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Voter Response to Peak and End Transfers: Evidence from a Conditional Cash Transfer Experiment†

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In a Honduran field experiment, sequences of cash transfers to poor households varied in amount of the largest (peak) and last (end) transfers. Larger peak-end transfers increased voter turnout and the incumbent party's vote share in the 2013 presidential election, independently of cumulative transfers. A plausible explanation is that voters succumbed to a common cognitive bias by applying peak-end heuristics. Another is that voters deliberately used peak-end transfers to update beliefs about the incumbent party. In either case, the results provide experimental evidence on the classic non-experimental finding that voters are especially sensitive to recent economic activity. (*JEL* C93, D72, I32, O15, O17)

Many countries implement variants of conditional cash transfers or CCTs (Fiszbein et al. 2009, Adato and Hoddinott 2010). The typical policy objectively targets poor households and offers transfers in exchange for using school and health services.[1] CCTs are not explicitly conditioned on political support, unlike vote buying (Stokes 2005, Finan and Schechter 2012). Even so, intrinsically reciprocal voters might express their gratitude in the polling booth (Sobel 2005, Finan and Schechter 2012, and Lawson and Greene 2014). Alternatively, transfer recipients with imperfect information about the competence or redistributive preferences of incumbents may update their beliefs and vote accordingly (Rogoff 1990; Drazen

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[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).

and Eslava 2006, 2010; Manacorda, Miguel, and Vigorito 2011; and Healy and Malhotra 2013).

An empirical literature finds mixed effects of transfers on voter preferences and behavior in national elections. Quasi-experimental studies in Uruguay, Romania, and Colombia find positive effects on incumbent political support,[2] as does an observational study in Brazil (Zucco 2013). However, a Ugandan experiment finds counter-intuitively negative results,[3] and there are conflicting findings 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 reanalysis finds that results are sensitive to the model specification and data (Imai, King, and Velasco Rivera 2017). Using discontinuous variation in the assignment of communities to Progresa, Green (2006) reports no effects on voter behavior.

This paper estimates the impact of a Honduran CCT—dubbed Bono 10,000—on voter turnout and incumbent party vote share in the 2013 presidential election. It does so with a three-arm randomized experiment that introduced variation in the timing and the amount of cash transfers. In this regard, our paper resembles field experiments in Colombia and Kenya that distributed lump-sum and evenly spaced transfers, although these experiments focused on consumption and human capital investments rather than voter behavior (Barrera-Osorio et al. 2011; Haushofer and Shapiro 2016; and Barrera-Osorio, Linden, and Saavedra forthcoming)

The Honduran experiment included 816 villages, randomly assigned to CCT1 (150), CCT2 (150), and CCT3 (516). A government agency was responsible for the distribution of transfers. CCT1 began receiving transfers in June 2012. CCT2 did not receive transfers until June 2013, just after both groups participated in a follow-up survey of consumption, education, and health, analyzed in a companion paper (Benedetti, Ibarrarán, and McEwan 2016). CCT3 also began receiving transfers in June 2012. The presidential election was held in November 2013.

Poor households in the three groups of villages were supposed to receive similarly sized and evenly distributed cash transfers, but there were substantial deviations. In CCT1, poor households received large catch-up payments just before the follow-up surveys began in March 2013. In CCT2, poor households also received large catch-up payments, but just after the completion of the follow-up surveys in June 2013. There were no similarly timed spikes in the volume and size of transfers in CCT3. In village-level regressions, we show that "peak" transfers per registered voter (i.e., the largest transfer in a sequence) were higher in both

[2] In Uruguay, recipients of monthly, unconditional transfers in the vicinity of an assignment cutoff were more likely to favor the government even after the transfers ended (Manacorda, Miguel, and Vigorito 2011). In Romania, the recipients of a one-time voucher for a computer purchase were more likely to support the incumbent governing coalition, also in the vicinity of an assignment cutoff (Pop-Eleches and Pop-Eleches 2012). In Colombia, Conover et al. (forthcoming) found that bimonthly, conditional transfers affected turnout and incumbent vote share in the 2010 presidential election, especially among women who were the direct recipients; they instrumented transfer participation with transfer eligibility, based on a proxy means test. Nupia (2011) also finds incumbent vote share effects in Colombia using a different (but less plausibly exogenous) source of variation in CCT exposure.

[3] Ugandan beneficiaries of a highly successful program to support skilled enterprises were more likely to support the opposition, perhaps because of the empowering effects of financial independence, which diminished the need for patronage (Blattman, Emeriau, and Fiala 2018).

CCT1 and CCT2, on average, relative to CCT3. So, too, were "end" transfers per registered voter (i.e., the final transfer in a sequence).

Voter turnout was 2.5 and 2.3 percentage points higher in CCT1 and CCT2, respectively, relative to CCT3. The incumbent party's vote share was 2.4 and 1.9 percentage points higher than CCT3. In each case, the effects are statistically distinguishable from zero at conventional levels, but not from each other. The results provide a prima facie case that peak and end transfers—rather than total transfers—affect voter behavior, because the cumulative transfers per registered voter were similar in CCT2 and CCT3, and smaller than CCT1. To assess this, we regress voting outcomes on two endogenous variables: total transfers per registered voter and the peak-end transfer midpoint per registered voter.[4] We instrument both variables with CCT1 and CCT2. The coefficients on total transfers are close to zero. However, the coefficients on the peak-end midpoint suggest that an increase of 100 lempiras per registered voter (about $5) increases voter turnout and the incumbent party's vote share by 0.6 and 0.5 percentage points, respectively.

A literature in behavioral economics and psychology suggests that voters may have succumbed to a common cognitive bias. When evaluating sequences of hedonic episodes, individuals rely on peak and end heuristics. That is, they overweight the episode of greatest pleasure or displeasure as well as the final episode.[5] Similar phenomena occur when subjects retrospectively evaluate sequences of payments—such as cash transfers—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).

It is possible that voter response to peak and end transfers is not the result of cognitive bias. Rational voters might intend to reward incumbents for the largest or most recent transfer, perhaps because they view it as the best signal of unobserved competence or redistributive preferences before an election (e.g., Rogoff 1990, Drazen and Eslava 2006, 2010). We cannot directly test this, although Healey and Lenz (2014) provide indirect evidence. In lab experiments, they found that subjects overweight the final year of economic growth when rating entire presidential terms. (This is despite survey evidence that voters intend to weight years more equally.) The effects disappear when subjects receive more transparent information regarding cumulative growth over four years, suggesting that a reliance on election-year economic growth depends, at least in part, on heuristics.

We evaluate and discard alternate explanations for the results. Unlike voters in CCT3, some in CCT1 and CCT2 were exposed to baseline and follow-up 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

[4] In practice, the separate peak and end variables are highly collinear with each other because the final payment is sometimes the largest, or there is only a single payment in a sequence (which is both largest and final). Thus, results are robust to the use of either peak or end transfers as an endogenous variable (rather than the peak-end midpoint). However, this precludes inferences about the relative salience of peak and end transfers.

[5] For reviews, see Kahneman, Wakker, and Sarin (1997) and Healy and Lenz (2014).

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—other than the cash transfers—received from public and private sources.

The paper makes three main contributions. First, it provides causal evidence outside a lab setting that voters respond to peak and end transfers. In so doing, the paper contributes to the voluminous theoretical and empirical literature on political budget and business cycles (Drazen 2008a,b). For example, a classic empirical literature argues that voters are responsive to macroeconomic conditions, especially in election years (Kramer 1971, Nordhaus 1975, Fair 1978, and Markus 1988). These studies face considerable challenges in identifying exogenous variation in economic conditions (Manacorda, Miguel, and Vigorito 2011). Political scientists have used lab experiments to confirm that subjects do a poor job of evaluating sequences of individual payments (Huber, Hill, and Lenz 2012) and macroeconomic outcomes (Healy and Lenz 2014). To our knowledge, ours is the only field experiment to demonstrate that voters respond to the timing of economic activity—specifically, cash transfers—just before a national election. While not conclusive, the results are consistent with the well documented use of peak-end heuristics in varied lab settings (e.g., Kahneman, Wakker, and Sarin 1997; Healy and Lenz 2014).

