The Effect of Conditional Cash Transfers on Voter Behavior: Evidence from Honduras

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Nandita Krishnaswamy

SUBMITTED IN PARTIAL FULFILLMENT
OF THE PREREQUISITE FOR
HONORS IN ECONOMICS

April 2012

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Acknowledgements

I would first and foremost like to extend my deepest appreciation to my adviser, Patrick McEwan, for his inspiring guidance at every stage of this project. He praised my work when it was deserved and provided thoughtful criticism where it was necessary – I am truly grateful for both.

My thanks also to Kristin Butcher and the ERS team, who made research so much fun, and who always reminded me that I was not alone in this long, and occasionally frustrating, process. Their feedback and encouragement played a big part in the success of this endeavor. Pinar Keskin, Robin McKnight, Kartini Shastry and Akila Weerapana weighed in with comments and suggestions that proved invaluable. I am also grateful to my teachers Adrienne Lucas, Susan Skeath, David Johnson and Dan Fetter for deepening my love for and understanding of Economics.

I want to thank Alan Shuchat in the Mathematics department for agreeing to read this thesis and be my thesis visitor. Carolin Ferwerda and Megan Brooks provided guidance and support in data collection and analysis.

I would like to thank the Jerome A. Schiff Fellowship Committee for providing me with financial support and recognition for this thesis.

I owe so much to my friends in Wellesley and beyond for the thesis-writing company, the laughter and the distractions. A final word of thanks to my parents, Radha and Mahesh, who have supported everything I have done over the years – I could not have done this (or anything else, really) without you.
Abstract

The choices that voters make are often influenced by economic circumstances. On a macroeconomic level, better economic indicators are often correlated with better outcomes for incumbents in elections. On a smaller scale, programs that provide fiscal support to poorer households have been shown to increase support for incumbent governments. There are a number of reasons why this might be the case—voters may reward incumbents for benefits received, or they may regard such programs as signals of the incumbent government’s investment in their well-being. Conditional cash transfer (CCT) programs are one such type of fiscal intervention. In the past decade, an increasing number of Latin American governments have implemented such programs, which distribute sums of money to targeted households that meet certain conditions such as school enrolment and attendance for school-aged children, and regular health checkups for younger children. In this paper, we consider the effect of one such program, PRAF, on voting outcomes in Honduras. The administration of the program is such that it allows for two empirical strategies: the randomization of treatment among eligible municipalities sets up an experimental estimation strategy, and the threshold of eligibility for the program provides scope for a regression discontinuity estimation at the threshold. Discontinuity estimates indicate that eligibility had a positive effect for the incumbent at the threshold, as expected, but experimental estimates indicate that among the eligible group, those who receive the program are more likely to support the opposition party, perhaps due to administrative difficulties in the distribution of the cash transfers.
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1. Introduction

Economics can play a significant role in the decision to cast a vote. Often, individual economic circumstances (that are perceived to somehow be linked to government policies or programs) conflict with pre-existing political ideologies in an individual’s choice of candidate. It is therefore interesting to know to what extent fiscal boosts from the government can influence voters’ political decision-making processes. A number of models of voter behavior have been developed, all of which try to provide some intuition for this answer to this question (Key, 1966; Downs, 1957; Drazen et al., 2006).

The link between fiscal interventions and political behavior is difficult to test empirically for two main reasons: (1) it is impossible to obtain accurate data on the final political decision made by the individual – that is, which candidate he or she votes for, and (2) government transfers are often targeted at a particular group of individuals, either because they are simply in greater need of such transfers or because the government favors them (for example, they may be swing voters who may be persuaded by the transfer to support the incumbent in the next election).

However, the relatively recent implementation of conditional cash transfer (CCT) programs in Latin America has provided us with a ready-made framework to analyze the effects of fiscal transfers on voting behavior. CCTs have implemented in the region with the aim of improving human capital outcomes, typically schooling and health. In CCT programs, cash transfers that amount to anywhere from 7-30% of household expenditure are given to households that meet certain conditions of school enrolment and attendance for school-going children, and various health requirements, such as regular health checkups (Schady and Fiszbein, 2009). CCTs are usually administered in a way that will allow the government or the NGO in charge to evaluate whether the program has achieved its developmental goals. In some cases like that of Progesa, a widely-known Mexican CCT program that served as a model for numerous subsequent programs in Latin America, treatment was initially randomly assigned to a
subset of the eligible population for an evaluation period. This makes an experimental approach viable to test the effect of the program. The program PANES in Uruguay, among others, was targeted on the basis of an eligibility score (determined based on an estimated income level) below which a household is granted the transfers, allowing for a regression discontinuity to be estimated at the threshold of eligibility to test for the effect of the program on the outcome of interest.

These methods can also be used to test the effect of CCTs on voting behavior. In these countries (Mexico: De la O, 2009; Uruguay: Manacorda et al., 2010), programs have shown positive and significant effects on incumbent vote share. However, results are not easy to generalize, even to other countries in the region, because the scale, administration, chronology and targeting of each CCT program is unique.

