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Electoral reciprocity in programmatic redistribution: Experimental Evidence
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ABSTRACT

We analyzed two conditional cash transfers experiments that preceded Honduran presidential elections in 2001 and 2013. In the first, smaller transfers had no effects on voter turnout or incumbent vote share. In the second, larger transfers increased turnout and incumbent share in similar magnitudes, consistent with the mobilization of the incumbent party base rather than vote switching. Moreover, we found that turnout and incumbent share increased when cumulative payments were similar, but larger payments were made closer to the elections. As in prior lab experiments, individuals seem to overweight "peak" and "end" payments in their retrospective estimation of net benefits. We further argue that a model of intrinsically-reciprocal voters is most consistent with the findings.

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I. Introduction

In poor countries, vote-buying is a pervasive form of redistribution in which voters receive benefits—cash or in-kind—from party brokers in exchange for their votes (Finan and Schechter, 2012; Stokes, 2005; Stokes et al., 2013). It is prevalent in Honduras, where the dominant Liberal and National Parties have traditionally eschewed ideology and cultivated clientelist networks of political support by distributing resources (Ruhl, 2010; Taylor-Robinson, 2010, 2013). In the 2009 campaign, for example, four percent of voters reported receiving a gift or favor, but this rose to 21% if the question was embedded within a list experiment (González-Ocantos, Kiewit de Jonge, and Nickerson, 2015). In Honduras and elsewhere, nonprogrammatic distribution such as vote-buying may introduce distortions in the economy (see, among others, Baland and Robinson, 2007; Bates, 1981; Lizzeri and Persico, 2001).

In contrast, programmatic redistribution is shaped by transparent and objective rules, and its receipt is not conditioned on political support (Stokes et al., 2013). Beginning in the late 1990s, many poor countries in Latin America implemented variants of one such policy: conditional cash transfers or CCTs (Fiszbein and Schady, 2009; Adato and Hoddinott, 2010). The typical CCT policy objectively identifies poor households using geographic and/or household-level targeting and offers payments in exchange for using school and health services. A largely experimental literature finds that CCTs are successful in increasing the consumption of poor households, increasing the use of school and health services, and reducing child labor on the intensive and extensive margins.[1] Beyond effects on the welfare of poor households, programmatic redistribution via CCTs holds promise for encouraging a shift towards healthier electoral competition that minimizes distortions (Diaz-Cayeros, Estévez, and Magaloni, 2016). At the

[1] See Benedetti, Ibarrarán, and McEwan (2016) and the citations therein.

same time, a growing literature suggests that CCTs yield electoral benefits for incumbent presidential parties. If this is the case, CCTs might cause distortions of their own, such as the allocation of resources away from policies with potentially larger social returns, but lower electoral ones.[2]

The study most similar to ours in context and design finds that randomly-assigned CCTs in Mexico increased voter turnout and the incumbent vote share in the 2000 presidential elections (De La O, 2013). The author concludes that CCTs mobilized the incumbent party base, rather than persuading voters to switch allegiance. However, Imai, King, and Velasco Rivera (2016) contend that there are zero effects, after correcting errors in data coding among other analyses. A programmatic transfer in Uruguay used a discontinuous assignment rule, and finds that its recipients were more likely to favor the government even after the transfers ended (Manacorda, Miguel, and Vigorito, 2011). Using varied evaluation designs and data, other papers find that incumbent presidential parties reap electoral support from programmatic transfers.[3]

This paper contributes to this literature by analyzing two CCT experiments in Honduras. Before the 2001 presidential elections, the PRAF-II experiment randomly assigned households in 40 of 70 municipalities to receive small conditional cash transfers, only intended to cover the costs of complying with education and health conditions (Galiani and McEwan, 2013; IFPRI, 2000). Then, in late 2011, the Bono 10,000 experiment randomly assigned 816 villages to three treatment arms. In a public lottery that took place in September 2011, 150 of these were

[2] Likely candidates include direct investments in the quantity and/or quality of education and health services. CCT evaluations frequently note the importance of investing in the quality of services used by transfer recipients (e.g., Fiszbein and Schady, 2009).
[3] Using discontinuous variation in exposure to a Colombian CCT, Zárate et al. (2013) find that it affected turnout and incumbent vote share in the 2010 presidential election. Nupia (2011) also finds incumbent vote share effects in Colombia using a different—but less plausibly exogenous—source of variation in CCT exposure. Using a matching strategy, Zucco (2013) finds incumbent effects in several Brazilian presidential elections.

randomly selected for the treatment group, and another 150 were randomly selected for the control group. The former received the treatment immediately after the baseline was completed, and the latter received it immediately after the evaluation's endline survey was completed (but 5 months before the 2013 elections, a choice that turned out to be useful for our research). The remaining 516 villages were not monitored by the evaluation team, and received transfers according to standard procedures. We refer to the three groups as CCT1, CCT2, and CCT3. CCT1 received the largest cumulative transfers (by design, substantially larger than PRAF-II). CCT2 and CCT3 received the same cumulative amounts, but much less than CCT1. However, CCT2 received its payments closer to the election than CCT3.

Thus, the two experiments provide variation in both the cumulative amount and the timing of payment sequences. However, both experiments were objectively targeted at poor geographic areas with more than 10% of Honduran voters, and both policies were both administered by the same government agency. Hence, this paper is about the electoral implications of a targeted and objectively implemented programmatic social program, where there is little scope for political or clientelist manipulation. Once the decisions on where and how to target the program were made (in agreement with multi-lateral development banks and based on a public lotteries), the ruling party did not overtly control where the program was implemented or which families received it. We verify this using a nationally-representative household survey collected during the rollout of Bono 10,000.

We find that the earlier and smaller PRAF-II transfers did not affect voter turnout or the incumbent party's vote share, on average. In the later experiment, CCT1's voter turnout was 3 percentage points higher than CCT3, while the incumbent vote share (expressed as a share of registered voters) was 3.4 percentage points higher. We interpret this as evidence that transfers mobilized incumbent party supporters, but did little to encourage vote switching.

More puzzlingly, we find that CCT2's turnout and incumbent vote share also exceeded those of CCT3, suggesting a key role for the timing of payments. Individuals in CCT2 were more likely to receive "catch-up" payments, closer to the election. The results are consistent with a literature in behavioral economics on the retrospective evaluation of payment sequences. In lab experiments, individuals who receive a sequence of payments tend to overweight the peak and the end payments when assessing cumulative payments. Indeed, they use the peak-end midpoint as a heuristic for evaluating an entire sequence (Fredrickson and Kahneman, 1993; Langer et al., 2005; Yu et al., 2008). Our field experiment provides some confirmation of this heuristic in the Honduran context. Despite the same cumulative payments, CCT2's peak-end midpoints—estimated using administrative payment data—were higher than those of CCT3.

We argue that the structure of transfers also mattered in the earlier PRAF-II experiment. Prior research showed that the two poorest strata accounted for all of the substantial increase in child enrollment and reduction in child labor (Galiani and McEwan, 2013). There were zero effects in the three less-poor strata. Our paper shows that voter turnout actually declined in the poorer strata, but not in the less-poor strata. A plausible explanation is that households in poorer strata retrospectively remembered net benefits as negative, for which they exhibited punishing behavior in the voting booth. Three facts support this explanation. First, more households in the poorer strata complied with enrollment conditions, thereby incurring schooling costs as a consequence of accepting the payment. Second, the end of the 2001 school year (just before the November election) coincides with the early part of the coffee harvesting season, in which child labor is especially important. Third, the payments were small and designed to exactly offset costs. Yet, some authors reported sporadic or irregular payments (Fiszbein and Schady, 2009; Moore, 2008) that could have preceded costs.

