End heuristics in retrospective voting:
Evidence from a conditional cash transfer experiment[*]

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Abstract: A Honduran field experiment allocated cash transfers that varied in their amount and timing. Voters were not indifferent to timing. Two groups of villages received similar cumulative payments per registered voter, but one received larger "catch-up" payments closer to election day. The latter treatment had larger effects on voter turnout and incumbent party vote share in the 2013 presidential elections. The results are consistent with lab experiments showing that individuals err in their retrospective evaluations of payment sequences. In Honduras, voters apparently used the amount of the final payment as an end heuristic for the sum of all payments received.

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1. Introduction

Beginning in the 1990s, many poor countries implemented variants of conditional cash transfers or CCTs (Fiszbein and Schady, 2009; Adato and Hoddinott, 2010). The typical CCT policy objectively identifies poor households using geographic and household targeting rules and offers payments in exchange for using school and health services.[1] CCTs are an example of programmatic redistribution, since the receipt of transfers is governed by transparent and well-known criteria (Stokes et al., 2013).

In sharp contrast to clientelist redistribution, programmatic transfers are not explicitly conditioned on political support.[2] Even so, theories of retrospective voting suggest that rational voters with imperfect information about the competence or redistributive preferences of incumbents may use the personal receipt of cash transfers to update their beliefs and vote accordingly (Rogoff, 1990; Drazen and Eslava, 2006, 2010; Manacorda, Miguel, and Vigorito, 2011; Healy and Malhotra, 2014). An alternative view is that some recipients of transfers are intrinsically-reciprocal (Sobel, 2005), and therefore express gratitude in the polling booth (Finan and Schechter, 2012; Lawson and Greene, 2014).

A growing empirical literature has estimated the effects of transfers on voter preferences and behavior. Studies in Uruguay, Romania, and Colombia—all using discontinuous variation in transfers—find positive effects on incumbent political support,[3] as do non-experimental studies.[4] In Mexico's well-known Progresa experiment, De La O (2013) reports positive effects on voter turnout and incumbent vote share in the 2000 presidential elections, though a re-analysis finds

[1] A largely experimental literature finds that cash transfers increase the use of school and health services (Baird et al., 2014; Gaarder, Glassman, and Todd, 2010), and reduce child labor on the intensive and extensive margins (de Hoop and Rosati, 2014)

[2] In clientelist redistribution, parties channel resources to voters via brokers, in exchange for political support. Voter compliance might be verified by the direct surveillance and violation of ballot secrecy (Stokes, 2005). Even when secret ballots prevent monitoring, party brokers may lessen the commitment problem by targeting resources to intrinsically-reciprocal voters (Finan and Schechter, 2012; Lawson and Greene, 2014).

[3] In Uruguay, recipients of unconditional transfers in the vicinity of the assignment cutoff were more likely to favor the government even after the transfers ended (Manacorda et al., 2011). In Romania, the recipients of a means-tested voucher for a computer purchase were more likely to support the incumbent governing coalition (Pop-Eleches and Pop-Eleches, 2012). In Colombia, Zárate et al. (2013) found that discontinuously-transfers affected turnout and incumbent vote share in the 2010 presidential election, especially among women who were the direct recipients.

[4] 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 of Brazil's national CCT in several presidential elections.

that results are sensitive to variable specification (Imai, King, and Velasco Rivera, 2017).[5] Using discontinuous variation in the assignment of communities to Progresa, Green (2006) found no effects on voter behavior.

Aided by a field experiment with three treatment arms, we estimate the impact of a large conditional cash transfer on turnout and incumbent party vote share in the 2013 Honduran presidential election. Our primary contribution to the literature is to demonstrate that the timing of payments affects retrospective voting, insofar as timing affects voters' ability to accurately recall a sequence of payments. Rational voters with imperfect information may view the total amount of transfers as signal of politicians' competence or preferences. Reciprocal voters may express gratitude in proportion to the total amount of transfers. In either case, theory assumes that voters are capable of accurate retrospection about a sequence of payments.

A large literature in psychology and behavioral economics finds that individuals make predictable errors when engaged in retrospection. When evaluating sequences of hedonic episodes (such as pleasant or aversive film clips), lab experiments show that individuals have a tendency to rely on heuristics. Specifically, they over-weight the final episode (the "end") and the episode of greatest pleasure or displeasure (the "peak"); for reviews, see Kahneman, Wakker, and Sarin (1997) and Healy and Lenz (2014). The same phenomenon occurs when subjects retrospectively evaluate sequences of payments, despite a transparent rule for aggregating payments over time. Subjects find it difficult to calculate a running sum of payments due to distraction from other tasks (Langer, Sarin, and Weber, 2005), inattention (Huber, Hill, and Lenz, 2012), or the sheer difficulty of doing so over a lengthy sequence (Yu, Lagnado, and Chater, 2008). As with hedonic episodes, subjects rely on peak and end payments as heuristics for their error-prone evaluations of sequences.

We find evidence that voters used end heuristics in a Honduran field experiment. The experimental sample includes 816 poor villages. In June 2012, households in 150 randomly-drawn villages—a treatment arm denoted CCT1—began receiving large conditional cash transfers, equivalent to roughly 18% of a median household's consumption. Households in another randomly-selected group of 150 villages—denoted CCT2—did not receive transfers until

[5] In another experiment, Blattman, Emeriau, and Fiala (2017) find that Ugandan beneficiaries of a highly successful program to support skilled enterprises were more likely to support the opposition. They attribute the surprising results to the empowering effects of financial independence, which diminished the need for patronage.

researchers completed endline surveys in June 2013. A companion paper to this one used endline data from CCT1 and CCT2 to estimate impacts on education, health, and poverty (Benedetti, McEwan, and Ibarrarán, 2016).

The exigencies of the experiment introduced variation in the amount and timing of payments across treatment arms. First, households in CCT1 received large payments just before the endline surveys. Second, households in CCT2 received large "catch-up" payments just after the endline surveys, but about 5 months before the presidential elections on November 23, 2013. In the meantime, the remaining group of 516 villages—denoted CCT3—received transfers at the discretion of the Honduran government (but, as we will show, without similarly-timed spikes). Using detailed administrative data, a later section shows that cumulative transfers per registered voter were similar in CCT2 and CCT3, but much higher in CCT1.

After linking villages' treatment status to village-level voting data, we find that voter turnout and incumbent vote share in CCT1 are, respectively, 2.7 and 2.5 percentage points higher than CCT3. This much is consistent with the large differences in the cumulative amount of transfers per registered voter. However, we also show that turnout and incumbent vote share in CCT2 are, respectively, 2.5 and 1.9 percentage points higher than CCT3. For both dependent variables, the coefficients on CCT1 and CCT2 are not statistically distinguishable.

The puzzling result is plausibly explained by differences in the sequences of payments offered to villages in the three treatment arms. Administrative data confirm that end payments per registered voters are higher in CCT1 and CCT2, relative to CCT3, but not statistically distinguishable from each other. There is a related pattern for peak payments and peak-end midpoints, although the coefficients on CCT1 are larger in each regression than coefficients on CCT2, in an economic and statistical sense. Given this, we conclude that voters are primarily influenced by end heuristics (for a related argument, see Healy and Lenz, 2014).

We empirically evaluate and discard alternate explanations for the pattern of results. Unlike voters in CCT3, some in CCT1 and CCT2 were exposed to baseline and endline surveys. It is possible that survey participation influenced voter perceptions of the competence or redistributive preferences of the incumbent. However, not all households participated in the survey, and we do not find that a proxy of survey participation moderates treatment effects in the expected direction. One might also hypothesize that the government surreptitiously allocated resources to CCT2 villages as a compensatory response to delayed transfers. However, households in CCT1 and CCT2 did not report substantial differences in a wide range of benefits received from public or private sources (other than the conditional cash transfer).

The paper makes several contributions to the literature. First, it provides causal evidence outside a lab setting that voters apply an end heuristic in the retrospective evaluation of economic outcomes. A classic literature in political economy documents that voters are responsive to macroeconomic conditions, especially in election years (Kramer, 1971; Nordhaus, 1975; Fair, 1978; Markus, 1988). As noted by Manacorda et al. (2011), these studies face considerable challenges in identifying exogenous variation in economic conditions. Political scientists have used lab experiments to confirm that subjects do a poor job of evaluating sequences of individual payments (Huber et al. 2012) and macroeconomic outcomes (Healy and Lenz, 2014). Subjects' behavior is consistent with the use of an end heuristic to evaluate economic outcomes. Our paper bridges a gap between these literatures: the results are internally valid and consistent with the use of end heuristics in a real political setting.

Second, the paper raises important issues related to the political economy of CCTs. Research has shown that deliberate or unintended variation in policy design can affect individual and household welfare. In one example, delayed transfers yielded larger impacts on education outcomes than bimonthly transfers (Barrera-Osorio et al., 2011, 2017). In another example, glitches in the enforcement of conditions facilitated tests of whether conditional transfers have effects on education enrollment beyond those of unconditional transfers (de Brauw and Hoddinott, 2011; Schady and Araujo, 2008).

There has been less attention to the political consequences of CCT design and implementation (and how rational politicians, attuned to the consequences, may wish to exert influence).[6] Returning to the examples above, the amount and timing of transfers in CCTs does not always conform to nominal payment schedules. In the Progresa experiment, the original evaluation report noted that payments deviated from a bimonthly schedule (Skoufias, 2005). There were "considerable delays in the processing of forms necessary for payment authorization" (p. 9). Administrative data show larger-than-expected payments in several months, "a consequence of Progresa's efforts to catch up…." (p. 9). This paper argues that large, delayed

[6] An exception is Fiszbein and Schady (2009), who note that the addition of education or health conditions to unconditional transfers—even in the absence of a marginal impact on outcomes—makes them more palatable for taxpayers and politicians (also see Baird et al., 2014).

payments will likely increase voters' retrospective evaluations of a payment sequence, even if the cumulative amount is held constant.