In this paper, we consider the effect of PRAF-II, a CCT program implemented in 1999 in Honduras, on the vote share in the 2001 national elections of the Partido Liberal, the incumbent party. Honduras is one of the poorest countries in the region, and its two key political parties (which together consistently secure more than 95% of the vote share) are not significantly ideologically polarized. People in the poorest regions of Honduras may then vote according to whether a political party has given them or will give them economic benefits that allow them to put food on the table more easily, rather than voting according to personal political ideologies. For these reasons, it seems likely that Honduras is an appropriate context in which to expect that voting may be significantly influenced by a fiscal transfer program like PRAF-II.

PRAF-II gave families with children below the 4th grade a cash transfer conditional upon enrolling their children in school and having them attend school regularly. The idea was that the transfer would cover the implicit and explicit costs of the child attending school, and provide some incentive for children, who would not otherwise go to school, to attend. An additional health transfer was granted to pregnant women and mothers of young children, conditional upon regular health checkups. In total, the
transfers amounted to roughly 9% of pre-transfer consumption (Schady and Fiszbein, 2009). The implementation of PRAF-II in 40 randomly chosen municipalities of the 70 that were initially eligible creates comparable treatment and control groups. We use these groups and the 1997 election results as a baseline for an experimental study. Under effective randomization of the transfers to 40 of the 70 eligible municipalities, the 40 treatment municipalities are comparable to the 30 eligible but untreated municipalities, and any differences in political outcomes between the two groups of municipalities can be attributed to the program. Data from the 2001 national census indicate similar demographics in treatment and control municipalities, supporting our assumption that these two groups of municipalities are comparable.

However, eligibility itself was assigned using a threshold average Height-for-Age Z-score (HAZ score) for first-graders in the municipality, a proxy for chronic malnutrition. We use this useful administrative detail to run a regression discontinuity at the threshold of eligibility. The regression discontinuity treats municipalities with average HAZ scores just above the cutoff as comparable to those with average HAZ scores just below the cutoff. The latter group of municipalities is eligible for the program, and the former is not, which means that any differences in political outcomes between them can be causally attributed to the program. The validity of this design relies on the arbitrary selection and strict enforcement of the threshold. If this does not hold, then the concern is that municipalities self-select into the program. This could make just-eligible municipalities inherently different from just-ineligible municipalities in ways that could affect political outcomes, which would bias our results.

The advantage of the setup of the program for our design is that the HAZ scores were measured before the program was implemented, and it is highly unlikely that anyone could actively choose to fall on either side of the threshold and in so doing, bias our results. Neither, arguably, would a first-grader see the need to manipulate his or her HAZ score. Further, even if an individual could manipulate his or
her HAZ score, the HAZ score of one individual would have little to no effect on the aggregate HAZ score of the municipality, which determines whether or not a municipality would be eligible.

The two empirical strategies allow us to test the effect of the program on voter behavior along two dimensions – eligibility and actual transfer receipt. To preview the results of this paper, the effect of the program on incumbent vote share is slightly negative and insignificant across all levels of Honduran elections – presidential, congress and mayoral. What is surprising is that there is a positive and significant effect of the program on the key opposition vote share, and we suggest that this phenomenon could be due to the incumbent Partido Liberal losing support and votes in the 2001 election due to inefficiency in transfer administration. Most of this effect seems to be a result of the voting patterns of people in what seem to be slightly richer municipalities, for whom the transfer may have been a less significant portion of their monthly expenditure. The effect persists somewhat till 2005.

The regression discontinuity estimates fit in better with our hypothesis that the program garnered the incumbents a greater measure of support – coefficients on eligibility are positive and significant. One potential explanation for the difference between these and the experimental results is that the political decisions of people in the eligible but untreated group of municipalities were perhaps also affected by the program due to some anticipatory effect.

The structure of the paper is as follows: Section 2 reviews the relevant literature on schooling and health outcomes of CCT programs and the empirical challenges of studying the effect of fiscal interventions, and CCTs in particular, on political outcomes, and evaluates studies done on the links between CCTs and politics in other Latin American countries; Section 3 describes the Honduran political context in the timeframe of our study; Section 4 describes the PRAF-II program in greater detail; Sections 5 and 6 discuss the relevant data and our two key empirical strategies; Sections 7 and 8 discuss results and robustness checks; Section 9 concludes.
2. Literature Review

2.1 Latin America’s Experiences with Fiscal Interventions

Conditional cash transfers became a popular form of fiscal intervention in Latin America in the late 1990s. The wave of cash transfer programs began with the Progresa/Oportunidades program in Mexico, which reached one-ninth of all Mexican households within two years of implementation (Skoufias, 2005), and the Bolsa Escola/Bolsa Familia program in Brazil, after which almost every other government in the region proposed or implemented a similar program. These programs have also garnered much support from the Inter-American Development Bank (IDB) and the World Bank (Handa and Davis, 2006). Programs have varied in duration, scale, size and frequency of cash transfers, and methods of compliance verification.