In summary, voters responded to the cumulative, net amount of transfers as well as their timing, insofar as timing influenced remembrance of net amounts. This leaves open the question of *why* voters changed their behavior in response to a programmatic sequence of payments and costs. A plausible explanation is that some voters in programmatically targeted households were intrinsically reciprocal (Finan and Schechter, 2012; Lawson and Greene, 2014; Manacorda et al., 2011; Sobel, 2005). Voters reciprocated because they derived utility from aiding political parties that helped them, and from punishing parties that hurt them.[4] On the other hand, voters might have been instrumentally reciprocal. Their putative generosity in the polling booth was a rational response in a repeated game between voters and political parties, in which voters maximized the present value of future payoffs from CCTs or similarly-targeted programs. However, instrumental reciprocity is not consistent with our findings of punishing behavior, unless voters somehow anticipated that punishment would lead to higher payoffs in the future (Sobel, 2005). Even if there is some role for instrumental reciprocity, intrinsic reciprocity makes it easier to maintain a repeated game between voters and political parties (Finan and Schechter, 2012; Sobel, 2005).

Finally, our results explain two stylized facts of CCT implementation in poor countries. First, the use of conditional rather than unconditional transfers is common, perhaps to assuage taxpayers (Fiszbein and Schady, 2009). However, the conditions themselves have often been imperfectly enforced (Baird et al., 2014; Ozler, 2013). Weaker conditions imply larger net

[4] Intrinsic reciprocity is also a compelling explanation for the persistence of clientelist redistribution even when voters cannot be easily monitored due to secret ballots (Lawson and Greene, 2014). To lessen the commitment problem, party brokers or middlemen explicitly target reciprocal individuals in Paraguay (Finan and Schechter, 2012).

benefits for a subset of households who would otherwise incur costs of complying with conditions. Second, due largely to operational complexities and fiscal constraints, the distribution of payments in CCT policies does not always conform to scheduled amounts and timing, in ways that our results suggest could influence electoral outcomes. In the early implementation of Mexico's Progresa, for example, Skoufias (2005) shows that payments were sometimes delayed and larger-than-expected due to "catch-up" payments. Even if politicians do not actively undermine CCT implementation, our evidence suggests that electoral incentives do not prod them to fix it.

II. Background

A. Honduran Politics

Since 1981, Honduran presidential elections have been held every four years (Taylor-Robinson, 2013).[5] Before the 2013 elections, the National and Liberal Parties dominated, with several small parties capturing a small percentage of the vote. Neither dominant party has consistently defended a strong ideological platform, and both have competing internal factions (Ruhl, 2010; Taylor-Robinson, 2013). Rather, the parties have cultivated clientelist networks of supporters by distributing resources and jobs, and these coexist with programmatic cash transfers.

[5] Elections are also held for a unicameral congress and the mayors of 298 municipalities. From 1981 to 1993, the ballots were fused, such that voters cast a single vote for a party. In 1997 and 2001, ballots were unfused but closed-list, such that voters cast a ballot for a party rather than specific congressional or mayoral candidates. Since 2005, ballots have been unfused and open-list. Eighteen departments have from 1 to 23 congressional representatives, in proportion to their population. Voters cast as many ballots for candidates chosen in primary elections, and winners are chosen with proportional representation rules (Taylor-Robinson, 2013).

Two-party dominance eroded after a 2009 *coup d'etat*. The Liberal president—Manuel Zelaya, elected in 2005—sought closer relations with Venezuela and a reversal of a ban on reelection (Ruhl, 2010). In June 2009, Zelaya was illegally removed from the country by the military. The Liberal president of the congress assumed power until the November 2009 elections won by the National Party candidate. By the 2013 elections, new parties had emerged (Otero-Felipe, 2014). The most prominent included a left-leaning party known by its Spanish acronym, LIBRE, and led by Zelaya's spouse, Xiomara Castro. The right-leaning Anti-Corruption Party (PAC) was headed by a well-known television personality, Salvador Nasralla.

The 2001 and 2013 elections were both preceded by the launch of conditional cash transfer programs described in the introduction. In 2001, the National Party candidate for the presidency, Ricardo Maduro, defeated the incumbent Liberal party with more than 50% of valid votes (Taylor-Robinson, 2003). In 2013, the incumbent National Party candidate, Juan Orlando Hernandez, won with a plurality of 39% of valid votes (Otero-Felipe, 2014).

B. PRAF-II (2000–2001)

Since the early 1990s, the *Programa de Asignación Familiar* (PRAF), or Family Allowance Program, has distributed cash to households with children and pregnant or nursing mothers. In its first phase (PRAF-I), the health and education conditions attached to the transfers were weakly enforced. Moreover, poverty targeting was weak and anecdotal evidence suggests that its benefits were targeted to political supporters (Moore, 2008).

PRAF-II was launched in 1998, functioning alongside rather than supplanting PRAF-I. It involved the explicit collaboration of a lender (the Inter-American Development Bank) and an external evaluator (IFPRI). IFPRI identified 70 (of 298) municipalities with the lowest mean height-for-age of first-graders (IFPRI, 2000). It then randomly assigned 40 of the 70 municipalities—within five strata defined by height-for-age—to receive conditional cash transfers (Glewwe and Olinto, 2004; IFPRI, 2000; Galiani and McEwan, 2013).

Households in treated municipalities received a per-child transfer of L 800 or about $50 for each child between ages 6 and 12 who enrolled in grades 1 to 4. The school year in Honduras runs from February to November. Households were also eligible for a per-child transfer of L 644 or about $40 for each child under 3 years of age and pregnant or nursing mothers who attended health centers. The transfers were calculated to compensate the typical out-of-pocket and opportunity costs that a household would incur to consume education and health services (IFPRI, 2000). The average household was eligible for annual transfers equal to 5% of median per-capita expenditure, in the lower range of other Latin American CCTs (Galiani and McEwan, 2013; Fiszbein and Schady, 2009).

Households were supposed to receive payments every 6 months, and Morris et al. (2004) report that payments were made in November 2000, May-June 2001, and October-November 2001, just before the presidential elections on November 25 (see Figure 1). However, Fiszbein and Schady (2009) state that payments were made irregularly, and Moore (2008) cites evidence from the evaluation's designers that payments were "sporadic."[6] There is unfortunately no administrative record of the amount or timing of payments.

Using the 2001 census, we estimate that PRAF disbursed L 459 per eligible voter in treated municipalities before the election (L 1014 in 2013 prices), and none in control municipalities. The treatment-control difference is an upper bound for three reasons. First, it assumes full take-

[6] Moore (2008) cites an IFPRI report that has since been removed from their website.

up among households in treated municipalities with eligible children.[7] Second, it assumes that a full annual payment was made on behalf of each eligible child. Third, it assumes zero payments were made to households residing in control municipalities.

C. Bono 10,000 (2012–2013)

Bono 10,000 was launched in 2010 with loans from the Inter-American Development Bank, the World Bank, and the Central American Bank for Economic Integration, as well as the requirement of an external evaluation. In contrast to PRAF-II, the transfers were larger and conditions were generally weaker. A household received L 10,000 per year if it: (1) resided in a poor village; (2) passed a proxy means test; and (3) enrolled at least one child between 6 and 18 in grades 1 to 9 (Benedetti et al., 2016). The enrollment condition was weaker than PRAF-II, since multi-child households still received the transfer if only one child enrolled. A household received the smaller transfer of L 5,000 if it: (1) included registered children under 6 and pregnant or nursing mothers in a health center, and (2) did not include older, school-aged children. The average household was eligible for annual transfers equal to 18% of median per-capita expenditure (Benedetti et al., 2016).