Moreover, the usual education and health conditions are often imperfectly enforced (for a review, see Baird et al., 2014). In an earlier Honduran CCT, for example, school attendance requirements were not enforced (though enrollment was), and participants were not removed from the program if they failed to comply with health conditions (Galiani and McEwan, 2013; Morris et al., 2004). This, too, may yield electoral benefits. Strong enforcement of onerous conditions may lead a subset of families to decline transfers, the so-called non-compliers (Baird, McIntosh, and Özler, 2011). Weaker enforcement may lead some non-compliers to accept the "conditional" cash transfer (with attendant consequences for voter behavior). In both examples, politicians may not be directly responsible for weak implementation. However, the evidence suggests that incumbent candidates and parties do not have strong incentives to improve the regularity of payments or the enforcement of conditions.[7]

Third, the paper highlights the necessity of describing the implementation of conditional cash transfers, including the amount and timing of actual payments. In our experiment, villages in CCT1 received substantially larger cumulative payments than villages in CCT2. In the absence of administrative payment data, we would have erroneously concluded that "cash transfers"—an overly broad construct—have no effects on voter behavior. An alternate view is that the timing of payments, in concert with flawed retrospection, led voters in CCT1 and CCT2 to believe that the treatments were similar. Individuals' reliance on heuristics might explain unanticipated results in other policy experiments.[8]

Section 2 provides background on the Honduran politics, as well as the conditional cash transfer experiment conducted before the 2013 presidential elections. Section 3 describes the estimation samples and election data that were scraped from official websites; it further demonstrates balance in baseline variables across the three treatment arms. Section 4 provides a

[7] One might argue that politicians have strong incentives to signal a commitment to public education by strictly enforcing attendance and enrollment conditions. However, the experiments in Bursztyn (2016) suggest that poor voters in Brazil have a stronger preference for redistributive programs than for investments in public education.
[8] For example, Imai et al. (2017) show that Mexico's Progresa had zero effects on voter behavior. The treatment group received payments for 31-32 months before the election, while the control group received payments for 3-8 months. The cumulative amount of transfers surely differed, but relative differences in voter behavior may have depended on the relative size of the last or largest payments, which did not always conform to bimonthly schedules (Skoufias, 2005). Contrary to this interpretation, Green (2006) finds no effect on voter behavior when comparing communities on either side of Progresa eligibility cutoffs; this was a plausibly cleaner treatment-control comparison. Imai et al. (2017) discuss this and other explanations for zero effects.

detailed description of implementation, including the fidelity of programmatic targeting criteria and the distribution of payments before elections. Section 5 reports estimates of effects on turnout and incumbent vote share. Drawing on related literatures in behavioral economics, it argues that the results are best explained by voters' use of end heuristics. It further presents robustness checks and complementary evidence from an earlier cash transfer experiment in Honduras. Section 6 summarizes and concludes.

2. Background

A. The 2013 presidential election

Since 1981, Honduran presidential elections have been held every four years (Taylor-Robinson, 2013). Prior to the 2013 election, the National and Liberal Parties dominated, with several small parties capturing a very small percentage of the vote. Neither party consistently defended a strong ideological platform, and both had competing internal factions (Ruhl, 2010; Taylor-Robinson, 2013). Traditionally, both parties cultivated strong clientelist networks of supporters by distributing resources and jobs in order to mobilize core supporters. In the 2009 election, for example, four percent of voters reported receiving a gift or favor during the campaign, but this rose to 21% if the question was embedded within a list experiment (González-Ocantos, Kiewit de Jonge, and Nickerson, 2015).

Two-party dominance eroded after a 2009 *coup d'etat*. The Liberal president—Manuel Zelaya—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 that were won by the National Party candidate, Porfirio Lobo. The *coup d'etat* catalyzed the formation of new parties (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 television personality, Salvador Nasralla. Ultimately, the incumbent National Party candidate, Juan Orlando Hernandez, won with a plurality of 36.9% of votes, followed by LIBRE (28.8%), the Liberal Party (20.3%), and PAC (13.4%). At 60.4% of registered voters, voter turnout was among the highest recorded in a presidential election (Otero-Felipe, 2014).

B. Conditional cash transfers prior to the election

Since the early 1990s, the *Programa de Asignación Familiar* (PRAF), or Family Allowance Program, has implemented variants of conditional cash transfers (for a history, see Benedetti et al., 2016; Moore, 2008).[9] In 2010, PRAF began implementing Bono 10,000, the program that is the subject of this paper. Three features distinguished the program from earlier versions.

First, it distributed larger transfers. Households received transfers of either 10,000 Lps. per year—about $500—or 5,000 Lps., depending on the presence and ages of children in the household. The typical household was eligible for annual transfers equal to 18% of median per-capita expenditure (Benedetti et al., 2016). This is comparable to other Latin American cash transfer programs such as Progresa/Oportunidades, and larger than earlier Honduran programs (Fiszbein and Schady, 2009; Galiani and McEwan, 2013).

Second, households were subject to a proxy means tests as a condition of participation, unlike earlier versions that only used the geographic targeting of poor municipalities (Galiani and McEwan, 2013). The proxy means test was based on a short household census conducted prior to roll-out. While the methodology was not disclosed to the public or researchers, it used income and wealth proxies such as the availability of electricity, sewers, and an index of household assets (Benedetti et al., 2016).

Third, the households were subject to weak conditions in exchange for receiving the transfers. Households received the larger amount—labeled an education transfer—if they enrolled at least one child between 6 and 18 in grades 1 to 9. Households received the smaller amount—labeled a health transfer—if children under 6 and/or pregnant women attended health center checkups (and if they were not eligible for the larger education transfer). Earlier programs had required all children to comply with education and health conditions (as did Bono 10,000 after the 2013 elections).

A companion paper reported the results of a randomized experiment embedded in the program roll-out (Benedetti et al., 2016). We analyze the same experiment in this paper, as described in the next section. In the months before the 2013 elections, the government sought to

[9] Between 2000 and 2002, a much-studied version—called PRAF-II—distributed small transfers to households conditional on school enrollment and health center use. The transfers were about 5% of median per capita consumption, in the lower range of other Latin American CCTs such as Progresa/Oportunidades in Mexico (Galiani and McEwan, 2013; Fiszbein and Schady, 2009). A randomized experiment showed that PRAF-II had substantial impacts on primary school enrollment and indicators of health service use (Glewwe and Olinto, 2004; Morris et al., 2004; Galiani and McEwan, 2013).

publicize the experimental findings. A ceremony was attended by the media and National Party officials, and news coverage emphasized the role of Bono 10,000 in poverty reduction (La Tribuna, 2013, Oct. 9).

C. Design of the Bono 10,000 experiment

PRAF began implementing Bono 10,000 in 2010, initially targeting the poorest of 3,727 villages (*aldeas*) in Honduras. At the same time, the research team collaborating with PRAF succeeded in delaying the roll-out in 816 villages that were also poor, but slightly less so than the earliest villages to receive transfers (Benedetti et al., 2016).[10] During a randomization ceremony on September 9, 2011, researchers blindly drew 300 numbered balls from a receptacle containing 816 (see the timeline in Figure 1). In alternating order, the 300 were assigned to a treatment group of 150 and a control group of 150. In this paper, we refer to each group, respectively, as CCT1 and CCT2. As described in Benedetti et al. (2016) and shown in Figure 1, baseline surveys were applied to a sample of eligible households in CCT1 and CCT2 in the first half of 2012.[11] Endline surveys were applied to the same sample between March and June 2013.

Villages in CCT1 received transfers immediately, while villages in CCT2 did not receive transfers until after the conclusion of endline surveys (but well before the elections on November 23, 2013). Benedetti et al. (2016) used the baseline and endline surveys to estimate the impact of transfers on child and household outcomes. Given the timing of endline surveys, CCT2 was a still a pure control group.

The remaining 516 villages—hereafter referred to as CCT3—did not participate in either baseline or endline surveys, and were not analyzed by Benedetti et al. (2016). However, they received transfers at PRAF's discretion. Given the randomization procedure, villages in CCT1, CCT2, and CCT3 should be similar, on average, prior to the roll-out of Bono 10,000. We empirically verify this in section 3. By election day on November 23, 2013, villages in the three treatment arms had received transfers that varied in their amount and timing (most notably a

[10] Despite this, Benedetti et al. (2016) estimate that rates of extreme poverty were very high in the experimental sample, exceeding 80%.
[11] A research firm—NORC at the University of Chicago—drew the sample from a list of eligible households that had passed a proxy means test devised and applied by PRAF. NORC's household sample drew a fixed number of households, 15, from each village, regardless of population size. Section 5 reports additional details.

significant delay in distribution of transfers to CCT2). In section 4, we describe these patterns using administrative payments data.

3. Data and estimation

A. Village samples

The Honduran *Tribunal Supremo Electoral* (TSE) is the agency responsible for conducting elections and certifying results. Voters are assigned to a voting center, usually located in a school. The voting center corresponds to a sector (or precinct). As required by law, the TSE published vote tallies for the 2013 elections on a website.[12] We scraped election outcomes for the 5,433 domestic voting centers that contributed to vote tallies.[13]

The scraped data do not include the geographic codes of the villages in which voting centers are located. As a labor-intensive alternative, we downloaded scanned images of the official *Actas* for each voting center (i.e., signed and certified vote tallies), which list the names of a voting center's department, municipality, and village (*aldea*).[14] We hand-matched 99.7% of voting centers to the geographic codes of their villages.[15]

Of the 3,727 villages in Honduras, 82% had at least one voting center in the 2013 elections. The remaining, sparsely-populated villages were assigned to voting centers in neighboring villages that we could not identify from publicly-available data. Of 816 villages in the experimental sample, 677 (83%) had at least one voting center (see Table 1). The proportion of villages with at least one voting center is similar across the three treatment arms.[16] This is expected, since random treatment assignment was independent of village population.

This paper's main estimation sample consists of 677 village-level observations. Nevertheless, it is possible that a village's voting centers include voters from outside the village. That is because the 677 villages may share a border with a sparsely-populated village without a voting

[12] http://siede.tse.hn/escrutinio/index.php
[13] Honduran consulates in some U.S. cities are also used as voting centers.
[14] To be more specific, the *Acta* lists either the *aldea, barrio,* or *caserío*. *Barrios* and *caseríos* are sub-units of *aldeas*, which facilitated the identification of *aldeas* not listed.
[15] It is important to emphasize that simply matching village names would yield spurious matches, given common village names used across different municipalities. Thus, we verified that departments, municipalities and villages had matching names (and did so by visual inspection to account for discrepancies in spelling or accents).
[16] In a sample of 816 experimental villages, we coded a dummy variable indicating whether the village had at least one voting center in the 2013 general election. We regressed it on dummy variables indicating villages in CCT1 and CCT2, and tested the null hypothesis that coefficients on CCT1 and CCT2 were jointly equal to zero. We were unable to reject the null, given a *p*-value of 0.17.

center. In the main estimation sample, this would introduce measurement error in the dummy variables indicating treatment assignment, since sparsely-populated villages do not necessarily share the treatment status of villages in the main sample.

