At the start of the pre-workshop session, applicants were reminded that selection to the workshop was completely random. Applicants were also told that we were not looking for any particular answers; they just needed to be honest when answering questions.

The session involved a series of tests designed to measure several dimensions of candidate quality: (1) public service motivation, (2) aptitude, (3) personality, (4) aspiration, and (5) integrity, to be discussed in the Data section below.

Out of the 720 eligible applicants, 569 attended and completed the pre-workshop session and were enrolled as study subjects. These study subjects represented 109 barangays from 9 municipalities in the Province of Sorsogon. Subjects were then randomly assigned into one of three treatment groups.[8] The three treatment groups are: (1) control, (2) workshop with conditional incentives, and (3) workshop with unconditional incentives. The results of the random selection of workshop participants were communicated to the study subjects by phone call and text messaging.

4.3.3 Leadership training workshop

Using a leadership training workshop primarily as a screening and sorting mechanism, to our knowledge, is a novel policy idea. But conducting leadership training to expose and prepare the youth for public service and political careers is certainly nothing new. In the United States, for instance, there are nonpartisan educational institutions (e.g. leadershipinstititute.org) and political party sponsored institutions (e.g. Democratic Leadership Institute in Wisconsin) that have institutionalized some form of leadership training for youth interested in joining politics. In the Philippines,

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[7]In particular, we conducted 3 sessions in Sorsogon City, 1 session in the Municipality of Casiguran, 1 session in the Municipality of Gubat, 1 session in the Municipality of Bulan, and 1 session in the Municipality of Matnog.

[8]Computer randomization was done in the office using Stata software.

the Kaya Natin! (trans. We can do it!) national movement, regularly conducts leadership training among youth leaders in university settings to promote good governance, ethical leadership and support effective and ethical leaders in government.

The leadership training workshop that we designed and implemented, called "Foundational Training for Aspiring Young Politicians," was held in Sorsogon City, the namesake capital of the Province of Sorsogon. There were four batches of the workshop, each one held over a weekend, from Friday afternoon to Sunday afternoon. The dates of the workshops were as follows: Batch 1 (Sep. 20-22), Batch 2 (Sep. 27-29), Batch 3 (Oct. 4-6), and Batch 4 (Oct. 11-13).

Study subjects selected to participate in the workshop were given a new set of consent and parent's permission forms to sign and have signed by their parent. Both participants and parents were also asked to read and agree to the house rules of the workshop.

The workshop was conducted by a hired consulting firm that specializes in conducting leadership training workshops for both private and public organizations in the Philippines, and has PEERRS certification from the University of Michigan IRB to conduct research on human subjects.

4.3.3.1 Workshop content

The Foundational Training for Aspiring Young Politicians (FTAYP) workshop provides aspiring young leaders a shared platform to interact with each other, to be grounded on servant–leadership principles that found application in the corporate and public sectors, and to evolve a plan of action that they can readily implement and deploy in their immediate community in the context of their prospective roles as elected youth council members. The workshop is a combination of plenary sessions; individual activities and small group discussions; and structured learning exercises (see Figure C.2 in the Appendix).

4.3.3.2 Scoring

Unbeknownst to the study participants, performance in the workshop was monitored and evaluated, and participants were assigned scores based on an established scoring rubric (see Figure C.3 in the Online Appendix). Study team members serving as small group leaders were the ones who assigned workshop participation and worksheet scores.

Each small group was assigned two leaders. Each leader graded each member of the small group for participation during Days 2 and 3 of the workshop. Participation score is the average of all the grades received by the participant. Workshop participation score is 20% of a participant's overall performance score.

Worksheets were anonymized and randomly redistributed to small group leaders for grading. Participants each had to accomplish 3 worksheets. Each worksheet is graded by two randomly assigned group leaders. The worksheet score is based on the average of the two grades. Worksheets 1 and 2 are each worth 20% of the participant's overall performance score. Worksheet 3 is worth 40%.

If a participant was assigned to a workshop with unconditional incentive, then he received the incentive regardless of his overall performance score. However, if a participant was assigned to a workshop with conditional incentive, then he received the incentive only if his performance score was above a pre-determined cutoff, which is known only to the Principal Investigator (PI).

4.3.3.3 Incentives

At the end of every workshop, all respondents receiving the incentive were awarded a plaque of merit. The study team also promised to donate 5 pieces of standard-sized campaign posters should they decide to file an official certificate of candidacy for the SK.

4.3.4 Deferment of the 2013 SK Elections

With the deferment of the October 2013 SK Elections, we could not measure the original outcomes of interest—who stands for election among the study subjects and, conditional on getting elected, their subsequent performance in office—until the elections are next held in October 2016.

However, with the creation of the Task Force on Youth Development in the interim, we gathered administrative data and conducted a follow-up survey among subjects a year after the workshop interventions to measure alternative outcomes of interest: (1) who got nominated to the Task Force; (2) who got designated to the Task Force; (3) change since baseline in interest in running for the next SK elections; and (4) engagement in village youth programs since the workshop interventions.

Until the elections are next held and subjects decide whether to file a candidacy, we cannot distribute the campaign posters as incentives, hence the only incentive at work is the plaque of merit that was awarded during the leadership training workshops. At the end of the follow-up survey, we did remind subjects who were awarded incentives that our promise to donate campaign posters still stands, should they subsequently file for candidacies in the next SK elections.

4.4 Framework

To inform the empirical exercise, we use a Principal–Agent model of political selection and screening which is a variation of the Spence (1973) signaling model of educational investment. The model elucidates the effects of the workshop interventions on agents' decision to serve in public office. The model assumes heterogeneous types of agents in the qualities of an effective public servant—for simplicity—low-types (L) and high-types (H). A key result is that a leadership training workshop with incentives induces low-types to select out of, and high-types to be nudged into,

serving in public office.

4.4.1 Assumptions

A Principal, which could be a political party or a non-governmental institution, cares about having a better pool of agents selecting into public office. The policy instrument available to the Principal is a leadership training workshop with incentives.

Agents—aspiring politicians—are heterogeneous in quality. Without loss of generality, agents are either high-types (H) or low-types (L). H-types are inherently more productive than L-types, that is, they have desirable qualities such as public service motivation, intellectual ability, good personality, aspirations, and integrity, which allow them to efficiently deliver public goods and services while in office.[9]

Agents would like to pursue a political career, but as first-time candidates, they are imperfectly informed about their own type. That is, whether they have the preferences and qualities suitable for a career in politics will likely be apparent to themselves only *after* some exposure to public service or learning about what it means to serve in public office. Much like taking specialized classes or on–the–job–training (OJT) helps agents "try out" jobs (Johnson, 1978)), we allow for the possibility that the leadership training workshop helps agents learn their own tastes and qualities with respect to serving in public office. This is the "self-selection and sorting" mechanism of the leadership training workshop.

Finally, we also assume that the leadership training workshop does not affect the productivity of agents, but that it helps the Principal learn agent types and award incentives conditional on revealed type.[10] The leadership training workshop

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[9]In this sense, this model is akin to the selection model conceptualized and contrasted with those of the sanctions model by Mansbridge (2009). Unlike in sanctions model, here, the agent is at least in part motivated to do public service. Moreover, an alignment of objectives between principal and agent is possible, based on the agent's public service motivation. Finally, the model requires reliable mechanisms of selection and sorting and that the principal's energy is concentrated on the selection process and not on the sanctioning post selection.

[10]Given that the workshop is a three-day affair, we take it as a plausible assumption that it does not affect the productivity of the participants the way schooling would increase productivity as

incorporates individual and group tasks which, by design, are easier (less costly) for H-types to perform than L-types. This is the "screening and incentives" mechanism of the workshop that induce type revelation and allows the Principal, with some margin of error, to identify H-types and award them incentives.

4.4.2 Model setup

The Principal has the policy instrument of a leadership training workshop with incentives. Agents attend the workshop and perform in the various individual and group tasks. The Principal then evaluates performance in the workshop, assigns score s to each agent, and offers incentive I with the following condition:

$$I = \mathbb{1}[s \geq \nu] \tag{4.1}$$

where $\mathbb{1}[.]$ is an indicator function so that a candidate with score s above the cutoff score ν is given incentive.

The agents are either H-type or L-type. Each agent i's decision problem is:

$$\max_{s} \quad P + B_i(i, \phi) + I_i(s) - C_i(s), \quad i = H, L \tag{4.2}$$

where P is the pecuniary and non-pecuniary benefits of standing for office; and

$$B_i(i, \phi) = -\phi \mathbb{1}[i = L] \tag{4.3}$$

is how we incorporate the "self-selection and sorting" mechanism of the workshop, which is a negative payoff for L-types as they learn about their type in the workshop,

generally argued (Mincer, 1974).

with $\phi \geq 0$ as an intensity parameter of learning; and

$$C_i(s) = \begin{cases} s & \text{if } i = L \\ s/2 & \text{if } i = H \end{cases} \tag{4.4}$$

is the "cost" of performing and achieving a certain score in the workshop.

The task of the Principal is to find the threshold value ν such that only H-types can and have the incentive to meet the cutoff score and are selectively incentivized, while the L-types may meet the cutoff score but have the incentive not to, and are therefore not incentivized. In short, the goal of the model is to find a separating equilibrium characterized by the parameters (ϕ, ν, s).

Take as given the value of $P = 3$. For a H-type agent, the payoff to performing well in the workshop such that he meets the cutoff score and obtains the incentive is $3 + 0 + 1 - s/2 = 4 - s/2$. On the other hand, his payoff for not bothering to meet the cutoff score in the workshop is 3. Hence, it is worthwhile for H-type to make an effort to meet the cutoff score if $\nu \leq 2$.

Meanwhile, for a L-type agent, the payoff to performing well in the workshop and obtaining the incentive is $3 - 1 + 1 - s = 3 - s$. On the other hand, his payoff for not bothering to meet the cutoff score is 2. Hence, it is not worthwhile for L-type to pretend to be H-type to receive the incentive if $\nu \geq 1$.

Therefore, in this example, if the Principal sets the cutoff score $\nu \in (1, 2)$, then it achieves a separating equilibrium in which L-types select out of public office and H-types are nudged into running for office. For as long as there are enough agents for which the set cutoff score induces a separating equilibrium, then the Principal can use a leadership training workshop with conditional incentives to attract a better pool of agents into public office.

Note that the model allows for the possibility that simply attending the workshop helps L-types realize that they may not be well-suited for a career in politics. For

example, keeping all parameter values as given, set $\phi = 4$. This would be true, for example, for an L-type candidate who, before the workshop, only considers the rewards of being in office, P, but after having attended the workshop, realizes that being in office requires a certain level of public service motivation and competence that they do not have or are costly for them to acquire.

The experiment has a control group and two treatment arms designed to test the "self-selection and sorting" mechanism as well as the "screening and incentives" mechanism of the leadership training workshop, which we formalize into hypotheses tests in the following subsection.

4.4.3   Hypotheses and econometric framework

We registered the pre-analysis plan (PAP) governing this analysis with Experiments in Governance and Politics (egap.org) on August 19, 2013 before recruitment and baseline survey was conducted. We also lodged the PAP in the American Economic Associations randomized control trial registry on May 5, 2014 (and modified on August 5, 2014), just before the follow-up survey began. This latter PAP took note of the deferment of the SK elections and the measurement of new outcomes on political attitudes and behavior in lieu of the original election-related outcomes.

Given the sample size, we do not have power to adjust for multiple hypothesis testing as we investigate the impact of the workshop interventions on many potential outcomes of interest and as we conduct subgroup heterogeneity analyses in the different measures of quality. Our approach, instead, is to specify and pre-commit in the PAP the three key outcomes that we will look at: (1) Interest in joining SK, (2) indicator for being nominated to the Task Force on Youth Development, and (3) indicator for being designated to the Task Force. We look at a fourth outcome, which is an indicator for having engaged in village youth programs, but we note that this is not in our PAP and should be taken as an exploratory analysis.

We also specified four dimensions of quality in our PAP: (1) Digit span score index as proxy for aptitude/intellectual ability, (2) public service motivation (PSM) index, (3) personality index based on the big five inventory (BFI), and (4) aspiration index developed by Kasser and Ryan (1996). We take each of these indexes as a family of hypotheses (e.g. PSM has six sub-components). By aggregating sub-components into indexes, we deal with the issue of multiple inference which is an approach taken in other studies (e.g. Casey, Glennerster and Miguel (2012); Kling, Liebman and Katz (2007)). However, we do not adjust across the four quality domains. To the four aforementioned quality domains, we add a fifth one, integrity index, which is based on a set of baseline survey questionnaires (details in the next section). Again, because this fifth dimension is not in our PAP, the analysis on it is only exploratory.

To estimate the effects of the workshop interventions on political attitudes and behavior of respondents by type, we estimate the following equation:

$$outcome_i = \beta_0 + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T1_i^* HighType_i + \beta_4 T2_i^* HighType_i + \beta_5 HighType_i + \mathbf{X}_i' \mathbf{\Gamma} + \epsilon_i$$

(4.5)

The dependent variable, $outcome_i$ is either: (1) change in interest in running for SK since baseline; (2) indicator variable for engaging in village youth programs; (3) indicator for being nominated as a youth leader; or (4) an indicator for being actually designated as a youth leader.

$T1$ is an indicator variable for being assigned to the workshop with unconditional incentives treatment arm. Likewise, $T2$ is an indicator variable for being assigned to the workshop with conditional incentives treatment arm.[11]

$HighType_i$ is an indicator for being above the median in the sample distribution of the quality index measures: public service motivation, intellectual ability, personality, aspiration, and integrity.

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[11]Ideally, we would have another treatment arm for workshop with no incentives so we can isolate the pure "learning" effect. But due to budget constraint, we decided to maximize power by dropping this treatment arm in the design.

X_i is a vector of controls including demographic characteristics such as gender, age, weight, height, body mass index, and baseline interest in joining SK (on a scale of 0–10). We also control for village fixed effects to account for heterogeneity in outcomes across the level of public office (i.e. village youth council). ϵ_i is an error term.

The hypotheses that we test are detailed in the pre-analysis plan and replicated here as follows:

$\beta_1 \lesseqgtr 0$	Low-types in T1 learn type but are unconditionally incentivized.
$\beta_2 < 0$	Low-types in T2 learn type and are screened-out and dis-incentivized.
$\beta_3 > 0$	High-types in T1 are unconditionally incentivized.
$\beta_4 > 0$	High-types in T2 are screened-in and incentivized.
$\beta_5 \leq 0$	High-types in C have lower outcome than low-types in C.
$\beta_4 - \beta_2 > 0$	Combined effect of screening-out low-types and screening-in high-types in T2.
$[\beta_4 - \beta_2] - [\beta_3 - \beta_1] \lesseqgtr 0$	Workshop with conditional incentives is more potent as a screening mechanism.

Where the hypothesis involves an ambiguous sign (i.e. $\lesseqgtr$) the test is two-sided, otherwise the test is one-sided in the direction indicated in the statement of the hypothesis.

4.5   Candidate qualities

The dimensions of quality that we consider in this study roughly corresponds to the four domains of personal variability according to Roberts (2006), and characterizes competent and honest politicians (Caselli and Morelli, 2004; Mansbridge, 2009). These dimensions are: (1) public service motivation, (2) intellectual ability, (3) personality, (4) aspiration, and (5) integrity. In what follows, we discuss how these were measured and describe the respondent pool.

4.5.1 Public service motivation

Research indicates that public sector workers have a different motivation profile in terms of values, inclination to public service activities, and volunteering (Bright 2005; Rotolo and Wilson 2006). Scholars of public administration have explored the idea that public service motivation is central to the effective delivery of public goods and services (Perry and Wise, 1990). Individuals with a strong desire to serve the public interest or who have higher levels of altruism are thought to not only be more attracted to public sector employment but also perform better on the job. While estimating the extent to which public service motivation affects job performance remains an active area of research, recent meta-studies suggest that public service motivation is positively correlated with job performance in the public sector, broadly defined (Petrovsky, 2009).

We measured subjects' public service motivation using Perry's 1996 scale of Public Service Motivation (Perry, 1996), which has become the gold standard in the literature on PSM. This index is constructed based on a questionnaire in which the subject must express agreement or disagreement with each of 40 statements. The questionnaire elicits opinions on the attractiveness of politics, public service, and prosocial activities. The questionnaire is subdivided into six modules labeled "Attraction to Policy Making," "Commitment to Policy Making," "Social Justice," "Civic Duty," "Compassion," and "Self-Sacrifice." Each dimension is an average of responses to several statements that are measured on a 5-point Likert scale, where a 5 represents strong agreement with the statement, and a 1 denotes strong disagreement.

We construct a public service motivation index, which is an equally weighted average of the z-scores of each dimension. Each dimension is standardized based on the mean and standard deviation of all subjects.

4.5.2   Intellectual ability

We take the view that a key aspect of competence relates to personal characteristics that make politicians more productive and valuable in public office. Given that the subjects are in their high school years (or early college at the latest) we could not measure wages as a signal of their ability (as valued by the market). Instead, we measured raw aptitude/intellectual ability and personality based on a vast body of research in psychology that documents the importance of both cognitive and non-cognitive traits for predicting earnings, job status, and job performance (Schmidt and Hunter, 1998).

To evaluate a study participant's intellectual or cognitive ability, the pre-workshop session involved a Test of Memory for Digit Span. In this test, the examiner reads out loud in one-second interval, a series of digits (e.g., '4, 8, 7') which participants must immediately repeat back. If participants are able to do so without mistake, they are given a longer list (e.g., '6, 3, 1, 0'). There are two rounds of the test. In the first round, participants are asked to repeat the digits in the same order as read (forward digit span), and in the second round, they are asked to repeat the digits in reverse order as read (backward digit span). The sum of the longest forward and backward digits a participant can recall without making two consecutive mistakes is that participant's digit span.

The digit span measures short-term memory, but it is a subcomponent of full-scale IQ tests, including the widely used Wechsler Adult Intelligence Scale (WAIS), and correlates well with the overall IQ measure. This is because a way to expand memory is to generate patterns as one repeats back the numbers, so more intelligent people can stretch their memory longer. Short-term memory, in turn, is involved in many everyday tasks, from remembering a friend's telephone number while entering it into a phone, to understanding long and difficult sentences.

4.5.3 Personality

To measure non-cognitive attributes, we examined a set of personality traits that, over time, psychologists have grouped into five categories labeled "the Big 5." These traits are openness to experience, conscientiousness, extraversion, agreeableness, and neuroticism. We measured the Big 5 personality traits using the Big Five Inventory (BFI) developed by John (1990). This is a 44-item questionnaire. John, Naumann and Soto (2008) report on extensive studies validating the BFI both for internal consistency in terms of test-retest reliability, as well as convergence with other personality inventories such as the NEO Five Factor scale (McCrae and Costa, 1992).

In the analysis to follow, we report results on an index of the Big 5. As with PSM, this index is constructed as an equally weighted average of the z-scores of each dimension, reverse-coding neuroticism, which is widely considered to be a negative characteristic (the negative of neuroticism is usually labeled "emotional stability"). The standardization was based on the mean and standard deviation of all study subjects.

4.5.4 Aspiration

Aspiration includes both intrinsic (affiliation, community feeling, self-acceptance) and extrinsic (financial success) goals. Kasser and Ryan (1993) developed an extensive measure of individual's aspirations with the aforementioned four goal contents. *Affiliation* aspirations concern the importance and realization of having a family life and good friends. *Community feeling* aspirations concern making the world a better place through one's actions. *Self-acceptance* aspirations concern individual psychological growth, self-esteem and autonomy. Lastly, *financial success*, refers to the aspiration to attain wealth and material success. We construct the aspiration index as an equally weighted average of the z-scores of each of these four goal contents.

4.5.5 Integrity

To capture a respondent's integrity level we construct an equally weighted average of two measures developed and used by Dal Bo, Finan and Rossi (2013): (1) Integrity - direct and (2) integrity - indirect. We then turn this average measure into a z-score. The direct measure of integrity is an indicator for whether or not the individual agrees with the statement that "laws are made to be broken", which is also a common proxy for a lack of respect for laws and moral standards. The indirect measure tracks a person's view about the likelihood that others will engage in honest behavior. In particular, the questions asked, "if you dropped a wallet with 200 pesos, what is the likelihood on a scale of 1 to 5, that a (neighbor, police, stranger) would return it intact." A pessimistic attitude towards the moral behavior of others is thought to correlate with weakness of one's own moral standards due to what psychologists' have termed projection bias (the belief that others must conform to our own inclinations.)

4.5.6 Summary statistics and baseline balance tests

We also measured respondent demographics and baseline interest in joining the youth council (SK). We have data on their gender, age, weight and height from which we construct the body mass index. Finally, we asked all respondents, on a scale of 0–10 (10 being most interested), how interested were they in standing for election for SK.

Table 4.1 presents summary statistics and baseline balance tests. The average age of subjects is 16. Sixty percent of the subjects are female. The average height and weight are 61.82 inches and 101.28 pounds, which implies an average body mass index (BMI) of 18.65.

The average baseline interest in joining the youth council (SK) is very high at 8.46 (out of 10). This is reassuring in that we are attracting individuals who are, to begin with, already inclined to pursue careers in politics. This allays concerns about

the policy intervention attracting individuals who have no political aspirations. All baseline variables are well balanced across the treatment arms.

[Table 4.1 about here.]

4.6 Results

We begin this section by examining whether the leadership training workshop works as a screening mechanism. We show that the workshop and the simple scoring system do remarkably well in revealing the qualities of respondents. Next, we examine the average treatment effects of the workshop interventions T1 and T2 on political attitudes and behavior. We then test for heterogeneous treatment effects in each of the quality measures to provide empirical evidence that workshops with conditional incentives screen-out individuals below the median of the quality measures and nudge those above the median to select into public service.

4.6.1 Is the leadership training workshop an effective screening mechanism?

A central premise of the screening theory presented earlier is that the leadership training workshop works as a screening mechanism by which better qualified individuals can be selectively given incentives. Figure 4.2 provide evidence that this is in fact the case.

[Figure 4.2 about here.]

As expected, subjects with below-median quality measures in T1 are just as likely to receive incentives as their above-median counterparts. In fact, all respondents in T1, by definition, receive the incentives.[12] On the other hand, subjects with below-median quality measures in T2 are significantly less likely to receive incentives than

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[12]The graphs show that only about 60% of below- and above-median respondents in T1 received the incentives (instead of 100% each), but this is because only 115 out of 190 respondents in T1

their above-median counterparts. Results hold across all quality measures, although the magnitude of difference is much less pronounced in the case of aspiration and integrity indexes. In short, the leadership training workshop with a simple scoring system to evaluate performance is an effective screening mechanism.

4.6.2 Average treatment effects of the leadership training workshop interventions

Table 4.2 presents summary results regarding the average treatment effects of the two workshop interventions: a workshop with unconditional incentives (T1) and a workshop with conditional incentives (T2). We do not find any evidence for T1's average treatment effects on the four outcomes of interest. In contrast, while we also do not find any average treatment effect of T2 on being nominated to the Task Force, we do find evidence that, on average, subjects in T2 are less likely to be designated to the Task Force, are less interested in running for the youth council (SK), and are less likely to engage in village youth programs.

[Table 4.2 about here.]

These results may lead us to believe that leadership training workshops with conditional incentives discourage individuals from serving in public office and hence, may adversely impact political selection (for example, if the "average" individual is a well qualified candidate for office). However, these average treatment effects mask heterogeneity among subjects within each treatment arm, in the dimensions of quality that are of interest to us. Recall that our primary goal is to investigate whether the workshop interventions cause a better quality of individuals to select into

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actually came to the workshop. Similarly, only 115 out of 190 respondents in T2 actually came to the workshop. All our analyses look at Intent–to–Treat (ITT) effects and so we look at the efficacy of the workshop as a screening device by treatment assignment. Looking only at treatment "takers" (i.e. those who actually attended the workshops) (not shown here) provide stronger evidence of its efficacy as a screening mechanism.

public service (and the relatively less qualified individuals to be screened-out of public office). To this end, we need to examine how the effects of the workshop interventions differ between subjects below and above the median of the quality measures.

4.6.3 Treatment effects heterogeneity in the dimensions of quality

We implement an OLS estimation of Eq. 4.5 to investigate the heterogeneous treatment effects of T1 and T2 on the four outcomes of interest, in each of the five dimension of quality of interest, namely, public service motivation, intellectual ability, personality, aspiration, and integrity.

4.6.3.1 Do the workshop interventions cause individuals with high Public Service Motivation (PSM) index to select into public service?

Figures 4.3, 4.4, 4.5, and 4.6 present summary results of the heterogeneous treatment effects of T1 and T2 in the dimension of public service motivation (PSM index), on being nominated as a youth leader, being designated as a youth leader, change in interest in running for the youth council since baseline, and engagement in village youth programs, respectively.