In 2011, researchers designed a randomized experiment to evaluate the program. Of 3,727 villages (aldeas) in Honduras, 816 were eligible for random assignment. In September 2011, 150 of these were randomly selected (without stratification) for the treatment group, and another 150 were randomly selected for the control group. The former received the treatment immediately, and the latter received it immediately after the evaluation's endline survey was completed (but 5

[7] Assuming full take-up among eligible households, we used the 2001 census (collected in July 2001) to calculate total transfers in treated municipalities according to household eligibility rules, and divided by the number of individuals 18 or older.

months before the November 2013 elections). The remaining 516 villages were not monitored by the evaluation team, and received transfers according to standard procedures. We refer to the three arms, respectively, as CCT1, CCT2, and CCT3.

According to program rules, treated households were to receive payments in three installments per year. The first was a small, unconditional payment (1/12 of the total) received at the time of household registration. The second and third were payable upon verification of compliance with the conditions. In practice, administrative data suggest variation in the amount and timing of the payments across the treatment arms. Villages in CCT1 received large payments, on average, just after the study's baseline surveys were conducted, and just before the endline surveys were to begin (see Figure 2). Villages in CCT2 were excluded from the treatment until the final endline survey was conducted on June 23, 2013, although they subsequently received "catch-up" payments.

Finally, villages in CCT3 received transfers at PRAF's discretion, with a steady increase in the transfers per eligible voter. By election day on November 23, the average CCT1 village had received L 1,773 more per voter than either CCT2 or CCT3. CCT2 and CCT3 received about the same cumulative amount, but CCT2 villages received it closer to the election. Even compared with the upper-bound estimates of PRAF-II payments, the data show that eligible voters in Bono 10,000 villages received substantially larger payments prior to the election.

D. Was Bono 10,000 Programmatic?

The earliest Honduran CCT, PRAF-I, was supposed to be programmatic, but resources were not targeted according to rules (Moore, 2008). Its successor, PRAF-II, used a simpler geographic targeting scheme in which all households with young children were eligible in treated municipalities. During the experiment, municipal targeting was monitored by IFPRI researchers, and available evidence indicates that PRAF followed household-level targeting rules.[8]

Were villages and households also targeted according to publicly-announced criteria in Bono 10,000?[9] We assess this question with the nationally-representative 2011-2012 Demographic and Health Survey (DHS). It was collected between September 2011 and July 2013, overlapping with the randomized experiment and the larger nationwide rollout of Bono 10,000. Overall, 16% of sampled households reported that they had received at least one Bono 10,000 payment (12% if weighted to account for survey design).

We linked surveyed households to the village-level poverty measure used by government officials for geographic targeting, as well as the probability that a household lived in one of the 300 villages in CCT1 and CCT2 (i.e., the villages most subject to external monitoring by researchers).[10] We further linked households to the municipal-level vote share of the National Party in the 2009 presidential election, as well as the absolute difference between this share

[8] Only three children per household were eligible to receive education transfers. Consistent with this, Galiani and McEwan (2013) found that enrollment effects were larger among children residing in households with 1 to 3 eligible children (instead of 4 or more).

[9] The opposition candidates frequently claimed that it was not programmatic, though each promised to keep the program if elected. During the 2013 campaign, the LIBRE candidate Xiomara Castro promised "to continue the program and ensure its availability for all needy families, regardless of political affiliation" (La Tribuna, 2013, Oct. 6). The Liberal candidate, Mauricio Villeda, called for the continuation of a "depoliticized" Bono 10,000 (La Tribuna, 2013, Oct. 23).

[10] This DHS survey measures the latitude and longitude of the census segment—the primary sampling unit—in which households are located. Because of privacy concerns, the coordinates are perturbed with a randomly chosen angle and radius (imposing a maximum radius depending on whether it is an urban or rural segment). Given these rules, we created a circular buffer around each census segment point and identified the proportion of a given circle falling into one or more villages. We estimated a household's value of a village-level variable as the average across all villages falling within the circle, weighted by the area of each village within the circle. In the case of the binary variable indicating experimental villages, it yields the probability that a household is located in such a village. We followed the same procedure for municipal-level vote shares.

(from 0 to 100) and 50. The former measures the amount of core support enjoyed by the party, and the latter measures whether it is a swing municipality (Schady, 2000).

The descriptive statistics in Appendix Table B1 suggest that recipients of Bono 10,000 reside in poorer, rural villages and are themselves more likely to be poor (as gauged by variables similar to those included on the proxy means test, such as household assets and utilities access). These patterns are robust to the linear probability regressions in Appendix Table B2. Households in poverty quintile 5 are much more likely to have received a transfer.

Residing in high-poverty villages is no guarantee of receiving a transfer from Bono 10,000, since households must pass the proxy means test described in Benedetti et al. (2016). Consistent with this, the probability of receiving a transfer is higher when households have fewer assets, a dirt floor, and no sewer access. All else equal, households with at least one child are much more likely to receive a transfer. As expected, the relationship between determinants of household eligibility (such as the number of children) is even stronger when households reside in the subsample of villages more likely to be eligible (in quintiles 4 and 5). Lastly, there is no evidence that households were more likely to receive transfers if they resided in core or swing municipalities.

III. Data and Estimation

A. Voting Datasets and Variables

1. 2001 Elections

The PRAF-II experiment included 70 municipalities. We obtained data on 2001 presidential elections from the *Tribunal Supremo Electoral* (TSE),[11] and merged it to treatment and strata

[11] See http://www.tse.hn/web/estadisticas/procesos_electorales.html.

indicators from Galiani and McEwan (2013).[12] We calculated three dependent variables for each election: (1) turnout (the percent of registered voters who cast a valid vote for any party), (2) National vote share, and (3) Liberal vote share. Vote share is calculated as a percent of registered voters. Municipal-level vote shares from 1997 presidential elections—employed as control variables—were obtained from printed tabulations (Tribunal Nacional de Elecciones, 1997). The tabulations did not report the number of registered voters, and so we calculated vote shares as the percent of valid votes.

2. 2013 Elections

Voters are assigned to a voting center that corresponds to a sector or precinct. We scraped center-level voting results from a TSE website, and then hand-matched each center to its department, municipality, and village (aldea).[13] Overall, there were 5,433 domestic voting centers that contributed to official tallies, and we identified the corresponding village for 99.7% of them. Of the 3,727 villages in Honduras, 82% had at least one voting center, and 9% had three or more (see Table A1). The more sparsely-populated villages were assigned to voting centers in neighboring villages that we could not identify. Of 816 experimental villages, 677 (or 83%) had at least one voting center. The proportion of villages with at least one voting center is similar across treatment arms. These villages constitute the estimation sample in this paper, after summing voting center results within villages and calculating shares. The dependent variables

[12] Note that geographic codes for some municipalities in the Department of Santa Bárbara are different between the treatment data (which uses 2001 territorial codes) and the TSE tabulations.
[13] See http://siede.tse.hn/escrutinio/index.php. Hand-matching was possible because the TSE also posts the scanned *Actas* that were filled out and signed by local voting officials. The *Acta* lists the department and municipality of a center. It further lists the aldea, barrio, or caserío (barrios and caseríos are sub-units of aldeas). We used this information in concert with complete national territorial records to identify the aldea of each voting center.

for 2013 elections were calculated in the same way as 2001 elections. We calculated village-level vote shares for the presidential election in 2009, and included those as controls.[14]

B. Estimation

We estimate the average treatment effect on 2001 election outcomes with the regression

$$V_{ij} = \alpha + \beta \cdot CCT_{ij} + X_{ij}\gamma + CCT_{ij} \cdot (X_{ij} - \bar{X})\theta + \delta_j + \varepsilon_{ij},$$

where V_{ij} is the 2001 voting outcome of municipality *i* in experimental strata (or block) *j*. CCT_{ij} is a dummy variable indicating municipalities treated with CCTs, the δ_j are strata fixed effects, and X_{ij} is a vector of two baseline covariates: the 1997 municipal-level vote shares of (1) the National Party and (2) all other parties except for the Liberal Party. The covariates, in deviations from their means, are interacted with CCT_{ij} as a simple way of improving precision (Imbens & Rubin, 2015). β is the average treatment effect, estimated via ordinary least squares. We report heteroskedasticity-consistent standard errors.