Second, the paper contributes to a growing literature in development economics that demonstrates how the timing of cash transfers can moderate effects on household consumption and human capital investment. In Colombia and Kenya, experiments awarded some households lump-sum transfers in addition instead of evenly spaced transfers (Barrera-Osorio et al. 2011; Haushofer and Shapiro 2016; and Barrera-Osorio, Linden, and Saavedra forthcoming).[6] In Colombia, the postponement of transfers increased effects on secondary and tertiary education attainment. In Kenya, lump-sum transfers increased the value of non-land assets, but did not affect many other outcomes. Both results suggest that lump-sum transfers might relax a saving or borrowing constraint. Our experiment is the first to show that lump-sum transfers may also have important political consequences.

This may help understand mixed results in other evaluations. For example, Imai, King, and Velasco Rivera (2017) shows 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"—much like CCT2 in this paper—received payments for 3–8 months. The cumulative amount of transfers surely differed between groups, but the difference in voter behavior may have depended on the relative size of peak and end transfers. Using administrative data, Skoufias (2005) documented that transfers deviated from a bimonthly schedule, although it is not clear whether this varied across treatment arms. There were "considerable delays in the processing of forms necessary for payment authorization," leading to larger-than-expected payments in several months, "a consequence of Progresa's

[6] In Indonesia, quasi-experimental estimates suggest that ignoring the timing of unconditional cash transfers can lead to underestimates since delayed receipt reduces household expenditures, while timely receipt has no effects (Bazzi, Sumarto, and Suryahadi 2015).

efforts to catch up . . .” (9). Nevertheless, Green (2006) still finds no effect on voter behavior when comparing communities on either side of Progresa eligibility cutoffs. This was a plausibly cleaner treatment-control comparison since controls did not receive transfers (Imai, King, and Velasco Rivera 2017).

Third, the paper provides a straightforward political explanation for common shortcomings in program implementation. In Honduras and Mexico, the evidence shows that some transfers were delayed and/or larger than prescribed. (This may well occur elsewhere, but data on program compliance is sparse.) Even if politicians are not directly responsible for weak implementation, our results suggest that incumbent candidates and parties do not have strong incentives to improve the regularity of payments. In a related example, education and health conditions—such as school attendance—are sometimes imperfectly enforced (Baird et al. 2014). Strong enforcement of onerous conditions may lead a subset of non-complier households to decline transfers (Baird, McIntosh, and Özler 2011). Weaker enforcement may lead some to accept the “conditional” cash transfer, with attendant consequences for voter behavior. As above, politicians may face weak incentives to improve implementation.[7]

I. The Bono 10,000 Conditional Cash Transfer Program

A. *Program Design*

Since the early 1990s, the Family Allowance Program—known by its Spanish acronym, PRAF—has administered variants of conditional cash transfer programs (Moore 2008, Galiani and McEwan 2013). In 2010, PRAF began the nationwide rollout of Bono 10,000, which continued after the 2013 presidential election. Program guidelines dictated that poor households would receive up to 10,000 lempiras per year (about $500). Household poverty (and thus eligibility) were determined via a proxy means test. The methodology was not disclosed to researchers or to the public, although it relied on wealth proxies obtained from a household census (Benedetti, Ibarrarán, and McEwan 2016).

Households received *either* an education or a health transfer. Households received the education transfer (10,000 lempiras per year) if they enrolled *at least one child* between 6 and 18 in grades 1 to 9. Households received the health transfer (5,000 lempiras per year) if children under 6 and pregnant women attended health center checkups (and if the household was not eligible for the larger education transfer). Relative to other CCT programs, the conditions in Bono 10,000 were weak (Benedetti, Ibarrarán, and McEwan 2016). In households with multiple children, households received the full education transfer even if no more than one child enrolled in school. Moreover, households were not obligated to comply with health conditions if the presence of an older child qualified the household for the education

[7] One might argue that politicians have strong incentives to signal a commitment to public education by strictly enforcing attendance and enrollment conditions. However, Bursztyn (2016) shows that poor voters in Brazil have a stronger preference for redistributive programs than for investments in public education.

transfer. The great majority (83 percent) of transfer-eligible households received the larger education transfer.[8]

According to program guidelines, households were supposed to receive transfers in no fewer than three installments per year. The first was a small, unconditional transfer equal to 1/12 of the annual total, received at the time of registration. Thereafter, two equally sized transfers were payable upon verification of compliance with the conditions. The annual transfers were equal to 18 percent of the median per capita expenditure of poor households (Benedetti, Ibarrarán, and McEwan 2016). This is comparable to Latin American cash transfer programs such as Progresa/Oportunidades, and larger than earlier Honduran programs (Fiszbein et al. 2009, Galiani and McEwan 2013).

B. *Experimental Design*

The national rollout of Bono 10,000 began in 2010 and continued past the 2013 election. PRAF focused early implementation on villages with high poverty rates.[9] The research team delayed the rollout in 816 villages with high poverty rates (Benedetti, Ibarrarán, and McEwan 2016). On September 9, 2011, researchers blindly drew 300 numbered balls from a receptacle containing 816. In alternating order, villages were assigned to a treatment group of 150 and a control group of 150. We refer to the groups as CCT1 and CCT2, respectively.

Baseline surveys were applied to a sample of poor households in CCT1 and CCT2 in the first half of 2012.[10] Follow-up surveys were applied to the same sample between March and June 2013 (but still more than four months before the presidential election on November 23, 2013). Villages in CCT1 received transfers from PRAF immediately, while villages in CCT2 received transfers immediately after the completion of follow-up surveys. In a companion paper, Benedetti, Ibarrarán, and McEwan (2016) used the survey data to estimate the impact of transfers on poverty, education, and health outcomes. The remaining 516 villages—referred to as CCT3—did not participate in baseline or follow-up surveys, although they received transfers at the discretion of PRAF.

C. *Characterizing the Distribution of Transfers*

Household transfers were supposed to follow the aforementioned guidelines, regardless of the treatment arm. In practice, PRAF deviated from the guidelines, particularly in CCT1 and CCT2. The histogram in Figure 1 describes the weekly volume of transfers in CCT1. There was a spike just before the application of the follow-up surveys. Indeed, more than 20 percent of all transfers in CCT1 occurred in a two-week period. The open circles show that average transfers in these weeks were among the largest made to CCT1 households. At more than

[8] The estimate is based upon the administrative payment data analyzed below.

[9] We empirically corroborate this, and other details of village and household targeting in online Appendix A.

[10] 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 15 households from each village, regardless of population size.

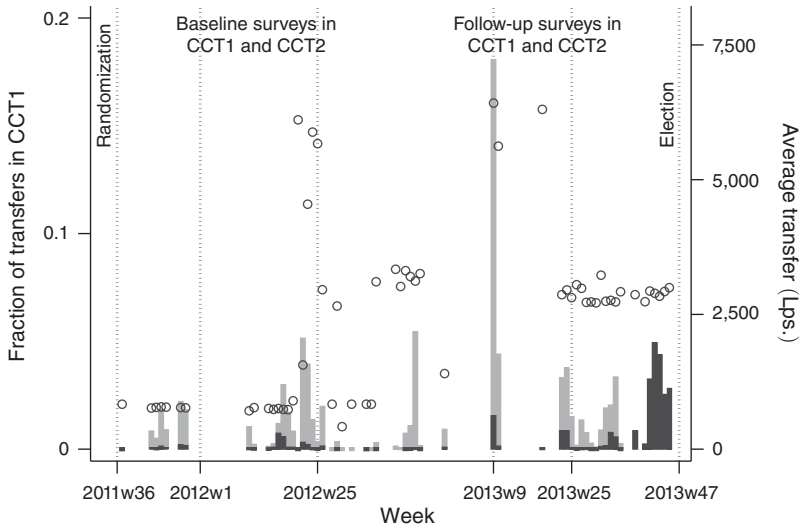

Figure 1. The Weekly Volume and Size of Household Transfers in CCT1

Notes: Lighter bars indicate the weekly volume of transfers to any household in CCT1, expressed as a fraction of all transfers in CCT1. Darker bars indicate the weekly volume of end transfers (i.e., transfers that are the final—or only— transfer in sequences received by households in CCT1), also expressed as a fraction of all transfers in CCT1. Circles indicate the average size of transfers made in a particular week. Vertical dotted lines indicate the dates of randomization (September 9, 2011), the application of baseline surveys (January 2 to June 18, 2012), the application of follow-up surveys (March 1 to June 23, 2013), and the presidential election (November 23, 2013).