In both T1 and T2, we find evidence that subjects who are below the median of PSM index are significantly less likely to be nominated as youth leaders than their counterparts in the control group. In particular, relative to the mean probability of being nominated among low-PSM subjects in the control group (0.43), low-PSM subjects in T1 are 13 percentage points less likely to be nominated, while low-PSM subjects in T2 are 16.3 percentage points less likely to be nominated. Likewise, we find evidence that subjects in both T1 and T2 who are above the median of PSM index are significantly more likely to be nominated as youth leaders. In particular, relative to the mean probability of being nominated among high-PSM subjects in the control group (0.31), high-PSM subjects in T1 are 18.1 percentage points more

likely to be nominated, while high-PSM subjects in T2 are 15.1 percentage points more likely to be nominated. The difference in the probability of being nominated between low– and high–PSM subjects in both T1 and T2 is 31.4 percentage points and are statistically significant. These results imply that, in terms of subjects being nominated as village youth leaders, both T1 and T2 are effective in screening-out the low-PSM subjects and nudging the high-PSM ones into public service.

[Figure 4.3 about here.]

In terms of being actually designated as youth leaders, both T1 and T2 provide evidence for the screening-out of low-PSM subjects. In particular, compared to a control group mean of 0.34, low-PSM subjects in T1 are 13.4 percentage points less likely to be designated, and low-PSM subjects in T2 are 12.8 percentage points less likely to be designated. Only T1 provides evidence of the nudging of high-PSM subjects into public service. Compared to a control group mean of 0.26, high-PSM subjects in T1 are 13.5 percentage points more likely to be designated. T2 subjects who are high-PSM are no more likely than their control group counterparts to be designated as youth leaders.

[Figure 4.4 about here.]

In terms of change in interest in running for the youth council, only T2 provide evidence for the screening-out of low-PSM subjects and the nudging of high-PSM subjects into public service. Compared to a control group mean of 0.02, low-PSM subjects in T2 are 23.5 percentage points less likely to be interested in running for office since baseline. Moreover, the difference in interest between high– and low-PSM subjects in T2 is statistically significant compared to the control group as well as compared to T1. These results imply that, as far as attitudes toward public service is concerned, T2 is effective in screening-out the low-PSM individuals and nudging the high-PSM individuals into public service.

[Figure 4.5 about here.]

Lastly, in terms of engagement in village youth programs, only T2 provides evidence for the screening-out of low-PSM subjects, but both T1 and T2 provide evidence for the nudging of high-PSM subjects. In addition, the difference in engagement in youth programs between high– and low-PSM subjects in T2 (but not in T1) is statistically significant compared to the control group.

[Figure 4.6 about here.]

So far, these set of results implies that, if the goal of policy were to attract a pool of individuals with high levels of public service motivation, then a leadership training workshop—with or without conditional incentives—generally does the job. However, nudging high-PSM individuals into public service (and screening-out low-PSM ones) may come at a a cost of losing high-aptitude individuals. Although we make no normative claim as to whether it is better to have high-PSM individuals than high-aptitude ones, we would like to investigate the extent to which there is a tradeoff, to inform policymaking. In the next subsection, we examine workshop treatment effects heterogeneity in our measure of aptitude / intellectual ability—the digit span score index.

4.6.3.2 Do the workshop interventions cause individuals with high aptitude (Digit Span Score) index to select into public service?

Figures 4.7, 4.8, 4.9, and 4.10 present summary results of the heterogeneous treatment effects of T1 and T2 in the dimension of aptitude / intellectual ability (Digit Span Score index), on being nominated as a youth leader, being designated as a youth leader, change in interest in running for the youth council since baseline, and engagement in village youth programs, respectively.

Only in T2, do we find evidence that subjects who are below the median of aptitude index are significantly less likely to be nominated as youth leaders than their counterparts in the control group. In particular, relative to the mean probability of being nominated among low-aptitude subjects in the control group (0.36), low-aptitude subjects in T2 are 16.4 percentage points less likely to be nominated. Likewise, only in T2 do we find evidence that subjects who are above the median of aptitude index are significantly more likely to be nominated as youth leaders. In particular, relative to the mean probability of being nominated among high-aptitude subjects in the control group (0.32), high-aptitude subjects in T2 are 14.2 percentage points more likely to be nominated. The difference in the probability of being nominated between low–and high–PSM subjects in T2 is 30.6 percentage points and is statistically significant.

[Figure 4.7 about here.]

In terms of the three other outcomes of interest—being designated as youth leader, change in interest in running for the youth council, as well as engagement in village youth programs, we find that only in T2 do we find generally consistent evidence that low-aptitude subjects are screened-out and high-aptitude ones are nudged into serving in public office.

[Figure 4.8 about here.]
[Figure 4.9 about here.]
[Figure 4.10 about here.]

Reassuringly, these results imply that attracting high-PSM individuals do not come at a cost of losing high-aptitude individuals. However, note that it is only in workshop with conditional incentives (T2) that we see a consistent evidence for low-aptitude subjects being screened-out and high-aptitude subjects being nudged into serving in public office. If policymakers were to care about attracting not only high-PSM individuals but also those with relatively high intellectual ability, then incentives

ought to be made conditional on performance, since they reinforce the self-selection and sorting mechanism of the leadership training workshop.

4.6.3.3 Do the workshop interventions cause individuals with high personality (Big Five Inventory) index to select into public service?

We also investigate the workshop treatment effects heterogeneity in three other dimensions of candidate quality: personality (measured by the Big Five Inventory index), aspiration (measured by Kasser and Ryan (1993) index) and integrity (an index based on a set of baseline survey questionnaires).

Figures 4.11, 4.12, 4.13, and 4.14 present summary results of the heterogeneous treatment effects of T1 and T2 in the dimension of personality (Big Five Inventory index), on being nominated as a youth leader, being designated as a youth leader, change in interest in running for the youth council since baseline, and engagement in village youth programs, respectively.

Overall, we find little evidence for systematic heterogeneity in the dimension of personality. Across the four outcomes of interest, only in the change in interest in running for the youth council since baseline do we find that T2 has heterogeneous effects. In particular, we find that low-personality subjects in T2 are less interested in running for office than their counterparts in the control group.

[Figure 4.11 about here.]

[Figure 4.12 about here.]

[Figure 4.13 about here.]

[Figure 4.14 about here.]

4.6.3.4 Do the workshop interventions cause individuals with high aspiration index to select into public service?

Figures 4.15, 4.16, 4.17, and 4.18 present summary results of the heterogeneous treatment effects of T1 and T2 in the dimension of aspiration (Kasser and Ryan (1993) index), on being nominated as a youth leader, being designated as a youth leader, change in interest in running for the youth council since baseline, and engagement in village youth programs, respectively.

Only in T2, do we find evidence that subjects who are below the median of aspiration index are significantly less likely to be nominated as youth leaders than their counterparts in the control group. In particular, relative to the mean probability of being nominated among low-aspiration subjects in the control group (0.46), low-aptitude subjects in T2 are 13.5 percentage points less likely to be nominated. In contrast, only in T1 do we find evidence that subjects who are above the median of aspiration index are significantly more likely to be nominated as youth leaders. In particular, relative to the mean probability of being nominated among high-aspiration subjects in the control group (0.37), high-aptitude subjects in T1 are 18.0 percentage points more likely to be nominated. The difference in the probability of being nominated between low– and high–aspiration subjects in T1 and T2 are 30.7 and 25.0 percentage points, respectively, and both are statistically significant.

[Figure 4.15 about here.]

In terms of the three other outcomes of interest—being designated as youth leader, change in interest in running for the youth council, as well as engagement in village youth programs, our findings mirror the heterogeneity of effects in aptitude. That is, we find that only in T2 do we find generally consistent evidence that low-aspiration subjects are screened-out and high-aspiration ones are nudged into serving in public office.

4.6.3.5 Do the workshop interventions cause individuals with high integrity index to select into public service?

Lastly, we investigate heterogeneity of workshop treatment effects in the dimension of integrity. As mentioned earlier, we did not include this dimension of quality in our pre-analysis plan, hence, our analyses here are exploratory. Scholars of political selection (e.g. Besley and Coate, 1997; Caselli and Morelli, 2004; Mansbridge, 2009) note that there are two broad categories of quality of politicians that ought to be considered—competence and honesty. One may view the four previous qualities as dimensions of competence, but not of honesty. Our analyses below attempts to get at heterogeneity in treatment effects in the dimension of integrity/honesty.

Figures 4.19, 4.20, 4.21, and 4.22 present summary results of the heterogeneous treatment effects of T1 and T2 in the dimension of integrity, on being nominated as a youth leader, being designated as a youth leader, change in interest in running for the youth council since baseline, and engagement in village youth programs, respectively.

We find that only in T2, do we find evidence that subjects who are below the median of integrity index are significantly less likely to be nominated as youth leaders than their counterparts in the control group. In particular, relative to the mean probability of being nominated among low-integrity subjects in the control group (0.44), low-integrity subjects in T2 are 16.3 percentage points less likely to be nominated. In contrast, only in T1 do we find evidence that subjects who are above the median of integrity index are significantly more likely to be nominated as youth leaders. In particular, relative to the mean probability of being nominated among high-integrity subjects in the control group (0.35), high-integrity subjects in T1 are 15.6 percent-

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age points more likely to be nominated. The difference in the probability of being nominated between low– and high–aspiration subjects in T2 (but not in T1) is 28.6 percentage points and is statistically significant.

[Figure 4.19 about here.]

In terms of the three other outcomes of interest—being designated as youth leader, change in interest in running for the youth council, as well as engagement in village youth programs, our findings mirror the heterogeneity of effects in aptitude. That is, we find that only in T2 do we find generally consistent evidence that low-integrity subjects are screened-out and high-integrity ones are nudged into serving in public office.

[Figure 4.20 about here.]

[Figure 4.21 about here.]

[Figure 4.22 about here.]

4.7   Conclusion

Using a novel data set combining survey data on respondent characteristics with data on behavior in a leadership training workshop as well as with survey and administrative data on interest and actual decisions to serve in public office, we show that individuals who attend a workshop with conditional incentives and are below the median of quality measures are less interested in standing in election, less likely to engage in youth programs and are less likely to be nominated and designated as youth leaders in their respective villages, and that those above the median of quality measures behave in the opposite way.

These findings offer two insights in our quest for improving governance. First, we need not wait for incompetent and dishonest individuals to hold public office

before holding them accountable, especially since "punishing" corrupt politicians can be difficult and can have adverse consequences (c.f. Bobonis, Camara-Fuertes and Schwabe (2013); Ramalho (2007)). Instead, we can influence political selection at the outset, by screening-in individuals that hold desirable qualities—public service motivation, ambition (but not avarice), aptitude, integrity, and good personality.

Second, we need to reevaluate policies that incentivize good politicians. Even small rewards such as a plaque of merit or campaign posters can be very effective in nudging individuals to stand for office. But with imperfect information, incompetent and dishonest ones can pretend to be otherwise, and this adverse selection can undermine the efficacy of incentives. While recent studies have shown that incentives work in motivating good quality citizens to respond to the call to public service (Dal Bo, Finan and Rossi, 2013; Besley, 2004; Gagliarducci and Nannicini, 2013) there is room for improving efficiency by designing mechanisms that can screen-in good types and implementing incentives conditional on revealed quality.

In particular, there is scope for political parties, nonprofit, and civic organizations to develop and scale up programs that can selectively nudge competent and honest politicians, especially at the onset of their careers. These can be in the form of leadership training workshops with conditional incentives (pecuniary and non-pecuniary), merit-based endorsements, and competitive internships and scholarships to attend specialized training for aspiring politicians, among others. More evaluations are needed, to be sure, to test their generalizability. The point is, such policies that employ screening mechanisms and use conditional incentives to improve political selection can complement policies that improve citizens' ability to hold politicians accountable and, ultimately, improve governance in developing democracies.

Tables

Table 4.1: Baseline survey summary statistics and balance tests

Variable	Full sample	Control group (C)	Workshop uncond'l (T1)	Workshop cond'l (T2)	C=T1	C=T2	T1=T2	C=T1=T2
						P-values		
Observations	569	189	190	190				
Panel A: Demographic characteristics								
Female (indicator)	0.61	0.61	0.62	0.59	0.802	0.707	0.530	0.819
	(0.49)	(0.49)	(0.49)	(0.50)				
Years of age	15.99	15.99	16.04	15.94	0.509	0.502	0.188	0.420
	(0.78)	(0.77)	(0.80)	(0.77)				
Weight (pounds)	101.28	101.77	100.03	102.05	0.282	0.870	0.200	0.374
	(15.81)	(16.78)	(14.58)	(16.01)				
Height (inches)	61.82	61.80	61.51	62.16	0.441	0.337	0.079	0.212
	(3.63)	(3.70)	(3.53)	(3.66)				
Body mass index (BMI)	18.65	18.72	18.64	18.60	0.739	0.621	0.879	0.879
	(2.56)	(2.41)	(2.66)	(2.62)				
Interest in joining SK (scale of 0–10)	8.46	8.39	8.56	8.44	0.371	0.805	0.525	0.649
	(1.96)	(1.99)	(1.86)	(2.02)				
Panel B: Candidate quality (z–scores)								
Public service motivation	0.00	-0.08	0.03	0.06	0.375	0.225	0.780	0.454
	(1.00)	(1.00)	(1.05)	(0.96)				
Intellectual ability	0.00	-0.03	-0.04	0.07	0.929	0.393	0.308	0.546
	(1.00)	(1.09)	(0.94)	(0.98)				
Personality	0.00	-0.05	0.03	0.02	0.517	0.565	0.923	0.776
	(1.00)	(0.99)	(1.05)	(0.97)				
Aspiration	0.00	-0.05	0.10	-0.05	0.157	0.969	0.160	0.260
	(1.00)	(0.98)	(0.96)	(1.05)				
Integrity	0.00	-0.07	0.02	0.05	0.403	0.246	0.785	0.487
	(1.00)	(0.99)	(1.05)	(0.96)				

Notes: Variables in Panels A and B are collected at baseline, administered from August 26 to September 14, 2013, prior to treatment assignment. Respondents randomized with equal (1/3) probability into the control group (C), workshop with unconditional incentives group (T1), or workshop with conditional incentives group (T2). Numbers reported are means. Numbers in parenthesis are standard deviations. P-values are for F-tests that mean of variables is equal across the specified treatment conditions. A test of joint orthogonality, an alternative balance test, based on a multinomial logit regression of treatment on baseline variables reported in panels A & B above gives a p-value of 0.8299.

Table 4.2: Reduced form effects of workshop treatments (Intent-to-Treat)

	Nominated to Task Force (1)	Designated to Task Force (2)	Change in interest in joining SK (3)	Engagement in youth programs (4)
T1: (Unconditional)	-0.04	-0.06	-0.07	-0.01
	(0.06)	(0.05)	(0.08)	(0.05)
T2: (Conditional)	-0.08	-0.10*	-0.20**	-0.09*
	(0.06)	(0.05)	(0.08)	(0.05)
Observations	559	559	559	535
C: No workshop (mean)	0.34	0.31	0.04	0.78

Notes: Each column is from a separate OLS regression of the outcome specified on the treatments. Controls include village fixed effects and variables in panels A and B in Table 4.1. Huber-White robust standard errors in parentheses. $^{***}p < 0.01,^{**}p < 0.05,^{*}p < 0.1$ based on two-sided hypothesis tests.

Figures

Figure 4.1: Study timeline and intervention flowchart.
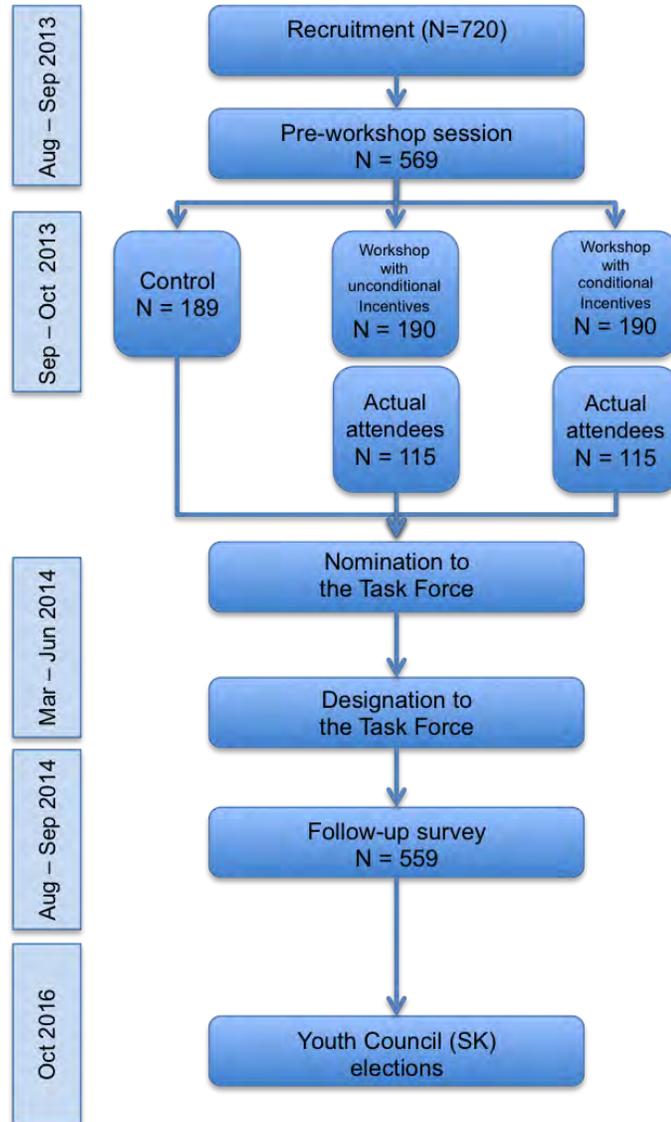
Figure 4.2: Is the leadership training workshop an effective screening mechanism?
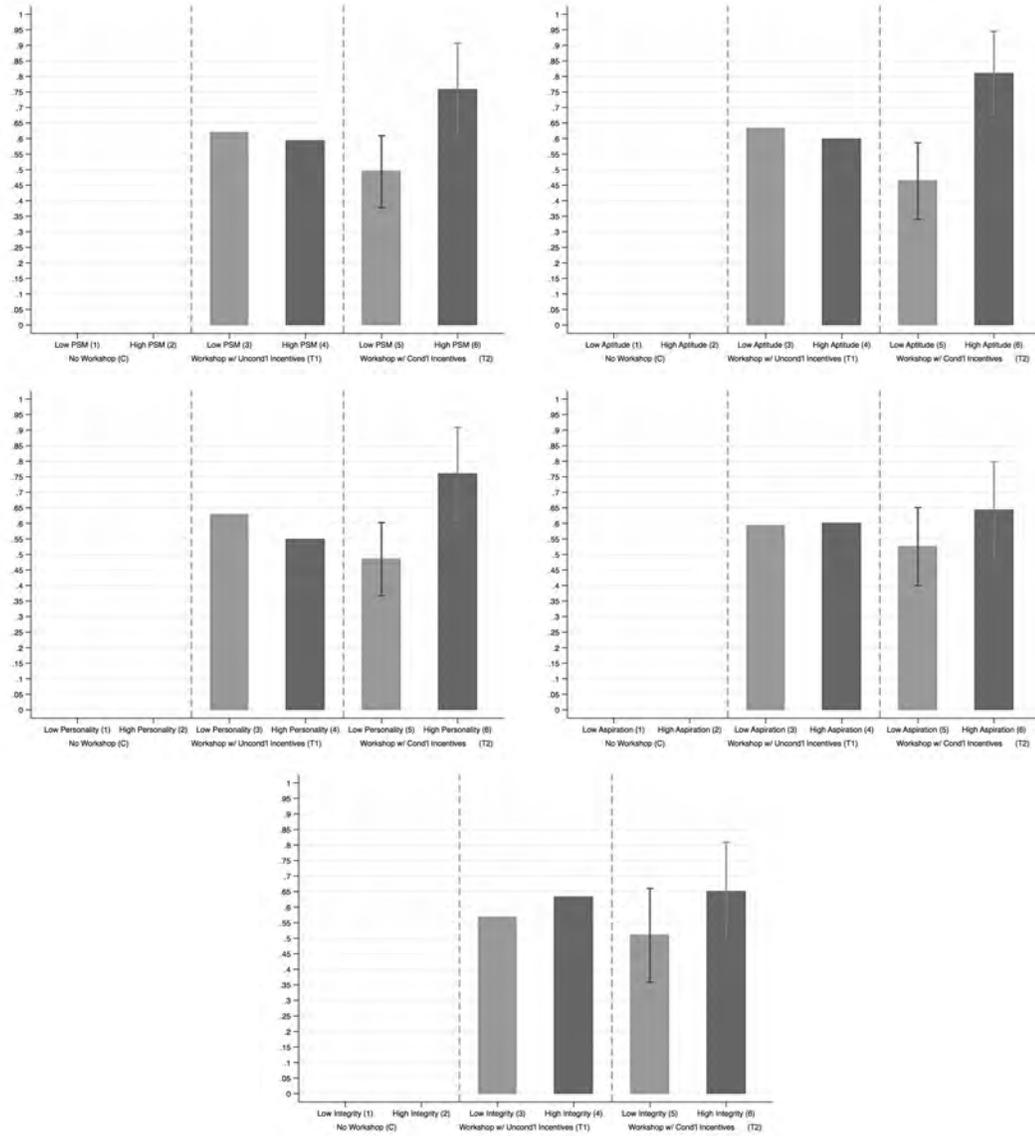
Notes: Each graph is from a separate OLS regression of the outcome, which is an indicator for receiving the incentives, on the treatments and their interactions with an indicator for being above the median of each of the five dimensions of quality. PSM, aptitude, personality, aspiration and integrity are z-scores based on the PSM index, digit span scores, BFI index, aspiration index, and survey questions on integrity, respectively. Controls include village fixed effects and variables in panels A and B in Table 4.1. Vertical lines represent 95% confidence intervals from two-sided hypothesis tests.

Figure 4.3: Heterogeneous effects of workshop treatments on nomination as a youth leader, among individuals with low and high public service motivation.
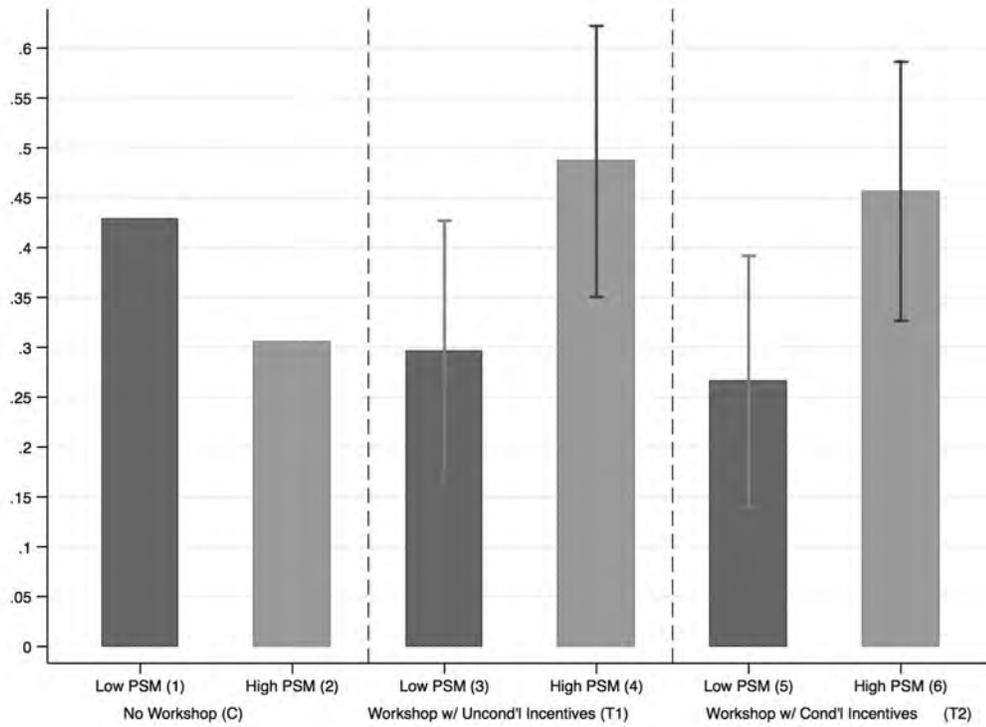
$$nominated_i = \beta_0 + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T1_i^* HighPSM_i +$$
$$\beta_4 T2_i^* HighPSM_i + \beta_5 HighPSM_i + X_i'\Gamma + \epsilon_i$$

Hypothesis Test	Graphical Test	Result
$\beta_1 \lesseqgtr 0$	[(3) - (1)]	-0.133 [0.0798]*
$\beta_2 < 0$	[(5) - (1)]	-0.163 [0.0766]**
$\beta_3 > 0$	[(4) - (2)]	0.181 [0.109]**
$\beta_4 > 0$	[(6) - (2)]	0.151 [0.108]*
$\beta_5 \leq 0$	[(2) - (1)]	-0.123 [0.081]*
$\beta_3 - \beta_1 \lesseqgtr 0$ [1]	[(4) - (2)] - [(3) - (1)]	0.314 (0.072)*
$\beta_4 - \beta_2 > 0$	[(6) - (2)] - [(5) - (1)]	0.314 (0.032)**
$[\beta_4 - \beta_2] - [\beta_3 - \beta_1] \lesseqgtr 0$	[(6) - (5)] - [(4) - (3)]	-0.000 (0.999)

Notes: Graph is from a separate OLS regression of the outcome, which is an indicator for nomination as youth leader on the treatments and their interactions with an indicator for being above the median of the PSM index. Controls include village fixed effects and variables in panels A and B in Table 4.1. Huber-White robust standard errors in brackets in third column of table. Prob > F based on Wald tests in parentheses in third column of table. Vertical lines in the graph represent 90% confidence intervals based on two-sided hypothesis tests. ***$p < 0.01$,** $p < 0.05$,* $p < 0.1$ in the table are from two-sided hypothesis tests when hypothesis has ambiguous sign and from one-sided hypothesis tests when hypothesis has an unambiguous sign. [1]Test not indicated in pre-analysis plan.