CCT programs reward households that actively engage in building human capital by sending children to school, or getting regular health checkups. The transfers are also meant to compensate families for the opportunity cost of sending children to school and taking time off work to have health checkups, and the direct costs of school supplies and transportation to health centers. Conditional cash transfers have usually included a significant monitoring and evaluation component, which in turn has led to the implementation being deliberately randomized across geographic areas. Among all programs in Latin America, PRAF, in fact, had one of the highest allocations of its funding towards monitoring and evaluation – around 35% of its budget (Schady and Fiszbein, 2009).

The evaluations of various cash transfer programs in Latin America are summarized below. Because the nature, timing and administration of each program are different, there is no common magnitude of effect that we would expect across countries. However, a general trend seems to be that CCT programs do some good in areas of health and schooling, increase consumption and reduce both acute and chronic malnutrition.
2.1.1 Mexico: Progresa/Oportunidades

*Progresa* began in 1997 with 140,000 households (Behrman, 2005), and has since been expanded to cover 5 million households. It had three components – a health intervention, a nutritional intervention and an educational intervention. On average, the transfers amount to around 25% of rural household income, and 15-20% of urban household income. *Progresa* was one the first programs of its kind to focus on purely cash-based interventions rather than the direct provision of certain food groups. Transfers were conditional on school attendance, giving infants micronutrient supplements, mothers attending sessions on health and nutrition, and family members attending regular health checkups. Compliance was carefully monitored, and full penalties enforced when necessary. Treatment was randomly assigned in the first 18 months of treatment among 506 rural communities, with approximately 300 treatment groups and 200 control groups. Within each community, household eligibility was determined by a poverty index (Behrman, 2005). Control groups began treatment in 2000.

As one of the earliest and largest CCT programs, *Progresa* has been carefully examined, particularly to evaluate its effects on human capital accumulation. Behrman and Hoddinott (2005) used panel data to estimate that the treatment group showed an increase in stature of more than 1cm per year, which translates to about 15% of growth. The predicted probability of stunting for the treatment group was also 66% lower. They also highlighted the need to control for unobserved heterogeneity at various levels because of the potential for non-random allocation of program resources. For example, a nutritional supplement shortage in Mexico caused administrators to favor children who were suffering from more severe malnutrition. As expected in this scenario, Behrman and Todd (1999) found that assignment was random at the community level, but not at the household level. That is, households in control municipalities did not receive the transfer, but that household selection within an eligible municipality was not random, which could have affected the external validity of the evaluation.
However, enrollment rates and schooling gap rates were similar in both the treatment and control groups.

In terms of the transfer linked to schooling, Behrman et al. (2009) found that the program resulted in an increase of 8-9% in grades completed. Schultz (2000) and Skoufias (2005) found similar positive results on grade completion and school enrollment. However, grade completion and other measures of continued school attendance did not translate into better academic performance. Six years after treatment, despite the positive results on grade completion, Behrman (2010, 2011) found no effect on achievement tests. He points out, however, that since Progresa encourages children who would otherwise not have attended school to enroll, selective enrollment could have affected average test scores in treatment districts, which could have dampened a significant effect.

2.1.2 Brazil: Bolsa Familiar/Bolsa Escolar

Brazil’s CCT program was of a scale similar to Mexico’s – it reached 11 million families, around 25% of the population. It, like Progresa, required that children aged 6-15 achieve 85% school attendance, although this was only over a three-month period, compared to Progresa’s annual check. Each household received R$ 15 per child, up to a maximum of R$ 45. A period of geographic targeting was carried out, with municipalities in micro-regions with a Human Development Index of less than 0.5 were eligible for the program (Denes, 2003). Households were identified through means-testing. However, means-testing was unverified, presenting the possibility of inaccurate eligibility measures¹. Additional conditions included vaccine and health checkup schedules for children and expectant and lactating mothers, and school enrollment. In general, Bolsa had less of an emphasis on conditionality monitoring, and was more focused on reducing short-term poverty (Soares, 2010).

¹ Formal sector employees’ earnings were cross-checked against a federal database, to provide some minimal verification (Soares, 2010).
Because *Bolsa* was not a randomized program, Propensity Score Matching (a method that compares households in the treatment group to similar non-beneficiary households) was used to evaluate the effects of the program on educational outcomes. A baseline survey was conducted immediately after the program started, with a follow-up survey conducted in 2005, and a nutrition survey conducted in 2007.