The average treatment effects are similarly estimated for 2013 election outcomes with the following regression:

$$V_i = \alpha + \beta_1 \cdot CCT1_i + \beta_2 \cdot CCT2_i + X_i\gamma + CCT1_i \cdot (X_i - \bar{X})\theta_1 + CCT2_i \cdot (X_i - \bar{X})\theta_2 + \varepsilon_i,$$

where V_i is the 2013 voting outcome of village *i*, $CCT1_i$ and $CCT2_i$ indicate treatment arms, and X_{ij} is a vector of covariates that include the 2009 village-level vote shares of (1) the National Party and (2) all other parties except for the Liberal Party.

C. Baseline Balance

[14] See http://consultas.tse.hn:1177/. Like the 1997 tabulations, the 2009 election data do not report the number of registered voters. Thus, we calculated 2009 vote shares as the percent of valid votes.

In both experiments, transfer-eligible households, children, and mothers in the treatment and control groups were similar on a variety of socioeconomic variables (Morris et al., 2004; Galiani and McEwan, 2013; Benedetti et al., 2016). Table 1 further confirms balance in large census samples of adults eligible to vote in 2001. The differences among treatment and control groups are small in magnitude, and statistically different from zero in only one instance. Table 2 further compares the baseline vote shares in 1997 (for PRAF-II) and 2009 (for Bono 10,000). The treatment-control differences are small and not statistically distinguishable from zero.

Table 1 highlights the coverage and external validity of each experiment. The PRAF-II sample of municipalities included 303,821 individuals 18 and older in 2001 (10% of the national total), while the Bono 10,000 villages included 11% of 2001 voters. Treated households in both experiments were disproportionately poor and rural, as intended by the selection rules of each experimental sample (Benedetti et al., 2016).

IV. Results and Interpretations

A. Main Results

Table 3 reports estimates for presidential voting outcomes in 2001 and 2013. In 2001, the coefficient on turnout suggests that CCTs lowered turnout by 1.4 percentage points (about 2% of the control-group mean), but the estimate is not statistically different from zero.[15] Given the standard error, we can rule out positive effects larger than 1.3 percentage points. There are no effects on vote share of the National party or the incumbent Liberal party. The point estimates

[15] In prior work, Krishnaswamy (2012) found no effects on turnout or vote share in the presidential election. Linos (2013) found no effect on incumbent vote share, but estimated a pooled effect across 2001 and 2005 elections.

are similar, but less precisely estimated, if one does not control for 1997 vote shares (see Table C1).[16]

In contrast, transfers had larger and statistically significant effects in 2013. Relative to CCT3, turnout in CCT1 villages was 3 percentage points higher (about 5% of the control-group mean. The vote share of the incumbent National Party was 3.4 percentage points higher (12% of the control-group share). The similar effects on turnout and vote share—both calculated as a percent of registered voters—suggest that transfers mobilized supporters, rather than persuading voters to switch parties. There was a negative and marginally significant effect of 0.6 percentage points on the vote share of the Anti-Corruption Party (PAC), a right-leaning party that was only officially recognized in August 2013. While small in an absolute sense, it represented a 15% reduction in PAC's modest vote share. This might also be viewed as a mobilizing effect for the National Party since recent PAC supporters were plausible defectors from the National Party.

Villages in CCT2 also had higher turnout relative to CCT3 (2.3 percentage points, or 4%), as well as a higher National vote share (1.6 percentage points, or 6%). We interpret this as evidence that similar payments, but received closer to the election, also mobilized National Party supporters. In this case, PAC vote share rose by 0.9 percentage points, or 22%. PAC's platform was focused on reducing corruption. Because the transfers in CCT2 were larger-than-normal catch-up payments, it may have signaled to voters that the program was not transparently run.

[16] They are also similar in unconditional quantile regressions estimated at the median (see Table C2). We also followed the original experimental design and controlled for 3 treatment dummies relative to a control (see Table C3). G1 applied CCTs, G2 applied CCTs and a grant program for schools and health centers, G3 applied the latter, and G4 was a control. Galiani and McEwan (2013) show that the grant program had no impact on children, likely because it was not implemented (also see Moore, 2008).

However, we found no evidence that the payments systematically violated targeting criteria or ignored the relatively weak schooling conditions imposed on households.[17]

B. Why Did Timing Matter in Bono 10,000?

CCT2 voters responded positively in turnout and incumbent support to being paid roughly the same amount, on average, as CCT3 voters, but closer to election. According to a literature in behavioral economics, when retrospectively evaluating a sequence of hedonic episodes, subjects tend to overweight the moment of highest pleasure (or worst discomfort), as well as the final moment. Fredrickson and Kahneman (1993) suggest that a peak-end rule—the midpoint of the peak and end evaluations of pleasure or discomfort—largely explains how subjects retrospectively evaluate an entire sequence (also see Kahneman, Wakker, and Sarin, 1997). More pertinent to our setting, Langer et al. (2005) found that subjects over-weighted peak and end payments when comparing sequences of payments, despite a more obvious aggregation rule than hedonic episodes. Yu, Lagnado, and Chater (2008) found similar effects in the context of gambling payouts.

For each village in the estimation sample, we calculated the cumulative payments per eligible voter.[18] Consistent with Figure 3, Table 4 shows that CCT1 villages received L 1,773 (about $89) more per eligible voter than CCT3. In contrast, the cumulative transfers per voter were

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[17] School codes corresponding to an enrolled child are recorded for all households receiving transfers. A small percentage of scheduled transfers were not completed (presumably due to failure to comply with eligibility requirements or conditions). This percentage was similar across the three arms.

[18] The administrative payments database of PRAF records transfers for adult payees in eligible households (but contains no records on the total size of the population). To infer a village's transfers per eligible voter, we divided the sum of village payments—recorded in the database—by the number of individuals 18 or older each village. We estimated the number of eligible voters per village in 2013 using data and methods described in the note to Figure 2.

statistically indistinguishable in CCT2 and CCT3. CCT1 and CCT2 look much more similar (relative to CCT3) when comparing average peak and end transfers per eligible voter. The average peak transfers in CCT1 and CCT2 are, respectively, L 997 and 490 higher than CCT3. The pattern is reversed for end transfers: L 423 and 554, respectively.

Recall that the point estimates on turnout and National vote share were slightly larger in CCT1 than CCT2 (though the null hypothesis of equality was not rejected). A similar pattern—with a similar ratio of point estimates—is evident for the average midpoint of the peak and end transfers. One interpretation is that the peak-end rule influenced voters' retrospective evaluations of payment sequences, which in turn affected electoral outcomes. We later revisit the question of *why* voters felt obligated to reward incumbent parties when their votes could not be monitored.

C. Did Timing Also Matter in PRAF-II?

The Bono 10,000 transfers were designed to increase short-run consumption, in addition to increasing investments in child schooling and health. The smaller PRAF-II transfers focused only on the latter. About 75% of the education transfer was meant to cover out-of-pocket costs, while the remainder covered the opportunity costs of schooling, or "about 9 days of [child] work during coffee harvest time" (IFPRI, 2000, p. 9).