Figure 4.4: Heterogeneous effects of workshop treatments on designation as a youth leader, among individuals with low and high public service motivation.
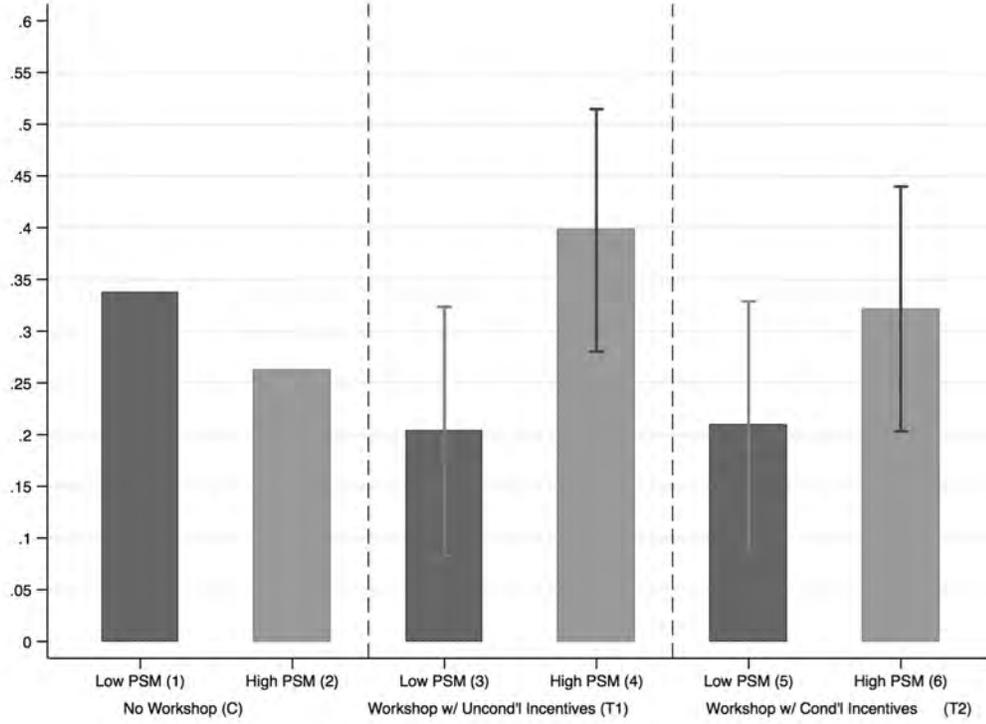

$$designated_i = \beta_0 + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T1_i^* HighPSM_i +$$
$$\beta_4 T2_i^* HighPSM_i + \beta_5 HighPSM_i + X_i'\Gamma + \epsilon_i$$

Hypothesis Test	Graphical Test	Result
$\beta_1 \lesseqgtr 0$	[(3) - (1)]	-0.134 [0.073]*
$\beta_2 < 0$	[(5) - (1)]	-0.128 [0.073]**
$\beta_3 > 0$	[(4) - (2)]	0.135 [0.097]*
$\beta_4 > 0$	[(6) - (2)]	0.059 [0.010]
$\beta_5 \leq 0$	[(2) - (1)]	-0.074 [0.074]
$\beta_3 - \beta_1 \lesseqgtr 0$ [1]	[(4) - (2)] - [(3) - (1)]	0.269 (0.090)*
$\beta_4 - \beta_2 > 0$	[(6) - (2)] - [(5) - (1)]	0.187 (0.121)
$[\beta_4 - \beta_2] - [\beta_3 - \beta_1] \lesseqgtr 0$	[(6) - (5)] - [(4) - (3)]	-0.082 (0.595)

Notes: Graph is from a separate OLS regression of the outcome, which is an indicator for designation as youth leader on the treatments and their interactions with an indicator for being above the median of the PSM index. Controls include village fixed effects and variables in panels A and B in Table 4.1. Huber-White robust standard errors in brackets in third column of table. Prob > F based on Wald tests in parentheses in third column of table. Vertical lines in the graph represent 90% confidence intervals based on two-sided hypothesis tests. ***$p < 0.01$,** $p < 0.05$,* $p < 0.1$ in the table are from two-sided hypothesis tests when hypothesis has ambiguous sign and from one-sided hypothesis tests when hypothesis has an unambiguous sign. [1]Test not indicated in pre-analysis plan.

Figure 4.5: Heterogeneous effects of workshop treatments on change in interest in joining SK since baseline, among individuals with low and high public service motivation.
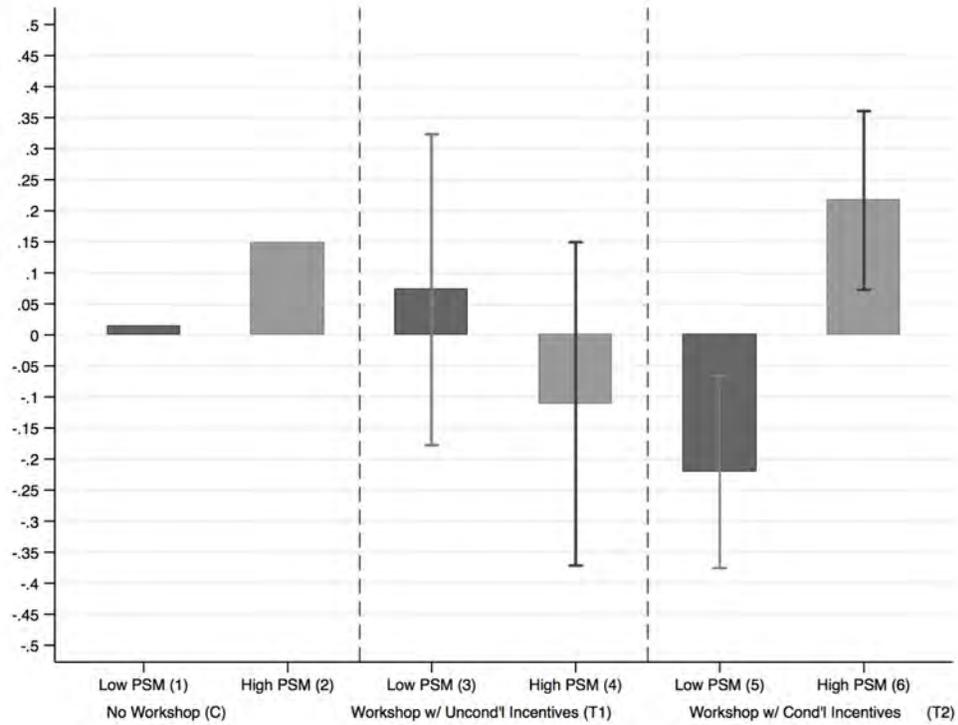

$$interest\_change_i = \beta_0 + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T1_i^* HighPSM_i +$$
$$\beta_4 T2_i^* HighPSM_i + \beta_5 HighPSM_i + X_i'\Gamma + \epsilon_i$$

Hypothesis Test	Graphical Test	Result
$\beta_1 \lessgtr 0$	[(3) - (1)]	0.059 [0.152]
$\beta_2 < 0$	[(5) - (1)]	-0.235 [0.094]***
$\beta_3 > 0$	[(4) - (2)]	-0.260 [0.231]
$\beta_4 > 0$	[(6) - (2)]	0.068 [0.151]
$\beta_5 \leq 0$	[(2) - (1)]	0.130 [0.149]
$\beta_3 - \beta_1 \lessgtr 0$ [1]	[(4) - (2)] - [(3) - (1)]	-0.318 (0.389)
$\beta_4 - \beta_2 > 0$	[(6) - (2)] - [(5) - (1)]	0.303 (0.084)*
$[\beta_4 - \beta_2] - [\beta_3 - \beta_1] \lessgtr 0$	[(6) - (5)] - [(4) - (3)]	0.622 (0.063)*

Notes: Graph is from a separate OLS regression of the outcome, which is the percentage point change in interest in joining SK since baseline, on the treatments and their interactions with an indicator for being above the median of the PSM index. Controls include village fixed effects and variables in panels A and B in Table 4.1. Huber-White robust standard errors in brackets in third column of table. Prob > F based on Wald tests in parentheses in third column of table. Vertical lines in the graph represent 90% confidence intervals based on two-sided hypothesis tests. ***$p < 0.01$,** $p < 0.05$,* $p < 0.1$ in the table are from two-sided hypothesis tests when hypothesis has ambiguous sign and from one-sided hypothesis tests when hypothesis has an unambiguous sign. [1]Test not indicated in pre-analysis plan.

Figure 4.6: Heterogeneous effects of workshop treatments on engagement in village youth programs, among individuals with low and high public service motivation.
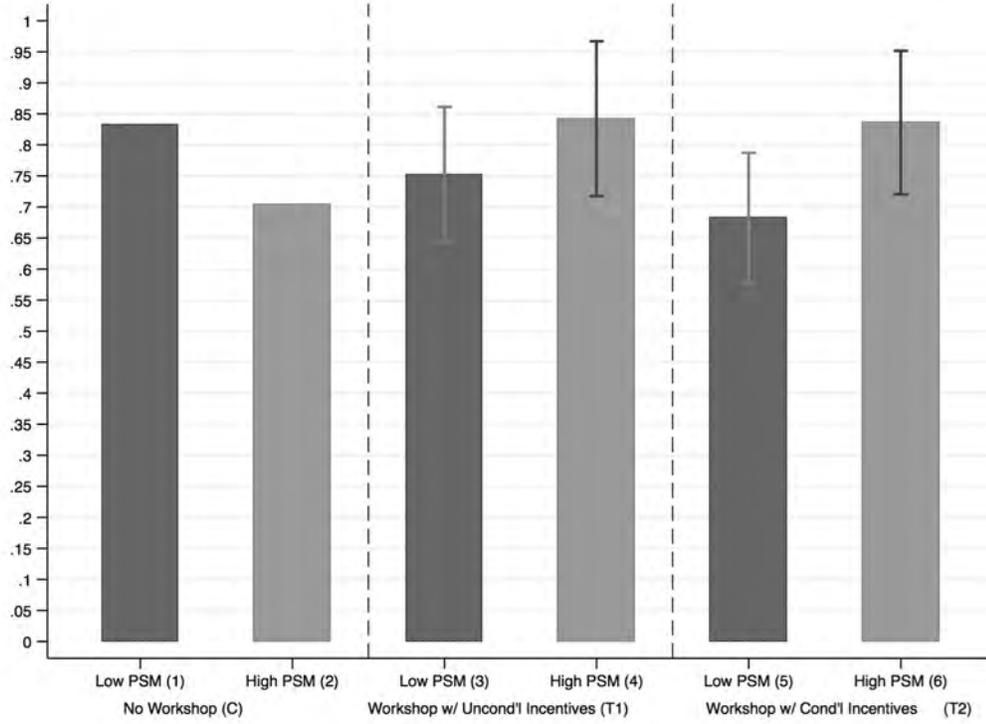
$$youth\_engage_i = \beta_0 + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T1_i^* HighPSM_i +$$
$$\beta_4 T2_i^* HighPSM_i + \beta_5 HighPSM_i + X_i'\Gamma + \epsilon_i$$

Hypothesis Test	Graphical Test	Result
$\beta_1 \lessgtr 0$	[(3) - (1)]	-0.079 [0.066]
$\beta_2 < 0$	[(5) - (1)]	-0.149 [0.065]***
$\beta_3 > 0$	[(4) - (2)]	0.139 [0.096]*
$\beta_4 > 0$	[(6) - (2)]	0.132 [0.096]*
$\beta_5 \leq 0$	[(2) - (1)]	-0.128 [0.075]**
$\beta_3 - \beta_1 \lessgtr 0$ [1]	[(4) - (2)] - [(3) - (1)]	0.218 (0.147)
$\beta_4 - \beta_2 > 0$	[(6) - (2)] - [(5) - (1)]	0.281 (0.028)**
$[\beta_4 - \beta_2] - [\beta_3 - \beta_1] \lessgtr 0$	[(6) - (5)] - [(4) - (3)]	0.064 (0.685)

Notes: Graph is from a separate OLS regression of the outcome, which is an indicator for engagement in village youth programs on the treatments and their interactions with an indicator for being above the median of the PSM index. Controls include village fixed effects and variables in panels A and B in Table 4.1. Huber-White robust standard errors in brackets in third column of table. Prob > F based on Wald tests in parentheses in third column of table. Vertical lines in the graph represent 90% confidence intervals based on two-sided hypothesis tests. ***$p < 0.01$,** $p < 0.05$,* $p < 0.1$ in the table are from two-sided hypothesis tests when hypothesis has ambiguous sign and from one-sided hypothesis tests when hypothesis has an unambiguous sign. [1]Test not indicated in pre-analysis plan.

Figure 4.7: Heterogeneous effects of workshop treatments on nomination as a youth leader, among individuals with low and high aptitude.
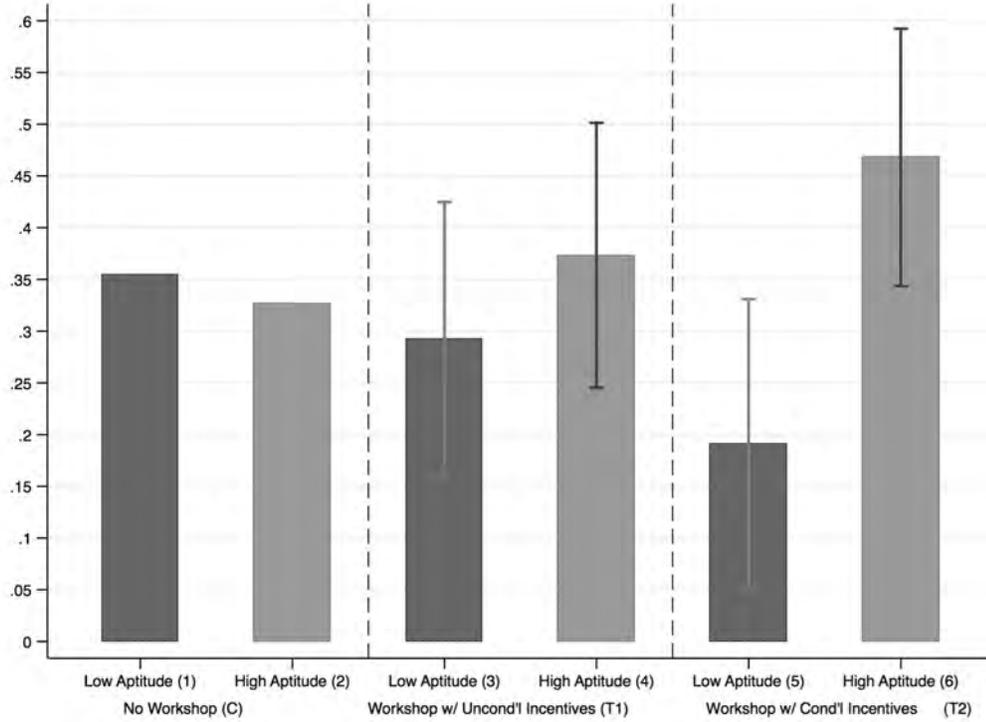
$$nominated_i = \beta_0 + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T1_i^* HighAptitude_i +$$
$$\beta_4 T2_i^* HighAptitude_i + \beta_5 HighAptitude_i + X_i'\Gamma + \epsilon_i$$

Hypothesis Test	Graphical Test	Result
$\beta_1 \lessgtr 0$	[(3) - (1)]	-0.063 [0.081]
$\beta_2 < 0$	[(5) - (1)]	-0.164 [0.085]**
$\beta_3 > 0$	[(4) - (2)]	0.047 [0.110]
$\beta_4 > 0$	[(6) - (2)]	0.142 [0.109]*
$\beta_5 \leq 0$	[(2) - (1)]	-0.028 [0.080]
$\beta_3 - \beta_1 \lessgtr 0$ [1]	[(4) - (2)] - [(3) - (1)]	0.109 (0.536)
$\beta_4 - \beta_2 > 0$	[(6) - (2)] - [(5) - (1)]	0.306 (0.046)**
$[\beta_4 - \beta_2] - [\beta_3 - \beta_1] \lessgtr 0$	[(6) - (5)] - [(4) - (3)]	0.196 (0.266)

Notes: Graph is from a separate OLS regression of the outcome, which is an indicator for nomination as youth leader on the treatments and their interactions with an indicator for being above the median of the Digit Span Score index. Controls include village fixed effects and variables in panels A and B in Table 4.1. Huber-White robust standard errors in brackets in third column of table. Prob > F based on Wald tests in parentheses in third column of table. Vertical lines in the graph represent 90% confidence intervals based on two-sided hypothesis tests. ***$p < 0.01$,** $p < 0.05$,* $p < 0.1$ in the table are from two-sided hypothesis tests when hypothesis has ambiguous sign and from one-sided hypothesis tests when hypothesis has an unambiguous sign. [1]Test not indicated in pre-analysis plan.

Figure 4.8: Heterogeneous effects of workshop treatments on designation as a youth leader, among individuals with low and high aptitude.
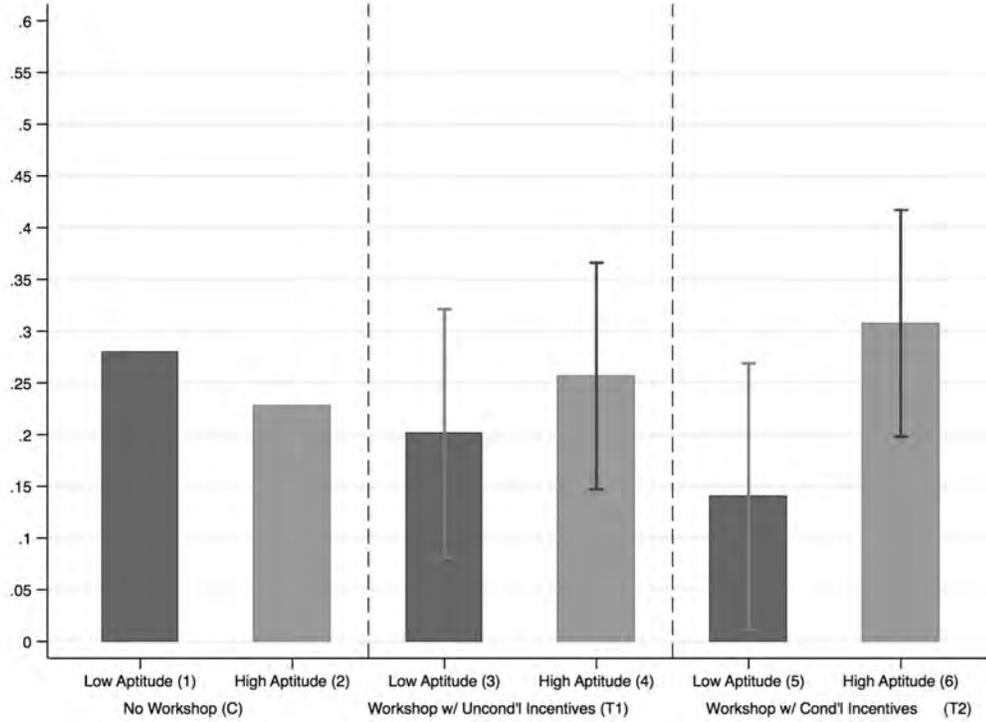

$$designated_i = \beta_0 + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T1_i^* HighAptitude_i +$$
$$\beta_4 T2_i^* HighAptitude_i + \beta_5 HighAptitude_i + X_i'\Gamma + \epsilon_i$$

Hypothesis Test	Graphical Test	Result
$\beta_1 \lessgtr 0$	[(3) - (1)]	-0.078 [0.073]
$\beta_2 < 0$	[(5) - (1)]	-0.139 [0.078]**
$\beta_3 > 0$	[(4) - (2)]	0.029 [0.098]
$\beta_4 > 0$	[(6) - (2)]	0.080 [0.101]
$\beta_5 \leq 0$	[(2) - (1)]	-0.052 [0.073]
$\beta_3 - \beta_1 \lessgtr 0$ [1]	[(4) - (2)] - [(3) - (1)]	0.107 (0.499)
$\beta_4 - \beta_2 > 0$	[(6) - (2)] - [(5) - (1)]	0.219 (0.094)*
$[\beta_4 - \beta_2] - [\beta_3 - \beta_1] \lessgtr 0$	[(6) - (5)] - [(4) - (3)]	0.112 (0.481)

Notes: Graph is from a separate OLS regression of the outcome, which is an indicator for designation as youth leader on the treatments and their interactions with an indicator for being above the median of the Digit Span Score index. Controls include village fixed effects and variables in panels A and B in Table 4.1. Huber-White robust standard errors in brackets in third column of table. Prob > F based on Wald tests in parentheses in third column of table. Vertical lines in the graph represent 90% confidence intervals based on two-sided hypothesis tests. ***$p < 0.01$,** $p < 0.05$,* $p < 0.1$ in the table are from two-sided hypothesis tests when hypothesis has ambiguous sign and from one-sided hypothesis tests when hypothesis has an unambiguous sign. [1]Test not indicated in pre-analysis plan.

Figure 4.9: Heterogeneous effects of workshop treatments on change in interest in joining SK since baseline, among individuals with low and high aptitude.
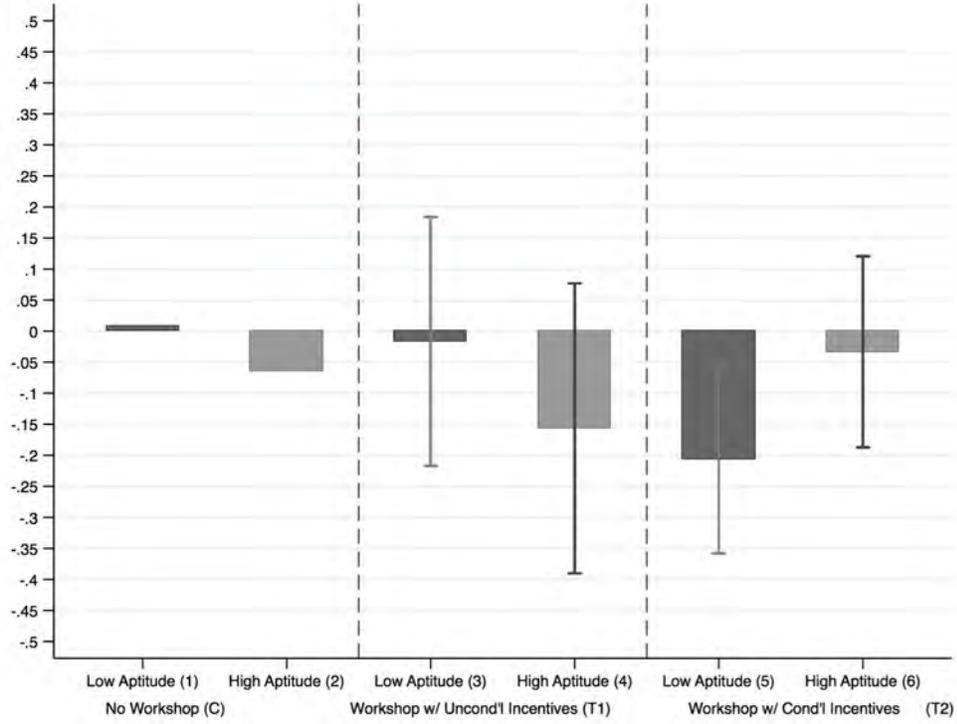

$$interest\_change_i = \beta_0 + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T1_i^* HighAptitude_i +$$
$$\beta_4 T2_i^* HighAptitude_i + \beta_5 HighAptitude_i + X_i'\Gamma + \epsilon_i$$

Hypothesis Test	Graphical Test	Result
$\beta_1 \lessgtr 0$	[(3) - (1)]	-0.025 [0.122]
$\beta_2 < 0$	[(5) - (1)]	-0.216 [0.0918]***
$\beta_3 > 0$	[(4) - (2)]	-0.092 [0.159]
$\beta_4 > 0$	[(6) - (2)]	0.032 [0.150]
$\beta_5 \leq 0$	[(2) - (1)]	-0.074 [0.143]
$\beta_3 - \beta_1 \lessgtr 0$ [1]	[(4) - (2)] - [(3) - (1)]	-0.067 (0.801)
$\beta_4 - \beta_2 > 0$	[(6) - (2)] - [(5) - (1)]	0.247 (0.128)
$[\beta_4 - \beta_2] - [\beta_3 - \beta_1] \lessgtr 0$	[(6) - (5)] - [(4) - (3)]	0.314 (0.239)

Notes: Graph is from a separate OLS regression of the outcome, which is the percentage point change in interest in joining SK since baseline, on the treatments and their interactions with an indicator for being above the median of the Digit Span Score index. Controls include village fixed effects and variables in panels A and B in Table 4.1. Huber-White robust standard errors in brackets in third column of table. Prob > F based on Wald tests in parentheses in third column of table. Vertical lines in the graph represent 90% confidence intervals based on two-sided hypothesis tests. ***$p < 0.01$,** $p < 0.05$,* $p < 0.1$ in the table are from two-sided hypothesis tests when hypothesis has ambiguous sign and from one-sided hypothesis tests when hypothesis has an unambiguous sign. [1]Test not indicated in pre-analysis plan.