An evaluation of *Bolsa Escolar* highlighted a tremendous fall in school dropout rates from 10% to 0.4% (Denes, 2003), a result that was echoed by Bourguignon et al. (2002). Similarly, Soares et al. (2010) found that *Bolsa Familia* (into which *Bolsa Escolar* was integrated) improved education outcomes and concluded in addition that it had no negative impact on adult labor market outcomes – that is, it did not result in a disincentive to work. However, they found no significant effects of the program on chronic malnutrition (measured by HAZ score) and acute malnutrition (measured by age-adjusted Body Mass Index) for children under the age of 7 (Soares, 2010). In fact, there was a slight negative effect of the program on the former outcome for children under the age of 5. Similar results from *Bolsa Família’s* predecessor, *Bolsa Alimentação*, were attributed to families’ fear that they might be excluded from the program if their children were to gain weight (Morris et al., 2004). A survey conducted on more recent data from the 2007 survey similarly showed no effect of *Bolsa Familia* on stunting or wasting of children aged 1-3 years.

### 2.1.3 Uruguay: Plan de Atención Nacional a la Emergencia Social (PANES)

The Uruguayan economy suffered an economic crisis in July 2002, in which per capita GDP plummeted by 11.4% in just a year (Amarante et al., 2009). After a new government took power in 2005, it quickly designed the *Plan de Atención Nacional a la Emergencia Social* (PANES), a temporary program to relieve families who had suffered severe losses in the financial crisis. In the longer term, it was hoped that PANES would build the human and social capital of the poor. PANES comprised three different...
interventions – a food aid card worth US$ 20 - 40 (depending on the number of children and pregnant women in the household), a household monthly cash transfer of US$ 56 (regardless of the size of the household), and an additional voluntary workfare program that offered training (Amarante et al., 2009). PANES was one of the most generous programs of its kind, being both unconditional and awarding a transfer roughly equivalent to half of average pre-program household income. The additional food aid transfers were awarded if there were children or expectant mothers in the household.

The PANES treatment was assigned based on a predicted income score calculated using baseline survey data taken from 188,671 applicant households. Issues of misreporting and bad short-term macroeconomic forecasts led to the predicted income being used instead of current income measures, which might have underestimated permanent income. The threshold eligibility score was independently determined by the Social Security Institute. Manacorda et al. (2010) report that 97% of eligible households report receiving PANES transfers, indicating that PANES was well-administered. PANES eventually reached almost 10% of all Uruguayan households. The threshold of eligibility lent itself to a quasi-experimental empirical strategy, in which a local average treatment effect was estimated at the threshold. In order to use this evaluation method, 3000 households that fell within 2% of the eligibility cutoff were surveyed again in 2006, 2000 of which had benefited from the program.

Although PANES was an unconditional program, the Uruguayan government expected there to be effects on child labor supply and school attendance, since the transfer would reduce the need for children to contribute to household income. However, Amarante et al. (2011) did not find any such effect, and suggested that cash transfers alone cannot reduce child labor outcomes. Amarante et al. (2008) also did not find significant effects on self-reported health. They found only modest positive effects of the program on health checkups for children less than 6 years of age and women of child-bearing age. Considering the generosity of the program, its lack of effect on schooling, labor and health
measures indicates that conditionality plays an important role in long-term effects of cash transfer
programs on human capital development.

2.1.4 Honduras: Programa de Asignación Familiar (PRAF)

PRAF was a program implemented in 2000 by the then-incumbent Partido Liberal of Honduras. The program granted families a sum of 55 lempiras per month (US$ 40-50 per year) for each pregnant woman or child under 3 years of age for regular visits to a health clinic, and a sum of 800 lempiras (US$ 50-60) per year for each child between the ages of 6-12 who regularly attended grades 1-4. The transfer amounts averaged around 7% of pre-transfer consumption, which is slightly less generous than other CCT programs in Latin America (Schady and Fiszbein, 2009).

The program used the average Height-for-Age Z-score of first-graders by municipality to identify the 70 poorest municipalities in the country. The municipality average of children’s HAZ score is a good indicator of the prevalence of chronic malnutrition in the municipality. The 70 municipalities all had a HAZ score of below -2.304. Of the 70 eligible municipalities, the program was randomly assigned to 40, to allow for the creation of a treatment group and a control group comprising the remaining 30 municipalities. Glewwe and Olinto (2004) and Galiani and McEwan (2011) assert that random assignment was largely successful in this program, with similar baseline characteristics for treatment and control groups, and low attrition rates. The cash transfers were also well-executed, with 79% of households eligible for the nutritional transfer reporting receipt (Morris et al., 2004). However, transfers were only distributed bi-annually, rather than monthly. A more detailed description of PRAF is given in section 4.

Moore (2008) points out that according to evaluation surveys carried out before the transfers and then again after the first two rounds of transfers in 2000, the program increased average school attendance by 4.3-4.6 percentage points and enrollment by 17 percentage points - nearly an additional
school day per month. Glewwe and Olinto (2004) find a positive effect of PRAF on school enrollment of 1-2 percentage points, a reduction in the dropout rate of 2-3 percentage points, and increased school attendance of around 0.8 days per month, conditional on enrollment. They find that the program has had significantly larger effects on poorer households, although they find no significant effect on child labor. Galiani and McEwan (2011) estimate a larger effect of the program, using the same empirical strategy but using control variables from the 2001 Honduran census. They conclude that children in treated municipalities were, on average, 8 percentage points more likely to enroll in school and 3-4 percentage points less likely to work than their counterparts in the control municipalities. In addition, they use two different discontinuity designs – one based on the initial targeting rule and another based on municipal boundaries – to estimate local average treatment effects in the neighborhood of the cutoff. They find no significant differences between eligible children on either side of the cutoff in the first RDD, which could be an indication of the lack of effect of the program among the richer of the eligible municipalities. However, the second regression discontinuity design, based on geographic boundaries between municipalities that were eligible and municipalities that were ineligible, produced estimates resembling the experimental estimates, though of smaller magnitudes.