To assess the empirical relevance of this critique, we report results in a restricted estimation sample. It consists of 383 experimental villages—a subsample of the 677—that are circumscribed by villages that also have at least one voting center.[17] In the restricted sample, therefore, each village's voting center only includes registered voters that that village. The restricted sample is 47% of the original sample of 816, and the proportion is similar across treatment arms.[18]

B. Dependent and independent variables

As dependent variables, we calculate the village-level turnout in the 2013 elections—aggregating up from center-level vote tallies—as the percent of registered voters who cast a valid vote for any party. We further calculate village-level vote shares for the incumbent National Party, the Liberal Party, LIBRE, and PAC. As with turnout, we calculate vote share as a percent of registered voters.

The number of registered voters in a village is a good proxy for the population of voting-age adults on the election date. This is mechanically true because individuals 18 and older with a national identify card are automatically registered to vote in the voting center nearest their residence.[19] To empirically validate this, we estimated the number of voting-age adults per village in 2013. We first calculated village-by-age totals using 2001 census microdata. We then projected 2013 totals, accounting for mortality with a life table (United Nations, 2013) and assuming no inter-village migration. In the main estimation sample, the correlation coefficient between voter registration and the imperfect population projection is 0.95. We further consider this issue in section 5.

[17] We ignore borders of neighboring villages when they are in different municipalities, since voting sectors do not cross municipal borders (República de Honduras, 2009).

[18] As before, in a sample of 816 experimental villages, we coded a dummy variable indicating whether the village is also in the restricted estimation sample. We regressed it on dummy variables indicating villages in CCT1 and CCT2, and tested the null hypothesis that coefficients on CCT1 and CCT2 were jointly equal to zero. We fail to reject the null, given a p-value of 0.42.

[19] The TSE is legally required to assemble a census of eligible voters using current data provided by the civil registry (the *Registro Nacional de Personas*). Before each election, the TSE publishes a website containing the preliminary census and voters may look up their assigned voting center (and correct if needed).

We construct two groups of independent variables. First, we scraped data from the previous presidential election in 2009—carrying out a similar hand-matching of village codes—and calculated village-level vote shares for the Liberal and National parties (noting that LIBRE and PAC did not yet exist in 2009).[20] Due to limitations in TSE data reporting on the 2009 website, the number of registered voters is not available, and so we calculate vote shares as a percent of total valid votes. This does not diminish the variables' utility in assessing baseline balance.

Second, we used microdata from the 2001 census—the only population data available before the 2013 election—to describe village-level demographics and socioeconomic status among voting-age adults (see Table 2). The variables include voters' gender, age, ethnicity, schooling, as well as attributes of dwellings that proxy income and wealth.

C. Estimation

Given randomized assignment, we estimate the average treatment effect on 2013 election outcomes with the regression

$$V_i = \alpha + \beta_1 CCT1_i + \beta_2 CCT2_i + \varepsilon_i, \qquad (1)$$

where V_i is the voting outcome of village i, and the dummy independent variables indicate villages in CCT1 and CCT2, relative to CCT3. Standard errors are adjusted for arbitrary forms of heteroscedasticity, but clustering is unnecessary since the unit of observation is the same as the unit of random assignment. To improve precision, we report two additional specifications. The first includes controls for Liberal and National vote shares in the 2009 elections, while the second further includes the census variables described in Table 2.

D. Baseline balance

Table 2 reports means of the independent variables in the main estimation sample. Across the three treatment arms—CCT1, CCT2, and CCT3—the average election outcomes in 2009 were very similar, as were the demographic and socioeconomic variables in the population of voting-age adults in 2001. Beyond the inspection of means, we regressed each baseline variable on CCT1 and CCT2, and tested the null hypothesis that coefficients were jointly equal to zero. None were significantly different from zero at conventional levels. For each variable, we also report two *p*-values from Kolmogorov-Smirnov tests of the equality of distributions between CCT3 and

[20] http://consultas.tse.hn:1177/

the other treatment arms. As above, we cannot reject equality of the distributions at conventional significance levels. We repeated these analyses in the restricted sample of villages described above (see Appendix Table A1). The table shows similarly good balance across treatment arms.

4. Implementation of conditional cash transfers

A. Programmatic targeting of villages and households

In Latin America, 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 et al., 2013). In contrast, programmatic redistribution—as exemplified by conditional cash transfer programs—is shaped by transparent and objective rules, and its receipt is not conditioned on political support (Stokes et al., 2013). During the 2013 campaign, opposition candidates claimed that transfers were contingent on political support (though each promised to keep the program if elected).[21]

We can empirically assess whether Bono 10,000 targeted villages and households according to programmatic or political criteria. The 2011-2012 Demographic and Health Survey (DHS) was collected between September 2011 and July 2013, overlapping with the 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).

Table 3 confirms that household participation is highly correlated with programmatic criteria. An increase of 21 percentage points in the extreme poverty rate of a household's village—about one standard deviation—increases the probability of participation by 5 percentage points (or 31%).[22] Rural households are 10 percentage points (or 63%) more likely to participate. Income and wealth proxies are correlated with participation, but only when they were used in the proxy

[21] During the campaign, the LIBRE candidate 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 called for the continuation of a "depoliticized" Bono 10,000 (La Tribuna, 2013, Oct. 23).

[22] Village-level poverty rates are based on an official poverty map; they were utilized by PRAF personnel to select villages during the roll-out of Bono 10,000. We merged poverty rates to the DHS survey using the latitude and longitude of DHS census segments—the primary sampling unit—in which households are located. Because of privacy concerns, the coordinates were 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 inside the buffer. We followed a similar procedure for municipal-level voting outcomes in 2009.

means test. For example, households with fewer assets are more likely to participate, but the schooling of the household head is not associated. Lastly, participation is strongly associated with the presence of children in the household. We would expect the relationship between household structure and participation to be even stronger when children reside in villages targeted by PRAF. And that is the case: splitting the household sample at the median village poverty rate shows stronger correlations when households are in poorer villages.

PRAF generally adhered to programmatic criteria in targeting villages and households. It may yet be the case that villages were favored because they included core supporters of the National Party or because they were closely contested in a prior elections. To test this, we also linked households to the municipal-level vote share of the governing National Party in the 2009 presidential election, as well as the absolute difference between this share (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). Coefficients on both variables are small and not statistically distinguishable from zero.

B. The amount and timing of transfers

Treated households were supposed 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 show variation in the amount and timing of the payments across CCT1, CCT2, and CCT3.

Figure 1 describes the evolution of transfers between the date of randomization (September 9, 2011) and the presidential election (November 23, 2013). We used administrative data on all transfers made to households residing in the main estimation sample of villages. For each village, we calculated the daily cumulative sums of payments to any household, and divided each sum by the number of adults in the village who were registered to vote in the 2013 elections. The lines in Figure 1 indicate daily means—taken within the three treatment arms—of village-level cumulative transfers per eligible voter.

In CCT1, transfers increased just as baseline surveys were completed in June 2012, and also rose sharply in the days before prior to endline surveys in March 2013. In CCT2, no transfers were made until the endline surveys were completed in June 2013, a constraint imposed by researchers concerned about the fidelity of the control group (Benedetti et al., 2016). After the surveys were finished, transfers sharply increased before the election. In fact, PRAF skipped most of the small payments and proceeded directly to larger ones. In CCT2, we calculated that only 12% of payees received the small unconditional transfer, compared with 89% of payees in CCT1 and 92% in CCT3. In contrast to CCT1 and CCT2, the amount and timing of transfers in CCT3 was solely influenced by PRAF.

By the election date, the village-level transfer per eligible voter was 1538 Lps. (about $77) higher, on average, in CCT1 than CCT3 (see Figure 1 and Table 4, panel A). Average transfers per voter in CCT2 were slightly lower than CCT3 (155 Lps.). Similar results are evident in the restricted estimation sample (Table 4, panel B). However, there is an important caveat. Figure 1 cannot reveal whether transfers increased on the intensive or extensive margins. PRAF could have made larger transfers to a stable number of participating households in each village (i.e., the intensive margin), or simply offered small payments to an ever-larger proportion of each village's households (i.e., the extensive margin).

To examine this, Figure 2 reports daily means of village-level cumulative transfers *per adult payee,* while Figure 3 reports daily means of the village-level percentage of registered voters that *ever received any payment.*[23] A joint inspection of the figures suggests the CCT1's jump in payments before the endline surveys was attributable to larger payments to a stable percentage of participating households. It further reveals that CCT3's post-endline jump is explained by both, since newly-registered payees received large initial payments (but not small registration payments). Finally, Figures 2 and 3 suggest that CCT1 had larger transfers per voter on the election date primarily because of higher mean participation rates in CCT1 villages (rather than larger cumulative transfers to payees). The lower mean participation rate in CCT3 villages—visible in Figure 3—is driven by the fact that PRAF made zero payments to 34% of the CCT3 villages. This is consistent with PRAF's intent to focus on higher-poverty villages: the extreme poverty rate in zero-payment villages is 42%, versus 59% in others.

C. Peak and end transfers

[23] Note that this is not necessarily the percentage of registered voters that resided in a household with a designated payee, since it does not include spouses or relatives of the payee. The administrative payments data do not include the structure of participating households. However, the baseline sample analyzed by Benedetti et al. (2016) indicate that participating households had approximately 2.7 individuals that were at least 18 years old.

For the moment, let us assume that voters' behavior in the polling booth is responsive to a sequence of payments received in the months preceding the election. A literature in psychology and behavioral economics—reviewed in section 5—suggests that voters will make predictable errors in their retrospective evaluations of these sequences (Langer et al., 2005; Yu et al., 2008; Huber et al., 2008; Healy and Lenz, 2014). Specifically, their evaluations may use peak payments, end payments, or peak-end midpoints as heuristics for retrospective evaluation.

The figures suggested that households in some villages—especially those in CCT1 and CCT2—received relatively larger transfers that were, in some cases, closer to election day. To further describe these patterns, we calculated the average peak payment among registered voters in each village (by summing the largest of all payments made to each payee and dividing by the number of registered voters in the village). We did the same for end payments and for the peak-end midpoint.[24] For all three variables, villages in CCT1 and CCT2 have uniformly larger values, on average, than CCT3, in both estimation samples (see Table 4). We can reject the null that coefficients on CCT1 and CCT2 are equal for peak payments and the peak-end midpoint, but not for end payments.

5. Impact on voting outcomes

A. Main results

Table 5 reports regression estimates for voter turnout and the incumbent party vote share. In addition to unadjusted mean differences based on equation (1), we report specifications that control for 2009 vote shares and census variables. In the main sample, the point estimates in all specifications are consistent with positive effects on both dependent variables (see panel A). However, the estimates are more precise when covariates are included. These coefficients show that turnout in CCT1 and CCT2 villages is, respectively, 2.7 and 2.5 percentage points higher than CCT3.[25] Both coefficients are statistically significant at 5%, and they are not statistically

[24] When a single payment is made or when payments are in equal amounts, then the peak-end midpoint has the same value.