Figure 4.10: Heterogeneous effects of workshop treatments on engagement in village youth programs, among individuals with low and high aptitude.
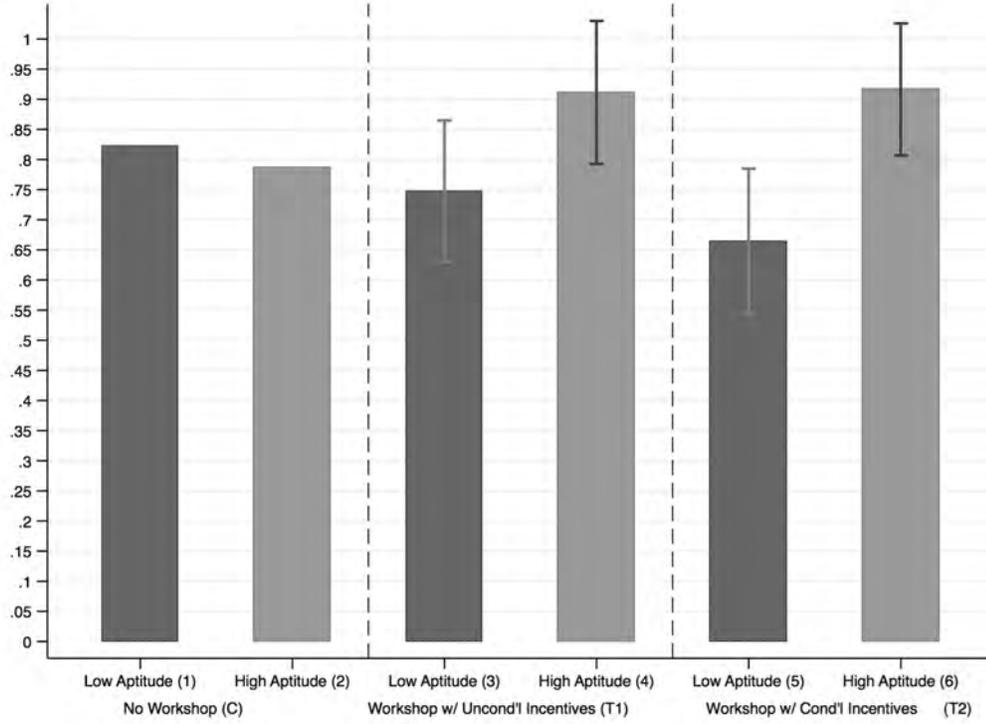

$$youth\_engage_i = \beta_0 + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T1_i^* HighAptitude_i +$$
$$\beta_4 T2_i^* HighAptitude_i + \beta_5 HighAptitude_i + X_i' \Gamma + \epsilon_i$$

Hypothesis Test	Graphical Test	Result
$\beta_1 \lesseqgtr 0$	[(3) - (1)]	-0.075 [0.071]
$\beta_2 < 0$	[(5) - (1)]	-0.158 [0.073]**
$\beta_3 > 0$	[(4) - (2)]	0.125 [0.096]*
$\beta_4 > 0$	[(6) - (2)]	0.129 [0.095]*
$\beta_5 \leq 0$	[(2) - (1)]	-0.035 [0.067]
$\beta_3 - \beta_1 \lesseqgtr 0$ [1]	[(4) - (2)] - [(3) - (1)]	0.199 (0.204)
$\beta_4 - \beta_2 > 0$	[(6) - (2)] - [(5) - (1)]	0.287 (0.036)**
$[\beta_4 - \beta_2] - [\beta_3 - \beta_1] \lesseqgtr 0$	[(6) - (5)] - [(4) - (3)]	0.088 (0.594)

Notes: Graph is from a separate OLS regression of the outcome, which is an indicator for engagement in village youth programs on the treatments and their interactions with an indicator for being above the median of the Digit Span Score index. Controls include village fixed effects and variables in panels A and B in Table 4.1. Huber-White robust standard errors in brackets in third column of table. Prob > F based on Wald tests in parentheses in third column of table. Vertical lines in the graph represent 90% confidence intervals based on two-sided hypothesis tests. ***$p < 0.01$,** $p < 0.05$,* $p < 0.1$ in the table are from two-sided hypothesis tests when hypothesis has ambiguous sign and from one-sided hypothesis tests when hypothesis has an unambiguous sign. [1]Test not indicated in pre-analysis plan.

Figure 4.11: Heterogeneous effects of workshop treatments on nomination as a youth leader, among individuals with low and high personality.
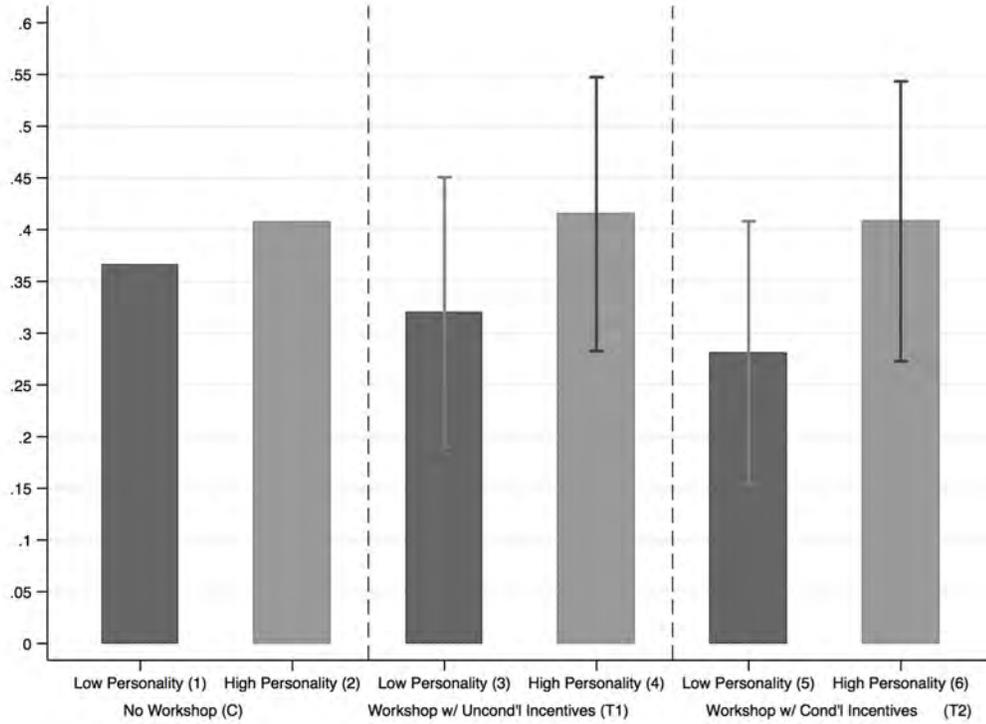
$$nominated_i = \beta_0 + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T1_i^* HighPersonality_i +$$
$$\beta_4 T2_i^* HighPersonality_i + \beta_5 HighPersonality_i + X_i'\Gamma + \epsilon_i$$

Hypothesis Test	Graphical Test	Result
$\beta_1 \lesseqgtr 0$	$[(3) - (1)]$	-0.045 [0.079]
$\beta_2 < 0$	$[(5) - (1)]$	-0.085 [0.077]
$\beta_3 > 0$	$[(4) - (2)]$	0.008 [0.108]
$\beta_4 > 0$	$[(6) - (2)]$	0.001 [0.111]
$\beta_5 \leq 0$	$[(2) - (1)]$	0.042 [0.082]
$\beta_3 - \beta_1 \lesseqgtr 0$ [1]	$[(4) - (2)] - [(3) - (1)]$	0.053 (0.758)
$\beta_4 - \beta_2 > 0$	$[(6) - (2)] - [(5) - (1)]$	0.086 (0.309)
$[\beta_4 - \beta_2] - [\beta_3 - \beta_1] \lesseqgtr 0$	$[(6) - (5)] - [(4) - (3)]$	0.032 (0.850)

Notes: Graph is from a separate OLS regression of the outcome, which is an indicator for nomination as youth leader on the treatments and their interactions with an indicator for being above the median of the Big Five Inventory index. Controls include village fixed effects and variables in panels A and B in Table 4.1. Huber-White robust standard errors in brackets in third column of table. Prob > F based on Wald tests in parentheses in third column of table. Vertical lines in the graph represent 90% confidence intervals based on two-sided hypothesis tests. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$ in the table are from two-sided hypothesis tests when hypothesis has ambiguous sign and from one-sided hypothesis tests when hypothesis has an unambiguous sign. [1]Test not indicated in pre-analysis plan.

Figure 4.12: Heterogeneous effects of workshop treatments on designation as a youth leader, among individuals with low and high personality.
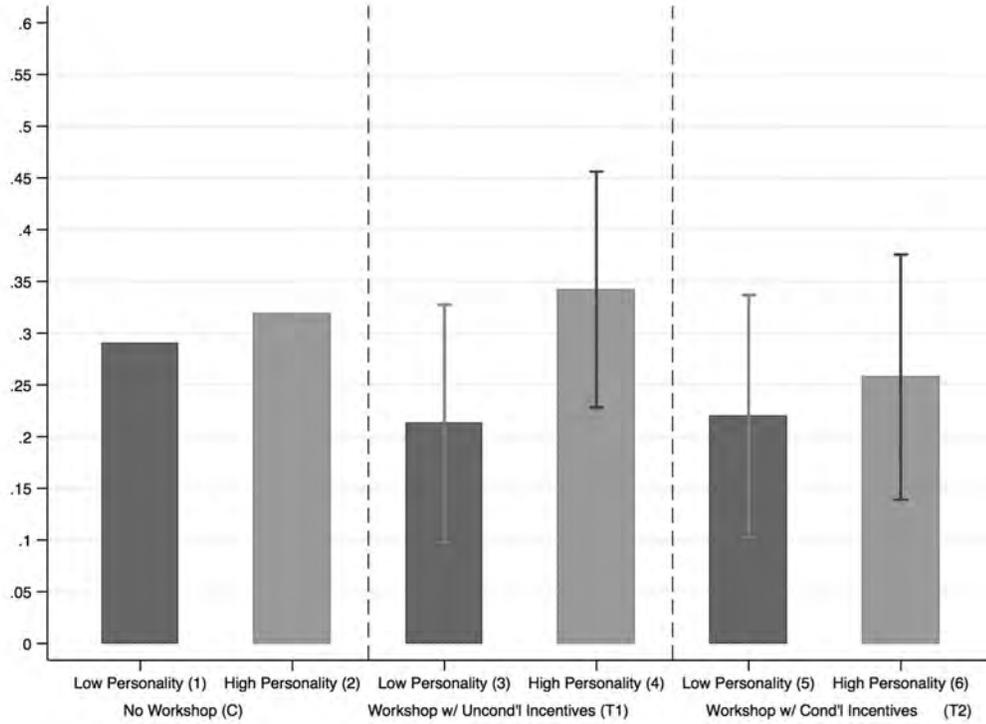

$$designated_i = \beta_0 + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T1_i^* HighPersonality_i +$$
$$\beta_4 T2_i^* HighPersonality_i + \beta_5 HighPersonality_i + X_i'\Gamma + \epsilon_i$$

Hypothesis Test	Graphical Test	Result
$\beta_1 \lessgtr 0$	[(3) - (1)]	-0.078 [0.070]
$\beta_2 < 0$	[(5) - (1)]	-0.071 [0.071]
$\beta_3 > 0$	[(4) - (2)]	0.023 [0.098]
$\beta_4 > 0$	[(6) - (2)]	-0.061 [0.100]
$\beta_5 \leq 0$	[(2) - (1)]	0.029 [0.076]
$\beta_3 - \beta_1 \lessgtr 0$ [1]	[(4) - (2)] - [(3) - (1)]	0.101 (0.510)
$\beta_4 - \beta_2 > 0$	[(6) - (2)] - [(5) - (1)]	0.010 (0.476)
$[\beta_4 - \beta_2] - [\beta_3 - \beta_1] \lessgtr 0$	[(6) - (5)] - [(4) - (3)]	-0.092 (0.531)

Notes: Graph is from a separate OLS regression of the outcome, which is an indicator for designation as youth leader on the treatments and their interactions with an indicator for being above the median of the Big Five Inventory index. Controls include village fixed effects and variables in panels A and B in Table 4.1. Huber-White robust standard errors in brackets in third column of table. Prob > F based on Wald tests in parentheses in third column of table. Vertical lines in the graph represent 90% confidence intervals based on two-sided hypothesis tests. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$ in the table are from two-sided hypothesis tests when hypothesis has ambiguous sign and from one-sided hypothesis tests when hypothesis has an unambiguous sign. [1]Test not indicated in pre-analysis plan.

Figure 4.13: Heterogeneous effects of workshop treatments on change in interest in joining SK since baseline, among individuals with low and high personality.
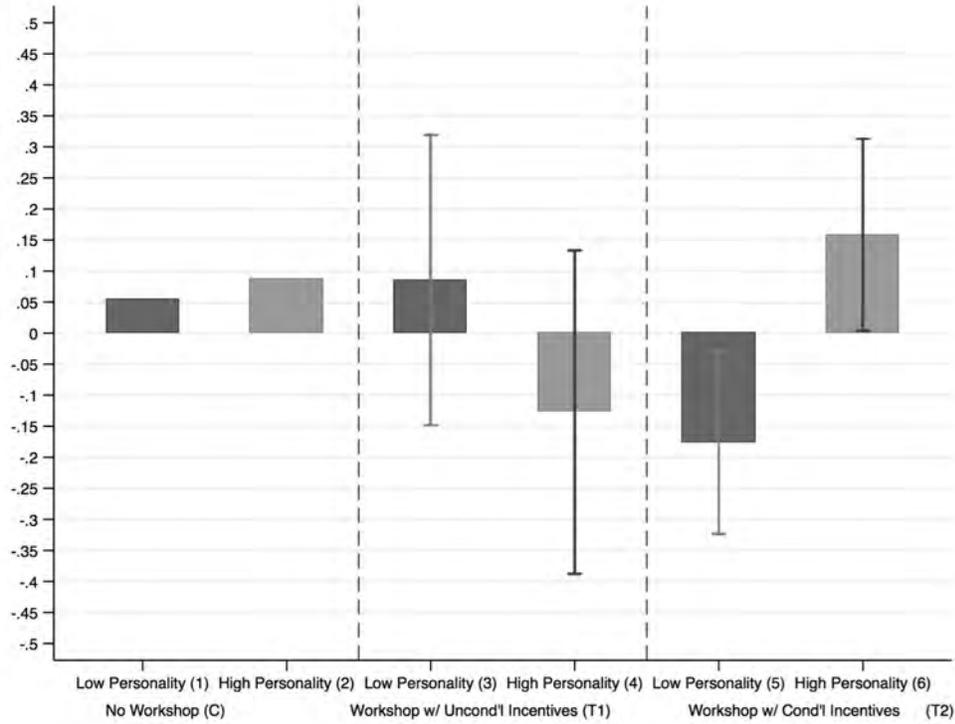

$$interest\_change_i = \beta_0 + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T1_i^* HighPersonality_i + \beta_4 T2_i^* HighPersonality_i + \beta_5 HighPersonality_i + X_i'\Gamma + \epsilon_i$$

Hypothesis Test	Graphical Test	Result
$\beta_1 \lessgtr 0$	[(3) - (1)]	0.031 [0.142]
$\beta_2 < 0$	[(5) - (1)]	-0.230 [0.090]***
$\beta_3 > 0$	[(4) - (2)]	-0.214 [0.214]
$\beta_4 > 0$	[(6) - (2)]	0.072 [0.136]
$\beta_5 \leq 0$	[(2) - (1)]	0.032 [0.151]
$\beta_3 - \beta_1 \lessgtr 0$ [1]	[(4) - (2)] - [(3) - (1)]	-0.245 (0.475)
$\beta_4 - \beta_2 > 0$	[(6) - (2)] - [(5) - (1)]	0.302 (0.067)*
$[\beta_4 - \beta_2] - [\beta_3 - \beta_1] \lessgtr 0$	[(6) - (5)] - [(4) - (3)]	0.546 (0.095)*

Notes: Graph is from a separate OLS regression of the outcome, which is the percentage point change in interest in joining SK since baseline, on the treatments and their interactions with an indicator for being above the median of the Big Five Inventory index. Controls include village fixed effects and variables in panels A and B in Table 4.1. Huber-White robust standard errors in brackets in third column of table. Prob > F based on Wald tests in parentheses in third column of table. Vertical lines in the graph represent 90% confidence intervals based on two-sided hypothesis tests. ***$p < 0.01$,** $p < 0.05$,* $p < 0.1$ in the table are from two-sided hypothesis tests when hypothesis has ambiguous sign and from one-sided hypothesis tests when hypothesis has an unambiguous sign. [1]Test not indicated in pre-analysis plan.

Figure 4.14: Heterogeneous effects of workshop treatments on engagement in village youth programs, among individuals with low and high personality.
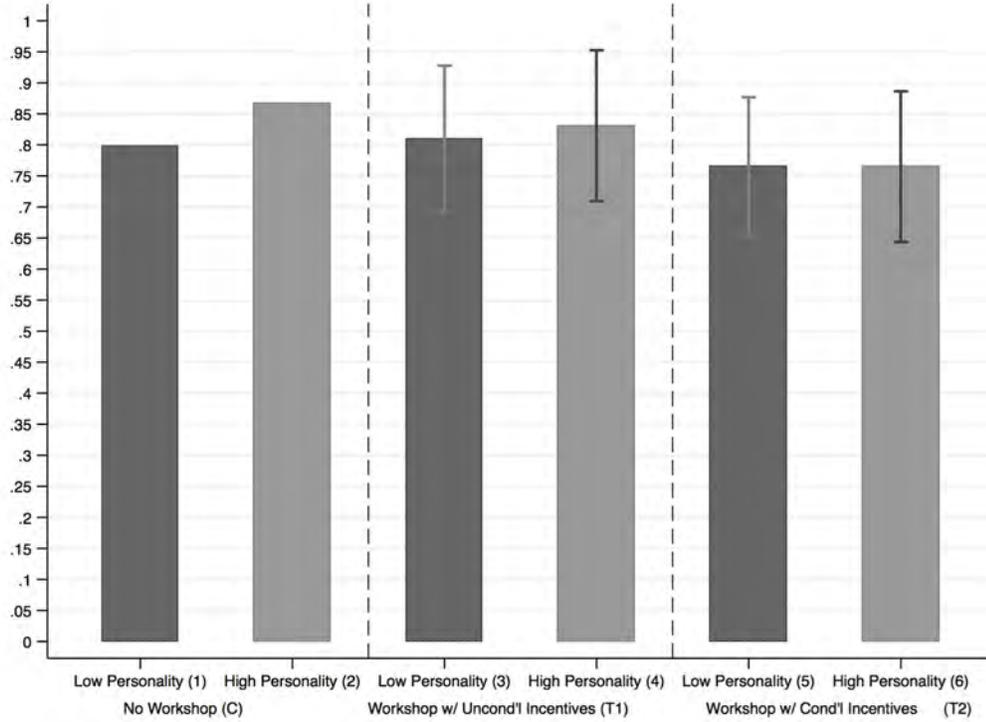

$$youth\_engage_i = \beta_0 + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T1_i^* HighPersonality_i +$$
$$\beta_4 T2_i^* HighPersonality_i + \beta_5 HighPersonality_i + X_i'\Gamma + \epsilon_i$$

Hypothesis Test	Graphical Test	Result
$\beta_1 \lessgtr 0$	[(3) - (1)]	0.012 [0.072]
$\beta_2 < 0$	[(5) - (1)]	-0.032 [0.068]
$\beta_3 > 0$	[(4) - (2)]	-0.035 [0.094]
$\beta_4 > 0$	[(6) - (2)]	-0.101 [0.095]
$\beta_5 \lessgtr 0$	[(2) - (1)]	0.069 [0.076]
$\beta_3 - \beta_1 \lessgtr 0$ [1]	[(4) - (2)] - [(3) - (1)]	-0.047 (0.763)
$\beta_4 - \beta_2 > 0$	[(6) - (2)] - [(5) - (1)]	-0.069 (0.324)
$[\beta_4 - \beta_2] - [\beta_3 - \beta_1] \lessgtr 0$	[(6) - (5)] - [(4) - (3)]	-0.022 (0.891)

Notes: Graph is from a separate OLS regression of the outcome, which is an indicator for engagement in village youth programs on the treatments and their interactions with an indicator for being above the median of the Big Five Inventory index. Controls include village fixed effects and variables in panels A and B in Table 4.1. Huber-White robust standard errors in brackets in third column of table. Prob > F based on Wald tests in parentheses in third column of table. Vertical lines in the graph represent 90% confidence intervals based on two-sided hypothesis tests. $^{***}p < 0.01,^{**}p < 0.05,^*p < 0.1$ in the table are from two-sided hypothesis tests when hypothesis has ambiguous sign and from one-sided hypothesis tests when hypothesis has an unambiguous sign. [1]Test not indicated in pre-analysis plan.

Figure 4.15: Heterogeneous effects of workshop treatments on nomination as a youth leader, among individuals with low and high aspiration.
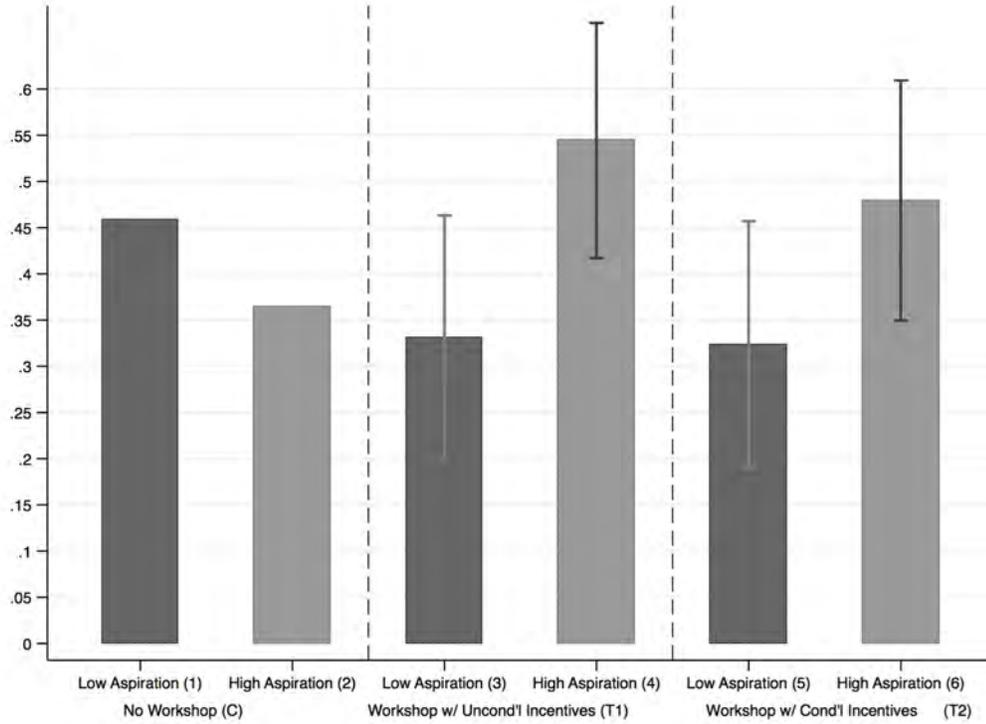
$$nominated_i = \beta_0 + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T1_i^* HighAspiration_i +$$
$$\beta_4 T2_i^* HighAspiration_i + \beta_5 HighAspiration_i + X_i'\Gamma + \epsilon_i$$

Hypothesis Test	Graphical Test	Result
$\beta_1 \lesseqgtr 0$	[(3) - (1)]	-0.127 [0.080]
$\beta_2 < 0$	[(5) - (1)]	-0.135 [0.081]**
$\beta_3 > 0$	[(4) - (2)]	0.180 [0.113]*
$\beta_4 > 0$	[(6) - (2)]	0.114 [0.112]
$\beta_5 \leq 0$	[(2) - (1)]	-0.094 [0.083]
$\beta_3 - \beta_1 \lesseqgtr 0$ [1]	[(4) - (2)] - [(3) - (1)]	0.307 (0.088)*
$\beta_4 - \beta_2 > 0$	[(6) - (2)] - [(5) - (1)]	0.250 (0.081)*
$[\beta_4 - \beta_2] - [\beta_3 - \beta_1] \lesseqgtr 0$	[(6) - (5)] - [(4) - (3)]	-0.057 (0.748)

Notes: Graph is from a separate OLS regression of the outcome, which is an indicator for nomination as youth leader on the treatments and their interactions with an indicator for being above the median of the Aspiration index. Controls include village fixed effects and variables in panels A and B in Table 4.1. Huber-White robust standard errors in brackets in third column of table. Prob > F based on Wald tests in parentheses in third column of table. Vertical lines in the graph represent 90% confidence intervals based on two-sided hypothesis tests. ***$p < 0.01$,** $p < 0.05$,* $p < 0.1$ in the table are from two-sided hypothesis tests when hypothesis has ambiguous sign and from one-sided hypothesis tests when hypothesis has an unambiguous sign. [1]Test not indicated in pre-analysis plan.