Morris et al. (2004) considered the effect of the program on two health outcomes – the use of health services (primary) and immunization and growth monitoring interventions (secondary). Children’s visits to health centers increased by 15-21 percentage points and the proportion of expectant mothers who received 5 or more pre-natal health checkups increased by 18-20 percentage points (Morris et al., 2004). However, according to the evaluations, these increases did not translate into better overall health outcomes, such as increased rate of immunization against measles and tetanus.
2.1.5 Colombia: Familias en Acción (FA)

*Familias en Acción* was a social welfare program set up to promote human capital accumulation in rural parts of Colombia along three dimensions – health, nutrition and schooling. Health supplements were dependent upon participation in a vaccination program and growth and development checks for children, and a health education program on nutrition, hygiene and contraception for their mothers. Conditional upon satisfying the health supplement requirements, families with children under the age of 7 were automatically eligible for the additional nutritional supplement without further conditionality. The school supplement gave mothers grants for keeping their children in school for a minimum amount of time, and was set at 12,000 pesos for primary school-going children and 24,000 pesos for secondary school-going children in 2001, and raised a year later to 14,000 and 28,000 pesos respectively.

Selection into the program was based on four criteria: eligible municipalities had to have a bank and a population of fewer than 100,000 people and have basic health and education infrastructure, and local authorities had to register the municipality into the program and provide information about its residents (including beneficiaries of other concurrent programs). In addition, the municipality could not be in the coffee region, which had already been allocated special resources. Within the municipality, the poorest quintile of households (assessed using a nation-wide survey in 1999) with children 0-17 was eligible to participate. By October 2002, 407,076 households in 622 eligible municipalities were registered and eligible, and 89% of those families received transfers (IFS Report, 2004).

Evaluations of *Familias* indicate that it had positive effects for beneficiaries. Attanasio et al. (2005b) use a differences-in-differences approach and find a 15% increase in average consumption due to the program, and an overall increase in the quality of food consumed within a year of implementation. A follow up study by Attanasio et al. (2005c) that compared eligible municipalities to similar (in baseline characteristics) ineligible municipalities finds a 6.9 percentage point decrease in
chronic undernutrition for children under the age of 2 due to the program, but no positive effects on the health of older children. Older children (aged 14-17) in beneficiary municipalities do show an increase in school attendance of 5.5 percentage points in both rural and urban areas (Attanasio, 2005a) relative to their non-beneficiary peers. Among younger children, effects are smaller and insignificant in urban areas, perhaps due to pre-existing high enrollment rates.

**2.1.6 Chile: Chile Solidario (CS)**

*Chile Solidario* was a cash transfer program that focused on improving labor market outcomes in target areas. Psycho-social support was provided to participating households by a social worker, whose main role was to connect families with a network of social and employment services. For example, the social worker would help families register in healthcare centers, enroll children in school, provide identity cards and access to improved housing conditions. The program was therefore not uniform across households and indeed, the unique nature of the program stemmed from the personalized attention of the social worker to individual and family circumstances. Participation in the program was conditional upon families signing contracts to make good-faith attempts to address what they (with the help of the social worker) identified as key causes of their poverty (Borzutzky, 2009).

An additional small cash transfer was provided to offset the potential costs of participating in the program. The transfer amount decreased over time, with the expectation that families’ financial situation would improve concurrently. The sum of this transfer and all other small transfers the families would have been eligible for amounted to only 2% of their median income. By the end of 2005, the program had reached 225,000 families. On the whole, the program was a small-scale endeavor that reached only 5% of the population.

CS targeted individuals using a proxy means test, and each municipality had a different threshold test score for eligibility. This allowed regression discontinuity evaluations to be conducted
across a continuum of means test scores. That is, individuals in a given municipality whose means scores were just below the threshold were compared to individuals in that same municipality whose means scores were just above the threshold. Means test score matching of treated households with untreated households also allowed for experimental evaluations to be conducted.

An experimental labor market evaluation (Carneiro et al., 2009) indicates a definite increase in the uptake of employment programs and subsidies, but no effect on probability of employment or income of the head of the family. Under a means test score-matching quasi-experimental evaluation, the program appears to have caused a significant increase in pre-school enrolment of 4-6 percentage points (Galasso, 2011) between 2002 and 2004, consistent across urban and rural areas. School enrolment among 6-15 year olds also saw a 7-9 percentage point increase, and adult education-completion programs saw a 4-5 percentage point increase in take-up due to the program. In terms of health, participating households had a higher likelihood of being enrolled in the public health system, but no other health outcomes appeared robustly affected by the program. A longer-term evaluation of labor market outcomes based on the matching technique and a broader dataset found positive effects on income and employment outcomes for individuals, as well as on take-up of post-natal care and dental examinations, suggesting perhaps that the effects of such programs take time to manifest.