[25] One village in CCT3—present in both estimation samples—had zero valid votes (and hence zero values for the dependent variables). Turnout ranged from 25.9% to 91.5% in other villages of the main sample. We verified that the observation was not an error of data collection and was part of the official tallies reported on the TSE website. When the observation is excluded, the coefficients from similarly-specified models are only slightly attenuated. In the main sample, the turnout effects in CCT1 and CCT2 are, respectively, 2.5 and 2.3 percentage points. The effects on National Party share are 2.4 and 1.9 percentage points. All are statistically distinguishable from zero at 1% or 5%.

different from one another. This is about a 4% increase over CCT3's turnout of 61% of registered voters.

The incumbent party's vote share—measured as a percent of registered voters—increased by 2.5 and 1.9 percentage points in CCT1 and CCT2. Again, both coefficients are significant at 5% and they are statistically indistinguishable from one another. They represent increases of 7 to 9% over CCT3's incumbent vote share of 27%. The similarity of point estimates on turnout and vote share is suggestive that transfers functioned by mobilizing the core supporters of the National Party, rather than by persuading supporters of other parties to switch.

Panel B repeats these analyses in the restricted sample of villages, which eliminates the threat of measurement error in the treatment variables, as described in section 3. Given the smaller sample, standard errors are larger. However, the pattern of point estimates and statistical significance reinforces the conclusion that turnout and incumbent party vote share in CCT1 and CCT2 are larger—and by a similar magnitude—than CCT3. In specifications with all control variables, all coefficients are larger than 2.5 for both turnout and for incumbent party vote share.

Table 6 reports the same estimates for the other parties with substantial vote shares: the Liberal Party, LIBRE, and PAC. In specifications with controls, the coefficients vary between -1 and 1 and none are statistically significant at 5% (see panel A). There is some evidence that villages in CCT2 were more likely to vote for PAC, relative to CCT3 and CCT1. However, the point estimate is smaller and no longer significant at 10% in the restricted sample.

B. Why do voters respond to transfers?

A classic literature in political economy posits that voters' decisions are affected by their economic well-being just before an election.[26] So-called pocketbook voting implies that gullible voters would—without forethought—vote for a party that pursues economic expansion or distributes cash just before elections (Stigler, 1973; Manacorda et al., 2011). Recent models of retrospective voting are more nuanced, assuming that "citizens consider information on past government performance in order to make forward-looking decisions" (Healy and Malhotra, 2013, p. 286). For example, rational voters with imperfect information may learn about the competence of an incumbent through the retrospective examination of her performance (Rogoff,

[26] See Manacorda et al. (2011) and the citations therein, including Kramer (1971) and Hibbs (1982).

1990; Fearon, 1999; Persson and Tabellini, 2002; Duch and Stevenson, 2008).[27] Before the election, higher economic activity signals a high-ability incumbent.[28] In related models, rational voters may be unaware of politicians' redistributive preferences for individuals or groups, and retrospection about payments allows voters to update their beliefs (Drazen and Eslava, 2006, 2010; Manacorda et al., 2011).

A second explanation for retrospective voting—rooted in behavioral economics—is that voters are intrinsically reciprocal (Finan and Schechter, 2012; Manacorda et al, 2011; Lawson and Greene, 2014). Individuals who receive transfers may be inclined to vote for incumbents "because they experience pleasure in increasing the material payoffs of the politician who has helped them" (Finan and Schechter, p. 864).[29] Reciprocal behavior could also be self-interested, or instrumental, if parties and voters interact in a repeated game and voters wish to "sustain a profitable long-term relationship," perhaps by ensuring that programmatic criteria for eligibility do not change (Sobel, 2005, p. 392). The two explanations are not mutually exclusive, since intrinsic reciprocity may enhance cooperation in a repeated game between parties and instrumentally-reciprocal voters (Finan and Schechter, 2012; Sobel, 2005).

We do not conduct sharp tests of theoretical explanations for retrospective voting. Rational voters in Honduras may have possessed imperfect information about the randomized assignment of villages and/or the programmatic targeting rules applied to villages and households (for a related example, see Manacorda et al., 2011).[30] Thus, they might have used their personal transfers—or rather, the remembered amount of such transfers—to update their views on the

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[27] The subsequent discussion and citations draw on Healy and Malhotra (2013). They also discuss models of retrospective voting as a sanction, in which rational voters attempt to the reduce moral hazard of politicians (and increase their accountability) by re-electing high-performing politicians (Barro, 1973; Ferejohn, 1986).
[28] See the review of Drazen (2008) and the citations therein, including Persson and Tabellini (1990) and Lohmann (1998).
[29] In models of clientelist politics, party brokers or middlemen offer favors to voters in exchange for votes as long as the vote can be explicitly verified by somehow subverting ballot secrecy (Stokes, 2005). The persistence of clientelism despite ballot secrecy has been explained as the result of intrinsically-reciprocal voters (Lawson and Greene, 2014; Finan and Schecter, 2014). As evidence of this, Finan and Schecter (2014) find that party middlemen in Paraguay target favors to more intrinsically-reciprocal voters.
[30] Instead suppose that rational voters had full information about the criteria for the allocation of transfers—including the random assignment of villages to treatment arms—and used this information to update their beliefs about the incumbent party's competence or redistributive preferences. In this case, voters in the three groups should have responded similarly, contrary to our findings. Given similar findings in Uruguay, Manacorda et al. (2011) argue that voters are rational but poorly informed; thus, the treatment group uses their personal receipt of transfers as a signal of redistributive preferences. In the present experiment, we cannot judge how much information voters possessed, but it seems plausible that imperfect information led some voters to use the remembered amount of transfers to update beliefs differently.

competence or redistributive preferences of the incumbent party (and then voted accordingly). It is also possible that Hondurans reciprocated the remembered amount of transfers by turning out on election day.

However, both models assume that voters can accurately recall a sequence of transfers. An obvious rule for doing so is the sum of transfers received by voters. In the present experiment, the cumulative transfers per registered voter in CCT2 villages were substantially lower than CCT1 and slightly lower than CCT3 (see Table 4). And yet, the same pattern was not reflected in voting outcomes: CCT1 and CCT2 had similar effects on voting outcomes, relative to CCT3 (see Table 5). The next section considers evidence that addresses this puzzle.

C. Why does timing affect voter retrospection?

Evaluation of hedonic episodes

A large literature in psychology and behavioral economics has shown that individuals have difficulty in accurately remembering the utility derived from a sequence of hedonic episodes (for reviews, see Kahneman, Wakker, and Sarin, 1997; Healy and Lenz, 2014). When evaluating sequences, subjects tend to overweight the peak—the moment of highest pleasure or worst discomfort—as well as the end or final hedonic episode.

For example, Fredrickson and Kahneman (1993) exposed subjects to a sequence of short film clips with pleasant or aversive content. The subjects rated the instant utility of each clip as they were exposed to it, and also provided final evaluations of remembered utility at the end of the sequence. The authors found that final evaluations were most influenced by the peak and end episodes, and that other episodes had no influence at all. Evidence of a "peak-end rule" for retrospective evaluation has been found in other settings, such as painful episodes (e.g., Redelmeier and Kahneman, 1996). In subsequent studies, the end part of the rule has been more robustly observed, perhaps suggesting that peak episodes are not as salient in varied contexts (e.g., Ariely, 1998; see the discussion in Healy and Lenz, 2014).

Evaluation of economic outcomes

In the present experiment, it is more pertinent to ask whether subjects can accurately evaluate sequences of monetary payments. It is plausible that individuals would make fewer errors in these settings. Indeed, the rule for evaluation of a payment sequence—the sum—is more obvious and universally-shared than rules for evaluating sequences of hedonic episodes (Langer et al., 2005). Nonetheless, four experiments suggest that accurate retrospection is challenging, especially in realistic circumstances when individuals cannot calculate a running sum of payments.

Langer et al. (2005) conducted an experiment in which participants viewed about 10 payments in one sequence, and a similar number of payments in another. Participants were asked to choose a preferred sequence (and had an incentive to choose correctly, since they received that amount). They showed no evidence of peak-end bias in their choice, and the authors inferred that the participants—all business students—simply calculated a running sum of payments, precluding the need for retrospection. In another experiment, however, the participants were distracted by a strenuous mental task, making aggregation more difficult. In this case, the participants were less likely to choose the correct sequence, and their choices were swayed by the end of the sequence. There was some evidence that peaks influenced choices, although the experiment did not include substantial peak variation.

A second experiment included variation in both peak and end payments. Participants played a slot machine for two sessions of 50 payouts each (Yu et al., 2008). Although participants were not distracted by another task, they evaluated longer sequences than in Langer et al. (2005), suggesting that running sums might not be easily calculated. In retrospective evaluations of the sequences, participants preferred those with higher peak-end midpoints despite lower total payouts.

A third experiment assigned each participant an "allocator" that would offer 32 payments of tokens in successive rounds, later convertible to cash (Huber et al., 2012). The size of each payment was subject to chance, although some "types" of allocators had higher or lower average payouts across many rounds. The allocator's type was not told to participants, although they could infer it from the sequence of payments.

The participants were allowed to draw a new allocator (with a potentially higher mean payments) after 16 of 32 rounds. Not surprisingly, the average payment received over 16 rounds was positively associated with the decision to retain an allocator. However, a random subset of participants only learned of this possibility after round 12, while the others were informed before any payments were made. When participants were informed later, their decisions leaned more heavily on payments in rounds 13 to 16, suggestive of end bias in the retrospective evaluation of the allocator's average payment. Similar to Langer et al. (2005), the results showed that inattention during a sequence of payments led participants to inaccurately evaluate the sequence.

The prior experiments focused on sequences of payments to individuals, but not in an explicitly political context. A fourth experiment called upon participants to compare and rate the economic growth during the terms of hypothetical presidents (Healy and Lenz, 2014). Each participant viewed two bar charts that described four years of annual economic growth rates during a hypothetical presidency. Despite having access to all years of growth data, participants evinced a preference for stronger growth in the fourth year, regardless of cumulative growth over the entire term. This occurred in spite of voters' stated desire to weight each year more equally in their overall decision (Healy and Lenz, 2014). The results suggest that participants relied an end heuristic in making judgments, rather than engaging in the mathematically-challenging task of evaluating the entire period. To further assess this, the authors gave additional information to a random subset of participants, including a bar chart of cumulative growth rates. In this case, participants' behavior was better aligned with the voters' stated intention to weigh years more equally.