Figure 4.16: Heterogeneous effects of workshop treatments on designation as a youth leader, among individuals with low and high aspiration.
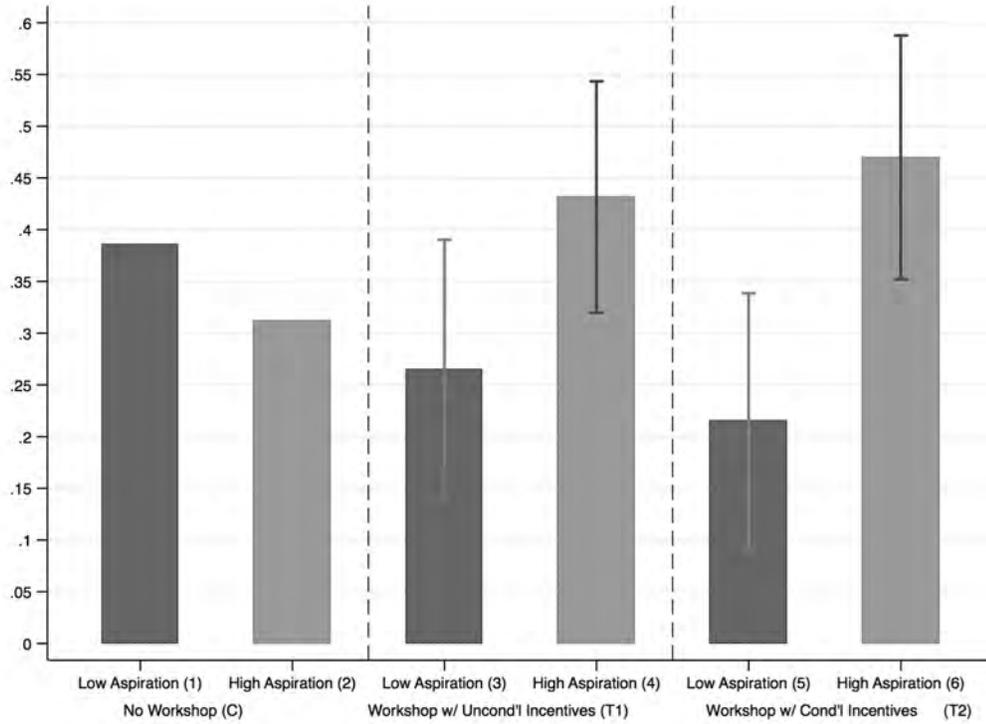

$$designated_i = \beta_0 + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T1_i^* HighAspiration_i +$$
$$\beta_4 T2_i^* HighAspiration_i + \beta_5 HighAspiration_i + X_i'\Gamma + \epsilon_i$$

Hypothesis Test	Graphical Test	Result
$\beta_1 \lesseqgtr 0$	[(3) - (1)]	-0.120 [0.076]
$\beta_2 < 0$	[(5) - (1)]	-0.171 [0.075]***
$\beta_3 > 0$	[(4) - (2)]	0.120 [0.102]
$\beta_4 > 0$	[(6) - (2)]	0.159 [0.103]*
$\beta_5 \leq 0$	[(2) - (1)]	-0.074 [0.078]
$\beta_3 - \beta_1 \lesseqgtr 0$ [1]	[(4) -0 (2)] - [(3) - (1)]	0.241 (0.150)
$\beta_4 - \beta_2 > 0$	[(6) - (2)] - [(5) - (1)]	0.329 (0.024)**
$[\beta_4 - \beta_2] - [\beta_3 - \beta_1] \lesseqgtr 0$	[(6) - (5)] - [(4) - (3)]	0.088 (0.580)

Notes: Graph is from a separate OLS regression of the outcome, which is an indicator for designation as youth leader on the treatments and their interactions with an indicator for being above the median of the Aspiration index. Controls include village fixed effects and variables in panels A and B in Table 4.1. Huber-White robust standard errors in brackets in third column of table. Prob > F based on Wald tests in parentheses in third column of table. Vertical lines in the graph represent 90% confidence intervals based on two-sided hypothesis tests. ***$p < 0.01$,** $p < 0.05$,* $p < 0.1$ in the table are from two-sided hypothesis tests when hypothesis has ambiguous sign and from one-sided hypothesis tests when hypothesis has an unambiguous sign. [1]Test not indicated in pre-analysis plan.

Figure 4.17: Heterogeneous effects of workshop treatments on change in interest in joining SK since baseline, among individuals with low and high aspiration.
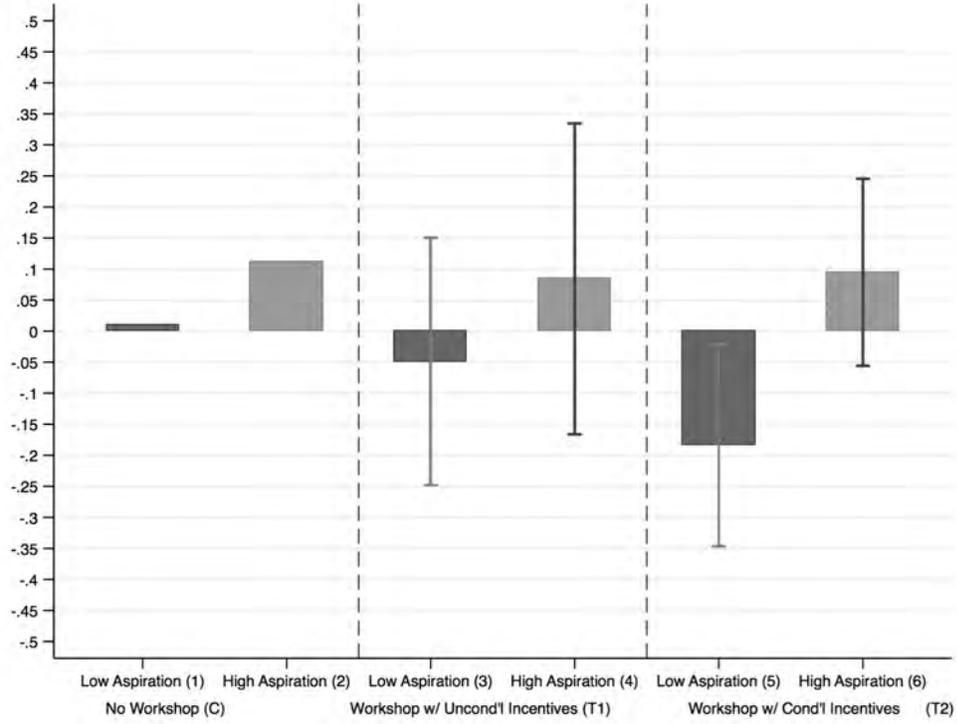
$$interest\_change_i = \beta_0 + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T1_i^* HighAspiration_i +$$
$$\beta_4 T2_i^* HighAspiration_i + \beta_5 HighAspiration_i + X_i'\Gamma + \epsilon_i$$

Hypothesis Test	Graphical Test	Result
$\beta_1 \lesseqgtr 0$	[(3) - (1)]	-0.058 [0.121]
$\beta_2 < 0$	[(5) - (1)]	-0.193 [0.099]**
$\beta_3 > 0$	[(4) - (2)]	-0.027 [0.219]
$\beta_4 > 0$	[(6) - (2)]	-0.016 [0.167]
$\beta_5 \leq 0$	[(2) - (1)]	0.101 [0.156]
$\beta_3 - \beta_1 \lesseqgtr 0$ [1]	[(4) - (2)] - [(3) - (1)]	0.032 (0.921)
$\beta_4 - \beta_2 > 0$	[(6) - (2)] - [(5) - (01)]	0.178 (0.231)
$[\beta_4 - \beta_2] - [\beta_3 - \beta_1] \lesseqgtr 0$	[(6) - (5)] - [(4) - (3)]	0.146 (0.664)

Notes: Graph is from a separate OLS regression of the outcome, which is the percentage point change in interest in joining SK since baseline, on the treatments and their interactions with an indicator for being above the median of the Aspiration index. Controls include village fixed effects and variables in panels A and B in Table 4.1. Huber-White robust standard errors in brackets in third column of table. Prob > F based on Wald tests in parentheses in third column of table. Vertical lines in the graph represent 90% confidence intervals based on two-sided hypothesis tests. ***$p < 0.01$,** $p < 0.05$,* $p < 0.1$ in the table are from two-sided hypothesis tests when hypothesis has ambiguous sign and from one-sided hypothesis tests when hypothesis has an unambiguous sign.
[1]Test not indicated in pre-analysis plan.

Figure 4.18: Heterogeneous effects of workshop treatments on engagement in village youth programs, among individuals with low and high aspiration.
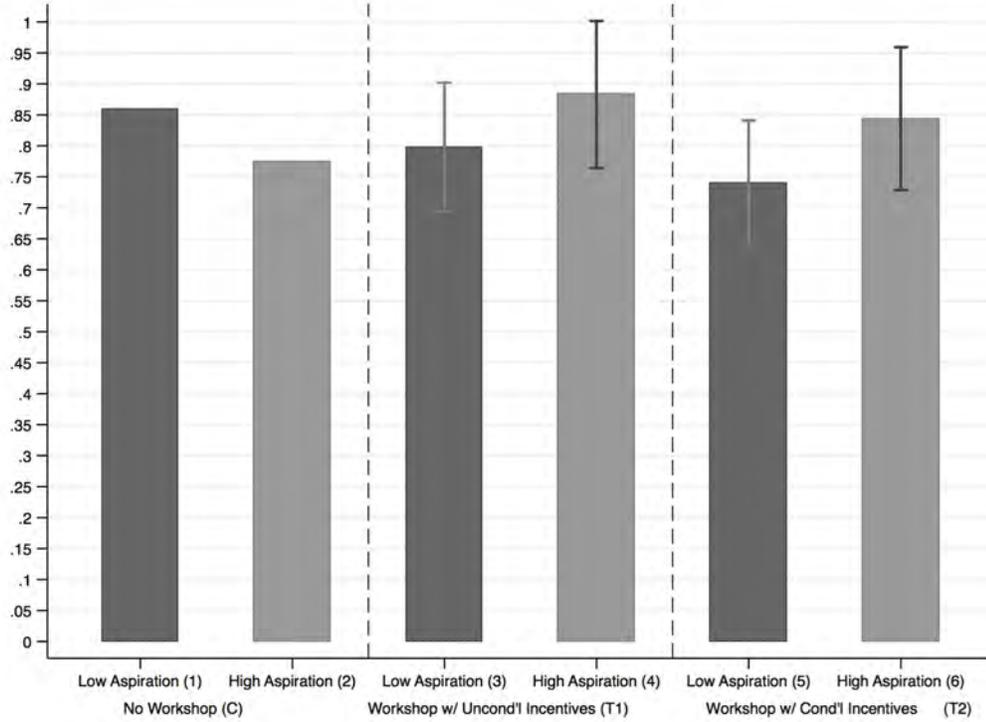

$$youth\_engage_i = \beta_0 + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T1_i^* HighAspiration_i +$$
$$\beta_4 T2_i^* HighAspiration_i + \beta_5 HighAspiration_i + X_i'\Gamma + \epsilon_i$$

Hypothesis Test	Graphical Test	Result
$\beta_1 \lesseqgtr 0$	[(3) - (1)]	-0.061 [0.063]
$\beta_2 < 0$	[(5) - (1)]	-0.119 [0.062]**
$\beta_3 > 0$	[(4) - (2)]	0.109 [0.095]
$\beta_4 > 0$	[(6) - (2)]	0.070 [0.096]
$\beta_5 \leq 0$	[(2) - (1)]	-0.084 [0.071]
$\beta_3 - \beta_1 \lesseqgtr 0$ [1]	[(4) - (2)] - [(3) - (1)]	0.169 (0.243)
$\beta_4 - \beta_2 > 0$	[(6) - (2)] - [(5) - (1)]	0.188 (0.094)*
$[\beta_4 - \beta_2] - [\beta_3 - \beta_1] \lesseqgtr 0$	[(6) - (5)] - [(4) - (3)]	0.019 (0.905)

Notes: Graph is from a separate OLS regression of the outcome, which is an indicator for engagement in village youth programs on the treatments and their interactions with an indicator for being above the median of the Aspiration index. Controls include village fixed effects and variables in panels A and B in Table 4.1. Huber-White robust standard errors in brackets in third column of table. Prob > F based on Wald tests in parentheses in third column of table. Vertical lines in the graph represent 90% confidence intervals based on two-sided hypothesis tests. ***$p < 0.01$,** $p < 0.05$,* $p < 0.1$ in the table are from two-sided hypothesis tests when hypothesis has ambiguous sign and from one-sided hypothesis tests when hypothesis has an unambiguous sign. [1]Test not indicated in pre-analysis plan.

Figure 4.19: Heterogeneous effects of workshop treatments on nomination as a youth leader, among individuals with low and high integrity.
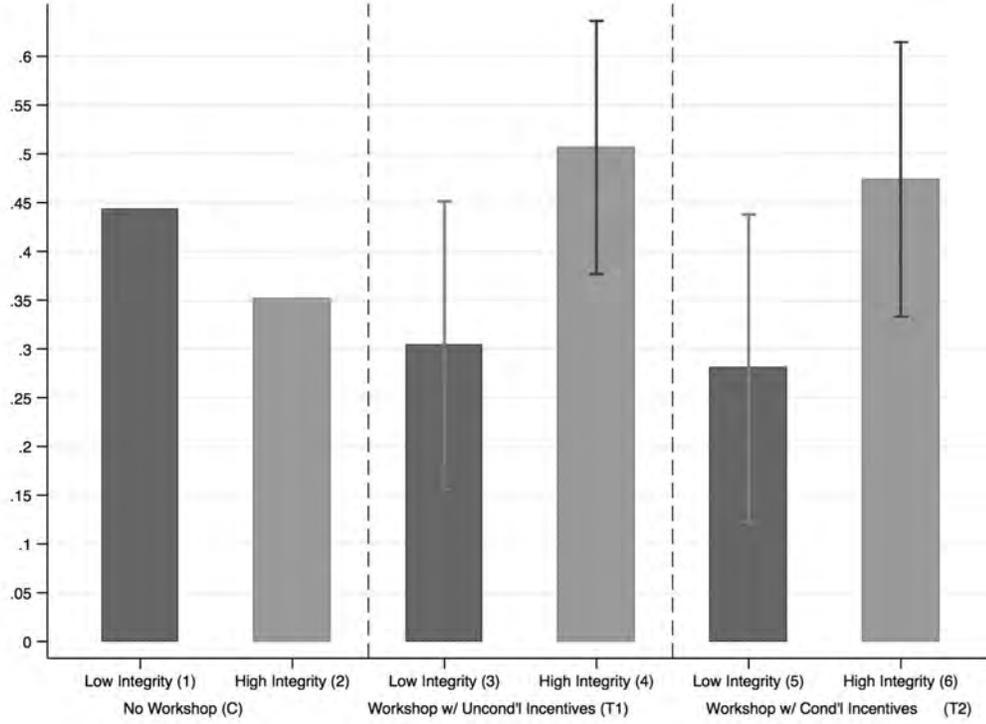
$$nominated_i = \beta_0 + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T1_i^* HighIntegrity_i +$$
$$\beta_4 T2_i^* HighIntegrity_i + \beta_5 HighIntegrity_i + X_i'\Gamma + \epsilon_i$$

Hypothesis Test	Graphical Test	Result
$\beta_1 \lessgtr 0$	[(3) - (1)]	-0.139 [0.090]
$\beta_2 < 0$	[(5) - (1)]	-0.163 [0.096]**
$\beta_3 > 0$	[(4) - (2)]	0.156 [0.113]*
$\beta_4 > 0$	[(6) - (2)]	0.123 [0.118]
$\beta_5 \leq 0$	[(2) - (1)]	-0.092 [0.081]
$\beta_3 - \beta_1 \lessgtr 0$ [1]	[(4) - (2)] - [(3) - (1)]	0.295 (0.123)
$\beta_4 - \beta_2 > 0$	[(6) - (2)] - [(5) - (1)]	0.286 (0.079)*
$[\beta_4 - \beta_2] - [\beta_3 - \beta_1] \lessgtr 0$	[(6) - (5)] - [(4) - (3)]	-.010 (0.960)

Notes: Graph is from a separate OLS regression of the outcome, which is an indicator for nomination as youth leader on the treatments and their interactions with an indicator for being above the median of the Integrity index. Controls include village fixed effects and variables in panels A and B in Table 4.1. Huber-White robust standard errors in brackets in third column of table. Prob > F based on Wald tests in parentheses in third column of table. Vertical lines in the graph represent 90% confidence intervals based on two-sided hypothesis tests. ***$p < 0.01$,** $p < 0.05$,* $p < 0.1$ in the table are from two-sided hypothesis tests when hypothesis has ambiguous sign and from one-sided hypothesis tests when hypothesis has an unambiguous sign. [1]Test not indicated in pre-analysis plan.

Figure 4.20: Heterogeneous effects of workshop treatments on designation as a youth leader, among individuals with low and high integrity.
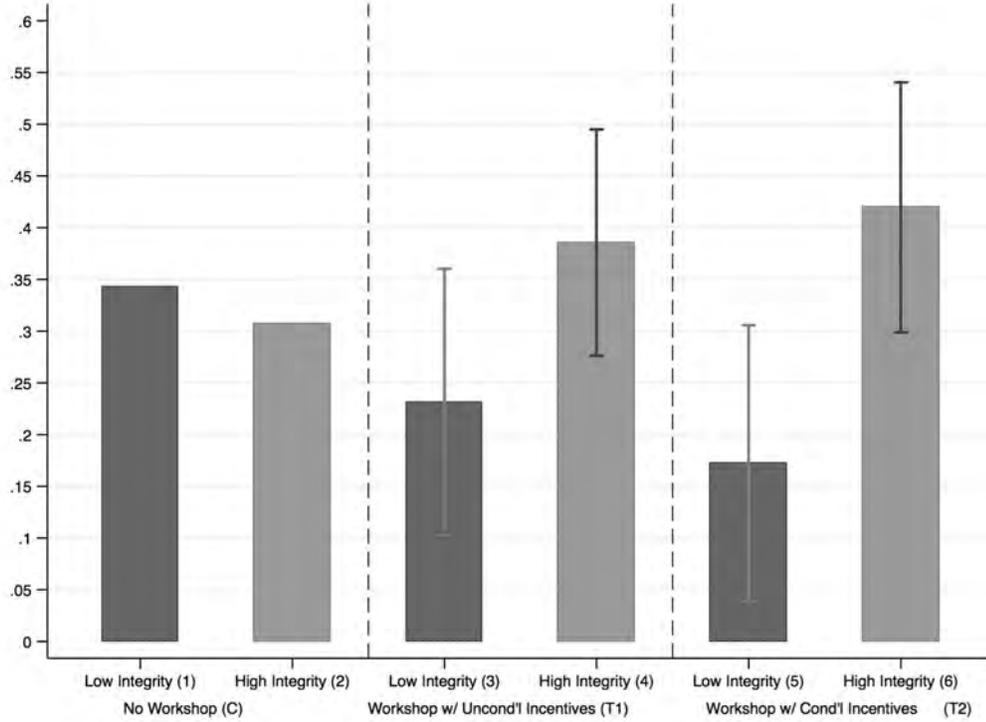

$$designated_i = \beta_0 + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T1_i^* HighIntegrity_i +$$
$$\beta_4 T2_i^* HighIntegrity_i + \beta_5 HighIntegrity_i + X_i'\Gamma + \epsilon_i$$

Hypothesis Test	Graphical Test	Result
$\beta_1 \lessgtr 0$	$[(3) - (1)]$	-0.111 [0.078]
$\beta_2 < 0$	$[(5) - (1)]$	-0.170 [0.081]**
$\beta_3 > 0$	$[(4) - (2)]$	0.079 [0.100]
$\beta_4 > 0$	$[(6) - (2)]$	0.113 [0.103]
$\beta_5 \leq 0$	$[(2) - (1)]$	-0.036 [0.074]
$\beta_3 - \beta_1 \lessgtr 0$ [1]	$[(4) - (2)] - [(3) - (1)]$	0.190 (0.256)
$\beta_4 - \beta_2 > 0$	$[(6) - (2)] - [(5) - (1)]$	0.283 (0.051)*
$[\beta_4 - \beta_2] - [\beta_3 - \beta_1] \lessgtr 0$	$[(6) - (5)] - [(4) - (3)]$	0.093 (0.577)

Notes: Graph is from a separate OLS regression of the outcome, which is an indicator for designation as youth leader on the treatments and their interactions with an indicator for being above the median of the Integrity index. Controls include village fixed effects and variables in panels A and B in Table 4.1. Huber-White robust standard errors in brackets in third column of table. Prob > F based on Wald tests in parentheses in third column of table. Vertical lines in the graph represent 90% confidence intervals based on two-sided hypothesis tests. $^{***}p < 0.01,^{**} p < 0.05,^* p < 0.1$ in the table are from two-sided hypothesis tests when hypothesis has ambiguous sign and from one-sided hypothesis tests when hypothesis has an unambiguous sign. [1]Test not indicated in pre-analysis plan.

Figure 4.21: Heterogeneous effects of workshop treatments on change in interest in joining SK since baseline, among individuals with low and high integrity.
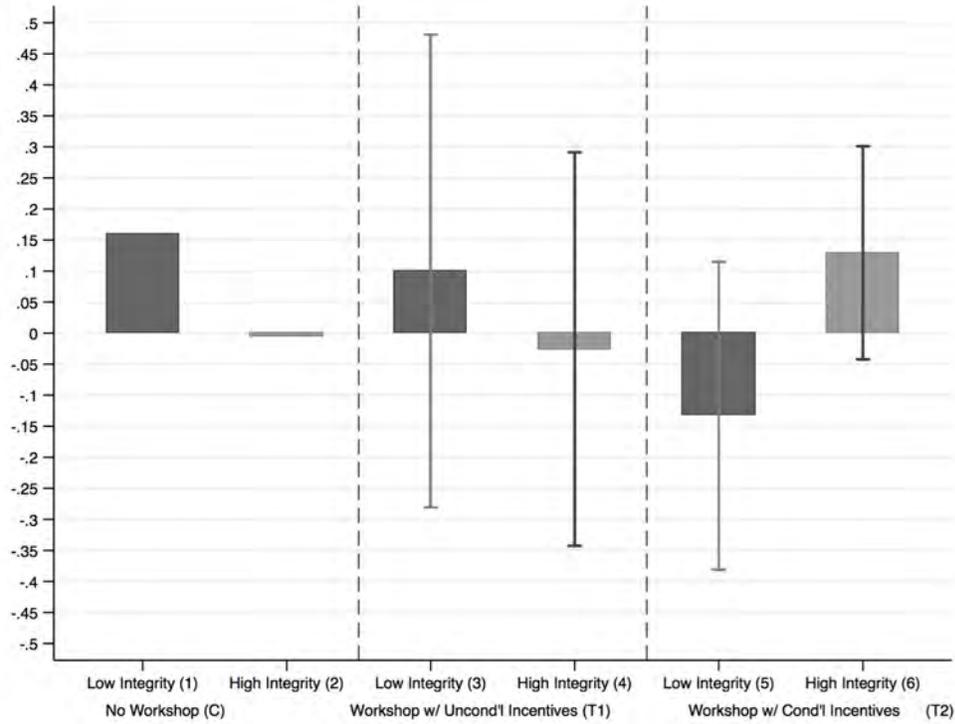
$$interest\_change_i = \beta_0 + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T1_i^* HighIntegrity_i +$$
$$\beta_4 T2_i^* HighIntegrity_i + \beta_5 HighIntegrity_i + X_i'\Gamma + \epsilon_i$$

Hypothesis Test	Graphical Test	Result
$\beta_1 \lessgtr 0$	[(3) - (1)]	-0.059 [0.232]
$\beta_2 < 0$	[(5) - (1)]	-0.292 [0.151]**
$\beta_3 > 0$	[(4) - (2)]	-0.020 [0.269]
$\beta_4 > 0$	[(6) - (2)]	0.135 [0.160]
$\beta_5 \leq 0$	[(2) - (1)]	-0.164 [0.166]
$\beta_3 - \beta_1 \lessgtr 0$ [1]	[(4) - (2)] - [(3) - (1)]	0.038 (0.938)
$\beta_4 - \beta_2 > 0$	[(6) - (2)] - [(5) - (1)]	0.427 (0.079)*
$[\beta_4 - \beta_2] - [\beta_3 - \beta_1] \lessgtr 0$	[(6) - (5)] - [(4) - (3)]	0.388 (0.328)

Notes: Graph is from a separate OLS regression of the outcome, which is the percentage point change in interest in joining SK since baseline, on the treatments and their interactions with an indicator for being above the median of the Integrity index. Controls include village fixed effects and variables in panels A and B in Table 4.1. Huber-White robust standard errors in brackets in third column of table. Prob > F based on Wald tests in parentheses in third column of table. Vertical lines in the graph represent 90% confidence intervals based on two-sided hypothesis tests. ***$p < 0.01$,** $p < 0.05$,* $p < 0.1$ in the table are from two-sided hypothesis tests when hypothesis has ambiguous sign and from one-sided hypothesis tests when hypothesis has an unambiguous sign. [1]Test not indicated in pre-analysis plan.