Evaluation of Chile’s program cannot be compared directly with those of Progresa or Bolsa because the transfer amount was so insignificant that nearly all the effects we see here are not a result of a direct monetary incentive to the families. Rather, the bulk of the effect was likely due to families’ improved knowledge of and access to social and employment services.
2.1.7 Nicaragua: Red de Protección Social (RPS)

The Red de Protección Social was first implemented in Nicaragua in 2000, and expanded in phases over the next six years. The pilot phase was implemented in just six municipalities, and the program expanded to 53 municipalities in 2002. The program focused on increasing household food consumption through cash transfers, reducing dropout rates among elementary school children and improving health indicators for children below the age of 5 (Maluccio and Flores 2005).

In the pilot program, half of 42 eligible localities were randomly selected to receive the cash transfers, and the other half formed a control group for evaluation. Beyond the pilot phase of the program, eligible localities were chosen based on a poverty index created using the 1995 Housing and Population Census. While targeting was initially purely geographic (that is, all households in the eligible municipalities could receive transfers), around 5% of households were excluded before transfer distribution for having “substantial resources” (Maluccio et al. 2006). Evaluations were carried out using a panel survey administered to randomly selected individuals in the treatment and control regions in 2000, before the Phase I transfers were distributed, and again in 2003, when the Phase I transfers stopped.

On average, RPS supplemented annual per capita expenditure by 18%, and surveys show that most of this increase was channeled towards purchasing food, as the administrators hoped. Funds were channeled into short-term consumption rather than long-term investment (Maluccio et al., 2010).

Education expenditures increased as well, and education outcomes saw some of the most dramatic improvements of all indicators evaluated by Maluccio and Flores (2005). The program led to a 13 percentage point increase in school enrollment and a 20 percentage point increase in current school attendance, coupled with a 5.6 percentage point decrease in the number of 5-13 year olds who worked.
This seems to suggest that the opportunity costs of school attendance had previously been a significant barrier to school enrollment and attendance, and that these costs were adequately covered by RPS. Maluccio and Flores’ comprehensive early evaluation study also finds significant improvements in grade advancement. These improvements seem to have triggered some supply-side improvements in schooling as well – a 2006 follow-up evaluation indicates that grade availability, number of school sessions per day and number of teachers all increased as a result of the program, potentially as a response to demand-side triggers.

Effects on health are less dramatic, but still significant. The program resulted in a 5.5 percentage point decline in the number of stunted children (Maluccio et al., 2005) and a large and significant increase in vaccination coverage (Barham et al., 2008).

2.1.8 Ecuador: Bono de Desarrollo Humano (BDH)

*Bono de Desarrollo Humano* (BDH) was launched to improve school and health outcomes in impoverished regions of the country. However, in order to minimize compliance monitoring costs, the program was technically unconditional. It was launched with an extensive media campaign encouraging beneficiaries to send their children to school and attend health centers for regular checkups. According to Oosterbeek et al. (2008), surveys indicated that 1/3 of program beneficiaries thought that the program was conditional. BDH provided monthly transfers of approximately US$ 15. This corresponded to 9% of the median beneficiary household’s pre-transfer expenditure (Schady et al., 2008), which is roughly the same size as the PRAF transfers.

When BDH was initially launched in 1999, eligibility criteria were not clearly defined. When the program was retargeted in 2003, household eligibility was determined using a proxy means test. Of the eligible households, half were randomly assigned to receive the transfers, allowing for an experimental evaluation. Evaluations of BDH were done based on surveys of schooling and labor decisions of
individuals in treatment and control households, and estimates of household assets and expenditures. The threshold of eligibility for the program also provided scope for a regression discontinuity estimation of program effects at the threshold.

Experimental evaluations of BDH estimate that the program resulted in a 10 percentage point increase in school enrolment rates for children aged 6-17 (Schady et al., 2008). A regression discontinuity estimation provided similar estimates of around 10 percentage points at the threshold of eligibility (Oosterbeek et al., 2008).

Perhaps because of the unconditional nature of the program, experimental evaluations by Paxson et al. (2007) concluded that the program had no effect on young children’s visits to healthcare centers, but that their overall nutrition improved (probably due to increases in quantity and variety of food consumed). Similarly, children’s social and emotional development improved due to the program, which is a striking endorsement of the wide-reaching benefits of CCTs. However, Ponce and Bedi (2008) and Paxson and Schady (2010) find little to no effect on cognitive outcomes.