The experiments show that individuals perform badly in the retrospective evaluation of economic outcomes, even with incentives or preferences to do otherwise (Langer et al., 2005; Huber et al., 2012; Healy and Lenz, 2014). Instead of accurately calculating sums, the experiments suggest that individuals are unduly swayed by the final economic outcome in a sequence. Two experiments suggest that peak payments also influenced the choice of payment sequences (Langer et al., 2005; Yu et al., 2008). The experiments vary in their explanations for participants' reliance on peak-end heuristics. These include distraction by other tasks (Langer et al., 2005), inattention (Huber et al., 2012), and participants' inability to calculate a running sum. The latter may be due to a lengthy sequence (Yu et al., 2008) or the inability to perform a desired calculation even when presented with the relevant data (Healy and Lenz, 2014).

The Honduran context is different from the lab experiments, but not in ways that might improve retrospection. The field experiment occurred over nearly two years, and adults were distracted by varied obligations in a predominantly rural and high-poverty setting. They were likely inattentive to the calculation of an accurate running sum, at least until the election increased the salience of doing so. Finally, the formal schooling of Honduran adults in poor villages is extremely low (Table 2), and their ability to calculate a running sum—even in the absence of distraction or inattention—is presumably lower than university students in richer countries (Langer et al., 2008; Yu et al., 2008) or experimental subjects recruited through Amazon's Mechanical Turk (Huber et al., 2012; Healy and Lenz, 2014).

Table 4 compared peak and end payments in the three treatment arms. CCT1 and CCT2 had larger end transfers per registered voter, relative to CCT3; the same pattern existed for peak transfers and peak-end midpoints. However, the coefficients on CCT1 and CCT2 were very similar and statistically indistinguishable for end transfers (as were the results on turnout and incumbent vote share in Table 5). For both peak transfers and peak-end midpoints, CCT1 had larger coefficients than CCT2, and we rejected the null of equality. We interpret this as evidence that registered voters applied an end heuristic in the retrospective evaluation of transfers. The evidence for the application of peak or peak-end heuristics is less consistent with the voting outcomes (but more so than the evidence on cumulative transfers).

D. Alternate interpretations

This section examines alternate interpretations of the results in Table 5. The first is that survey exposure, rather than the transfers, might explain the similar voting outcomes in CCT1 and CCT2. The second is that effects on CCT2 villages were partly influenced by resources from other sources, assuming that politicians wished to compensate for delays in transfers. The third is that the transfers improved the health of registered voters, which may in turn have directly influenced voting behavior. A fourth interpretation is that the point estimates in Table 5 do not exclusively reflect the effect of transfers on the numerators of voting variables. Rather, they might also influenced the denominator, via an effect on voters' decisions to consult and correct errors in the voter rolls.

Survey exposure as a form of treatment

Households CCT1 and CCT2—unlike those in CCT3—participated in baseline and endline surveys (Benedetti et al., 2016). One might hypothesize that participating in the lengthy surveys—which included questions on household income and consumption—influenced voters' perceptions of the incumbent party's competence or redistributive preferences.[31] This, in turn, may have influenced voter behavior independently of the transfers, and could account for the similar point estimates.

However, all registered voters in CCT1 and CCT2 villages did not participate in the survey. A household sample was drawn in three steps. First, PRAF conducted a household census in all villages that would receive transfers. Second, PRAF applied a proxy means test with these variables and constructed a list of eligible households in the villages. Third, a survey firm—NORC at the University of Chicago—randomly drew 15 households from each village, regardless of village population.

Using the baseline survey data from Benedetti et al. (2016), we calculated that the average village in CCT1 and CCT2 had 42 voting-age adults residing in a fixed number of 15 surveyed households (an average of 7% of registered voters in the villages). A natural question is whether effects in CCT1 and CCT2 villages are moderated by the percent of voters exposed to the survey. NORC did not draw a household sample in CCT3, and so we cannot directly calculate the same percentage.

Instead, we control for Z—defined as the z-score of the number of registered voters in a village—and interact Z with CCT1 and CCT2. Given NORC's sampling methodology, the percent of voters exposed to the survey is inversely related to Z ($r = -0.57$ in the sample of CCT1 and CCT2 villages). If survey exposure positively affects voting outcomes, then we anticipate negative signs on the interaction terms.

In Table 7, the point estimates on CCT1 and CCT2—interpreted as effects at the sample mean of registered voters—are consistent with the estimates from Table 5. Only one interaction term is statistically significant at 5%, and its positive sign is consistent with a *larger* effect on National Party share in villages with more voters. A one standard deviation increase in Z increases the coefficient on CCT1 by 1.7 to 1.9 percentage points, depending on the estimation sample. The pattern of heterogeneity may be due to the correlation of Z with unobserved moderators. However, there is no evidence that survey exposure alone is responsible for the effects of transfers.

[31] The survey contained modules on consumption, income, education, and maternal and child health, among others (Bendetti et al., 2016). It was administered by non-government survey personnel affiliated with Esa Consultores, a Honduran partner of NORC at the University of Chicago.

Endogenous responses of politicians

Transfers to CCT2 were delayed because it was a control group in the experiment reported in Benedetti et al. (2016). One might hypothesize that the incumbent party was concerned about the electoral consequences of restricting transfers to 150 villages, and chose to compensate by targeting other resources to those villages. To assess this, we used the endline data from Benedetti et al. (2016). Households responded whether any member of the household had received a variety of government transfers in the 12 months prior to the survey (see Table 8). As expected, 80% of CCT1 households but only 4% of CCT2 households received a transfer from Bono 10,000. However, no more than 2% of households had received other types of government transfers.

The survey also asked whether anyone in the households had received generic categories of benefits (e.g., "food donation") in the last 12 months, without specifying that the benefit was provided by a public or private organization. The most common benefit was "school lunch," and households in CCT1 were 4.7 percentage points more likely to have received it. This is consistent with the magnitude of impacts on school enrollment (Benedetti et al., 2016). The incidence of other benefits is small and differences are not statistically significant.

The exception is "health or vaccination campaign," which was 5 percentage point more common in CCT2. One interpretation is that pre-existing vaccination programs were redirected from CCT1 villages, given the expectation that young children would receive vaccinations in health centers (a consequence of the imposed health conditions). Whatever the case, Benedetti et al. (2016) reported no differences in the vaccination rates of young children across CCT1 and CCT2, suggesting that CCT2 villages did not receive substantially different health resources. In summary, the clearest compensatory response of the government was to swiftly disburse Bono 10,000 payments after the conclusion of endline surveys.

Effects on adult health

Some evidence suggests that adult health has direct effects on voter turnout (Mattila et al., 2013). It is possible that Bono 10,000 affected the health of at least some voting-age adults. This could be due to health-related conditions imposed on pregnant and nursing mothers, spillovers to adult health as a result of healthier children, or health-related expenditures facilitated by the transfers.

Benedetti et al. (2016) report no full-sample effects on the use of health services by mothers—the only adults subjected to health conditions—and no effects on the health and nutritional outcomes of children under 4, including child hemoglobin, parent-reported child illness, vaccination rates, and anthropometric variables. The authors do show that household consumption increased by 9% in CCT1 relative to CCT2, for both food and non-food consumption. This gap likely narrowed before the election, but not entirely, given catch-up transfers to CCT2 (see Table 4). If increased household consumption directly improved adult health and then voter turnout, we might have expected larger turnout effects in CCT1 relative to CCT2. However, this was not the case (see Table 5).

Effects on voter registration

We noted in section 3 that the number of registered voters in a village is a good proxy for the voting-age population. That is because the TSE—the Honduran election agency—constructs voter rolls prior to each election using data from the civil registry on voting-age adults with a national identify card (República de Honduras, 2009). Even so, it is possible that the treatment affects voter registration. The voter rolls are made available on a website before the election, and individuals may consult their assigned voting center by entering a national identity number. If individuals are not registered—or not assigned to a voting center near their home—then individuals may request a correction up to 90 days before the election.

We have assumed that transfers only increased the numerators of the dependent variables, which are all calculated as a percent of registered voters. Instead let us suppose that transfers have a positive effect on the denominator by motivating individuals to consult and correct errors in the voter rolls, in anticipation of voting on election day. As a consequence, the estimates in Table 5 could understate the magnitude of effects on the mobilization of voters.

As a simple test, Table 9 reports regressions with Z—the z-score of the number of registered voters—as a dependent variable.[32] In all specifications, the coefficients on CCT1 and CCT2 are less than 10% of a standard deviation. When all controls are included the point estimates are less

[32] The results are similar if we use the natural log of registered voters as the dependent variable.

than 3% of a standard deviation. We conclude that the estimates in Table 5 are consistent with a causal effect of transfers on the mobilization of previously-registered voters.

E. Complementary evidence from an earlier experiment

We have shown that the amount and timing of transfers had a causal impact on voters' behavior in the 2013 election. We argued that the size of end payments was an especially plausible explanation for the similar impacts on CCT1 and CCT2, relative to CCT3. The findings imply that effects in similar contexts should be smaller when the size of the final payment is smaller, as in the PRAF-II experiment conducted before the 2001 elections.

The PRAF-II treatment

PRAF-II transfers were smaller than Bono 10,000 (Glewwe and Olinto, 2004; Morris et al., 2004; Galiani and McEwan, 2013; Moore, 2008). This was by design, since the education and health transfers were calculated to compensate households for the costs of complying with education and health conditions, but not to substantially increase income and consumption. 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).

Households received up to three per-child transfers of 800 Lps. for each child between ages 6 and 12 who enrolled in grades 1 to 4. Households were also eligible for up to two per-child transfers of 644 Lps. for each child under 3 years of age and pregnant or nursing mothers who attended health centers. Given household structure, Galiani and McEwan (2013) estimated that the average household was eligible for an annual transfer of 1127 Lps. per year, to be made in two installments of 564 Lempiras. Inflating to 2013 prices, therefore, the average household would have received an end payment of 1245 Lps. This is much smaller than the "catch-up" payments made to CCT2 households in 2013 (Figure 2).

The PRAF-II experiment and data

In the PRAF-II experiment, 40 of 70 municipalities—rather than villages, as in Bono 10,000—were randomly assigned to receive transfers, while 30 received no treatment before the

2001 presidential elections.[33] The first transfers to the treatment group reportedly occurred in November 2000, with the second round in May and June 2001 (Morris et al., 2004). A third set of transfers—kicking off the second year of treatment—occurred just before the presidential elections on November 25, 2001.

We obtained 2001 election data, already aggregated to the municipal level, from a TSE website[34] and merged it to variables indicating the treatment group and experimental strata (Galiani and McEwan, 2013). We calculated turnout and vote shares for the incumbent Liberal Party and the National Party (noting that LIBRE and PAC did not emerge until after the 2009 *coup d'etat*).