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Figure 4.22: Heterogeneous effects of workshop treatments on engagement in village youth programs, among individuals with low and high integrity.
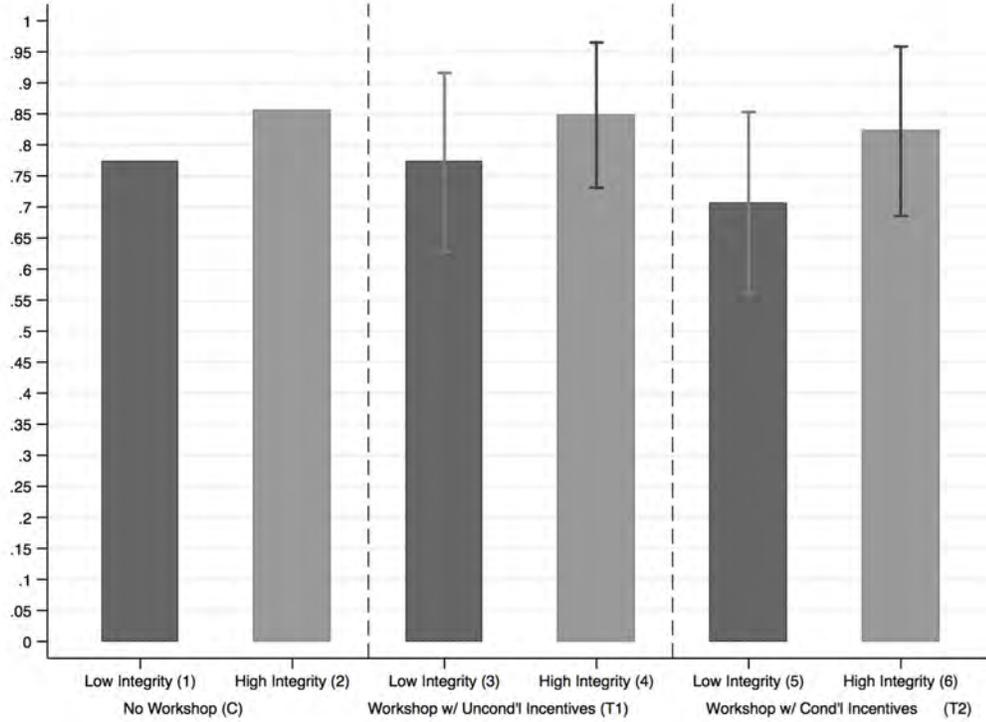

$$youth\_engage_i = \beta_0 + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T1_i^* HighIntegrity_i +$$
$$\beta_4 T2_i^* HighIntegrity_i + \beta_5 HighIntegrity_i + X_i'\Gamma + \epsilon_i$$

Hypothesis Test	Graphical Test	Result
$\beta_1 \lessgtr 0$	[(3) - (1)]	-0.000 [0.087]
$\beta_2 < 0$	[(5) - (1)]	-0.066 [0.089]
$\beta_3 > 0$	[(4) - (2)]	-0.007 [0.102]
$\beta_4 > 0$	[(6) - (2)]	-0.033 [0.108]
$\beta_5 \leq 0$	[(2) - (1)]	0.083 [0.072]
$\beta_3 - \beta_1 \lessgtr 0$ [1]	[(4) - (2)] - [(3) - (1)]	-0.007 (0.970)
$\beta_4 - \beta_2 > 0$	[(6) - (2)] - [(5) - (1)]	0.033 (0.431)
$[\beta_4 - \beta_2] - [\beta_3 - \beta_1] \lessgtr 0$	[(6) - (5)] - [(4) - (3)]	0.040 (0.831)

Notes: Graph is from a separate OLS regression of the outcome, which is an indicator for engagement in village youth programs on the treatments and their interactions with an indicator for being above the median of the Integrity index. Controls include village fixed effects and variables in panels A and B in Table 4.1. Huber-White robust standard errors in brackets in third column of table. Prob > F based on Wald tests in parentheses in third column of table. Vertical lines in the graph represent 90% confidence intervals based on two-sided hypothesis tests. ***$p < 0.01$,** $p < 0.05$,* $p < 0.1$ in the table are from two-sided hypothesis tests when hypothesis has ambiguous sign and from one-sided hypothesis tests when hypothesis has an unambiguous sign. [1]Test not indicated in pre-analysis plan.

APPENDICES

APPENDIX A

Partisan Motives in Pork Distribution

A1 Theoretical Derivations

Proof of Proposition 1a

Define $\underline{\tau} = E[\tau|P_i = 0, MMV_i^0 = 0] = lim_{MMV_i^0 \uparrow 0\%}\tau_i$ as the optimal pork allocations to unaligned municipalities in close mayoral races, and $\bar{\tau} = E[\tau|P_i = 1, MMV_i^0 = 0] = lim_{MMV_i^0 \downarrow 0\%}\tau_i$ as the optimal pork allocations to aligned municipalities in close mayoral races. These quantities are derived from the following first-order conditions, respectively: $(1 - \theta)U'(\underline{\tau}) - \phi[\bullet]\left[\frac{\theta R f'(\underline{\tau})}{\sigma_{MMV}}\right]\Pr[CMV_d > 0] + \phi[\bullet]\left[\sum_i R * \Pr[MMV_i > 0] + W\right]\left[\frac{(1-\theta)h'(\underline{\tau})}{\sigma_{CMV}}\right] - C'(\underline{\tau}) = 0; (1-\theta)U'(\bar{\tau}) + \phi[\bullet]\left[\frac{\theta R f'(\bar{\tau})}{\sigma_{MMV}}\right]\Pr[CMV_d > 0] + \phi[\bullet]\left[\sum_i R * \Pr[MMV_i > 0] + W\right]\left[\frac{(1-\theta)h'(\bar{\tau})}{\sigma_{CMV}}\right] - C'(\bar{\tau}) = 0$. It follows that: $\underline{\tau} = \tau' - z < \tau' < \bar{\tau} = \tau' + k$, with $z, k > 0$, which proves Proposition 1a.

Proof of Proposition 1b

Define $\underline{\tau} = E[\tau|P_i = 0, CMV_i^0 = 0] = lim_{CMV_i^0 \uparrow 0\%}\tau_i$ as the optimal pork allocations to unaligned municipalities in close House races, and $\bar{\tau} = E[\tau|P_i =$

$1, CMV_i^0 = 0] = lim_{CMV_i^0 \downarrow 0\%} \tau_i$ as the optimal pork allocations to aligned municipalities in close House races. These quantities are derived from the following first-order conditions, respectively: $(1 - \theta)U'(\underline{\tau}) - \phi[\bullet] \left[\frac{\theta R f'(\underline{\tau})}{\sigma_{MMV}} \right] \Pr[CMV_d > 0] - \phi[\bullet] \left[\sum_i R * \Pr[MMV_i > 0] + W \right] \left[\frac{\theta MMV_i^0 g'(\underline{\tau}) + (1-\theta)h'(\underline{\tau})}{\sigma_{CMV}} \right] - C'(\underline{\tau}) = 0$; $(1 - \theta)U'(\bar{\tau}) + \phi[\bullet] \left[\frac{\theta R f'(\bar{\tau})}{\sigma_{MMV}} \right] \Pr[CMV_d > 0] + \phi[\bullet] \left[\sum_i R * \Pr[MMV_i > 0] + W \right] \left[\frac{\theta MMV_i^0 g'(\bar{\tau}) + (1-\theta)h'(\bar{\tau})}{\sigma_{CMV}} \right] - C'(\bar{\tau}) = 0$. It follows that: $\underline{\tau} = \tau' - z < \tau' < \bar{\tau} = \tau' + k$, with $z, k > 0$, which proves Proposition 1b.

Proof of Proposition 2

Define the first-oder condition in equation (6) as $g(\tau_i, MMV_i^0) = 0$. Therefore, at $P = 1$: $(\partial \tau_i / \partial MMV_i^0) = -(\partial g / \partial MMV_i^0)/(\partial g / \partial \tau_i) \lesseqgtr 0$. In fact: $(\partial g / \partial \tau_i) < 0$ because of the second-order condition; and $(\partial g / \partial MMV_i^0)$ is as follows:

$$\left(\frac{\partial g}{\partial MMV_i^0}\right) = -\left(\frac{R\theta f'(\tau_i)}{\sigma_{MMV}}\right)\left(\frac{\rho MMV_i^0 + \theta f(\tau_i)}{\sigma_{MMV}}\right)\phi(\bullet)\Pr[CMV_d > 0]+$$

$$\phi(\bullet)\left(\sum_i R\Pr[MMV_i > 0] + W\right)*$$

$$\left[\left(\frac{\theta g(\tau_i)}{\sigma_{CMV}}\right) - \left(\frac{\psi CMV_d^0 + \sum_i \theta MMV_i^0 g(\tau_i) + \sum_i (1-\theta)h'(\tau_i)}{\sigma_{CMV}}\right)\left(\frac{\theta MMV_i^0 g(\tau_i) + (1-\theta)h'(\tau_i)}{\sigma_{CMV}}\right)\right] \lesseqgtr 0$$

Similarly, at $P = 0$: $(\partial \tau_i / \partial MMV_i^0) = -(\partial g / \partial MMV_i^0)/(\partial g / \partial \tau_i) \lesseqgtr 0$, as because in this case:

$$\left(\frac{\partial g}{\partial MMV_i^0}\right) = \left(\frac{R\theta f'(\tau_i)}{\sigma_{MMV}}\right)\left(\frac{\rho MMV_i^0 - \theta f(\tau_i)}{\sigma_{MMV}}\right)\phi(\bullet)\Pr[CMV_d > 0]+$$

$$\phi(\bullet)\left(\sum_i R\Pr[MMV_i > 0] + W\right)*$$

$$\left[\left(\frac{\theta g(\tau_i)}{\sigma_{CMV}}\right) - \left(\frac{\psi CMV_d^0 + \sum_i \theta MMV_i^0 g(\tau_i) + \sum_i (1-\theta)}{\sigma_{CMV}}\right)\left(\frac{\theta MMV_i^0 g(\tau_i) + (1-\theta)h'(\tau_i)}{\sigma_{CMV}}\right)\right] \lesseqgtr 0$$

A2 Robustness Checks

Table A1: Congressional pork releases by implementing agency, 2001-2010 (in millions of Philippine pesos).

Implementing Agency	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010
Priority Development Assistance Funds (PDAF)										
Special Financial Assistance to LGUs										
Province	*284*	.	381	294	357	121	*143*	*107*	1,032	1,051
Municipalities & Cities	1,274	.	1,842	1,318	1,495	409	593	445	2,729	1,253
Barangays (Villages)	*44*	.	95	27	48	13	11	23	342	225
National Government Agencies										
Agriculture	*79*	.	358	511	534	67	*144*	*127*	165	106
Education	*103*	.	305	458	319	98	*76*	*15*	92	114
Finance	.	.	37	83	179	64	.	.	453	1,160
Health	*162*	.	111	186	85	31	*30*	*25*	199	265
Interior & Local Government	.	.	302	138	0	0	.	.	18	1
Labor	*17*	.	22	38	71	10	*19*	*27*	131	138
Other Executive Offices	.	.	21	50	30	15	.	.	205	152
Philippine General Hospital	.	.	27	40	31	7	.	.	0	9
Public Works & Highways	*640*	.	183	396	447	123	*128*	*71*	546	621
Social Welfare & Development	*100*	.	270	356	447	123	*128*	*71*	546	621
State Universities & Colleges	*79*	.	67	90	95	26	*41*	*38*	201	240
Trade & Industry	.	.	0	26	8	0	.	.	5	5
Transportation & Communication	.	.	86	85	0	0	.	.	0	0
Dept. of Public Works & Highways Congress Allocations (DPWH-CA)	3,374	.	9,956	10,014	3,513	.	1,229	1,429	1,816	1,421

Notes: Numbers in italics are imputed values using percentages reported in the Department of Budget and Management's National Expenditure Program (NEP) publication for the relevant years. A cell value of "." means no data available.

Table A2: Effect of Legislator-Mayor Alignment on DPWH-CA, OLS, DD, and RD Estimates.

Dependent Variable: Per Capita DPWHCA (PHP)	OLS (1)	DD (2)	RD in Mayor's Vote-Share Margin of Victroy				RD in Legislator's Vote-Share Margin of Victroy			
			Spline Polynomial (3)	Local Linear Regression (4)	OLS in an interval [-5;+5] (5)	[-2.5;+2.5] (6)	Spline Polynomial (7)	Local Linear Regression (8)	OLS in an interval [-5;+5] (9)	[-2.5;+2.5] (10)
Aligned = 1	15.46***	11.59**	-4.16	-7.15	-8.14	-22.43	-5.60	2.67	8.63	20.72
	(4.09)	(4.65)	(9.27)	(10.36)	(8.96)	(18.49)	(12.94)	(11.87)	(21.39)	(28.41)
Constant	117.80***	63.18***	127.90***	133.30***	62.13***	60.12***	132.70***	69.29***	58.22*	128.2***
	(11.49)	(6.45)	(12.85)	(15.74)	(15.06)	(22.92)	(12.77)	(11.01)	(33.24)	(41.08)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bandwidth h	.	.	.	14	.	.	.	30	.	.
R^2	0.44	0.59	0.44	0.39	0.46	0.43	0.44	0.51	0.21	0.33
Clusters	1,578	1,578	1,578	1,089	562	290	175	154	60	38
Observations	6,671	6,671	6,671	2,858	1,104	508	6,058	3,549	615	333

Notes: Dependent variable: Per Capita Department of Public Works and Highways - Congressional Allocations distributed by a legislator to a municipality within his district. Independent variable: 'Aligned', which is a binary variable that takes on a value of 1 if the mayor is a partisan ally of the legislator, and 0 otherwise. Estimation methods: (1) OLS; (2) Difference-in-differences; (3) RD specification with 3rd-order spline polynomial approximation as in equation 7; (4) local linear regression with optimal bandwidth as in equation 8; (5)-(6) OLS in restricted intervals around the threshold of zero vote-share margin of victory, i.e., [-5;+5] and [-2.5;+2.5]; (7) RD specification with 3rd-order spline polynomial approximation as in equation 9a; (8) local linear regression with optimal bandwidth as in equation 9b; and (9)-(10) OLS in restricted intervals around the threshold of zero vote-share margin of victory, i.e., [-5;+5] and [-2.5;+2.5]. Robust standard errors are clustered at the municipality level for columns (1) - (6) and clustered at the district level for columns (7) - (10), and are in parentheses below the coefficient estimates. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table A3: Party/coalitions of House and Mayoral Candidates for Election Years 2001, 2004, 2007, 2010.

Party/Coalition	2001		2004		2007		2010	
	Legislators	Mayors	Legislators	Mayors	Legislators	Mayors	Legislators	Mayors
no data	11	45	.	49	11	55	.	132
ABAG/PROMDI	.	.	.	1	.	.	.	.
AIM	.	.	.	.	.	1	.	2
AIM/NPC	.	.	4	3	.	1	.	.
AKBAYAN	.	.	.	.	.	.	.	1
AKSYON	17	12	16	12	.	1	.	.
AKSYON DEMOKRATIKO	.	.	.	.	.	.	.	7
ALAYON	32	18	.	5	.	1	.	.
ALAYON/KAMPI	.	.	.	.	.	2	.	.
ALAYON/NPC	.	.	.	4	.	.	.	.
ALLIANCE FOR BARANGAY	.	.	.	.	.	1	.	.
ALYANSA	.	.	.	1	.	.	.	.
ALYANSA/LM/REPORMA	.	.	.	1	.	.	.	.
ATUN	.	1	.	.	.	.	.	.
BAGUMBAYAN	.	.	.	.	.	.	.	1
BAGUMBAYAN/VNP	.	.	.	.	.	.	.	5
BAKUD	.	.	.	.	.	5	.	.
BAKUD-ALAYON	11	9	.	.	.	.	.	.
BALANE	.	15	9	23	9	5	.	.
BALANE/KAMPI	.	.	.	.	8	13	.	.
BANGON POLIPINAS	.	.	.	.	.	.	.	1
BAK	.	.	.	.	.	.	.	10
BIGKIS PINOY MOVEMENT	.	.	.	.	.	.	.	11
BILEG TI ILOCANO	.	1	.	.	.	.	.	.
BILEG TI LA UNION	.	.	.	.	.	.	.	1
BISKEG/CMD/LAKAS	.	.	.	.	.	1	.	.
BUKLOD	.	.	.	1	.	.	.	.
BUKLOD CAPAMPANGAN	.	2	.	.	.	.	.	.
BUKLOD/CAPAMPANGAN	.	.	.	2	.	.	.	.
CITIZENS ACTION PARTY	.	.	.	.	.	1	.	.
CMD/KAMPI/LAKAS	.	.	.	2	.	9	.	.
CMD/KMP/LAKAS	.	.	.	.	.	.	900	785
CMD/KMP/LAKAS/UNA	.	.	.	.	.	.	.	4
CMD/LAKAS	.	.	723	768	677	689	.	.
CMD/LAKAS/LDP	.	.	.	.	.	2	.	.
CMD/LAKAS/LIHOK	.	.	.	6	.	.	.	.
CMD/LAKAS/LP	.	.	8	6	10	5	.	.
CMD/LAKAS/NP	.	.	.	1	.	.	.	.
CMD/LAKAS/NPC	.	.	11	18	.	2	.	.
CMD/LAKAS/NPC/UNA	.	.	.	1	.	.	.	.
CMD/LAKAS/OMPIA	.	.	.	1	.	.	.	.
CMD/LAKAS/PDP	.	.	.	.	.	1	.	.
CMD/LAKAS/PDSP	.	.	.	2	.	1	.	.
CMD/LAKAS/SARRO	.	.	.	6	7	.	.	.
CMD/LAKAS/UNA	.	.	5	2	5	.	.	.
DIL	.	.	.	.	.	.	.	1
IBID	.	2	.	.	.	.	.	.
INA/NPC	.	.	7	7	.	.	.	.
INDEPENDENT	56	60	44	64	30	39	46	95
KABAYANI	2	2	.	.	.	.	.	.
KAMPI	4	27	15	16	408	432	.	.
KAMPI/LDP	.	.	.	.	7	3	.	.
KAMPI/LDP/PMP	.	.	.	1	.	.	.	.
KAMPI/LP	.	.	.	.	.	3	.	.
KAMPI/PDSP	.	.	.	.	.	1	.	.
KAMPI/PPS	.	.	.	.	.	1	.	.
KAMPI/SARRO	.	.	.	.	.	6	.	.
KAMPI/UNA	.	.	.	.	6	1	.	.
KASALIGAN	.	.	.	1	.	.	.	.
KBL	11	5	11	6	11	5	11	.
KDT	.	.	.	2	2	2	.	.

Table A3, cont'd: Party/coalitions of House and Mayoral Candidates for Election Years 2001, 2004, 2007, 2010.

Party/Coalition	2001		2004		2007		2010	
	Legislators	Mayors	Legislators	Mayors	Legislators	Mayors	Legislators	Mayors
KNP/LABAN/LDP	.	.	.	1	.	.	.	.
KNP/LDP	.	.	11	8	.	.	.	.
KNP/PMP	.	.	.	4	.	.	.	.
LABAN	.	.	.	1	.	.	.	.
LABAN/PDP	.	.	9	11	3	14	.	.
LABAN/PDP/UNO	.	.	.	.	1	3	.	.
LAKAS NUCD-UMDP	616	605	.	.	.	.	.	.
LAKAS/PDP	.	.	.	.	.	.	2	8
LAMMP	.	7	.	.	.	.	.	.
LAPIANG BAGONG LAKAS	.	.	.	.	.	.	.	10
LM	.	.	.	.	.	.	10	1
LDP	175	164	110	84	10	20	23	15
LDP/MAGDALO	.	.	11	11	.	.	.	.
LDP/PMP	.	.	.	1	.	.	.	.
LDP/SARRO	.	.	.	1	.	.	.	.
LDP/UNO	.	.	.	.	1	1	.	.
LM/REPORMA	.	.	10	11	.	.	.	.
LP	180	92	209	142	133	100	202	201
LP/LPKKK	.	.	.	.	.	.	5	1
LP/UNO	.	.	.	.	12	2	.	.
LP/USWAG	.	.	.	1	.	.	.	.
MAGDALO	.	.	.	1	.	.	.	.
NP	.	.	7	1	35	13	159	184
NP/UNO	.	.	.	.	7	2	.	.
NPC	378	294	364	251	191	156	240	126
NPC/SST	.	.	.	3	.	.	.	.
NPC/UNA	.	.	14	4	6	5	.	1
NPC/UNO	.	.	.	.	.	2	.	.
OMPIA	.	5	.	.	.	1	.	.
OMPIA/PDSP	.	.	.	.	.	1	.	.
ONE CEBU	.	.	.	.	.	.	.	2
PADAYON PILIPINO	.	3	.	.	.	.	.	.
PARTIDO DEL PILAR	.	.	.	.	.	.	.	3
PDS	.	.	.	.	.	.	27	3
PARTIDO MAGDIWANG	.	.	.	.	.	.	.	1
PARTIDO NAVOTEO	.	.	.	.	.	1	1	1
PARTIDO PADAJON	.	.	.	.	.	8	.	.
PARTIDONG PAGBABAGO	.	.	.	.	.	.	.	13
PCM	.	.	.	.	.	.	7	5
PDP-LABAN	19	39	.	.	.	.	.	.
PDSP	27	25	8	32	5	12	.	.
PM	18	14	.	.	.	.	.	.
PMP	48	101	43	57	24	11	26	19
PMP/UNO	.	.	.	.	1	3	.	.
PPB	.	2	.	.	.	.	.	.
PPC	10	6	.	.	.	.	.	.
PROMDI	11	26	.	1	.	.	.	.
PRP	.	.	.	.	.	.	.	1
REPORMA	20	21	.	.	.	.	.	.
REPORMA-LM	13	37	.	.	.	.	.	.
SAMA-SAMA TARLAC	.	.	.	.	.	.	.	1
SST	.	10	.	.	.	.	.	.
UGYON	.	.	.	9	.	.	.	1
UMMAH	.	1	.	.	.	.	.	.
UNA	5	13	.	1	.	.	.	.
UNANG SIGAW	.	.	.	.	.	.	.	9
UNO	.	.	.	.	30	21	.	.
KKK-BAYAN NG HAGONOY	.	.	.	.	.	.	.	1
KMP	.	.	.	.	.	.	.	1
KNP	.	.	.	16	.	.	.	.

Table A4: Difference in income sources between Treated (Ally=1) and Control (Ally=0) in Close Races.