Upon being offered transfers—well before election day—households responded in one of three ways. First, households accepted CCTs if eligible children were already enrolled and would have done so in the absence of the transfer. Their net benefits were positive, since no additional costs were incurred as a result of accepting the CCT. Second, households declined CCTs if they felt the costs of complying with the conditions outweighed the benefits. Their net benefits were zero. Third, households with unenrolled children accepted CCTs (and enrolled their children) if they felt the benefits of doing so outweighed the costs. Their anticipated net benefits were positive—or the households would not have participated—but their "remembered" net benefits on election day were plausibly influenced by the peak-end heuristic.

Under what circumstances could they be negative? Suppose the household incurred costs during the school year that were (1) larger than specific transfer payments and/or (2) occurred closer to election day than final payments. The first cannot be verified, but the second is plausible because the school year ends in early November, while the coffee harvest season begins as early as September. Moreover, the biannual payments arrived in a sporadic or irregular fashion, according to some reports (Fiszbein and Schady, 2009; Moore, 2008). This might have diminished households' retrospective estimation of net payments under the peak-end rule, even rendering it negative.

We cannot empirically distinguish between household types in the census microdata, which would allow us to estimate voting effects for each group. However, Galiani and McEwan (2013) showed that the full-sample effects of PRAF-II on school enrollment and child labor participation are concentrated in the two (of five) strata with the lowest mean height-for-age z-scores. In these strata, school enrollment in late July increased by 15 percentage points and child labor in the prior week decreased by 7 percentage points (Galiani and McEwan, 2013). The effects on enrollment and child labor were small and not statistically different from zero in the other strata.

In Table 5, we estimate separate voting effects for the two groups. There is a negative and statistically significant effect of 4.2 percentage points on turnout in Blocks 1-2, and a smaller and statistically insignificant coefficient for Blocks 3-5. The coefficients on both vote share variables are less negative and imprecisely estimated. However, the coefficient on the incumbent Liberal vote share is consistent with negative effects as large as the turnout coefficient. A plausible explanation is that some voters in blocks 1-2 retrospectively evaluated a sequence of payments *and* costs as negative, and punished the incumbent by not voting, when they would otherwise have preferred to do so. Voters in blocks 3-5 either received zero payments (because they did not comply with conditions) or positive payments (because their children would have complied anyway). On average, eligible voters in these blocks did not find net benefits sufficiently appealing to reward the incumbent party.

D. Intrinsic Reciprocity and Voter Behavior

In models of clientelist exchanges, brokers (or middlemen) deliver benefits to voters in exchange for their votes (Finan and Schechter, 2012; Lawson and Greene, 2014; Stokes et al., 2013). Yet, short of undermining secret ballots, how can parties ensure that voters commit to the exchange? In Paraguay, middlemen are far more likely to reward voters with higher levels of intrinsic reciprocity (Finan and Schechter, 2012). These individuals are perhaps more likely to return the favor in the ballot box, “because they experience pleasure in increasing the material payoffs of the politician who has helped them,” thus overcoming the commitment problem (p. 864).

Finan and Schechter offer another explanation for the suggestive correlation. Suppose that ballot secrecy is compromised, or that Paraguayan voters simply believe it is. In this case, reciprocal behavior could be self-interested, or “instrumental,” if middlemen and voters interact in a repeated game and voters wish to “sustain a profitable long-term relationship” (Sobel, 2005, p. 392). The two explanations are not mutually exclusive, since intrinsic reciprocity may enhance cooperation in a repeated game (Finan and Schechter, 2012; Sobel, 2005).

The same reciprocal relationships could easily persist in programmatic distribution, and even co-exist with clientelist relationships. If ballots are secret, then intrinsically reciprocal voters could reward politicians and parties who include their group in programmatic targeting criteria. If ballot secrecy is compromised (or believed to be), then instrumentally reciprocal voters may cooperate in a repeated game to ensure the continued inclusion of their group in targeting criteria.

In Honduras, there are two reasons why intrinsic reciprocity cannot be ruled out. First, the 2013 elections were among the most heavily monitored in recent years. Independent observation of local vote tallies found results consistent with official tallies, and over 90% of sampled polling places had adequate provisions for ballot secrecy (Hagamos Democracia, 2014). Second, the 2001 elections showed evidence that voters engaged in punishing behavior, or "destructive reciprocity" (Sobel, 2005). This is certainly consistent with intrinsic reciprocity. However, it would not be expected in a repeated game unless voters expected that punishing the incumbent party would result in larger benefits to voters in the future (Sobel, 2005), which seems unlikely.

Manacorda et al. (2011) view their Uruguyan findings as consistent with a model of reciprocal voters. However, they further consider a model in which voters do reward or punish politicians for past benefits or costs. Rather, "voters use policy outcomes as signals to infer politicians' competence…or their preferences for redistribution towards particular social groups…." (p. 4).[19] Suppose that voters in both the treatment and control groups are rational and well-informed about the rules of a newly-implemented programmatic transfer. In this case, both groups should update their views similarly and, on average, make similar voting decisions. On

[19] They present a model based on Drazen and Eslava (2006) in which voters update their views on a party's redistributive preferences. It can be similarly applied to situations in which voters update their views of a party's competence, as in Rogoff (1990).

the other hand, if voters are rational but poorly-informed about the rules of programmatic transfers, then the treatment group's receipt of the transfer might lead it to update its views differently from the control group. Manacorda et al. (2011) argue that the second view could explain the Uruguayan results, because the program's targeting criteria were not publicly disclosed to recipients or even government employees.

In contrast, Honduran voters had extensive information about Bono 10,000 before the 2013 elections. This included publicity campaigns emphasizing the poverty targeting, household eligibility requirements, and conditions (for an example, see Appendix D). The results of the large-scale randomized experiment were presented in a public ceremony—attended by the President and media—a month prior to elections.[20] News coverage emphasized the role of Bono 10,000 in poverty reduction (Benedetti et al., 2016; La Tribuna, 2013, Oct. 9). In short, potential voters in both treatment and control groups had opportunities to update their views of the National Party's competence and redistributive preferences. In this situation, one would expect rational voters in otherwise identical groups to vote similarly, on average, which is inconsistent with our pattern of results. In fact, Manacorda et al. (2011) describe a context like the Honduran one as well-suited to conducting sharper tests of reciprocity.

V. Conclusions

We analyzed two CCT experiments that preceded Honduran presidential elections in 2001 and 2013. The same government agency implemented both treatments, and both experiments could be generalized to disproportionately poor and rural areas. In the first, relatively smaller transfers had no effects on turnout or incumbent vote share. In the second, we found that

[20] A co-author of this paper (McEwan) presented the results.

relatively larger transfers increased turnout and incumbent vote share in similar magnitudes. Since both variables were measured as a percent of registered voters, we inferred that transfers mobilized the incumbent party base rather than encouraging voters to switch parties. Moreover, we found that when comparing groups that received similar cumulative transfers, the effects on both turnout and incumbent share were larger in villages that received the transfers closer to the elections.

We argued that the timing of payments (which were determined by the evaluation design) mattered for voting behavior because individuals mis-remembered net benefits. In lab experiments, individuals tend to overweight peak and end payments when ranking sequences of payments (Fredrickson and Kahneman, 1993; Langer et al., 2005; Yu et al., 2008). In fact, the groups with larger effects had higher peak-end midpoints, suggesting that memories of benefits are more important to voters than actual benefits.