The Ecuador CCT shows that conditionality is important. The Schady and Araujo study (2008) concludes that the school enrolment effects they estimate are larger for individuals who were under the impression that the transfers were conditional. On the other hand, while health center visits did not show significant change (presumably because they were not required and because they have a relatively large opportunity cost in terms of hours of labor lost), the transfers themselves improve nutrition and consequently, overall health.
### 2.2 Fiscal Interventions and Political Outcomes

The distribution of fiscal interventions is closely tied to political support. On a macroeconomic level, a country’s economy being strong has been shown to correlate positively with how incumbents fare in elections (MacKuen et al., 1992; Alesina, 1993; Pacek, 1995). While this correlation is significant, it is difficult to address issues of causality, given the number of omitted factors that can affect both macroeconomic cycles and political outcomes – administration of economic policy, for example. People’s and households’ financial situation is also linked to the country’s general economic health to very different degrees. Therefore, while most people are likely to be in a better financial position when other macroeconomic indicators are good, it is impossible to estimate how direct improvements to households’ financial situations affect their view of the incumbent government and opposition parties.

To address some of these issues, our particular question deals with specific government interventions that provide transfers to households directly, rather than interventions that influence income levels through macroeconomic cycles. Numerous models have been written to describe how people think about voting, in relation to receiving a direct individual or household transfer. These models assume there is no inherent ambiguity in how much of the transfer reaches the beneficiaries. This fits particularly well with the concept of conditional cash transfers, which are direct payments from the program administration to the household representative.

A classic model first developed by Key (1966) is one of rewards to the incumbent for programs already implemented, and by extension, punishments for programs that are implemented at the expense of that voter’s community. Anderson (1988) points out that this reward-punishment system is most applicable when responsibility for a particular economic policy or situation is easily attributed – generally in strong two-party systems, like in Honduras. Most importantly, the quality or ideology of the
opposition party does not factor into this particular model, again fairly applicable in Honduras where the main opposition party is ideologically similar to the incumbent.

However, there is a great deal of ambiguity in how conditional cash transfers, in particular, affect patterns of voting and political outcomes. Such interventions are generally targeted at poorer areas of the country, and Diaz-Cayeros et al. (2007) contend that less-well-off voters are more likely to respond to cash transfers than to focus on party histories and ideologies - what he calls “symbolic appeals”. This is especially true in Honduras, where as a result of a tumultuous political and economic history, “ordinary [citizens] are much less committed to democratic institutions than most other Latin Americans” (Ruhl, 2010). What this implies is that political ties are less rigid in the case of Honduras. If this is true, Hondurans’ political affiliations are much more likely to change as a result of programs such as PRAF. While those who benefit from the program are more likely to support the incumbent party, those who do not may, out of resentment, choose to support challengers from other parties instead – the “punishment” aspect of the model. In addition, in the case of CCTs, assignment rules are kept a carefully guarded secret in the initial stages of the program in order to prevent manipulation of the system – which in turn is perceived among non-beneficiaries of the program as a frustrating lack of transparency (de la Brière, 2006), which leads to a more significant “punishment” effect.

Drazen et al. (2006) develop a model in which voters use a fiscal intervention as a signal that their political representative is acting in the best interest of the eligible group, and therefore alter their political preferences towards the incumbent in response to an expected future benefit. This could have two implications in the case of CCT programs. Particularly in the case of experimental CCT programs, non-beneficiaries of fiscal interventions also assimilate the information that the incumbent party is working to help the ordinary citizen, and could then choose to support the incumbent party in the hope of receiving future benefits. This would dampen any positive effect on incumbent support due to the
program. The second potential effect of applying this model of voter behavior is that people receiving the transfer would increase their support for the incumbent in future elections, which may lead to some persistent effects of the program in the future election years. The presence of a persistent effect would differentiate this model of behavior from the reward-punishment Keysian model.

Another famous model by Downs (1957) is one of prospective voting in which people focus on the future economic policies that the candidate promises to enact. The Downsian model tends to better suit opposition parties rather than the incumbents, since the only information the public has about the priorities of the challengers is through the policies favored by the challengers. In the case of Honduras and Mexico, in particular, opposition parties made it clear that the program would continue under their power if they were elected, which implies, under the prospective model of voting, that the opposition may have reaped some benefit from the program, which could mask any positive effect of the program on popularity of the incumbent among the electorate. Our empirical strategy can shed some light on what type of model may be applicable in the Honduran context.

The relative ease of access to transfers and high take-up rates of conditional cash transfers indicates that these programs will be a good way to test the applicability of such models. Governments often set up transfer distribution centers (banks, ATMs etc.) in a way that allows everyone who is eligible to have access to the transfer. In Honduras in particular, conditionality enforcement was lacking, which meant a fairly low cost to receiving the transfer. The school intervention was essentially conditional only on school enrollment, while health intervention condition-enforcement was really only implemented in late 2001, after the second round of transfers (Morris et al., 2004). This implies that Honduras may fit in even better with the existing voting models than some of the more stringent cash transfer programs.
A large body of literature deals with the issue of how fiscal interventions are distributed (Cox and McCubbins, 2006; Lindbeck and Weibull, 1987; Dixit and Londregan, 1995, 1996). This class of models focuses particularly on different groups of voters within a single district and the best ways to target such interventions to maximize vote share for the incumbent, or to maximize legislative leverage. However, this literature is much less relevant to the study of cash transfers, since many CCT programs are jointly administered by external organizations such as the Inter-American Development Bank, which implies greater accountability and less scope for program manipulation on the part of the incumbent. Furthermore, CCT programs are often allocated at the municipality or village level, which implies that intra-district manipulation does not occur. We later show this in the case of Honduras, by illustrating that a number of control variables that could affect voting patterns are in fact similar for treatment and control municipalities.