As control variables, we obtained municipal-level vote shares from the 1997 presidential elections (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. Finally, we use the same census controls described in Table 2, noting that the July 2001 census preceded the November elections. Appendix Table A3 reports municipal-level means for the CCT treatment group and the control group, revealing good balance for 1997 election outcomes as well as census variables.

Effects on 2001 elections

Table 10 reports the same regression specifications as Table 5. Given the unit of random assignment, the standard errors are only adjusted for arbitrary forms of heteroscedasticity. The point estimates are negative, regardless of the specification.[35] The estimates in this experiment are less precise than earlier ones because of sample size. However, the 95% confidence intervals on turnout and the incumbent Liberal vote share allow us to rule out effects larger than 1.9 percentage points. Viewed alongside results from Bono 10,000, the results suggest that voters' responses are muted when end payments are smaller.

However, there are two caveats. First, we do not have administrative payments data and cannot verify the sequence of payments actually received by voters before the 2001 election.

[33] The 70 municipalities (of 298) were selected for inclusion because they had the highest rates of child stunting, a proxy for municipal well-being. The randomization was conducted within 5 equally-sized strata defined by the stunting rate.

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

[35] 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.

Second, the context of the 2001 election, ultimately lost by the incumbent Liberal party, was unique (Taylor-Robinson, 2003). In late 1998, Hurricane Mitch killed thousands and destroyed large amounts of productive infrastructure throughout the country. Voters tend to punish incumbent parties for weather events beyond their control (Cole, Healy, and Werker, 2012). In this context, it is possible that voters responded even more favorably to PRAF-II transfers than in the absence of Mitch, perhaps because the natural disaster increased the salience of poverty-relief as a signal of politician competence. The opposite might be true if voters perceived the modest size of PRAF-II transfers—against the backdrop of Mitch's devastation—as evidence of insufficient commitment to redistribution.

6. Conclusions

This paper analyzed a Honduran cash transfer experiment with three treatment arms: CCT1, CCT2, and CCT3. Villages in CCT2 and CCT3 received similar cumulative transfer per registered voters, while villages in CCT1 received substantially more. Despite this, voter turnout and the incumbent party's vote share in both CCT1 and CCT2 were higher, on average, than CCT3, and by a similar magnitude.

By way of explanation, we noted that villages in both CCT1 and CCT2 had larger end payments per registered voter than CCT3, suggesting that voters used an end heuristic when evaluating the sum of transfers received by election day. The findings are consistent with lab experiments that call upon subjects to evaluate sequences of hedonic episodes (Fredrickson and Kahneman, 1993; Redelmeier and Kahneman, 1996; Ariely, 1998) and economic outcomes (Langer et al., 2005; Yu et al., 2008; Huber et al., 2012; Healy and Lenz, 2014). Individuals predictably over-weight the final economic outcome when evaluating a sequence, even when they have an incentive or preference to calculate an accurate sum. In addition to lab experiments, the results are consistent with non-experimental findings that voters tend to disproportionately reward politicians for economic outcomes in election years (Kramer, 1971; Nordhaus, 1975; Fair, 1978; Markus, 1988).

The experiment illustrates that CCT implementation does not necessarily conform to drawing-board guidelines. Variation in the amount and timing of payments was guided by a payment schedule, but it did not conform perfectly (in part, because of researcher-imposed constraints on the distribution of payments to CCT2). Fiszbein and Schady (2009) reviewed 23 transfer policies in 16 Latin American countries. Twenty-one policies specified either monthly or bimonthly payments, but the literature does not provide substantial evidence on whether payment schedules were followed. The most telling evidence is from Mexico's Progresa, a blueprint for subsequent Latin American CCTs. Administrative data showed that some payments were delayed and followed by larger-than-expected catch-up payments (Skoufias, 2005).

There are two important implications. First, politicians are unlikely to be indifferent to the details of CCT implementation, including the timing of payments. Even if they are not directly responsible for deviations from nominal payment schedules, our paper shows that they have weak electoral incentives to enforce a regular sequence of smaller payments. Second, the details of actual payment sequences merit further description in impact evaluations of CCTs, especially when hypothesized impacts—on voter behavior or other variables—plausibly depend on individual retrospection about the sequences. Without such data, it cannot be discerned whether "larger" cash transfers (i.e., larger cumulative amounts) have greater effects on outcomes, since voters' perception of "large" could depend on the use of heuristics.

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Table 1: Sample size in the main and restricted estimation samples

	All treatment arms	CCT1	CCT2	CCT3
Total number of villages	816	150	150	516
<u>Main estimation sample</u>				
Villages with ≥1 voting center:				
Number	677	120	129	428
% of total	83%	80%	86%	83%
<u>Restricted estimation sample</u>				
Villages with ≥1 voting center, *and* circumscribed by villages with ≥1 voting centers:				
Number	383	76	69	238
% of total	47%	51%	46%	46%

Table 2: Baseline characteristics of villages in main estimation sample

	Mean (standard deviation)			p-value (jointly equal)	p-value (K-S)
	CCT1	CCT2	CCT3		
Panel A: Village-level vote share in 2009 Presidential elections					
National Party vote share	56.99	57.26	57.28	0.99	0.29/0.80
	(15.98)	(15.11)	(15.13)		
Liberal Party vote share	39.46	39.30	39.72	0.96	0.81/0.95
	(16.01)	(14.91)	(14.96)		
Panel B: Village-level variables from 2001 census; individuals 18 and older					
% female	0.482	0.486	0.488	0.23	0.60/0.69
	(0.03)	(0.03)	(0.04)		
Mean age	38.10	37.97	38.21	0.45	0.46/0.57
	(2.04)	(1.88)	(2.08)		
% Lenca (indigenous)	0.055	0.056	0.066	0.66	0.34/35
	(0.14)	(0.15)	(0.16)		
Mean years of schooling	2.931	3.088	3.048	0.20	0.14/0.74
	(0.74)	(0.71)	(0.68)		
% literate	0.653	0.670	0.667	0.34	0.35/0.97
	(0.11)	(0.10)	(0.10)		
% who worked week before census	0.498	0.499	0.497	0.97	0.06/0.98
	(0.08)	(0.09)	(0.08)		
% with dirt floor in dwelling	0.542	0.507	0.525	0.38	0.73/0.34
	(0.19)	(0.21)	(0.20)		
% with piped water in dwelling	0.720	0.697	0.696	0.62	0.75/0.34
	(0.24)	(0.26)	(0.24)		
% with electric light in dwelling	0.199	0.233	0.212	0.52	0.49/0.47
	(0.23)	(0.25)	(0.23)		
% with sewer/septic in dwelling	0.321	0.344	0.338	0.69	0.90/0.99
	(0.22)	(0.23)	(0.22)		
N of villages	120	129	428		

Notes: See the text for a description of the null hypotheses corresponding to the p-values.

Table 3: Correlates of household participation in Bono 10,000

	Dependent variable: Household had received any transfer at time of DHS survey		
	All households	Households below median village poverty	Households above median village poverty
Extreme poverty rate of household's village	0.252***	0.102**	0.421***
	(0.024)	(0.048)	(0.083)
Dwelling located in rural census segment	0.099***	0.034***	0.197***
	(0.010)	(0.011)	(0.016)
Dwelling has dirt floor	0.036***	0.014	0.024*
	(0.010)	(0.013)	(0.012)
Dwelling connected to sewer or septic	-0.019**	-0.015*	-0.022*
	(0.008)	(0.009)	(0.013)
Index of 15 household assets (z-score)	-0.032***	-0.016***	-0.029***
	(0.004)	(0.005)	(0.007)
Years of schooling (household head)	0.001**	-0.001*	0.002**
	(0.001)	(0.001)	(0.001)
Number of household members	0.020***	0.008***	0.024***
	(0.002)	(0.002)	(0.003)
≥1 child between 0 and 5 years old	0.012**	0.010*	0.022**
	(0.006)	(0.006)	(0.009)
≥1 child between 6 and 18 years old	0.079***	0.031***	0.148***
	(0.007)	(0.007)	(0.011)
Anyone pregnant in household	-0.019*	-0.013	-0.025
	(0.011)	(0.010)	(0.017)
National Party vote share in 2009	0.000	-0.001	0.001
(municipal-level)	(0.002)	(0.001)	(0.003)
Absolute deviation from 50 of National	0.001	0.001	-0.003
Party vote share (municipal-level)	(0.002)	(0.002)	(0.003)
R^2	0.18	0.05	0.19
N	20,446	10,230	10,216

Source: 2011-2012 Demographic and Health Survey.
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. All regressions include a constant and dummy variables indicating year-by-month cells in which the survey was completed.

Table 4: Transfers per registered voter on day of presidential election (1 USD~20 Lps.)

	Total transfer per registered voter	Peak transfer per registered voter	End transfer per registered voter	Peak-end midpoint per registered voter
Panel A: Main sample of villages				
CCT1	1,538***	865***	369***	617***
	(153)	(79)	(37)	(57)
CCT2	-155*	323***	389***	356***
	(84)	(42)	(33)	(37)
Constant	1,233***	494***	308***	401***
	(69)	(26)	(16)	(21)
R^2	0.17	0.24	0.23	0.23
N of villages	677	677	677	677
p-value (CCT1=CCT2)	<0.01	<0.01	0.65	<0.01
Panel B: Restricted sample of villages				
CCT1	1,561***	885***	371***	628***
	(201)	(108)	(49)	(77)
CCT2	-332***	291***	385***	338***
	(117)	(57)	(44)	(50)
Constant	1,484***	578***	354***	466***
	(100)	(37)	(23)	(30)
R^2	0.18	0.23	0.21	0.22
N of villages	383	383	383	383
p-value (CCT1=CCT2)	<0.01	<0.01	0.80	<0.01

Notes: *** indicates statistical significance at 1%, ** at 5%, and * at 10%. Robust standard errors are in parentheses. See the text for definitions of the main and restricted samples.