5,000 lempiras, the average transfers exceeded program guidelines. Anecdotally, PRAF rushed to distribute catch-up payments before the application of follow-up surveys, so that CCT1 households would receive the prescribed (cumulative) transfers. Similarly large payments occurred just after baseline survey collection, although the volume of such payments was much lower.

In CCT2, there were almost no transfers before the completion of the follow-up surveys (see Figure 2). This constraint was imposed by researchers in order to preserve the fidelity of the control group in the original impact evaluation (Benedetti, Ibarrarán, and McEwan 2016). After the follow-up surveys, PRAF rapidly increased the volume of catch-up transfers. Over an 11-week period, the average size of these payments was nearly 5,000 lempiras. Many such transfers were the final ones received by households before the presidential election (illustrated by the darker bars in the histogram of Figure 2).

Transfers in CCT3 hewed more closely to program guidelines (see Figure 3). There were fewer spikes in the volume and average size of transfers, relative to CCT1 and CCT2. There were larger-than-prescribed transfers in several weeks of mid-2012 that mirrored CCT1. However, the larger transfers did not occur in weeks of especially high volume. Overall, Figures 1, 2, and 3 provide suggestive evidence that poor households in CCT1 and CCT2—relative to CCT3—were more likely to receive specific transfers that exceeded program guidelines.

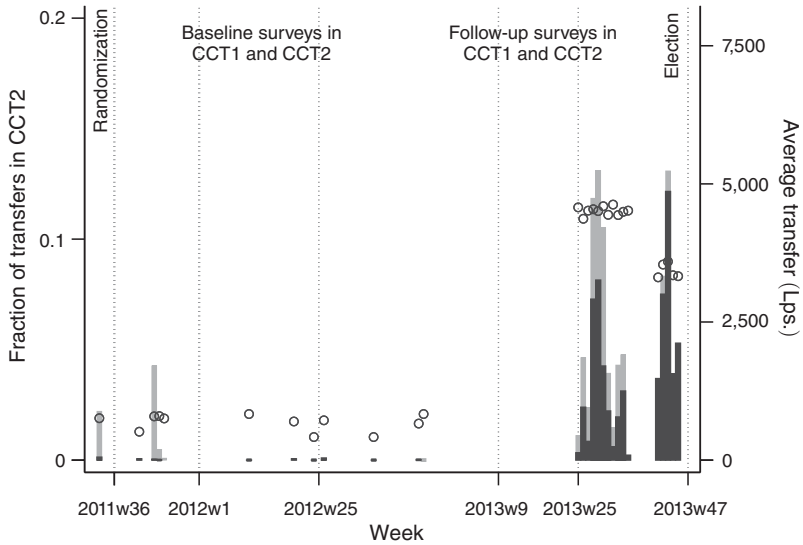

FIGURE 2. THE WEEKLY VOLUME AND SIZE OF HOUSEHOLD TRANSFERS IN CCT2

Notes: Lighter bars indicate the weekly volume of transfers to any household in CCT2, expressed as a fraction of all transfers in CCT2. Darker bars indicate the weekly volume of end transfers (i.e., transfers that are the final—or only—transfer in sequences received by households in CCT2), also expressed as a fraction of all transfers in CCT2. Circles indicate the average size of transfers made in a particular week. See Figure 1 for the dates corresponding to dotted lines.

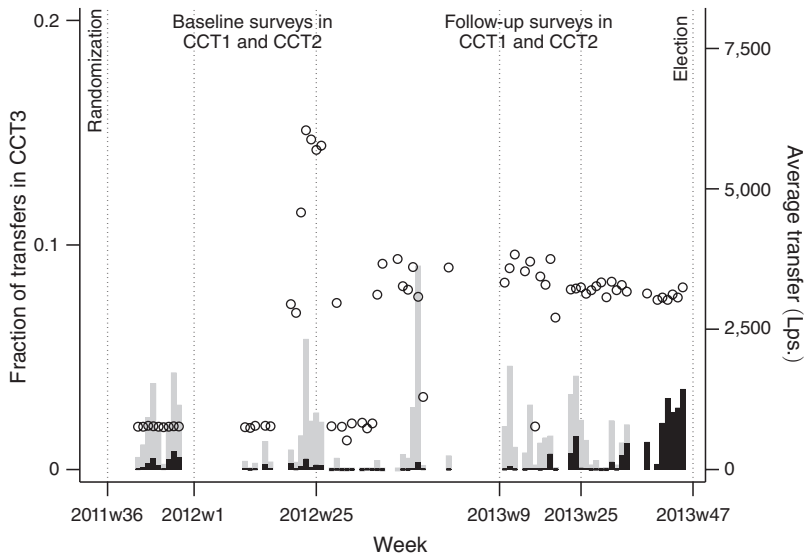

FIGURE 3. THE WEEKLY VOLUME AND SIZE OF HOUSEHOLD TRANSFERS IN CCT3

Notes: Lighter bars indicate the weekly volume of transfers to any household in CCT3, expressed as a fraction of all transfers in CCT3. Darker bars indicate the weekly volume of end transfers (i.e., transfers that are the final—or only—transfer in sequences received by households in CCT3), also expressed as a fraction of all transfers in CCT3. Circles indicate the average size of transfers made in a particular week. See Figure 1 for the dates corresponding to dotted lines.

D. *The Potential Salience of Peak and End Transfers*

What are the implications for voter behavior? For the moment, assume that voters *intend* to reward incumbents in proportion to the sum of transfers received by an election date (we later revisit this assumption). This requires a retrospective evaluation of the sequence of transfers on election day. A well-known literature in behavioral economics and psychology suggests that individuals make predictable errors when retrospectively evaluating sequences of hedonic episodes.[11] Subjects tend to overweight the peak episode—the moment of highest pleasure or worst discomfort—as well as the final episode. Intuitively, individuals might commit fewer errors when evaluating a sequence of economic outcomes, such as monetary transfers. The rule for evaluation of a sequence—the sum—is more obvious and universally shared than rules for evaluating hedonic episodes (Langer, Sarin, and Weber 2005).

Nonetheless, lab experiments show that subjects perform badly in the retrospective evaluation of economic outcomes, even with incentives or preferences to do otherwise (Langer, Sarin, and Weber 2005; Huber, Hill, and Lenz 2012; Healy and Lenz 2014; and Yu, Lagnado, and Chater 2008). 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, Sarin, and Weber 2005; Yu, Lagnado, and Chater 2008). The reliance on peak and end heuristics is most evident when subjects are distracted by other tasks,[12] inattentive,[13] or simply unable to calculate a running sum because of lengthy sequences or mathematical ability.[14]

[11] For reviews, see Kahneman, Wakker, and Sarin (1997) and Healy and Lenz (2014). 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, Healy and Lenz 2014).

[12] In Langer, Sarin, and Weber (2005), 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—calculated running sums, precluding the need for retrospection. In another experiment, however, the participants were distracted by a strenuous mental task. 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.

[13] Huber, Hill, and Lenz (2012) assigned subjects an "allocator" that would offer 32 payments of tokens in successive rounds, later convertible to cash. 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 payment) 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.

[14] In Yu, Lagnado, and Chater (2008), participants played a slot machine for two sessions of 50 payouts each, preferring those with higher peak-end midpoints despite lower total payouts. Healy and Lenz (2014) called upon participants to compare and rate economic growth during the terms of hypothetical presidents. 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

The Honduran setting is unlikely to diminish susceptibility to peak-end bias. The field experiment occurred over nearly two years, and voters were distracted by varied obligations in a high-poverty setting. They were plausibly 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 poor adults is low. Their ability to calculate a running sum—even without distraction or inattention—is plausibly lower than university students in richer countries (Langer, Weber, and Sarin 2005; Yu, Lagnado, and Chater 2008) or experimental subjects recruited through Amazon's Mechanical Turk (Huber, Hill, and Lenz 2012; Healy and Lenz 2014).