The peak-end rule also provided a helpful explanation for the negative turnout effects that we observed in poorest strata of the PRAF-II experiment. Perhaps not coincidentally, these strata had the largest gains in school enrollment and reductions in child labor (Galiani and McEwan, 2013). In those strata, some households incurred out-of-pocket and opportunity costs as a direct consequence of accepting the transfers. Child labor costs might have been incurred close to the election, given the overlap of the end of the school year in November and the beginnings of the coffee harvest in September and October. Weighed against smaller and, by some reports, sporadic payments, it is possible that some individuals mis-remembered net benefits as negative.

Finally, we argued that our findings were consistent with intrinsically reciprocal voters dispensing rewards and punishment in the voting booth, in proportion to their retrospective estimation of net benefits. There are at least two alternate explanations. First, it is possible that voters are instrumentally reciprocal, insofar as their voting behavior is a self-interested move in a repeated game with political parties. However, this is inconsistent with punishing behavior in PRAF-II that seemed unlikely to increase future benefits in the context of a repeated game. Second, it is possible that voters rationally used their receipt of transfers to update their views on the competence or redistributive preferences of parties (Manacorda et al., 2011; Drazen and Eslava, 2006; Rogoff, 1990). But if the control group is also well-informed about the criteria for distribution—as in the Honduran context—then one would not expect positive effects.

Our results provide insight into two common challenges of implementing CCTs. First, school and health conditions are often weakly implemented (Baird et al., 2014; Ozler, 2013). Second, payments do not always adhere to the announced amounts or schedules, although there is surprisingly little administrative data on CCT treatments. Administrative data from Mexico's Progresa CCT suggest that the largest payments to the experimental treatment group were always made in December, notwithstanding rules to the contrary (Skoufias, 2005). These were an effort to "catch up with the distribution of payments owed to the beneficiary families" (p. 9). In both cases, implementation challenges might increase individuals' perceptions of net benefits (by reducing costs, or by increasing peak-end midpoints on election day). Even without direct manipulation of CCT implementation, politicians have some electoral incentive not to improve it.

Finally, one must consider our results in light of Mexico's Progresa CCT. De La O (2011) found strong effects on turnout and incumbent vote share, but a compelling re-analysis by Imai et al. (2016) found zero effects. It is puzzling if the latter is true, since both Progresa and Bono 10,000 payments were a similar and generous percentage of household consumption (Benedetti et al., 2016; Fiszbein and Schady, 2009).

One hypothesis for the divergent effects is related to the implementation and perception of net benefits in the Progresa experiment. Treated villages received payments beginning in November 1998 (or as early as May 1998 according to administrative data in Skoufias, 2005). Control villages received payments for anywhere from 3 to 8 months, between November 1999 and April 2000 (Imai et al., 2016). The election was held on July 2, 2000. While the treatment group's cumulative payments clearly exceeded those of the control group, the difference in peak-end payments is unknown, and could be influenced by "catch-up" payments in the control group. In Honduras, this is precisely what occurred in CCT1 and CCT2: cumulative payments diverged but peak-end payments were similar (see Table 4). Ultimately, it is an empirical question best resolved by Mexican data on the amount and timing of payments.

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Table 1: Descriptive statistics from the 2001 census (individuals 18 and older)

	PRAF-II experiment			Bono 10,000 experiment			
	CCT	Control	p-value	CCT1	CCT2	CCT3	p-value
Female (0/1)	0.50	0.49	0.11	0.49	0.50	0.49	0.55
Age (years)	37.1	37.5	0.18	38.0	37.6	37.9	0.24
	(16.2)	(16.4)		(16.7)	(16.5)	(16.7)	
Self-identifies as Lenca (0/1)	0.33	0.28	0.36	0.05	0.05	0.06	0.63
Years of schooling	3.1	2.7	0.05	3.1	3.3	3.2	0.40
	(3.3)	(3.0)		(3.1)	(3.2)	(3.1)	
Self-identifies as literate (0/1)	0.65	0.62	0.14	0.66	0.69	0.68	0.27
Worked last week outside home (0/1)	0.52	0.52	0.58	0.49	0.48	0.48	0.77
Dirt floor in dwelling (0/1)	0.68	0.66	0.56	0.52	0.46	0.49	0.28
Piped water in dwelling (0/1)	0.67	0.67	0.99	0.71	0.68	0.70	0.74
Electric light in dwelling (0/1)	0.20	0.18	0.59	0.28	0.35	0.29	0.26
Sewer/septic access in dwelling (0/1)	0.35	0.30	0.11	0.31	0.33	0.35	0.32
N of municipalities	40	30		78	80	165	
N of villages (*aldeas*)	426	278		120	129	428	
Max N of individuals ≥18 years old	195,541	108,280		61,169	69,576	204,869	

Notes: The p-values for the PRAF-II experiment are obtained by regressing each row variable on a treatment dummy variable and dummy variables indicating strata, and testing the null that the coefficient on the treatment variable is zero. Robust standard errors are clustered by municipalities. The p-values for the Bono 10,000 experiment are obtained by regressing each row variable on two treatment dummies, and testing the null that the coefficients are jointly zero. Robust standard errors are clustered by villages. For details on the census variables, see Galiani and McEwan (2013).

Table 2: Baseline presidential vote shares in 1997 and 2009

	Mean vote share (standard deviation)			p-value
1997 (before PRAF-II)	CCT	Control		
National vote share	49.3	49.5		0.95
	(8.4)	(9.2)		
Liberal vote share	45.7	45.6		0.95
	(7.8)	(9.3)		
N of municipalities	40	30		
2009 (before Bono 10,000)	CCT1	CCT2	CCT3	
National vote share	57.0	57.3	57.3	0.98
	(16.0)	(15.1)	(15.1)	
Liberal vote share	39.5	39.3	39.7	0.96
	(16.0)	(14.9)	(15.0)	
N of villages (*aldeas*)	120	129	428	

Notes: The p-values in the top panel are obtained by regressing each row variable on a treatment dummy variable and dummy variables indicating randomization strata, and testing the null that the coefficient on the treatment variable is zero. The p-values in the bottom panel are obtained by regressing each row variable on two treatment dummies, and testing the null that the coefficients are jointly zero. All regressions use robust standard errors.

Table 3: Effects of transfers on 2001 and 2013 presidential elections

	Turnout	Vote share			
		National	Liberal	LIBRE	PAC
<u>2001 elections</u>					
CCT	-1.43	-0.48	-0.79	--	--
	(1.38)	(0.88)	(0.89)		
R^2	0.22	0.62	0.67		
Mean of control group	74.0	38.0	33.0		
<u>2013 elections</u>					
CCT1	2.99**	3.37***	-0.58	0.72	-0.60*
	(1.17)	(0.98)	(0.86)	(1.17)	(0.34)
CCT2	2.32**	1.61*	-0.40	0.23	0.86*
	(1.17)	(0.88)	(0.81)	(1.06)	(0.47)
R^2	0.08	0.31	0.23	0.14	0.03
Mean of CCT3	61.1	27.3	11.7	17.9	3.9
p-value (CCT1=CCT2)	0.65	0.12	0.86	0.73	0.00

Note: *** indicates statistical significance at 1%, ** at 5%, and * at 10%. Robust standard errors are in parentheses. The 2001 sample includes 70 municipal observations. The 2013 sample includes 677 villages (*aldeas*). The 2001 regressions control for dummy variables indicating strata; the 1997 municipal-level vote shares of (1) the National Party and (2) other parties not including the Liberal Party; and interactions of CCT with each vote share. The 2013 regressions control for the 2009 village-level vote shares of (1) the National Party and (2) other parties not including the Liberal Party; and interactions of CCT1 and CCT2 with each vote share.