	Mayor's Vote-Share Margin of Victory		Legislator's Vote-Share Margin of Victory	
OLS Interval	[-5;+5]	[-2.5;+2.5]	[-5;+5]	[-2.5;+2.5]
Total Income	21.35	104.20	658.20	931.50
	(76.88)	(210.10)	(628.10)	(824.80)
Internal Revenue Allotment	5.17	114.80	480.50	800.70
	(65.87)	(197.60)	(417.40)	(539.70)
Real Property Tax	1.98	-5.86	125.30	136.70
	(9.48)	(4.90)	(139.20)	(184.80)
Business Tax	7.60	4.84	23.06	17.20
	(5.74)	(5.27)	(35.72)	(49.29)
Other Local Taxes	0.25	-0.45	7.78	10.18
	(0.46)	(0.58)	(8.27)	(11.23)
Regulatory Fees	1.98	1.26	22.89	27.16
	(1.90)	(1.76)	(21.10)	(27.81)
Service/User Charges	0.42	2.49	5.25	5.34
	(1.18)	(2.36)	(6.90)	(9.19)
Economic Enterprises	0.29	-1.13	30.15	37.23
	(5.19)	(11.75)	(31.68)	(44.27)
Other Local Non-tax	-0.10	-8.84	-1.97	-2.13
	(3.38)	(6.22)	(5.35)	(6.11)
Non-IRA National Revenue Shares	6.59	-39.51	0.59	-8.52
	(14.66)	(32.76)	(15.24)	(32.70)
Inter-Local Transfers	-1.25	-1.58	-2.14	-3.67
	(1.24)	(2.11)	(2.14)	(3.61)
Aid	1.67	-4.92	-0.94	-23.87
	(2.98)	(6.43)	(14.44)	(19.64)
Loans & Borrowings	-6.31	-33.65	17.59	1.96
	(9.81)	(23.30)	(25.23)	(26.52)

Notes: Estimated discontinuities of municipality income sources by type at the threshold of zero vote-share margin of victory. OLS specifications shown are restricted to the close intervals [-5;+5] and [-2.5;+2.5]. Robust standard errors clustered at the municipality level for the first three columns, and at the district level for the last two columns. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Figure A1. Graphical Representation of the Regression Discontinuity Using Optimal
Bandwidths as in Imbens and Kalyanaraman (2012).
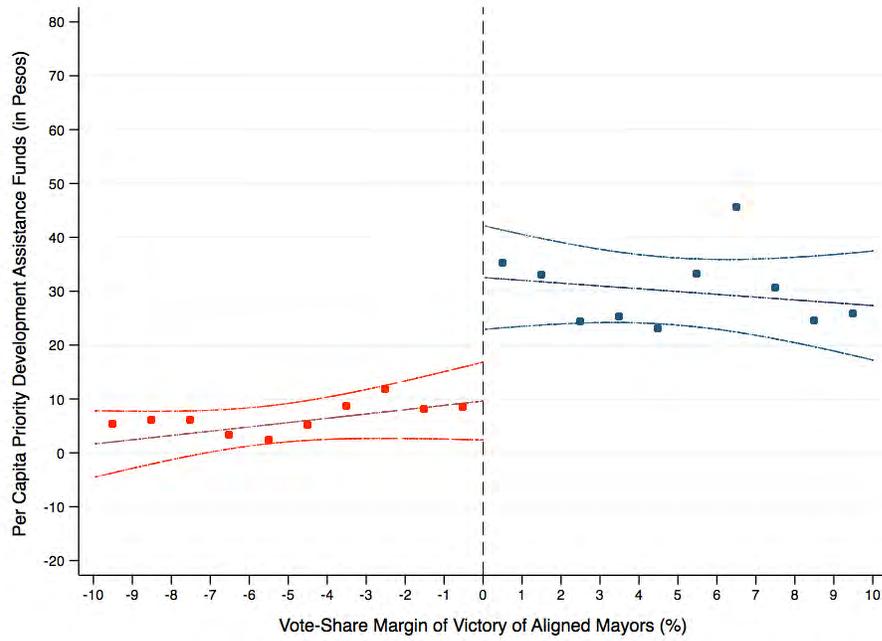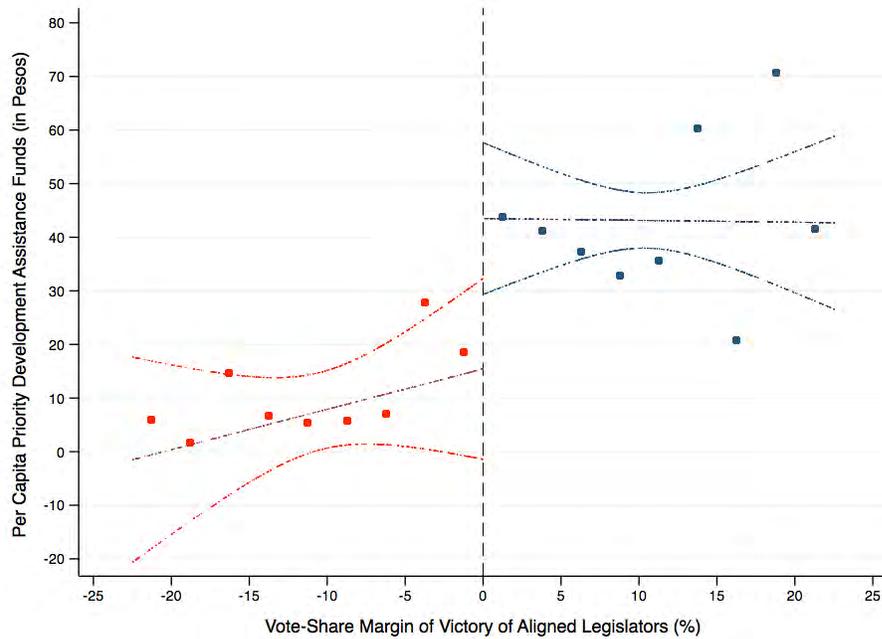
Figure A1.1

Figure A1.2

Notes: The central line is a local linear regression fit over the optimal bandwidth as in Imbens and
Kalyanaraman (2012). Figure A1.1 is in the vote-share margin of victory of aligned mayors, with
optimal bandwidth $h_{IK} = 10$. Figure A1.2 is in the vote-share margin of victory of aligned legislators,
with optimal bandwidth $h_{IK} = 26$. The lateral lines represent the 95% confidence interval. Scatter
points are averages over 0.5-unit intervals in Figure A1.1, and over 2.5-unit intervals in Figure A1.2.

Figure A2. How Competition Facing Mayors Shapes Partisan Pork Distribution
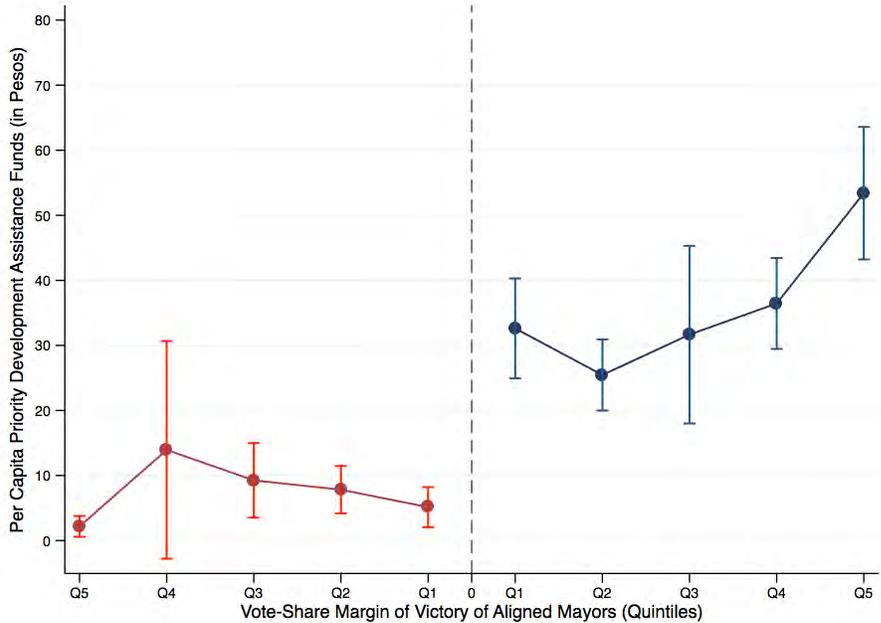
Figure A2.1
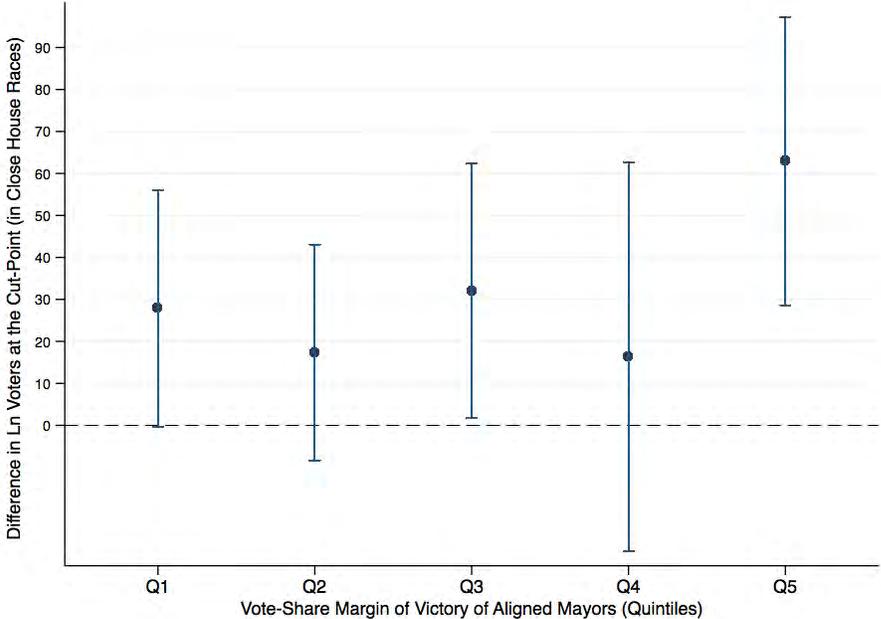
Figure A2.2

Notes: In Figure A2.1, points shown are the means of per capita PDAF, by partisan alignment and by quintiles of mayor's vote-share margin of victory. In Figure A2.2 point estimates shown are based on the regression results using the specification in Equation 10 with optimal bandwidths as in Imbens and Kalyanaraman (2012). Vertical lines represent 95% confidence intervals. Q1 represents vote-share margin in the 1st quintile, Q2 in the 2nd quintile, Q3 in the 3rd quintile, Q4 in the 4th quintile, and Q5 in the 5th quintile.

Figure A3. Histogram of Vote-Share Margin of Victory in Bins of Size 2.5%, 0.5%.
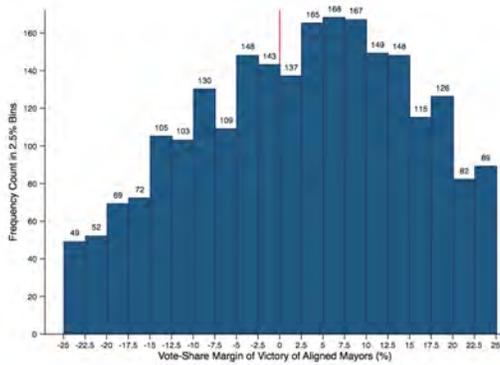
Figure A3.1
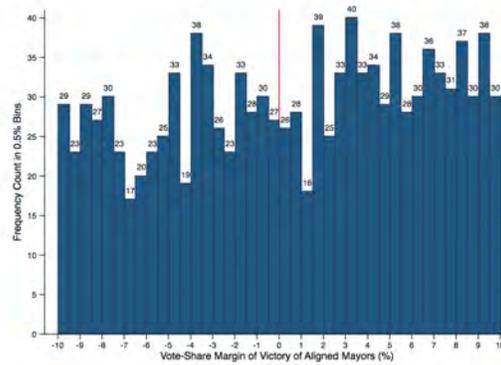
Figure A3.2
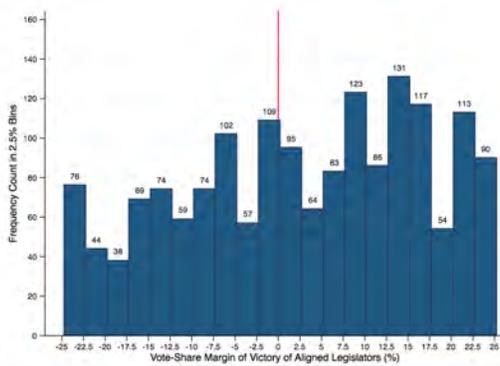
Figure A3.3
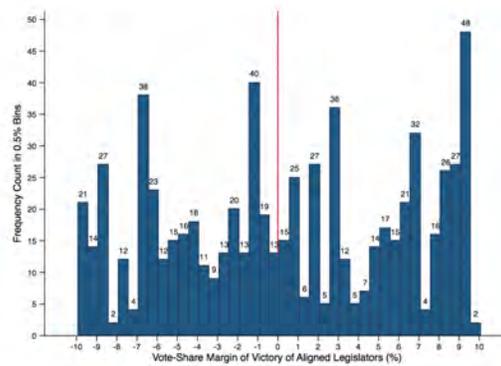
Figure A3.4

Notes: Figures A3.1 and A3.2 show the frequency of municipalities according to the vote share margin of victory of aligned mayors. Figures A3.3 and A3.4 show the frequency of municipalities according to the vote share margin of victory of aligned legislators.

Figure A4. Testing the Continuity of the Density in Close Races.
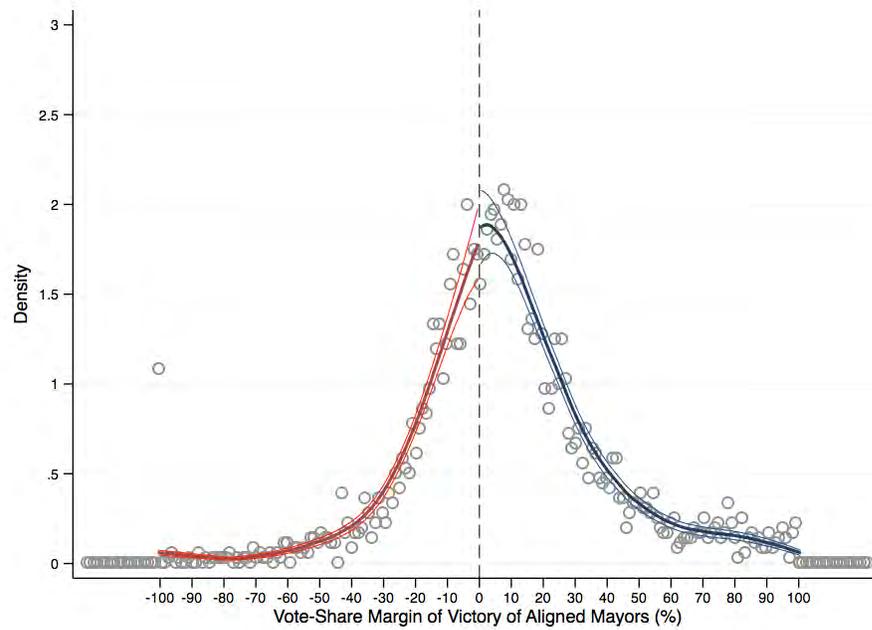
Figure A4.1
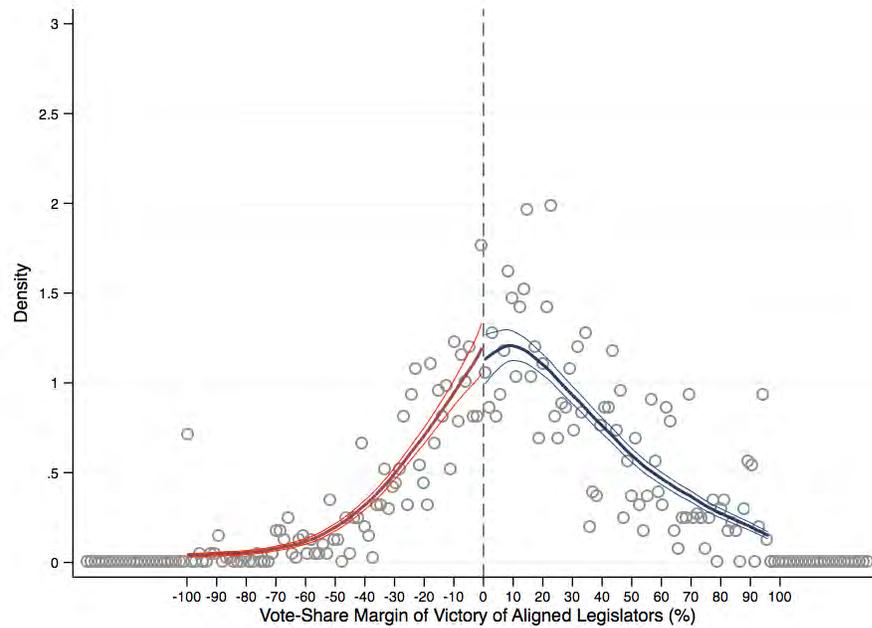
Figure A4.2

Notes: Weighted kernel estimation of the log density according to the vote share margin of victory of the aligned mayor (Figure A4.1) and of the aligned legislator (Figure A4.2), performed separately on either side of the zero threshold. Optimal bandwidth and bin size as in McCrary (2008).

Figure A5. Placebo Tests of the Effect of Legislator-Mayor Alignment on Pork Distribution.
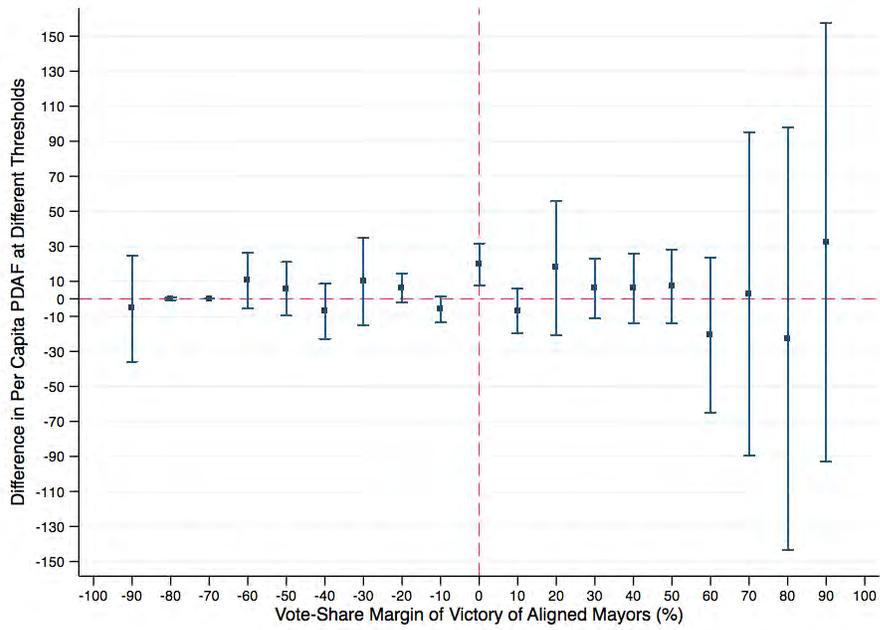

Figure A5.1
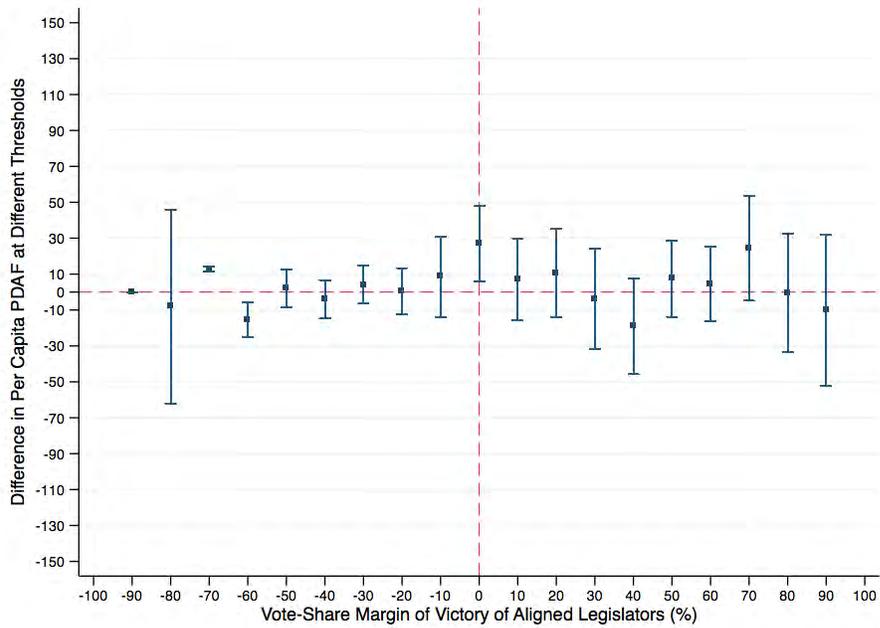
Figure A5.2

Notes: Graphs show placebo estimates using local linear regression with IK-optimal bandwidths at fake thresholds. Figure A5.1 is in the vote-share margin of victory of aligned mayors. Figure A5.2 is in the vote-share margin of victory of aligned legislators. Lateral lines represent the 95% confidence interval.

APPENDIX B

Temptation in Vote-Selling: Evidence from a Field Experiment in the Philippines

Appendix

Model Details

Partially Sophisticated Voters

<u>Promise 1</u>

Case 1: Relatively Naive Voter - (3) does not hold

At $t = 0$, if he makes and fulfills the promise the voter will get $U = v_1 + \gamma$. If he doesn't he expects to accept both gifts and vote for Candidate 1 ($U = v_1 + g_1 + g_2$). He will make the promise if $\gamma \geq g_1 + g_2$. This is then sufficient to turn down the gifts at $t = 1$.

Case 2: Relatively Sophisticated Voter - (3) holds and $g_2 > v_1 - v_2$

At $t = 1$, if the voter has made the promise he can accept neither gift ($U = v_1 + \gamma$), only the gift from Candidate 1 ($U = v_1 + g_1 - \gamma$), or both gifts ($U = v_2 + g_1 + g_2 - \gamma$). Accepting both gifts dominates accepting only one gift because $g_2 > v_1 - v_2$. Therefore, the voter will keep his promise not to accept money from candidates if $\gamma \geq 1/2[g_1 + g_2 - (v_1 - v_2)]$. A voter will never make a promise he expects to break, so assume the condition holds. At $t = 0$, the voter can promise ($U = v_1 + \gamma$), or not promise ($U = v_2 + g_1 + g_2$). He will promise if $\gamma \geq [g_1 + g_2 - (v_1 - v_2)]$.

<u>Promise 2</u>

Case 1: Relatively Naive Voter - (4) does not hold

Since the voter does not expect gifts to change his vote, he will accept any gift offered at $t = 1$ (with or without the promise). Since he expects to accept both gifts and keep his promise, at $t = 0$ he will make the promise. For the promise to reduce switching we need the promise to be effective if made, i.e. (2) does not hold, and for the voter to switch without the promise, i.e. (1) holds and either (3) doesn?t hold or $g_2 > v_1 - v_2$.

Case 2: Relatively Sophisticated Voter - (4) holds

At $t = 1$ he expects the gift from Candidate 2 to change his vote, so he can accept it and vote for 2 ($U = v_2 + g_1 + g_2 - \gamma$) or turn it down and vote for 1 ($U = v_1 + g_1 + \gamma$). For the promise to prevent vote switching we need $\gamma \geq 1/2[g_2 - (v_1 - v_2)]$. Then at $t = 0$, he can promise and get $U = v_1 + g_1 + \gamma$ or not promise and get $U = v_2 + g_1 + g_2$. He will promise if $\gamma \geq g_2 - (v_1 - v_2)$.

Reported Voting Model

For a given candidate and race, let S_0, S_1 and S_2 denote the number of voters switching in the Control group, the Promise 1 treatment and the Promise 2 treatment respectively. With truthful reporting this will coincide with the observed switching. For the Control group, S_0 will consist of two groups of voters : Group 1 voters who accept the gift from Candidate 2 expecting to switch (equations 1 and 3 hold and $g_2 > v_1 - v_2$) and Group 2 voters who accept the gift from Candidate 2 expecting not to switch (equation 1 holds and 3 does not hold). For the Promise 1 treatment, S_1 will consist of subsets Groups 1 and 2. Specifically, S_1 will consist of those in Group 1 who do not make the promise ($\gamma < g_1 + g_2 - (v_1 - v_2)$) and those Group 2 who do not make the promise ($\gamma < g_1 + g_2$). Therefore, we expect that $S_1 \leq S_0$, and that the difference is smaller for races with large monetary gifts. S_2 will also consist of subsets Groups 1 and 2. Specifically, there will be two separate subsets of Group 1: relatively sophisticated voters who do not want to make the promise (equation 4 holds but $\gamma < g_2 - (v_1 - v_2)$) and relatively naive voters who switch after making the promise (equation 4 does not hold but equation 2 does). Additionally, from Group 2 there will be those that switch after making the promise (equation 2 holds). Lastly, there will be additional voters not part of S_0 who now accept the gift from Candidate 2 expecting not to switch but who are wrong (equations 2 and 3 hold, equation 4 does not hold, and $g_2 < v_1 - v_2$). S_2 can therefore be either larger or smaller than S_0.

To model social desirability bias suppose that any voter who actually switched his vote will only report switching with probability $p < 1$. In this case the observed switching rates would be biased

towards zero: pS_0, pS_1 and pS_2. However, there will only be a difference in observed switching rates if there is a difference in true switching rates: $p(S_i - S_j) > 0$ only if $(S_i - S_j) > 0$. Therefore this form of bias cannot create the treatment differences our model predicts.

If social desirability bias only affects promise-breaking, rather than vote selling generally, then the observed switching rates will be S_0, S_1 and pS_2, since only voters in Promise 2 would both promise and switch. If instead the bias affects any voter asked to make a promise comparisons between races can demonstrate that the promise must be having an effect. If the bias affects both races equally, then any difference between races in a promise treatment can only be generated by differences in true switching rates: $p(S_i - S'_i) > 0$ only if $(S_i - S'_i) > 0$. If voters exhibit different biases depending on the race, we would generally expect voters to distort more in the more important races than in the less important races: $pM < pVM < pCC$. This would lead to a larger apparent treatment effect in the mayoral race than the city council race, and would never lead to an apparent increase in switching.