Table 5: Effects on turnout and incumbent party vote share in 2013 presidential election

	Dependent variable: Turnout			Dependent variable: National Party share		
Panel A: Main sample of villages						
CCT1	2.64**	3.09***	2.68**	2.68**	3.24***	2.51***
	(1.17)	(1.16)	(1.15)	(1.20)	(0.97)	(0.94)
CCT2	1.90	2.29**	2.47**	1.29	1.66*	1.94**
	(1.23)	(1.17)	(1.07)	(1.07)	(0.89)	(0.80)
Adjusted R^2	0.01	0.07	0.20	0.01	0.29	0.44
N of villages	677	677	677	677	677	677
CCT3 mean	61.1	61.1	61.1	27.3	27.3	27.3
p-value (CCT1=CCT2)	0.62	0.58	0.88	0.32	0.17	0.60
Panel B: Restricted sample of villages						
CCT1	2.56*	3.19**	2.54*	1.81	3.21***	2.53**
	(1.37)	(1.34)	(1.34)	(1.43)	(1.12)	(1.06)
CCT2	2.74*	3.13**	2.85**	2.82*	2.93**	2.98**
	(1.58)	(1.53)	(1.38)	(1.47)	(1.23)	(1.21)
Adjusted R^2	0.01	0.08	0.23	0.01	0.32	0.45
N of villages	383	383	383	383	383	383
CCT3 mean	62.3	62.3	62.3	28.6	28.6	28.6
p-value (CCT1=CCT2)	0.92	0.97	0.85	0.56	0.84	0.74
Controls:						
2009 vote shares	N	Y	Y	N	Y	Y
Census variables	N	N	Y	N	N	Y

Note: *** indicates statistical significance at 1%, ** at 5%, and * at 10%. Robust standard errors are in parentheses. All regressions include a constant. See the text for definitions of the main and restricted samples.

Table 6: Effects on other parties' vote shares in 2013 presidential election

	Dependent variable: Liberal Party share			Dependent variable: LIBRE share			Dependent variable: PAC share		
Panel A: Main sample of villages									
CCT1	-0.93	-0.68	-0.33	1.34	1.09	0.88	-0.52	-0.62*	-0.47
	(0.98)	(0.84)	(0.80)	(1.26)	(1.18)	(1.15)	(0.34)	(0.34)	(0.31)
CCT2	-0.60	-0.34	-0.13	0.25	0.09	-0.06	0.94**	0.86*	0.72*
	(0.87)	(0.82)	(0.72)	(1.13)	(1.09)	(1.03)	(0.48)	(0.47)	(0.43)
Adjusted R^2	<0.01	0.21	0.35	<0.01	0.10	0.17	<0.01	0.02	0.18
N of villages	677	677	677	677	677	677	677	677	677
CCT3 mean	11.7	11.7	11.7	17.9	17.9	17.9	3.9	3.9	3.9
p-value (CCT1=CCT2)	0.77	0.73	0.83	0.48	0.49	0.49	0.00	0.00	0.01
Panel B: Restricted sample of villages									
CCT1	-0.70	-0.60	-0.66	1.67	0.95	1.02	-0.36	-0.50	-0.48
	(1.22)	(1.04)	(1.03)	(1.62)	(1.54)	(1.52)	(0.42)	(0.42)	(0.38)
CCT2	-1.21	-0.87	-0.76	0.45	0.44	0.00	0.68	0.62	0.62
	(1.21)	(1.19)	(1.10)	(1.59)	(1.52)	(1.42)	(0.55)	(0.55)	(0.56)
Adjusted R^2	<0.01	0.19	0.28	<0.01	0.10	0.17	<0.01	0.02	0.13
N of villages	383	383	383	383	383	383	383	383	383
CCT3 mean	11.3	11.3	11.3	18.5	18.5	18.5	3.6	3.6	3.6
p-value (CCT1=CCT2)	0.73	0.85	0.94	0.55	0.79	0.59	0.08	0.06	0.06
Controls:									
2009 vote shares	N	Y	Y	N	Y	Y	N	Y	Y
Census variables	N	N	Y	N	N	Y	N	N	Y

Note: *** indicates statistical significance at 1%, ** at 5%, and * at 10%. Robust standard errors are in parentheses. All regressions include a constant. See the text for definitions of the main and restricted samples.

Table 7: Heterogeneity in effects on turnout and incumbent party vote share in 2013 presidential election

	Turnout	National Party share
Panel A: Main sample of villages		
CCT1	2.69**	2.55***
	(1.14)	(0.93)
CCT1 × Z	0.10	1.67**
	(0.98)	(0.77)
CCT2	2.48**	1.94**
	(1.08)	(0.81)
CCT2 × Z	-0.16	0.28
	(0.55)	(0.46)
Adjusted R^2	0.20	0.44
N of villages	677	677
Panel B: Restricted sample of villages		
CCT1	2.51*	2.38**
	(1.36)	(1.08)
CCT1 × Z	0.46	1.86**
	(1.07)	(0.80)
CCT2	2.84**	2.98**
	(1.39)	(1.24)
CCT2 × Z	-0.38	0.14
	(0.76)	(0.70)
Adjusted R^2	0.23	0.45
N of villages	383	383
Controls:		
2009 vote shares	Y	Y
Census variables	Y	Y

Note: *** indicates statistical significance at 1%, ** at 5%, and * at 10%. Robust standard errors are in parentheses. All regressions include a constant and control for Z, the z-score of villages' registered voters. See the text for definitions of the main and restricted samples.

Table 8: Transfers and benefits available to households in the year prior to the endline survey

	Percentage of households in:		
	CCT1	CCT2	p-value
In the 12 months prior to endline survey, any member of household received:			
Bono 10,000	79.50%	3.86%	<0.01
Other government transfer:			
Mother-child transfer	0.96%	0.06%	<0.01
Old-age transfer	1.65%	1.51%	0.76
Agriculture transfer	0.85%	0.84%	0.96
Secondary school transportation transfer	0.05%	0.22%	0.17
Assistance to female-managed businesses	0.05%	0.17%	0.30
In the 12 months prior to endline survey, any member of household benefitted from:			
School lunch	72.90%	68.20%	0.01
Food donation	3.47%	4.23%	0.41
Literacy campaign	1.39%	1.06%	0.40
Health or vaccination campaign	27.40%	32.40%	0.01
Growth monitoring for young children	6.13%	6.12%	0.99
Training in health	2.24%	2.45%	0.69
Latrine project	2.67%	2.39%	0.79
Potable water project	2.51%	2.78%	0.78
Sewer project	0.05%	0.39%	0.17
Electricity project	3.20%	1.45%	0.02
Construction or improvement of dwelling	2.99%	1.95%	0.16
Support for farmers	1.87%	1.45%	0.46
Support for small business owners	0.37%	0.28%	0.75
N of households in endline survey	1875	1797	

Note: For a detailed description of variables in the endline surveys, see Benedetti et al. (2016). The sample includes all households with non-missing endline surveys in CCT1 and CCT2. In the bottom panel, the questions did not specify whether the benefits were received by a public or private organization. The p-value corresponds to a test of the null that percentages are equal (adjusting for clustering of households within villages).

Table 9: Effects on the number of registered voters in 2013 presidential election

	Dependent variable: Z (z-score of the number of registered voters)		
Panel A: Main sample of villages			
CCT1	-0.020	-0.049	0.006
	(0.086)	(0.086)	(0.076)
CCT2	0.064	0.041	0.004
	(0.116)	(0.115)	(0.103)
Adjusted R2	<0.01	0.02	0.20
N	677	677	677
p-value (CCT1=CCT2)	0.51	0.48	0.98
Panel B: Restricted sample of villages			
CCT1	0.043	-0.008	0.017
	(0.128)	(0.128)	(0.115)
CCT2	0.019	-0.001	-0.032
	(0.153)	(0.154)	(0.144)
Adjusted R2	<0.01	0.02	0.16
N	383	383	383
p-value (CCT1=CCT2)	0.89	0.96	0.75
Controls:			
2009 vote shares	N	Y	Y
Census variables	N	N	Y

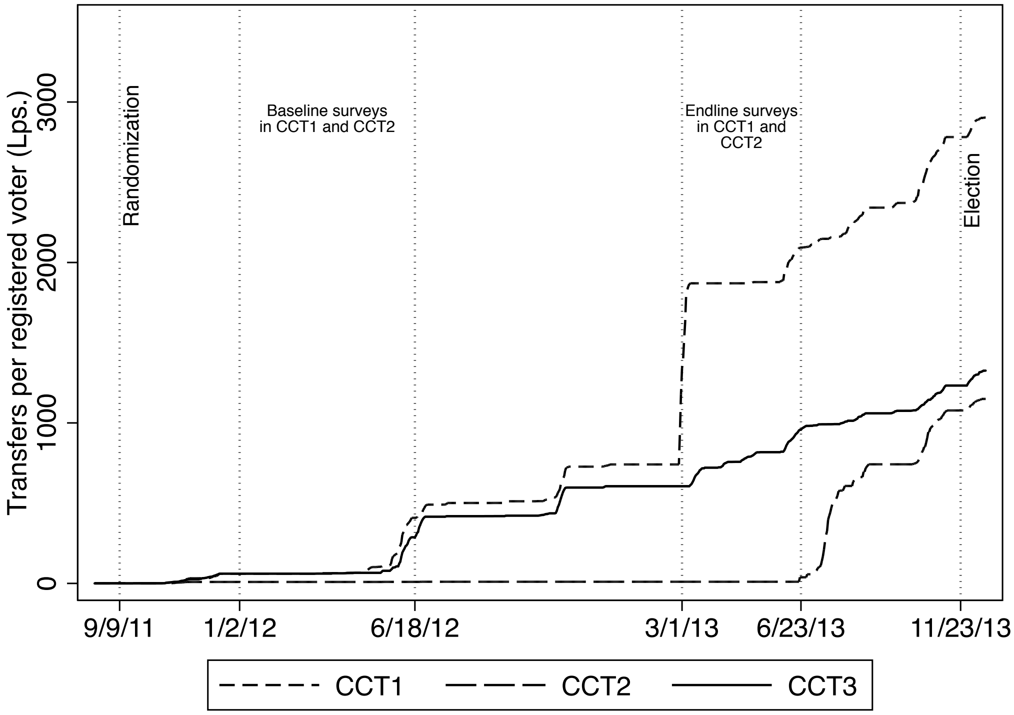
Note: *** indicates statistical significance at 1%, ** at 5%, and * at 10%. Robust standard errors are in parentheses. All regressions include a constant. See the text for definitions of the main and restricted samples.