Table 4: Transfers per eligible voter by 2013 election

	Hundreds of Lempiras per eligible voter (1 USD~20 L)			
	Average amount of transfers by election day	Average peak transfer	Average end transfer	Average midpoint between peak and end transfers
CCT1	17.73***	9.97***	4.23***	7.10***
	(1.28)	(0.66)	(0.30)	(0.47)
CCT2	-0.37	4.90***	5.54***	5.22***
	(1.07)	(0.66)	(0.54)	(0.60)
Constant	14.45***	5.80***	3.58***	4.69***
	(0.72)	(0.27)	(0.16)	(0.22)
R^2	0.21	0.29	0.28	0.28
p-value (CCT1=CCT2)	<0.01	<0.01	0.02	0.01

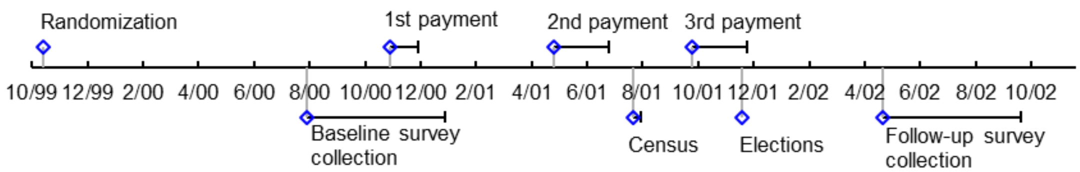
Notes: *** indicates statistical significance at 1%, ** at 5%, and * at 10%. Robust standard errors are in parentheses. All regressions include 677 villages with at least one voting center in the general elections; see Appendix Table A1.

Table 5: Effects of transfers on 2001 elections (by experimental blocks)

	Turnout	Vote share	
		National	Liberal
President			
CCT * (Blocks 1-2)	-4.24**	-2.57*	-1.46
	(1.87)	(1.37)	(1.43)
CCT * (Blocks 3-5)	0.44	0.91	-0.34
	(1.97)	(1.27)	(1.10)
R^2	0.25	0.64	0.67
p-value	0.10	0.09	0.53

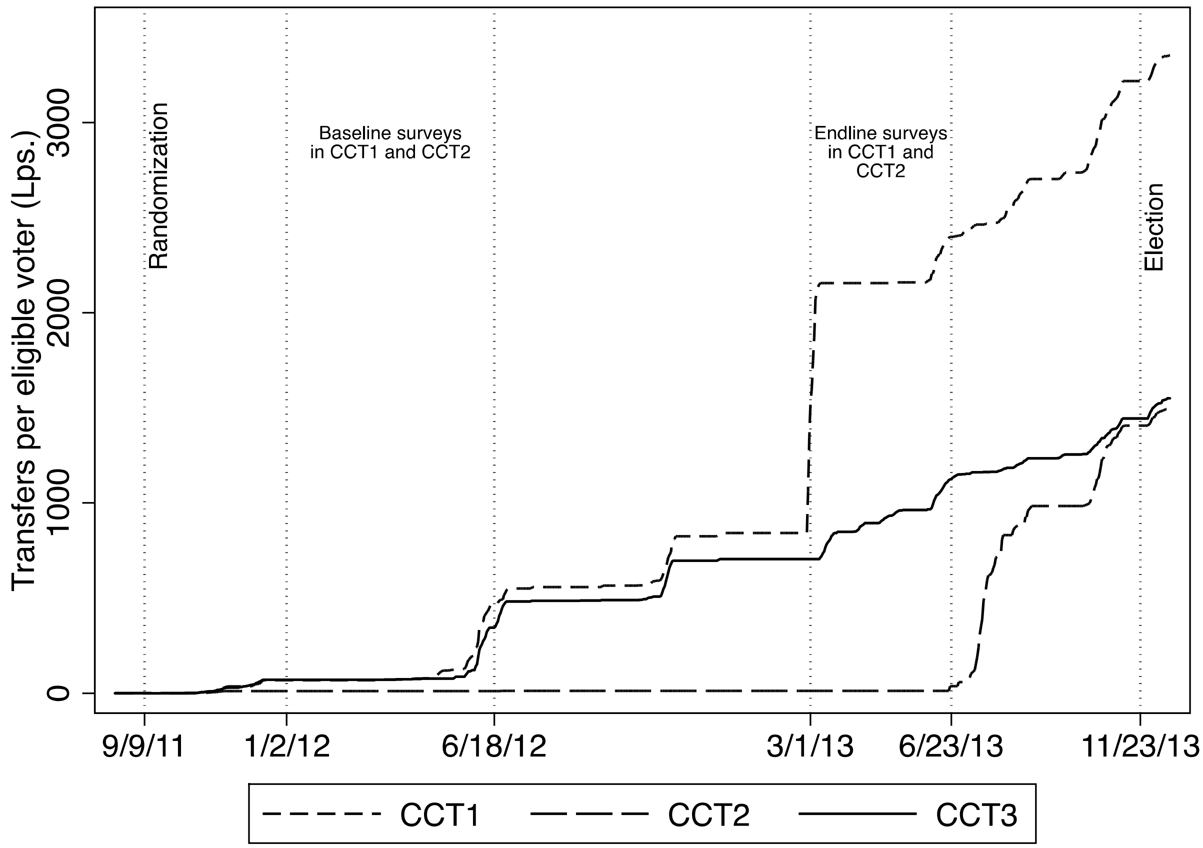
Note: *** indicates statistical significance at 1%, ** at 5%, and * at 10%. Robust standard errors are in parentheses. The 2001 sample includes 70 municipal observations. The 2001 regressions control for dummy variables indicating strata; the 1997 municipal-level vote shares of (1) the National Party and (2) other parties not including the Liberal Party; and interactions of CCT with each vote share.

Figure 1: Timeline of the PRAF-II experiment



Source: Galiani and McEwan (2013).
Notes: IFPRI (2000) reports the date of randomization (October 13, 1999). Morris et al. (2004) report the dates of payments, including a fourth payment not shown on the timeline that "partly coincided with the post-intervention survey" (p. 2031) from May to October 2002.

Figure 2: Transfers per eligible voter in the Bono 10,000 experiment



Note: The sample includes 677 (of 816 experimental villages) with voting centers in the 2013 election. We used administrative records to calculate village-by-day cumulative payments. To estimate the total number of eligible voters in each village in 2013, we calculated the percentage change in the number of eligible voters in municipalities between the 2001 and 2013 population censuses (village-level data are not available in 2013) and projected village-level growth using the percentages.

Appendix A: Data

Table A1: Villages in 2013 election sample

	National	Bono 10,000 experiment			
		Total	CCT1	CCT2	CCT3
Total villages	3727	816	150	150	516
Number of voting centers in village					
≥1	3069	677	120	129	428
1	2208	500	88	103	309
2	509	115	21	12	82
≥3	352	62	11	14	37
% of villages with ≥1 centers	82%	83%	80%	86%	83%

Note: In the 2013 general elections, 5,433 domestic voting centers contributed to official vote tallies. We matched the village (*aldea*) code to 5,417 centers.