II. Data

A. *The 2013 Presidential Election*

Before 2013, Honduran presidential elections were dominated by the National and Liberal Parties. Neither party defended a strong ideological platform, and both cultivated clientelist networks of supporters by distributing resources and jobs in order to mobilize core supporters (Ruhl 2010, Taylor-Robinson 2014).[15] Two-party dominance eroded after a 2009 coup d'état. The Liberal president—Manuel Zelaya—sought closer relations with Venezuela and the 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 presidential elections—in November 2009—that were won by the National Party candidate, Porfirio Lobo.

The coup d'état 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. In the presidential election held on November 23, 2013, the incumbent National Party candidate, Juan Orlando Hernandez, won with a plurality of 36.9 percent of votes. He was followed by the candidates of LIBRE (28.8 percent), the Liberal Party (20.3 percent), and PAC (13.4 percent). At 60.4 percent of registered voters, voter turnout was among the highest recorded in a presidential election (Otero-Felipe 2014).

B. *Village Samples*

The *Tribunal Supremo Electoral* (TSE) is 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). We scraped the

each year more equally in their overall decision. 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 aligned with their intention to weigh years more equally.

[15] In the 2009 election, for example, a list experiment found that 21 percent of voters reported receiving a gift or favor during the campaign (González-Ocantos, Kiewiet de Jonge, and Nickerson 2015).

Table 1—Number of Villages in the Main and Restricted Estimation Samples

	All treatment arms	CCT1	CCT2	CCT3
Total number of villages in experiment	816	150	150	516
Main estimation sample				
Villages with $\geq$1 voting center:				
Number	676	120	129	427
Percent of total	83	80	86	83
Restricted estimation sample				
Villages with $\geq$1 voting center, *and* circumscribed by villages with $\geq$1 voting centers:				
Number	382	76	69	237
Percent of total	47	51	46	46

Notes: The main estimation sample includes experimental villages with at least one voting center in the 2013 presidential election. The restricted sample includes experimental villages with at least one voting center that are also circumscribed by villages with at least one voting center. Both samples exclude one village in CCT3 that reports zero valid votes.
Source: Author calculations

2013 election results for 5,433 domestic voting centers from a TSE website.[16] The scraped data did not include the numerical geographic codes of the villages in which voting centers are located. As a labor-intensive alternative, we downloaded scanned images of certified TSE vote tallies, which named the department, municipality, and village (*aldea*) of the voting center.[17] We hand-matched 99.7 percent of voting centers to the geographic codes of their villages.[18]

Of the 3,727 villages in Honduras, 82 percent had at least one voting center in the 2013 elections (see Table 1). 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 had at least one voting center. We further exclude one village that reported zero valid votes (the mean and median villages have 873 and 569 registered voters, respectively). This is plausibly due to manipulation, and so we conservatively exclude this outlier. Because it is in CCT3, its inclusion slightly increases the magnitude of estimates reported in subsequent tables. The main estimation sample includes 83 percent of all experimental villages, and the proportion is similar across the treatment arms.[19] This is expected, since treatment assignment was independent of village population.

[16] See http://siede.tse.hn/escrutinio/index.php. Honduran consulates in some US cities are also used as voting centers.

[17] To be more specific, the certified vote tallies list either the aldea, barrio, or caserío. Barrios and caseríos are sub-units of aldeas, which facilitated the identification of unlisted aldeas.

[18] We emphasize that simply matching village names would yield spurious matches, given many common village names across municipalities. Thus, we verified that departments, municipalities, and villages had matching names (and did so by visual inspection to account for small discrepancies in spelling and diacritics).

[19] In a sample of 816 experimental villages, a dummy variable indicates whether the village is in the main 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 were unable to reject the null, given a p-value of 0.37 on the F-test.

Villages in the main estimation sample may share a border with a sparsely populated village without a voting center. Therefore, a village's voting centers might include voters from outside the village. This introduces measurement error in the dummy variables indicating treatment groups, 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 construct a restricted estimation sample of 382 experimental villages that are circumscribed by villages that also have at least one voting center. In the restricted sample, therefore, each village's voting center only includes registered voters of that village. We ignore borders of neighboring villages when they are in different municipalities, since sectors do not cross municipal borders (República de Honduras 2009). The restricted sample is 47 percent of the original sample of 816, and the proportion is similar across treatment arms.[20]

C. *Dependent and Independent Variables*

We calculated the village-level turnout in the 2013 elections—aggregating up from voting-center tallies—as the percent of registered voters who cast a valid vote for any party. We further calculated village-level vote shares for the incumbent National Party, the Liberal Party, LIBRE, and PAC. As with turnout, we calculated vote shares as percentages of registered voters. The number of voters registered in the 2013 election is a good proxy for the population of voting-age adults.[21] That is because individuals 18 and older with a national identify card are automatically registered to vote in the voting center nearest their residence.

We constructed two groups of control 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).[22] In 2009, the TSE did not report the number of registered voters, and so we calculated vote shares as percentages of valid votes. 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, and attributes of dwellings that proxy income and wealth.

[20] As before, in a sample of 816 experimental villages, a dummy variable indicates whether the village is 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 0.58.

[21] We verified this by estimating 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 (Department of Economic and Social Affairs 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.

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

Table 2—Baseline Characteristics of Villages in the Main Estimation Sample

	Mean (standard deviation)			p-value (jointly equal)	p-values (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	0.98	0.31/0.81
	(15.98)	(15.11)	(15.15)		
Liberal Party vote share	39.46	39.30	39.72	0.96	0.82/0.96
	(16.01)	(14.91)	(14.98)		
Panel B. Village-level variables from 2001 census; Individuals 18 and older					
Percent female	0.482	0.486	0.488	0.23	0.59/0.67
	(0.03)	(0.03)	(0.04)		
Mean age	38.10	37.97	38.22	0.41	0.43/0.54
	(2.04)	(1.88)	(2.08)		
Percent Lenca (indigenous)	0.055	0.056	0.067	0.65	0.33/34
	(0.14)	(0.15)	(0.16)		
Mean years of schooling	2.931	3.088	3.052	0.19	0.13/0.72
	(0.74)	(0.71)	(0.68)		
Percent literate	0.653	0.670	0.668	0.33	0.33/0.96
	(0.11)	(0.10)	(0.10)		
Percent who worked week before census	0.498	0.499	0.496	0.96	0.06/0.99
	(0.08)	(0.09)	(0.08)		
Percent with dirt floor in dwelling	0.542	0.507	0.525	0.38	0.71/0.34
	(0.19)	(0.21)	(0.20)		
Percent with piped water in dwelling	0.720	0.697	0.695	0.61	0.73/0.32
	(0.24)	(0.26)	(0.24)		
Percent with electric light in dwelling	0.199	0.233	0.212	0.52	0.47/0.48
	(0.23)	(0.25)	(0.23)		
Percent with sewer/septic in dwelling	0.321	0.344	0.337	0.70	0.91/0.98
	(0.22)	(0.23)	(0.22)		
Number of villages	120	129	427		

Notes: Each cell in the column titled "p-value (jointly equal)" reports the p-value from an F-test of the null hypothesis that the means in the three groups are equal. Each cell in the column titled "p-values (K-S)" reports p-values from two-sample Kolmogorov-Smirnov tests of the equality of distributions of the baseline variables (across CCT1/CCT3 and CCT2/CCT3, respectively).
Source: Author calculations

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 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 the other two treatment arms. We cannot reject equality of the distributions at conventional significance levels. We repeated these analyses in the restricted sample of villages, and there is similarly good balance across treatment arms (see Table B1 in the online Appendix).