Vote buying in Sorsogon

While the experiment was in the field, two of the authors were on the ground in Sorsogon observing election campaigning, interviewing local candidates and campaign workers, and on occasion, accompanying candidates and campaign workers as they worked to buy votes. Almost all candidates and workers were very open about the fact that their campaigns engaged in vote buying.[1] Many expressed regret that they were forced to do so, but in the words of one candidate, "We have no choice. Voters expect it." While the particulars differ from candidate to candidate, the following is typical of what we heard from candidates and their campaign workers.

Pre-campaign preparation: patronage and organization building

The groundwork for an election campaign is laid long before formal campaigning begins. Those intending to run for office work assiduously to cultivate relationships with voters in the years and months prior to election day by providing services to constituents. These can include everything from providing free food (one candidate regularly provides a free breakfast for anyone who shows up at his door), help with medical bills or school fees, to "KBL"?attending and giving financial support at weddings (*kasal*), baptisms (*binyag*), and burials (*libing*). Obviously, the expectations of such goods and services are higher from a politician currently in office than from a challenger, but even

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[1]However, since such activities violate election law, we will not identify individuals by name.

challengers need to engage in these types of patronage efforts if they hope to be a viable candidate.

Candidates also begin very early building an effective campaign organization. The goal is to have campaign leaders who are loyal to the candidate in every *barangay* (roughly, a village; the smallest official administrative unit in the Philippines). Interestingly, even where there are party or clan ties among candidates for different offices, candidates tend not to share campaign organizations or workers. "You have to have your own leaders who work just for you" one village councilor candidate reported. Well before the campaign period begins these local canvassers develop lists of voters by household and categorize them as supporters of the candidate, supporters of an opponent, and "loose" or uncommitted voters.[2]

Early campaign period: rallies, visits and handouts

As the official campaign period begins, candidates spend time attending official campaign events, sometimes on their own, sometimes with candidates for other offices. Typical at these events are free food and entertainment for participants, as well as other (generally non-monetary) handouts, including t-shirts, foodstuffs (rice, noodles, coffee), etc. Candidates also spend time meeting with voters in less formal settings, including visiting local markets, and being present at events where large numbers of voters are likely to be gathered. Most candidates reported that they spent little time talking to voters about policy issues. Small sectors of the community might care about such issues, "but for a majority, all they care about is personal help." Accordingly candidates must typically also deal with a deluge of requests for assistance during the campaign period. While there is some variation, at this stage of the campaign candidates are not doing much to distinguish between likely supporters and non-supporters. All who come to the rallies receive handouts, and candidates respond positively to all those petitioners that they can. To do otherwise would risk the candidate?s reputation.

Late campaign period: Vote buying

Things change dramatically in the week before election day. It is during this period that vote buying occurs. Using the voter lists each campaign has developed, vote canvassers approach households directly, offering money or goods in exchange for the vote. This is a massive logistical enterprise, involving the movement of large amounts of cash and goods, and the coordination of a large network of vote canvassers. Two campaigns we visited had their vote canvassers visiting the campaign head-

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[2]We were able to view one such voter list from a candidate running for councilor.

quarters throughout the day, three days before the election, to pick up their allotment of money and pre-packaged noodles (Appendix Figure B.1). We accompanied another campaign during a midnight run to distribute money and noodles to canvassers a few days prior to the election.

Candidates report that vote buying is generally done systematically and strategically. The more experienced local leaders with established networks in their *barangay* or *purok* (roughly translated as district or neighborhood) typically ask household heads to come to a designated place (usually the local leader's house) at a designated time (typically at night) to pick up the packets to be given to the voting members of the household. "Inpatawag ka ni kap," ("Our barangay captain is asking for you,") is understood to mean the packets are ready to be claimed. This is most efficient, since money stays in one secure place, there are fewer intermediaries required to conduct the vote-buying, and household heads can be made to sign a document that they, in fact, received the money.

Not all households and voters are brazen enough to make the trip to a local leader?s camp to pick up money. To reach out to these voters, local canvassers make house-to-house visits, sometimes in broad daylight, to hand out packets to individual voters or to household heads. "Pwede tabi maghatag?" ("Can I give you something?") or "May iharatag tabi ako saindo." ("I have something to give you,") are some of the phrases vote canvassers use when handing out the packets. Voters may then accept or refuse these packets. These packets would typically contain the name of the voter and the voter's assigned precinct number, so the campaign can track who accepted and who did not (Appendix Figure B.2).

Campaigns do not attempt to buy the votes of all voters. The top priority is individuals identified as likely supporters, followed by those who are viewed as unaffiliated or uncommitted. Few vote buying resources are wasted on those already believed to be supporters of another candidate. While unaffiliated voters may receive offers of money from multiple rival candidates, most voters receive offers from only a single candidate. Candidates may also engage in a second round of vote buying if they learn that a challenger is offering more money than they are.

Campaigns are very careful to be sure voters clearly associate the money or gift with the correct candidate. For example, the candidate?s flyer may be stapled to packages of instant noodles handed out to voters (Appendix Figure B.3), or cash may be attached to the flyer or letter from the candidate (Appendix Figure B.4). The most common strategy, however, is for candidates to distribute money attached to a sample ballot. They encourage voters to take the ballots with them to the polls as a guide. The sample ballot includes not just the candidate?s name, but also candidates from other races up and down the ticket. Appendix Figure B.5 contains four examples of vote buying using

sample ballots.[3]

Monitoring local leaders' efforts and voter behavior

Philippine polls have used automated ballot readers since 2010, making monitoring how voters actually vote challenging for candidates.[4] It is possible for candidates to connive with election officers who administer polling places. Election officers can, in limited circumstances, find excuses to help voters insert their ballots into ballot readers and observe contents of the ballot. However, such situations are rare and are kept in check by independent election monitors. In the vast majority of cases voters are likely to be able to keep their ballots secret. Candidates use election results as an imprecise mechanism to audit the performance of local leaders who are buying votes for them. For example, candidates providing funds to a *barangay* meant to buy 500 votes would expect to receive at least 500 votes from that *barangay*. This, of course, is a very crude method, and cannot be used to check whether any individual sold his or her vote.

Recruitment script

In English:

Hello, my name is _____. I am working with Innovations for Poverty Action along with the University of Michigan. We are researching voter education campaigns that can promote civic competence. We are asking you to participate because you are a resident of and a bona fide registered voter in Sorsogon City.

If you agree to be part of the research study, you will be asked to participate in a survey. We

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[3]One candidate went to great lengths to explain why the bottom left ballot was superior to the bottom right ballot (Figure B.5). Both candidates were offering the same amount (300 pesos) but the candidate on the bottom right made the rookie mistake of not "fanning" the money so that all of the bills were immediately visible, and then compounded this mistake by stapling the money to back of the ballot rather than front.

[4]Before automated polling, indirect methods for confirming vote-buying transactions included what were known as "clave" ("key") methods, in which voters would signal in some way on the ballot itself that they had sold their vote. For example, when voting, voters who received vote-buying payments would agree to vote for only the single candidate who had bought the vote (and not other candidates on the ballot for other elected positions). The manual process of tabulating votes involved reading out in public the votes on every single paper ballot, and so the number of ballots with votes for just a single candidate could be tallied by election observers allied with the vote-buying candidate. A variation on the "clave" method involved having vote-sellers write in the name of someone not running for another elected position (e.g., the name of a popular media personality). The number of write-in votes for the media personality would be counted to track the number of votes sold.

will ask questions about your voting behavior in the past and voting intentions for the upcoming May elections. We will also ask you to watch a 3-minute video clip relating to the elections.

While you may not receive a direct benefit from participating in this research, we hope that this study will contribute to improving the efficacy of voter education campaigns and promoting greater civic competence among Filipino.

Are you available to participate in our study?

(If asked) This interview will take approximately 30 minutes. If you do not have time to do the interview right now, we can arrange to come back at a later time.

In Bicol:

Dios marhay na adlaw, ako tabi si _____. Nagtatrabaho ako sa Innovations for Poverty Action kasabay san University of Michigan. Igwa tabi kami sin research tungkol sa voter education campaigns o mga kampanya para sa edukasyon kan mga botante. Iniimbitaran ka tabi namon magpartisipar sa research na ini, bilang registradong botante nan residente kan Sorsogon City.

Inhahagad tabi namon an partisipasyon nindo sa paagi san pagsimbag nindo sa saro na sarbey. An mga hapot sa sarbey tungkol sa mga hinimo mo kaugnay sa mga nakaaging eleksyon asin mga intensyon mo sa pag-boto niyan na maabot na eleksyon. Igwa man po kami sin halip-ot na video na ipapakita saimo na may kaugnayan sa eleksyon.

Maski ngani wara kami maipo-promisa sa imo na anuman na direktang benepisyo sa pagpartic-ipar sa pagaadal na ini, inlalayon namon na makabulig an pag-aadal na ini para mapagayon asin mapamarhay pa an mga kampanya para sa edukasyon kan botanteng Pilipino.

May panahon ka tabi na mag-partisipar sa pag-aadal na ini?

(If asked) An interview na ini malawig sin mga 30 minutos. Kun wara ka tabi panahon sa interview na ini sa niyan, pwede man tabi kita mag-iskedyul sin iba na oras kun sano may panahon ka na.

Analysis of attrition from baseline to endline

To be included in the endline sample for analysis of a particular electoral outcome, a baseline respondent had to have: 1) completed the endline survey, 2) actually turned out to vote in the election, and 3) reported who they voted for in a given electoral race.

Appendix Table B.1 presents regression estimates of the impact of the treatments on various measures of attrition from the endline sample for analysis. All regressions include the full set of control variables. For each dependent variable, the mean of the dependent variable in the control group is presented in the bottom row of the table.

In column 1, the dependent variable is an indicator for completion of the endline survey. In column 2, the dependent variable is an indicator for completion of the endline survey and turning out to vote. These outcomes are examined to shed light on any treatment-related differentials in preliminary stages of attrition. In the control group, the mean of the dependent variable is 0.966 and 0.938 respectively. As it turns out, the treatments have no large or statistically significant effects on these outcomes.

In columns 3 to 5, the dependent variables are indicators for completion of the endline survey, turning out to vote, and reporting one?s vote for a particular electoral race (mayor, vice-mayor, or city council). These variables represent whether an individual in the baseline sample ends up being used in the endline analysis of voting for a particular electoral race. In the control group, the mean of the dependent variable for the mayor?s race is 0.845. The corresponding numbers for the vice-mayor and city council races are 0.852 and 0.897 respectively. This measure of attrition is also very similar across treatment conditions: for neither treatment is there a statistically significant difference vis-à-vis the control group.

In the remaining columns of the table, we examine two other related dependent variables: whether a baseline respondent ends up reporting his or her vote in all races (column 6), and in at least one race (columns 7). The promise treatments do not have any large or statistically significant effect on these attrition measures either.

All told, we conclude that attrition bias is not a worry in the empirical analyses of this paper.

Figure B.1: Cases of instant noodles awaiting distribution under the red house, and campaign workers waiting to pass those noodles out to vote canvassers as they arrive.

Figure B.2: Vote buying packet distributed to voter with voter name and information written in, and candidate name printed in large bold letters at the bottom.
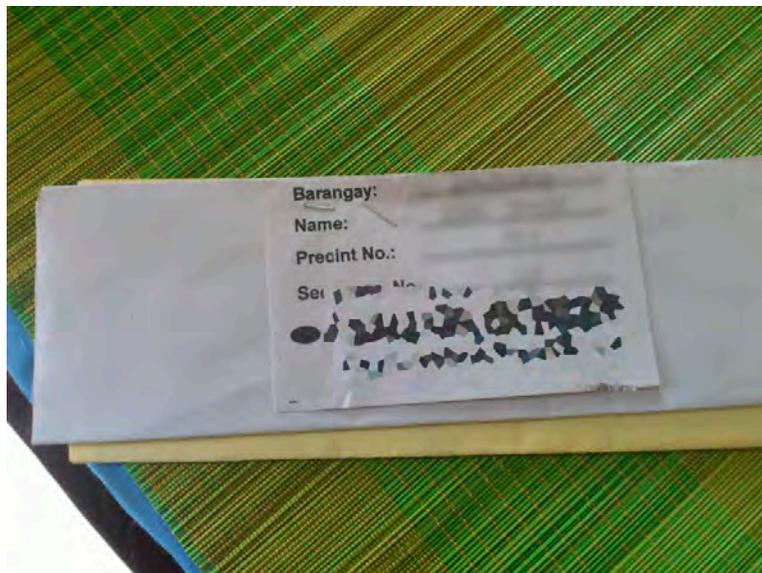
Figure B.3: Candidate flyer attached to package of instant noodles.

Figure B.4: Money attached to a letter urging voters to support a particular candidate.
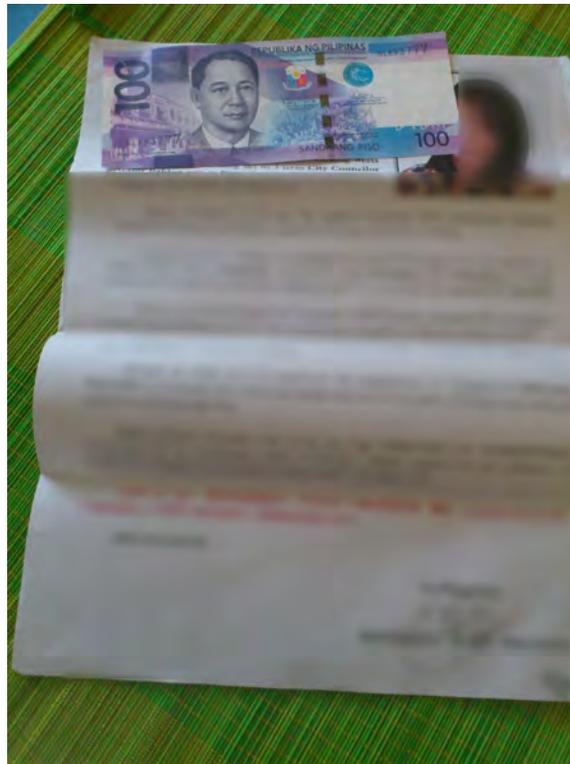
Figure B.5: Sample ballots with money attached.

Table B.1: Impact of treatments on completion of voting reports at endline (ordinary least-squares regressions)

Dependent variable:	Completed endline survey,	Completed endline survey, turned out	Completed endline survey, turned out, reported vote for mayor	Completed endline survey, turned out, reported vote for vice-mayor	Completed endline survey, turned out, reported vote for city council	Completed endline survey, turned out, reported vote for all races	Completed endline survey, turned out, reported vote for at least one
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Promise 1 treatment	-0.0120	-0.0103	0.0180	-0.00903	-0.00521	-0.0112	0.00294
("Don't take money")	(0.0162)	(0.0206)	(0.0284)	(0.0296)	(0.0255)	(0.0328)	(0.0236)
Promise 2 treatment	-0.00529	-0.00403	0.00688	-0.00756	-0.00263	0.00295	-0.00817
("Take money, vote conscience")	(0.0151)	(0.0196)	(0.0288)	(0.0297)	(0.0250)	(0.0322)	(0.0238)
Control variables	Y	Y	Y	Y	Y	Y	Y
Observations	883	883	883	883	883	883	883
R-squared	0.057	0.042	0.070	0.039	0.037	0.060	0.037
Mean of dep. var. in control group	0.966	0.938	0.845	0.852	0.897	0.801	0.911

***$p < 0.01$,** $p < 0.05$,* $p < 0.1$

Notes: Robust (Huber/White) standard errors in parentheses. Dependent variables are all indicator variables. Respondents randomized with equal (1/3) probability into the control group, Promise 1 treatment group, or Promise 2 treatment group. Control variables are listed in Panel A of Table 1 and were reported in baseline survey prior to treatment.

Table B.2: Impact of Treatments on Vote-Switching (regressions without control variables)

Dependent variable:		Switched Vote in Any Race (1)		Switched Vote for Mayor (2)		Switched Vote for Vice-Mayor (3)		Switched Vote for City Council (4)
Promise 1 treatment	β_1	-0.0699	β_{1m}	0.00969	β_{1v}	-0.00802	β_{1c}	-0.0869**
("Don't take money")		(0.0429)		(0.0279)		(0.0359)		(0.0429)
Promise 2 treatment	β_2	0.0444	β_{2m}	0.0406	β_{2v}	0.0504	β_{2c}	0.0060
("Take money, vote conscience")		(0.0426)		(0.0297)		(0.0378)		(0.0437)
Control variables		N		N		N		N
Observations		806		759		751		793
R-squared		0.009		0.003		0.004		0.007

***$p < 0.01$,** $p < 0.05$,* $p < 0.1$

Notes: Robust (Huber/White) standard errors in parentheses. Dependent variable in columns 1-10 equal to 1 if respondent switched his/her vote in the given race or set of races, 0 otherwise. Vote switching in mayor and vice-mayor races defined as voting for a candidate not receiving respondent's highest favorability rating in baseline (pre-election) survey. Vote switching in city council race defined as voting for a candidate not among the respondent's top-four highest-favored candidates in baseline survey. Respondents randomized with equal (1/3) probability into the control group, Promise 1 treatment group, or Promise 2 treatment group.

Table B.3: Tests of theoretical predictions (using coefficients from regressions without control variables)

	Races pooled	Mayor race	Vice-mayor race	City council race

A. Testing predictions of partially sophisticated theoretical case
(within race, effects more negative for Promise 1 than Promise 2)

$\beta_1 - \beta_2$ -0.114*** $\beta_{1m} - \beta_{2m}$ -0.031 $\beta_{1v} - \beta_{2v}$ -0.058 $\beta_{1c} - \beta_{2c}$ -0.093**
(0.042)   (0.030)   (0.037)   (0.043)

P-value of F-test: $(\beta_{1m} - \beta_{2m} = 0)$ & $(\beta_{1v} - \beta_{2v} = 0)$ & $(\beta_{1c} - \beta_{2c} = 0)$ 0.009

B. Testing prediction of differential effects across races
(within promise, effects more negative for city council than in either mayor or vice-mayor races)

Comparing across races, for Promise 1: $\beta_{1c} - \beta_{1m}$ -0.097** $\beta_{1c} - \beta_{1v}$ -0.079
    (0.049)   (0.053)

Comparing across races, for Promise 2: $\beta_{2c} - \beta_{2m}$ -0.035 $\beta_{2c} - \beta_{2v}$ -0.044
    (0.051)   (0.054)

P-value of F-test: $(\beta_{1c} - \beta_{1m} = 0)$ & $(\beta_{1c} - \beta_{1v} = 0)$ & $(\beta_{2c} - \beta_{2m} = 0)$ & $(\beta_{2c} - \beta_{2v} = 0)$ 0.119

C. All theoretical predictions in A. and B. combined

P-value of F-test: $(\beta_{1m} - \beta_{2m} = 0)$ & $(\beta_{1v} - \beta_{2v} = 0)$ & $(\beta_{1c} - \beta_{2c} = 0)$
$(\beta_{1c} - \beta_{1m} = 0)$ & $(\beta_{1c} - \beta_{1v} = 0)$ & $(\beta_{2c} - \beta_{2m} = 0)$ & $(\beta_{2c} - \beta_{2v} = 0)$ 0.015

***$p < 0.01$,** $p < 0.05$,* $p < 0.1$

Notes: Table reports tests of linear combinations of coefficients suggested by theory. Robust (Huber/White) standard errors in parentheses. β_{ij} is impact of promise i on vote-switching in race j in regressions reported in Appendix Table 2.

APPENDIX C

Nudging Good Politicians

Appendix

Figure C.1: Call for applications poster for the leadership training workshop.

Figure C.2: Leadership training workshop content.

Foundational Training for Aspiring Young Politicians

PROGRAM

DAY 1		
TIME	ACTIVITY	OUTPUT/ REMARKS
1:00-06:00pm	Arrival and Registration Check-in	Participants are each given a workshop kit with materials needed for the workshop (e.g. pen, sheets of paper, name tag with ID number, program, handouts, and worksheets).
06:00-07:00pm	Dinner	
07:00-08:15pm	Opening Activity • Welcoming of Participants • Orientation to the Summit & House Rules • Expectations Setting • Objectives, Framework, and Program Schedule	Participants will be informed that there will be worksheets that they need to accomplish and submit for each plenary session.
08:15-09:30pm	PLENARY 1 Lead2Serve: The Case for Servant Leadership in Youth Councils • Interactive Exercise: The Evolution Game • Understanding Servant Leadership • Servant Leaders Models in Business • Servant Leadership Models in the Philippine Setting • The Compelling Need for Servant Leaders in Youth Councils Individual Activity/ Group Discussion (IA/GD) • What qualities of servant leaders do I demonstrate? • What can I contribute to our community?	Participants submit filled-in Worksheet 1 with answers to the 2 key questions during the IA/GD. They will need to indicate their ID number on the Worksheet but not their names.
09:30-10:00pm	Announcements for Day 2	Group faciliators will submit score sheets for the day after announcements
DAY 2		
07:00-08:00am	Breakfast	
08:00-10:00am	PLENARY 2 The Four Main Tasks of a Leader: • Personal Growth • Building Relationships • Developing People: The Two-Point Test • Enhancing Momentum in the Community	
10:00-10:30am	Refreshments	
10:30-12:00pm	Individual Activity/ Group Discussion (IA/GD) • What is the present youth situation in our community? • How is the present leadership addressing this situation and is it sufficient?	Participants submit filled-in Worksheet 2 with answers to the 2 key questions during the IA/GD.

12:00-01:15pm	Lunch	
01:15-03:30pm	STRUCTURED LEARNING EXERCISES Series of experiential learning activities, games, and challenges that will serve as practical application of concepts learned in the plenary OIL: Observation, Insight, Learning	
03:30-04:00pm	Refreshments	
04:00-07:00pm	Individual Activity/ Group Discussion (IA/GD) • If I were to assume an elective position in our community's youth council, what can I do differently? What program of action/ platform of government can I espouse?	Participants submit filled-in Worksheet 3 with answers to the 2 key questions during the IA/GD.
07:00-08:00pm	Dinner	
08:30-10:00pm	Synthesis of Day 1 & 2 Suggested Dynamics: Bonfire • Creative Presentation on Selected Program Themes • Burning the Boat Activity	Group faciliators will submit score sheets for the day after synthesis
DAY 3		
07:00-08:00am	Breakfast	
08:00-10:00am	PLENARY 3 THE EXCELLENCE OF A YOUNG LEADER • Defining excellence • The Law of Solid Ground • The Four Human Dimensions • Case Study	
10:00-10:30am	Refreshments	
10:30-12:00nn	Group Discussion • What is the most striking learning point did I encounter in this Summit? • How can I apply this concretely? • What could prevent or stop me from applying my learning?	Participants submit filled-in Worksheet 4 with answers to the 3 key questions during the IA/GD.
12:00-01:30pm	Lunch and Check Out	Group facilitators will submit score sheets for the day during lunch.
01:30-03:00pm	CLOSING • Commitment Ceremony • Closing Remarks: A Commitment to Lead in Order to Serve • Awarding of Certificate of Merit and announcement of recipients of the campaign posters sponsored by the Angara Centre [This information will not show in the actual program. It will be a surprise announcement.]	Worksheets will all be returned to the participants. PI will determine which participants make the cutoff score if the workshop has conditional incentive. All participants will receive the incentive if the workshop has unconditional incentive.

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Figure C.3: Workshop performance scoring rubric.

Workshop Participation Rubric and Guide

	3	2	1
Peer Interaction	Actively supports, engages, and listens to peers	Makes a sincere effort to interact with peers	Limited or no interaction with peers even when encouraged
Contribution to small group discussion	Comments advance level and depth of dialogue	Comments are relevant and clearly stated	Demonstrates weak level of interest and comments made are vague
Group dynamics	Small group dynamic and level of discussion are often better because of participant's presence	Small group dynamic and level of discussion are occassionally better, but not worse, because of participant's presence	Small group dynamic and level of discussion are sometimes disrupted by participant's presence

Consider upgrading score when:
1. Participant is becoming more active and/or making more effective comments that raise overall level of discussion and set examples for others.
2. Participant is asking thoughtful questions that enhance discussion and engage peers.

Consider downgrading score when:
1. Participant is not present for a significant portion of the time alloted for group discussions.
2. Participant is dominating discussions to the point that others are restricted from participating.
3. Participant is making negative, offensive, and/or disrespectful comments.
4. Participant is using electronic devices for reasons unrelated to the discussion.

Workshop Participation Rubric and Guide

	3	2	1
Content (80%)	Gives thorough and well thought out response. Responses demonstrate intensive thoughts and answers to questions that go beyond primary answers.	Gives a complete and relevant response. Responses demonstrate some thought and answers to questions are adequate.	Response includes extraneous points or completely misses the point of the question. Answers to questions provide little or no evidence of understanding of the questions.
Composition (20%)	Answers are proofread carefully; the point is clear.	There are some spelling and grammatical errors but the point is made with acceptable clarity	Answers have some problems in language and sentence structure that result in lack of clarity

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