Appendix B: Programmatic allocation of Bono 10,000

Table B1: Descriptive statistics on households in the 2011-2012 Demographic and Health Survey

Variable	Mean (standard deviation)		
	Full sample	Received Bono	Did not receive Bono
Household had received Bono 10,000 payment on survey date (1/0)	0.16	1.00	0.00
Poverty quintile 1 (1/0)	0.20	0.05	0.23
Poverty quintile 2 (1/0)	0.20	0.07	0.23
Poverty quintile 3 (1/0)	0.20	0.18	0.21
Poverty quintile 4 (1/0)	0.20	0.27	0.19
Poverty quintile 5 (1/0)	0.20	0.43	0.16
Rural (vs. urban) census segment (1/0)	0.58	0.91	0.52
Household resides in experimental village (probability)	0.07	0.08	0.07
	(0.16)	(0.17)	(0.16)
Dwelling has dirt floor (1/0)	0.22	0.42	0.18
Dwelling connected to sewer or septic (1/0)	0.49	0.22	0.54
Household asset index (z-score)	0.00	-0.57	0.11
	(1.00)	(0.74)	(1.00)
Schooling of household head (years)	5.10	3.30	5.44
	(4.37)	(2.97)	(4.51)
Total household size	4.70	5.96	4.46
	(2.36)	(2.31)	(2.29)
Household has zero children 0-5 years old (1/0)	0.53	0.38	0.56
Household has one child 0-5 years old (1/0)	0.32	0.37	0.31
Household has two or more children 0-5 years old (1/0)	0.15	0.25	0.13
Household has zero children 6-18 years old (1/0)	0.31	0.10	0.35
Household has one child 6-18 years old (1/0)	0.25	0.19	0.27
Household has two or more children 6-18 years old (1/0)	0.44	0.71	0.39
Anyone pregnant in household (1/0)	0.06	0.07	0.06
National Party vote share in 2009 (municipal-level)	56.44	57.51	56.24
	7.30	8.17	7.14
Absolute deviation from 50 of vote share (municipal-level)	7.32	8.18	7.15
	(5.75)	(6.65)	(5.54)
Number of households	20,446	3,265	17,181

Table B2: Village and household determinants of Bono 10,000 treatment (DHS households)

	Dependent variable: Had received Bono 10,000 payment at time of survey				
	Full sample			Quintiles 1 and 2	Quintiles 4 and 5
Poverty quintile:					
2	-0.046***	-0.061***	-0.061***	-0.015	
	(0.013)	(0.012)	(0.012)	(0.011)	
3	0.024	-0.008	-0.008		
	(0.016)	(0.015)	(0.015)		
4	0.090***	0.049***	0.049***		
	(0.015)	(0.014)	(0.014)		
5	0.177***	0.108***	0.107***		0.034*
	(0.017)	(0.016)	(0.016)		(0.018)
Rural	0.161***	0.109***	0.109***	0.020*	0.235***
	(0.010)	(0.010)	(0.010)	(0.010)	(0.018)
Household resides in experimental village (probability)	-0.027	-0.016	-0.014	0.038	-0.036
	(0.040)	(0.040)	(0.040)	(0.077)	(0.084)
Dirt floor		0.028***	0.028***	0.006	0.024*
		(0.009)	(0.009)	(0.013)	(0.013)
Sewer/septic		-0.023***	-0.023***	-0.014	-0.005
		(0.008)	(0.008)	(0.009)	(0.014)
Asset index		-0.027***	-0.027***	-0.010***	-0.023***
		(0.004)	(0.004)	(0.004)	(0.008)
Household head schooling		0.001	0.000	-0.002***	0.001
		(0.001)	(0.001)	(0.001)	(0.001)
Household size		0.008***	0.008***	0.002	0.005
		(0.002)	(0.002)	(0.002)	(0.003)
Number of children 0-5:					
1		0.024***	0.024***	0.013*	0.042***
		(0.006)	(0.006)	(0.007)	(0.011)
≥2		0.053***	0.053***	0.018*	0.093***
		(0.010)	(0.010)	(0.010)	(0.017)
Number of children 6-18:					
1		0.050***	0.049***	0.018***	0.110***
		(0.006)	(0.006)	(0.006)	(0.013)
≥2		0.144***	0.144***	0.050***	0.269***
		(0.009)	(0.009)	(0.010)	(0.018)
Anyone pregnant		-0.013	-0.013	-0.010	-0.019
		(0.011)	(0.011)	(0.010)	(0.019)
National Party share (municipal-level)			0.001	-0.000	0.003
			(0.002)	(0.001)	(0.004)
Abs. deviation from 50 (municipal-level)			-0.001	0.000	-0.003
			(0.002)	(0.002)	(0.004)
R^2	0.13	0.19	0.19	0.03	0.20
N	20,446	20,446	20,446	8,161	8,187

Notes: *** indicates statistical significance at 1%, ** at 5%, and * at 10%. Robust standard errors, clustered by census segments (the primary sampling unit), are in parentheses. See Table B1 for variable definitions and descriptive statistics. All regressions include dummy variables indicating the month in which the survey was completed.

Appendix C: Additional specifications

Table C1: Effects of transfers on 2001 and 2013 presidential elections (no controls)

	Turnout	Vote share			
		National	Liberal	LIBRE	PAC
2001 presidential					
CCT	-1.41	-0.53	-0.72	--	--
	(1.46)	(1.22)	(1.43)		
R^2	0.10	0.21	0.07		
Mean of control group	74.0	38.0	33.0		
2013 presidential					
CCT1	2.64**	2.68**	-0.93	1.34	-0.52
	(1.17)	(1.20)	(0.98)	(1.26)	(0.34)
CCT2	1.90	1.29	-0.60	0.25	0.94**
	(1.23)	(1.07)	(0.87)	(1.13)	(0.48)
R^2	0.01	0.01	0.00	0.00	0.01
Mean of CCT3 group	61.1	27.3	11.7	17.9	3.9
p-value (CCT1=CCT2)	0.62	0.32	0.77	0.48	0.00

Note: *** indicates statistical significance at 1%, ** at 5%, and * at 10%. Robust standard errors are in parentheses. The 2001 sample includes 70 municipal observations. The 2013 sample includes 677 villages (*aldeas*). The 2001 regressions only control for dummy variables indicating strata. The 2013 regressions do not include any control variables.

Table C2: Effects of transfers on 2001 and 2013 presidential elections (median regressions with no controls)

	Turnout	Vote share			
		National	Liberal	LIBRE	PAC
<u>2001 presidential</u>					
CCT	-0.34	-0.94	-1.04	--	--
	(1.97)	(2.20)	(2.12)		
<u>2013 presidential</u>					
CCT1	3.35**	2.50**	-1.61	1.01	-0.32
	(1.48)	(1.09)	(1.16)	(1.82)	(0.25)
CCT2	2.20	1.50	-0.60	1.58	0.58
	(1.67)	(1.18)	(0.99)	(1.20)	(0.37)
p-value (CCT1=CCT2)	0.52	0.43	0.47	0.78	0.02

Note: *** indicates statistical significance at 1%, ** at 5%, and * at 10%. Robust standard errors are in parentheses. The 2001 sample includes 70 municipal observations. The 2013 sample includes 677 villages (*aldeas*).

Table C3: Effects of transfers on 2001 elections (by 3 treatment groups)

	Turnout	Vote share	
		National	Liberal
President			
G1 (CCT)	-0.10	0.71	-0.62
	(1.62)	(1.02)	(1.05)
G2 (CCT)	-1.60	-1.32	-0.07
	(1.84)	(1.13)	(1.23)
G3	1.72	0.54	1.34
	(2.32)	(1.61)	(1.46)
R^2	0.24	0.64	0.67
p-value (G1=G2)	0.43	0.08	0.66
p-value (G2=G3)	0.20	0.28	0.38

Note: *** indicates statistical significance at 1%, ** at 5%, and * at 10%. Robust standard errors are in parentheses. The 2001 sample includes 70 municipal observations. The 2001 regressions control for dummy variables indicating strata; the 1997 municipal-level vote shares of (1) the National Party and (2) other parties not including the Liberal Party; and interactions of CCT with each vote share.

Appendix D: Example of publicity ("Bono 10,000 works like this")