Table 3—Effects on Turnout and Incumbent Party Vote Share in the 2013 Presidential Election

	Turnout		National Party share	
Panel A. Main sample of villages				
CCT1	2.50	2.50	2.62	2.42
	(1.16)	(1.14)	(1.20)	(0.93)
CCT2	1.76	2.31	1.22	1.86
	(1.23)	(1.06)	(1.07)	(0.80)
Adjusted R^2	<0.01	0.21	0.01	0.45
Number of villages	676	676	676	676
CCT3 mean	61.2	61.2	27.3	27.3
p-value (CCT1 = CCT2)	0.62	0.89	0.32	0.60
Panel B. Restricted sample of villages				
CCT1	2.30	2.23	1.69	2.37
	(1.35)	(1.31)	(1.42)	(1.06)
CCT2	2.48	2.56	2.70	2.83
	(1.56)	(1.34)	(1.46)	(1.20)
Adjusted R^2	0.01	0.27	<0.01	0.47
Number of villages	382	382	382	382
CCT3 mean	62.6	62.6	28.7	28.7
p-value (CCT1 = CCT2)	0.92	0.84	0.56	0.74
Control variables?	No	Yes	No	Yes

Notes: Robust standard errors are in parentheses. See the notes to Table 1 for definitions of the main and restricted samples. All regressions include a constant; additional controls in some specifications include the variables in Table 2.
Source: Author calculations

III. Effects on Voting Outcomes

A. *Reduced-Form Estimates*

Given randomized assignment, we estimate the reduced-form effect of assignment to CCT1 and CCT2—relative to CCT3—with the regression

$$V_i = \alpha + \beta_1 CCT1_i + \beta_2 CCT2_i + \varepsilon_i, \tag{1}$$

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. We also report estimates from regressions that control for the baseline variables in Table 2.

Table 3 reports regression estimates for voter turnout and the incumbent party vote share. 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, 2.5 and 2.3 percentage points higher than CCT3. Both coefficients are statistically significant at conventional

levels, and we fail to reject the null that they are equal. The magnitude of the effect represents a 3.8 to 4.1 percent increase over CCT3's election turnout of 61 percent.

The incumbent party's vote share—also measured as a percent of registered voters—increased by 2.4 and 1.9 percentage points in CCT1 and CCT2. Again, both coefficients are statistically significant at conventional levels, though not statistically different from one another. They represent increases of 7 to 8.8 percent over CCT3's incumbent vote share of 27 percent. Recall that turnout and vote share both are calculated with the same denominator of registered voters. Thus, the similarity of the point estimates on turnout and incumbent party vote share is suggestive that assignment to the CCT1 and CCT2 primarily increased the turnout among the National Party base, rather than encouraging vote-switching among those already inclined to vote.[23]

Panel B repeats these analyses in the restricted sample of villages, which reduces the threat of measurement error in the treatment variables. 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 2.2 for both turnout and for incumbent party vote share.

Table 4 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 conventional levels (see panels A and B). 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 the 10 percent level in the restricted sample.

Viewed alongside Figures 1 to 3, the reduced-form estimates are prima facie evidence that voters in CCT1 and CCT2 responded to larger peak and/or end transfers. However, this straightforward interpretation is muddied by three issues. First, transfer-recipients in CCT2 receive fewer cumulative transfers, on average, than households in CCT1 and CCT3.[24] Second, 33 percent of villages in CCT3 did not receive transfers at PRAF's discretion, which did not occur in other groups. Third, some registered voters within participating villages did not receive transfers, including non-poor households that did not pass a proxy means test, and poor households that did not comply with the conditions. However, both exclusions were consistently applied across the treatment arms.

[23] In this respect, the results seem consistent with the historically entrenched nature of party identification in rural and poor areas (in which National and Liberal parties employed clientelist strategies primarily to mobilize a political base). In Bono 10,000, resources were distributed to poor households without regard for party affiliation. However, it seems plausible that voters' party identification and past experiences with clientelist redistribution mediate the degree to which intrinsically reciprocal voters (e.g., Finan and Schechter 2012) obtain pleasure from rewarding a political party. Alternatively, voters' past experiences might affect whether they perceive transfers as a credible signal of party competence or redistributive preferences (e.g., Manacorda, Miguel, and Vigorito 2011).

[24] Among transfer-recipients in each treatment arm, the average household in CCT1, CCT2, and CCT3 received a total of 12,066; 6,029; and 11,527 lempiras, respectively.

Table 4—Effects on Other Parties' Vote Shares in the 2013 Presidential Election

	Liberal Party share		LIBRE share		PAC share	
Panel A. Main sample of villages						
CCT1	−0.96	−0.36	1.29	0.83	−0.53	−0.48
	(0.98)	(0.80)	(1.26)	(1.15)	(0.34)	(0.31)
CCT2	−0.63	−0.15	0.21	−0.11	0.93	0.71
	(0.87)	(0.72)	(1.13)	(1.03)	(0.48)	(0.43)
Adjusted R^2	<0.01	0.35	−0.00	0.17	0.01	0.18
Number of villages	676	676	676	676	676	676
CCT3 mean	11.7	11.7	18.0	18.0	3.9	3.9
p-value (CCT1 = CCT2)	0.77	0.83	0.48	0.50	<0.01	0.01
Panel B. Restricted sample of villages						
CCT1	−0.74	−0.71	1.60	0.92	−0.37	−0.49
	(1.22)	(1.03)	(1.62)	(1.52)	(0.42)	(0.38)
CCT2	−1.26	−0.81	0.37	−0.09	0.67	0.61
	(1.21)	(1.10)	(1.59)	(1.42)	(0.55)	(0.56)
Adjusted R^2	<0.01	0.28	−0.00	0.18	<0.01	0.12
Number of villages	382	382	382	382	382	382
CCT3 mean	11.4	11.4	18.6	18.6	3.6	3.6
p-value (CCT1 = CCT2)	0.73	0.94	0.55	0.59	0.08	0.06
Control variables?	No	Yes	No	Yes	No	Yes

Notes: Robust standard errors are in parentheses. See the notes to Table 1 for definitions of the main and restricted samples. All regressions include a constant; additional controls in some specifications include the variables in Table 2.
Source: Author calculations

B. *First-Stage Estimates*

We therefore calculated the total transfers *per registered voter* in each village.[25] Figure 4 illustrates averages of the village-level variable within the three treatment arms. On the date of the presidential election, the total transfers per registered voter are roughly similar in CCT2 and CCT3, but much larger in CCT1. This is particularly influenced by the fraction of CCT3 villages that did not participate in the program before the election. In Table 5, the first two columns in panel A report analogous regression estimates. On average, the total transfers per registered voter are 1,511 lempiras higher in CCT1 than in CCT3 (about $76), a difference that is economically and statistically significant. In contrast, the coefficient on CCT2 is slightly negative, but not statistically distinguishable from zero at conventional levels. As Table 5 shows, these estimates are not sensitive to the exclusion of baseline control variables, or to the use of the restricted estimation sample in panel B.

Figure 4 also corroborates the uneven distribution of transfers before the election. The total transfers per registered voter increased sharply in CCT1 villages just before the follow-up surveys, and in CCT2 villages just after the follow-up surveys.

[25] To calculate the total transfer per registered voter in a village, we divide the total transfers received by a particular date (taken from administrative payment data) by the number of registered voters (taken from TSE voting data). In other words, registered voters in excess of the recipients are assumed to have received zero payments.

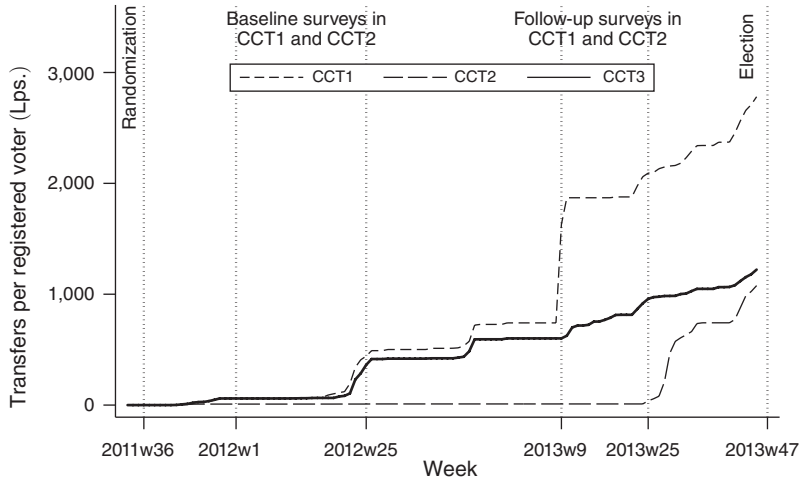

Figure 4. Cumulative Transfers per Registered Voter in CCT1, CCT2, and CCT3

Notes: The sample includes 676 villages in the main estimation sample (120 in CCT1, 129 in CCT2, and 427 in CCT3). For each village, we calculated village-by-week cumulative transfers, divided by the number of registered voters in the village. The lines indicate averages of village-level observations in each week. See Figure 1 for the dates corresponding to dotted lines.