Table 10: Effects of PRAF-II on the 2001 presidential election

	Dependent variable: Turnout			Dependent variable: Liberal vote share			Dependent variable: National vote share		
CCT	-1.41	-1.43	-1.05	-0.72	-0.79	-0.26	-0.53	-0.48	-0.10
	(1.46)	(1.44)	(1.49)	(1.43)	(0.96)	(1.09)	(1.22)	(0.89)	(0.93)
Adjusted R^2	0.03	0.05	0.08	<0.01	0.56	0.54	0.15	0.56	0.63
N of municipalities	70	70	70	70	70	70	70	70	70
Control-group mean	74.0	74.0	74.0	33.0	33.0	33.0	38.0	38.0	38.0
Controls:									
1997 vote shares	N	Y	Y	N	Y	Y	N	Y	Y
Census variables	N	N	Y	N	N	Y	N	N	Y

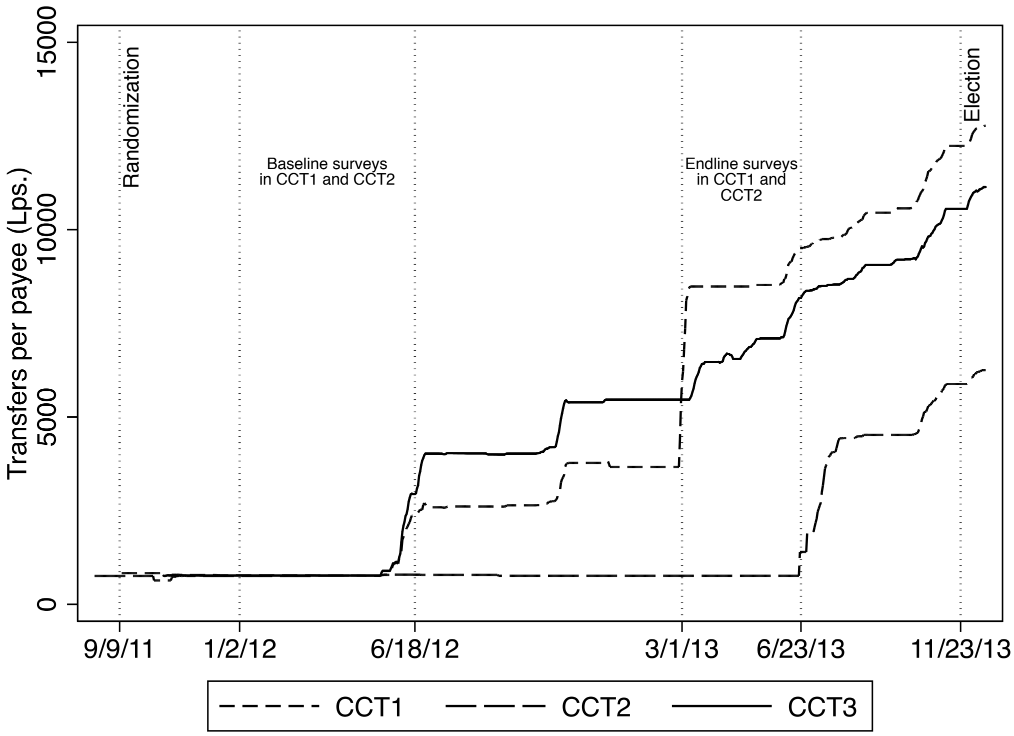
Note: *** indicates statistical significance at 1%, ** at 5%, and * at 10%. Robust standard errors are in parentheses. All regressions include a constant and dummy variables indicating 4 of 5 experimental strata (as described in Galiani and McEwan, 2013).

Figure 1: Cumulative transfers per registered voter in the Bono 10,000 experiment



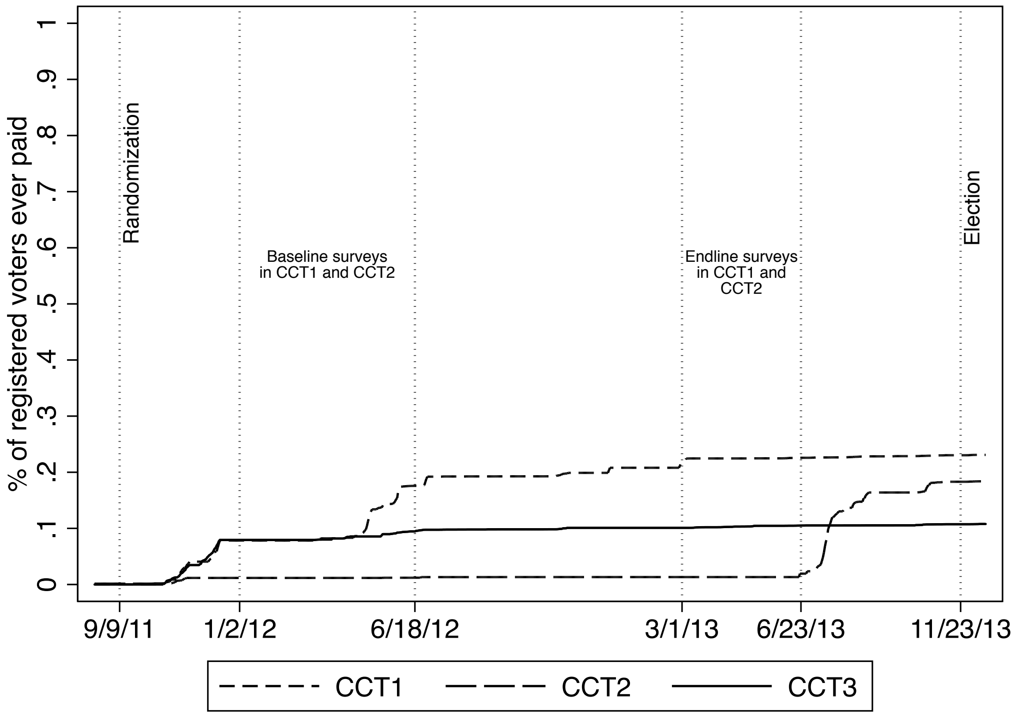
Note: The sample includes 677 villages in the main estimation sample. We used administrative records to calculate village-by-day cumulative payments, dividing by the number of registered voters in the village. The lines are within-group averages of village-level observations.

Figure 2: Cumulative transfers per payee in the Bono 10,000 experiment



Note: The sample includes 677 villages in the main estimation sample. We used administrative records to calculate village-by-day cumulative payments, dividing by the number of individuals who had ever received a transfer. The lines are within-group averages of village-level observations.

Figure 3: Cumulative participation rates in the Bono 10,000 experiment



Note: The sample includes 677 villages in the main estimation sample. We used administrative records to calculate village-by-day cumulative sums of the number of individuals who had ever received a transfer, dividing by the number of registered voters in the village. The lines are within-group averages of village-level observations.

Appendix A

Table A1: Baseline characteristics of villages in restricted estimation sample

	Mean (standard deviation)			p-value (jointly equal)	p-value (K-S)
	CCT1	CCT2	CCT3		
Panel A: Village-level vote share in 2009 Presidential elections					
National Party vote share	56.29	58.42	57.81	0.67	0.53/0.41
	(15.65)	(14.59)	(15.14)		
Liberal Party vote share	39.83	38.23	39.14	0.81	0.97/0.81
	(15.84)	(14.24)	(14.92)		
Panel B: Village-level variables from 2001 census; individuals 18 and older					
% female	0.486	0.489	0.491	0.51	0.87/0.39
	(0.03)	(0.04)	(0.04)		
Mean age	37.99	38.03	38.21	0.63	0.57/0.79
	(1.94)	(1.78)	(2.23)		
% Lenca (indigenous)	0.0765	0.078	0.0905	0.76	0.65/0.50
	(0.17)	(0.17)	(0.18)		
Mean years of schooling	2.922	3.05	2.999	0.57	0.52/0.97
	(0.75)	(0.75)	(0.63)		
% literate	0.65	0.663	0.663	0.62	0.77/0.92
	(0.10)	(0.09)	(0.09)		
% who worked week before census	0.505	0.498	0.501	0.86	0.23/0.69
	(0.08)	(0.09)	(0.09)		
% with dirt floor in dwelling	0.573	0.563	0.558	0.83	0.91/0.99
	(0.18)	(0.20)	(0.20)		
% with piped water in dwelling	0.712	0.741	0.713	0.66	0.95/0.03
	(0.23)	(0.24)	(0.21)		
% with electric light in dwelling	0.212	0.212	0.207	0.98	0.92/0.23
	(0.24)	(0.22)	(0.23)		
% with sewer/septic in dwelling	0.304	0.332	0.329	0.65	0.75/0.92
	(0.22)	(0.21)	(0.23)		
N of villages	76	69	238		

Notes: See the main text for a description of the null hypotheses tested by the reported p-values.

Table A2: 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 received any transfer by survey date	0.16	1.00	0.00
Extreme poverty rate of household's village	0.48	0.62	0.46
Dwelling located in rural census segment	0.58	0.91	0.52
Dwelling has dirt floor	0.22	0.42	0.18
Dwelling connected to sewer or septic	0.49	0.22	0.54
Index of 15 household assets (z-score)	0.00	-0.57	0.11
	(1.00)	(0.74)	(1.00)
Years of schooling (household head)	5.10	3.30	5.44
	(4.37)	(2.97)	(4.51)
Number of household members	4.70	5.96	4.46
	(2.36)	(2.31)	(2.29)
≥1 child between 0 and 5 years old	0.50	0.49	0.50
≥1 child between 6 and 18 years old	0.46	0.30	0.65
Anyone pregnant in household	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 National Party vote share (municipal level)	7.32	8.18	7.15
	(5.75)	(6.65)	(5.54)
N of households	20,446	3,265	17,181

Table A3: Baseline characteristics of municipalities in PRAF-II sample

	Mean (standard deviation)		p-value	p-value
	CCT	Control	(equal)	(K-S)
Panel A: Municipal-level vote share in 1997 Presidential elections				
National Party vote share	49.33	49.45	0.96	0.88
	(8.44)	(9.23)		
Liberal Party vote share	45.7	45.58	0.95	0.78
	(7.76)	(9.30)		
Panel B: Municipal-level variables from 2001 census; individuals 18 and older				
% female	0.496	0.495	0.84	0.88
	(0.02)	(0.02)		
Mean age	37.37	37.58	0.52	0.5
	(1.33)	(1.43)		
% Lenca (indigenous)	0.29	0.278	0.84	0.73
	(0.22)	(0.26)		
Mean years of schooling	2.816	2.69	0.43	0.97
	(0.73)	(0.59)		
% literate	0.63	0.618	0.56	0.5
	(0.09)	(0.08)		
% who worked week before census	0.51	0.525	0.22	0.15
	(0.06)	(0.04)		
% with dirt floor in dwelling	0.71	0.662	0.27	0.4
	(0.17)	(0.19)		
% with piped water in dwelling	0.653	0.66	0.86	0.15
	(0.11)	(0.17)		
% with electric light in dwelling	0.171	0.182	0.79	0.92
	(0.15)	(0.17)		
% with sewer/septic in dwelling	0.341	0.285	0.09	0.27
	(0.13)	(0.14)		
N of municipalities	40	30		

Notes: See the main text for a description of the null hypotheses tested by the reported p-values.