Both patterns are consistent with the household-level data reported in Figures 1 and 2. To further examine these patterns, Table 5 reports estimates for three additional dependent variables: the peak transfer per registered voter, the end transfer per registered voter, and peak-end transfer midpoint per registered voter.[26] For each transfer recipient, the peak and end transfers (and the midpoint) were equal if there was only a single payment in the sequence, or the end transfer also happened to be the largest. This was the case in 44 percent of households that received a transfer. Not surprisingly, the peak and end variables are highly correlated with each other ($r = 0.92$), and more so with the midpoint variable.

On average, the peak transfer per registered voter is 855 lempiras higher in CCT1 villages, relative to CCT3. It is 329 lempiras higher in CCT2 (see panel A in Table 5). Both estimates are statistically distinguishable from zero and from each other at conventional levels. Neither is sensitive to the exclusion of baseline controls or to the use of the restricted estimation sample in panel B. For the end transfer per registered voter, the coefficients on CCT1 and CCT2 are closer in magnitude (360 and 391 lempiras, respectively) and statistically indistinguishable. When using the peak-end midpoint variable, the coefficient on CCT1 is again somewhat larger than that of CCT2.

[26] As with total transfers per registered voter, we simply assume that registered voters received peak and end transfers of zero if they did not appear in administrative payment data.

Table 5—Effects on Transfers per Registered Voter (Hundreds of Lempiras) by the 2013 Presidential Election

	Total transfer per registered voter		Peak transfer per registered voter		End transfer per registered voter		Peak-end transfer midpoint per registered voter	
Panel A. Main sample of villages								
CCT1	15.60	15.11	8.72	8.55	3.72	3.60	6.22	6.07
	(1.58)	(1.62)	(0.83)	(0.89)	(0.38)	(0.39)	(0.60)	(0.63)
CCT2	−1.44	−1.18	3.24	3.29	3.88	3.91	3.56	3.60
	(0.83)	(0.85)	(0.42)	(0.40)	(0.32)	(0.31)	(0.37)	(0.35)
Adjusted R^2	0.18	0.34	0.24	0.33	0.23	0.33	0.23	0.32
Number of villages	676	676	676	676	676	676	676	676
CCT3 mean	12.2	12.2	4.9	4.9	3.0	3.0	4.0	4.0
p-value (CCT1 = CCT2)	0.00	0.00	0.00	0.00	0.71	0.50	0.00	0.00
F (CCT1 = CCT2 = 0)	64	49	72	73	102	108	85	88
Panel B. Restricted sample of villages								
CCT1	15.96	15.87	8.99	8.97	3.78	3.74	6.38	6.36
	(2.10)	(2.09)	(1.15)	(1.19)	(0.50)	(0.51)	(0.82)	(0.84)
CCT2	−3.13	−2.98	2.95	3.00	3.88	3.88	3.42	3.44
	(1.16)	(1.20)	(0.56)	(0.55)	(0.43)	(0.41)	(0.49)	(0.48)
Adjusted R^2	0.18	0.36	0.23	0.31	0.23	0.32	0.22	0.31
Number of villages	382	382	382	382	382	382	382	382
CCT3 mean	14.6	14.6	5.7	5.7	3.5	3.5	4.6	4.6
p-value (CCT1 = CCT2)	0.00	0.00	0.00	0.00	0.86	0.81	0.00	0.00
F (CCT1 = CCT2 = 0)	48	42	37	35	57	59	45	44
Control variables?	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Robust standard errors are in parentheses. See the notes to Table 1 for definitions of the main and restricted samples. All regressions include a constant; additional controls in some specifications include the variables in Table 2. 100 lempiras ∼5 US dollars.
Source: Author calculations

C. *Two-Stage Least Squares Estimates*

The reduced-form estimates in Table 3 show that assignment to CCT1 or CCT2 increased turnout and incumbent party vote share by roughly the same magnitude, relative to CCT3. The first-stage estimates in Table 5 imply that total transfers per registered voter cannot easily explain this result, since CCT2 was, if anything, slightly lower than CCT3. In contrast, the first-stage estimates showed that peak and end transfers were higher in both CCT1 and CCT2, relative to CCT3.

In Table 6, we empirically assess whether total transfers are less important than peak-end transfers. In the first column of panel A, we report estimates of second-stage regressions that instrument total transfers per registered voter and the peak-end transfer midpoint per registered voter with CCT1 and CCT2. (All first-stage and second-stage regressions control for baseline variables.) The coefficient on total transfers is negative (−0.08). Its 95 percent confidence interval allows us to rule out effects on turnout that are larger than 0.14 (or 0.14 percentage points per 100 lempiras, or $5). However, a 100 lempira increase in the peak-end transfer midpoint

Table 6—Two-Stage Least Squares Regressions of the Effects of Transfers (Hundreds of Lempiras) on Turnout and Incumbent Party Vote Share in the 2013 Presidential Election

	Turnout			National Party share		
Panel A. Main sample of villages						
Total transfer per registered voter	−0.08	−0.19	0.02	−0.04	−0.13	0.04
	(0.11)	(0.15)	(0.08)	(0.08)	(0.11)	(0.06)
Peak-end transfer midpoint per registered voter	0.62	–	–	0.50	–	–
	(0.26)			(0.20)		
Peak transfer per registered voter	–	0.63	–	–	0.52	–
		(0.26)			(0.20)	
End transfer per registered voter	–	–	0.60	–	–	0.49
			(0.25)			(0.19)
Number of villages	676	676	676	676	676	676
p-value (jointly equal)	0.04	0.04	0.05	0.04	0.03	0.05
Panel B. Restricted sample of villages						
Total transfer per registered voter	−0.12	−0.22	−0.01	−0.13	−0.25	−0.02
	(0.11)	(0.15)	(0.08)	(0.10)	(0.14)	(0.07)
Peak-end transfer midpoint per registered voter	0.64	–	–	0.71	–	–
	(0.29)			(0.27)		
Peak transfer per registered voter	–	0.63	–	–	0.70	–
		(0.29)			(0.27)	
End transfer per registered voter	–	–	0.65	–	–	0.71
			(0.30)			(0.27)
Number of villages	382	382	382	382	382	382
p-value (jointly equal)	0.05	0.05	0.05	0.02	0.02	0.02

Notes: Robust standard errors are in parentheses. See the notes to Table 1 for definitions of the main and restricted samples. The two endogenous variables in each second-stage regression are instrumented with CCT1 and CCT2 (Table 5 reports the first-stage estimates). All regressions include a constant and the control variables in Table 2. Each *p*-value corresponds to the *F*-test of the null hypothesis that the coefficients on the two endogenous variables are equal. 100 lempiras ~5 US dollars.
Source: Author calculations

per registered voter increases turnout by 0.6 percentage points.[27] We can reject the null hypothesis at conventional levels that the coefficients are equal. The results are similar for incumbent party vote share, and in the restricted estimation sample in panel B.

Naturally, it would be preferable to include three endogenous variables that measure total, peak, and end transfers. This is not possible because the number of endogenous variables would exceed the number of instruments. In any case, peak and end transfers are highly collinear.[28] If we instead specify peak transfers or end

[27] It is challenging to directly compare the magnitude of estimates to other settings with varied treatments, evaluation designs, and measures of political preference, but it does not appear unreasonably high. In Uruguay, for example, poor households at eligibility cutoff for a cash transfer were 11 percentage points more likely to support the current government; there was full compliance with the treatment offer, or lack thereof (Manacorda, Miguel, and Vigorito 2011). The monthly transfer—and therefore the peak and end transfer—was $70, implying that $5 per household increased support by 0.8 percentage points. Some households also received food cards worth $15 to $41 per month, although this was less well implemented.

[28] As an additional exercise, we included peak and end transfers in the second-stage regression—instrumenting both with CCT1 and CCT2—and excluded total transfers. Not surprisingly, given the collinearity, the standard errors were considerably larger. Given this, and the strong assumption that total transfers do not belong in the regression, we do not report these estimates in Table 6.

transfers as endogenous (see Table 6), the substantive results do not change. The strongest conclusion to emerge from Table 6 is that voters are not responsive to the total amount of transfers. However, they are responsive to larger payments in a sequence.

D. *Cognitive Bias versus Rational Updating*

When voters respond to peak-end transfers, are they necessarily succumbing to a cognitive bias? The answer hinges on understanding voters' intentions. On the one hand, they might intend to reward incumbents in proportion to the sum of transfers, but a reliance on peak-end heuristics leads them astray. On the other hand, voters may deliberately—and rationally—reward incumbents based on the amount of recent transfers. We briefly consider the evidence for each view.

Suppose that voters are intrinsically reciprocal (Finan and Schechter 2012, 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 2012, 864).[29] In such a model, voters' intentions would be to reciprocate in proportion to the total benefit received (i.e., the cumulative amount of transfers). This, in turn, would require a retrospective evaluation of the cumulative amount. Unless voters can easily recall the running sum, laboratory experiments suggest they would rely on peak and end heuristics (Langer, Sarin, and Weber 2005; Huber, Hill, and Lenz 2012; and Healy and Lenz 2014).

Some evidence suggests that voting behavior in Latin America is—at least in part—influenced by voters' intrinsic reciprocity. In lower income countries, including Honduras and Paraguay, party brokers or middlemen routinely offer favors to voters with the expectation that voters will return the favor on election day (Finan and Schechter 2012; González-Ocantos, Kiewiet de Jonge, and Nickerson 2015). Vote-buying can be sustained if the secret ballot is compromised, allowing middlemen to directly monitor and sanction non-complying voters (Stokes 2005). But unless voters are intrinsically reciprocal, it is hard to explain the stubborn persistence of vote-buying even when ballot secrecy precludes direct monitoring (Lawson and Greene 2014). As indirect evidence of this, Finan and Schechter (2014) finds that party middlemen in Paraguay strategically target favors to more intrinsically reciprocal voters, as a plausible means of lessening the commitment problem.

In other models, rational voters with imperfect information may infer the unobserved competence of an incumbent through a retrospective assessment of her performance (Rogoff 1990, Fearon 1999, Persson and Tabellini 2000, and Duch and Stevenson 2008).[30] Prior to an election, for example, increased economic output

[29] Reciprocal behavior could also be self-interested if parties and voters interact in a repeated game and voters wish to "sustain a profitable long-term relationship" (Sobel 2005, 392), perhaps by ensuring that criteria for transfer eligibility do not change. The two explanations are not mutually exclusive, since intrinsic reciprocity may enhance cooperation in a repeated game between political parties and instrumentally reciprocal voters (Sobel 2005, Finan and Schechter 2012).

[30] The discussion and citations draw on Healy and Malhotra (2013).

may signal a high-ability incumbent.[31] In related models, rational voters may be unaware of politicians' redistributive preferences for individuals or groups, and the retrospective assessment of payments allows voters to update their beliefs (Drazen and Eslava 2006, 2010; Manacorda, Miguel, and Vigorito 2011). In these models, it is plausible that voters believe that recent economic activity—such as election-year economic growth or last month's cash transfer—provides the most credible signal of an incumbent's competence or redistributive preferences (Healey and Malhotra 2013). Therefore, voters' reliance on recent activity may not be the result of cognitive bias.

To investigate this, Healy and Lenz (2014) surveyed potential voters about the weights they *intend* to place on four years of economic growth when evaluating the economy during a president's term. The typical voter declared an intention to weight years similarly (with a slight preference for later years). Even so, laboratory experiments found that subjects' judgments were consistently swayed by election-year growth rates, even when presented with four years of growth data. Most compellingly, they found that subjects' overweighting of election year growth disappeared when subjects received clearer information about the cumulative growth (or levels) of income over four years. Their evidence suggests that voters' reliance on election-year outcomes is the unintended consequence of relying on end heuristics. In a field experiment such as ours, one could randomly assign a fraction of households in CCT1, CCT2, and CCT3 to receive a clear summary of cumulative transfers just before an election. Better information might attenuate the influence of peak-end transfers, to the extent that voters had used them as heuristics for cumulative transfers.

IV. Alternate Interpretations

This section examines alternate interpretations of the voting effects. The first is that survey exposure, rather than peak-end transfers, explains the similar voting outcomes in CCT1 and CCT2. The second is that effects on CCT2 villages are partly influenced by unmeasured benefits from other sources, assuming that politicians wished to compensate for delays in cash transfers. The third is that transfers to CCT1 and CCT2 improved non-voting outcomes relative to CCT3, which in turn mediated the influence on voting outcomes. The fourth is that transfers to CCT1 and CCT2 influenced the denominator of turnout and incumbent vote share—the number of registered voters—via an effect on voters' decisions to consult and correct errors in the voter rolls.

A. *Survey Exposure*

Some households in CCT1 and CCT2 (but not in CCT3) participated in baseline and follow-up surveys. The survey included questions on consumption, income, education, and maternal and child health, among others (Bendetti, Ibarrarán, and

[31] See the review of Drazen (2008b) and the citations therein, including Persson and Tabellini (1990) and Lohmann (1998).

McEwan 2016). It was administered by nongovernment personnel affiliated with Esa Consultores, a Honduran partner of NORC at the University of Chicago. Survey participation might have influenced beliefs regarding the incumbent party's competence or redistributive preferences, independently of the transfers. Thus, it might explain the similar point estimates in CCT1 and CCT2.

All registered voters in CCT1 and CCT2 villages did not participate in the survey. NORC randomly drew 15 households from each village's roster of poor households. Using the baseline survey data from Benedetti, Ibarrarán, and McEwan (2016), we calculated that an average of 42 voting-age adults—or 7 percent of registered voters—resided in the households of each village's sample. A natural question is whether the effects of treatment assignment are moderated by potential survey exposure. To assess this, we control for Z—the within-sample z-score of the number of registered voters in a village—and interact Z with CCT1 and CCT2. Given the sample design, the percent of voters potentially 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.

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 (see Table C1 in the online Appendix). Only one interaction term is statistically significant at conventional levels, and its 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 1.8 to 2.2 percentage points, depending on the estimation sample. The pattern of heterogeneity may also be due to the correlation of Z with unobserved variables that also moderate the effects. However, there is no evidence that survey exposure alone is responsible for the effects of transfers.

B. *Endogenous Responses of Politicians*

Transfers to CCT2 were delayed because it was a control group in the original experiment (Benedetti, Ibarrarán, and McEwan 2016). Perhaps the National Party was justifiably concerned about the electoral consequences of delaying transfers in 150 villages, and chose to compensate by targeting other resources to other villages in CCT2. To assess this, we used the follow-up data from Benedetti, Ibarrarán, and McEwan (2016). Households responded whether any member of the household had received a variety of government benefits in the 12 months prior to the survey (see Table C2 in the online Appendix). As expected, 80 percent of CCT1 households but only 4 percent of CCT2 households had received a cash transfer from Bono 10,000. No more than 2 percent of households had received cash transfers from other government programs.

The survey also asked whether anyone in the households had received generic categories of benefits (e.g., a 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 a 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, Ibarrarán, and McEwan 2016). The incidence of other benefits is small and differences are not statistically significant.

The exception is a health or vaccination campaign, which was 5 percentage points more common in CCT2. One interpretation is that vaccination programs were redirected from CCT1 villages, given the expectation that young children would receive vaccinations in health centers as a consequence of the imposed health conditions. In any case, Benedetti, Ibarrarán, and McEwan (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 inputs. In summary, the clearest compensatory response in CCT2 was to swiftly disburse transfers after the follow-up surveys.

C. *Mediating Effects of Non-voting Outcomes*

A small experimental literature tests whether non-voting outcomes—including household consumption, education, and health—are influenced by lump-sum rather than evenly spaced transfers (Barrera-Osorio et al. 2011, Haushofer and Shapiro 2016, Barrera-Osorio, Linden, and Saavedra forthcoming). Our concern is that the putative effects of peak-end transfers on voting behavior are mediated by non-voting outcomes. In Kenya, unconditional lump-sum transfers (instead of monthly transfers) increased the value of non-land assets in households, but did not increase other outcomes, including non-durable expenditures, psychological well-being, education, or health (Haushofer and Shapiro 2016). In Colombia, the postponement of conditional cash transfers increased their eventual effects on secondary and tertiary education outcomes, relative to monthly distribution (Barrera-Osorio et al. 2011; Barrera-Osorio, Linden, and Saavedra forthcoming). In both cases, lump-sum transfers plausibly reduced saving or borrowing constraints.

In Honduras, one might hypothesize that durable assets and ongoing school investments were visible and constant reminders of the cumulative cash transfer (which obviated the need to rely on peak-end heuristics). However, the importance of this channel is not consistent with the Honduran results (recalling that CCT1 and CCT2 had similar effects on voting outcomes, relative to CCT3). First, the Kenyan experiment also showed that larger transfers—independently of timing—increased the value of non-land assets. Yet, our experiment found that total transfers did not affect voter behavior, suggesting durable asset accumulation cannot fully explain the results.

Second, children in CCT1 were 3.8 percentage points more likely to be enrolled in school than CCT2, by the date of the follow-up survey (Bendetti, Ibarrarán, and McEwan 2016). For school enrollment to plausibly explain voting effects in Table 3, we must assume that children in CCT2 closed the enrollment gap by the election (and that CCT1 and CCT2 maintained their advantage relative to CCT3). It is a generous assumption because CCT2 received approximately half the cumulative transfers of CCT1. The Colombian experiment did not independently vary the total transfer, although evidence from Mexico's CCT suggests that larger total grants improve education attainment (Araujo et al. 2018).

Finally, some evidence suggests that adult health affects voter turnout (Mattila et al. 2013). Bono 10,000 may have affected the health of voting-age adults via health-related conditions imposed on pregnant and nursing mothers, spillovers from healthier children to adults, and health-related expenditures facilitated by higher incomes. The first two were relatively unaffected by Bono 10,000 (Benedetti, Ibarrarán, and McEwan 2016). By the date of the follow-up surveys, mothers in CCT1 were no more likely to use health services than mothers in CCT2 (Benedetti, Ibarrarán, and McEwan 2016). Moreover, there were no effects on the health and nutritional outcomes of young children, including child hemoglobin, parent-reported child illness, vaccination rates, and anthropometric variables. There is no direct evidence on health-related expenditures, although household consumption increased by 9 percent in CCT1 relative to CCT2, for both food and non-food items. This gap likely narrowed before the election, but not entirely, given the much larger cumulative transfers in CCT1. This is inconsistent with the similar magnitude of voting outcomes in the two treatment arms.

D. *Voter Registration*

Section II noted that the number of registered voters proxies 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 (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 increased the numerators of the dependent variables, which are calculated as a percent of registered voters. Instead, let us suppose that transfers increase 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 3 might understate the magnitude of effects on turnout. To assess this, Table C3 in the online Appendix reports regressions with Z—the z-score of the number of registered voters—as a dependent variable.[32] In all specifications, the absolute values of coefficients are less than 7 percent of a standard deviation. With baseline controls, the absolute values are less than 4 percent of a standard deviation. We conclude that the estimates in Table 3 are consistent with a causal effect of transfers on the turnout of previously registered voters.

V. Complementary Evidence from the PRAF-II Experiment

Cash transfers were smaller in the earlier PRAF-II experiment, implemented before the 2001 presidential election (Glewwe and Olinto 2004, Morris et al. 2004, Moore 2008, and Galiani and McEwan 2013). This was by design, since

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

the education and health transfers were intended to compensate households for the costs of complying with education and health conditions, but not to substantially increase income and consumption. About 75 percent 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, 9).

In PRAF-II, households received up to three per child transfers of 800 lempiras 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 lempiras for each child under 3 years of age and pregnant or nursing mothers who attended health centers. Given household structure, the average household was eligible for an annual transfer of 1,127 lempiras per year, to be made in two installments of 564 lempiras (Galiani and McEwan 2013). Inflating to 2013 prices, therefore, the average household would have received peak and end payments of 1,245 lempiras. This is smaller than the catch-up payments made to either CCT1 or CCT2 households.

In the 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, 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'état). As control variables, we obtained municipal-level vote shares from the 1997 presidential elections (Departamento de Computo 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. Table D1 in the online Appendix reports municipal-level means for the CCT treatment group and the control group; it is consistent with good covariate balance for the baseline variables.

Table D2 in the online Appendix reports estimates from specifications like those in Table 3. The point estimates are negative, regardless of the specification.[35] Given the smaller sample, the estimates in this experiment are less precise. However, the 95 percent confidence intervals on turnout and the incumbent Liberal Party's vote share allow us to rule out effects larger than 1.9 percentage points. Viewed alongside results from Bono 10,000, the results are consistent with the finding that voters' responses are muted when peak and end transfers are smaller.

[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) also found no effects on turnout or vote share in the 2001 presidential election. Linos (2013) found no effect on incumbent vote share, but estimated a pooled effect across 2001 and 2005 elections.

There are two caveats. First, we do not have administrative data and cannot verify the sequence of payments actually received by voters before the 2001 election. Second, the context of the 2001 election, ultimately lost by the Liberal Party candidate, was unique (Taylor-Robinson 2003). In late 1998, Hurricane Mitch killed thousands and destroyed productive infrastructure throughout the country. Voters tend to punish incumbents 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.

VI. Conclusions

This paper analyzed a Honduran cash transfer experiment with three treatment arms: CCT1, CCT2, and CCT3. On average, the peak and end transfers per registered voters were higher in CCT1 and CC2 villages, relative to CCT3. This was also the case for voter turnout and the incumbent party's vote share in the 2013 presidential election. Two-stage least squares regressions suggest that the cumulative transfer per registered voter does not affect voting behavior, but that the peak-end transfer midpoint has an economically and statistically significant effect. Because of the collinearity of peak and end transfers, we cannot convincingly assess whether one or both are relevant. We evaluate and rule out alternate explanations for the results in CCT1 and CCT2, including the effects of survey exposure, the compensatory behavior of politicians, and the mediating effects of non-voting outcomes affected by peak-end transfers. The results are consistent with voters' substitution of peak-end heuristics for the sum of payments (e.g., Langer, Sarin, and Weber 2005). They might also indicate that voters deliberately and rationally respond to peak-end transfers as signals of party competence or preferences (Rogoff 1990, Drazen and Eslava 2006, 2010). However, the latter is not consistent with voters' stated intentions, or their behavior when given more information about cumulative economic activity (Healy and Lenz 2014).

Whatever voters' intention, the results help explain classic (but non-experimental) results in political economy that voters respond more strongly to election-year economic activity (e.g., Kramer 1971, Nordhaus 1975, Fair 1978, and Markus 1988). In addition, a small literature shows that timing of cash transfers affects non-voting outcomes, including consumption and human capital investment (Barrera-Osorio et al. 2011; Haushofer and Shapiro 2016; and Barrera-Osorio, Linden, and Saavedra forthcoming). This is the first paper to extend these results to voting outcomes. Finally, the results provide a political explanation for common shortcomings in the implementation of cash transfers, including delayed and larger-than-expected catch-up payments. Even if politicians are not directly responsible for deviations from nominal payment schedules, they have weak electoral incentives to enforce a regular sequence of smaller payments.

There are two obvious ways of building upon these results in subsequent field experiments. First, the Honduran experiment varied peak and end transfers, but they were highly collinear. In any case, there were too few instruments to separately identify the effects of cumulative, peak, and end transfers. A future experiment could systematically vary peak, end, and cumulative transfers. Second, the results provide suggestive but not conclusive evidence that voter behavior is driven by the use of peak-end heuristics. To obtain such evidence, one could systematically vary the information about cumulative transfers available to voters just before elections. If this attenuates voter response to peak-end transfers, all else equal, then it suggests that peak-end heuristics are indeed important.

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