2.3 The Empirical Study of Fiscal Interventions

Empirical analysis of political support is challenging. Fundamentally, incumbents are not homogeneous. For instance, the local government in a particular municipality may appear to spend more and make larger fiscal interventions, but may actually be known to be siphoning off money into its own pockets – larger “spending” does not necessarily lead to an increase in the provision of public services. The same party’s government in another municipality might be re-elected to reward exceptional governance and a significant increase in the provision and efficiency of public services. Similar to this form of geographic heterogeneity, the quality of incumbent representatives does not stay constant over time. Therefore, fluctuations in political outcomes over time are clearly not uniform across regions, and need not even be in the same direction from one region to the next. This tends to make clear trends difficult to identify.
Research shows that in many countries, fiscal interventions are often targeted specifically towards districts with a large number of “swing voters”, in the hope that they can be persuaded to vote in favor of the incumbent, and sometimes even towards groups of “core voters” with the aim of increasing their participation in the forthcoming elections. Furthermore, with the generally increasing trend of democracy in the region, politicians now have an even greater incentive to target these programs non-randomly. This phenomenon is not new, nor is it uncommon, but its main implication for empirical study is that people who receive fiscal benefits are more likely to have views aligned with that of the incumbent party, and may, in addition, differ from non-beneficiaries in demographic and other unobserved attributes. In these cases, we cannot directly compare the outcomes of beneficiaries with those who did not benefit from the fiscal interventions.

The lack of political data is another constraint we face in empirical analysis. In order to ensure fairness of elections and to protect voters, many governments have mandated that voting be anonymous. Because of the fact that individual-level data on voting behavior is difficult to find, most studies have relied on individual surveys that capture political preference, or have used anonymous voting data aggregated to the smallest level at which they are available.

Further, given the difficulty of effectively randomizing household income levels in the absence of programs like CCTs, some studies have considered the larger-scale distribution of federal funding. Litschig and Morrison (2010) consider federal funding to local governments and its effect on Brazilian elections as far back as 1988. Funding was allocated based on a discontinuous population rule, and so the authors implement an RD design under the assumption that municipalities are unable to precisely control their populations in order to get additional funding. They find that a 20% increase in spending per capita resulted in a 10 percentage point increase in the probability of re-election of the incumbent party at the municipal level. In another approach to studying federal funding distribution, Levitt and
Snyder (1997) consider the distribution of federal funding in the United States, and find positive effects on incumbent vote-share in congressional elections.

However, the advent of CCT programs has provided a useful alternative to analyzing the effects of large-scale federal funding on voting behavior. Federal funding may be budgeted in different ways in different districts, and households in a given district can benefit both directly (through channels like school subsidies) and indirectly (better roads and cleaner public spaces) from such funding. This makes it difficult to pinpoint the channels through which federal funding affects individual political support. CCT programs have the added benefit of being direct household-level transfers, which gives a more accurate estimate of the effect of a boost to household income on political outcomes, since there is no ambiguity in how much of the transfer reaches the household.

2.4 The Empirical Study of CCT Programs and Their Political Impact

Many CCTs are administered with a view to evaluating their effects on developmental outcomes. Some CCT programs and the results of their evaluations are summarized in section 2.1. Many such programs are evaluated using an experimental design, which involves the creation of a comparable treatment group which receives the cash transfer, and a control group, which is indistinguishable from the treatment group except for the fact that it does not receive the cash transfer. In some cases, the CCT is randomly assigned to some geographic area, with a time lag before it is assigned to control groups, which allows us to estimate a plausible counterfactual to the program. A differences-in-differences type specification, for example in the Honduras and Mexico cases, would help to estimate the causal effect of the program on schooling and health outcomes. Extending such a strategy to political outcomes is therefore a viable empirical strategy that eliminates most of the concerns we have listed in the previous section.
Other CCT programs are administered according to some threshold eligibility rule (for example, the level of some poverty index) set by an independent agency, such as in Honduras and Uruguay. Due to the interest in effective monitoring of these programs, the rules are usually strictly followed, which leaves little room for program targeting. The non-political rule-based assignment of CCTs allows for a quasi-experimental approach to estimating the effect of fiscal interventions on political outcomes at the threshold of assignment, with the assumption that regions, municipalities or households that fall just below the threshold of eligibility are characteristically similar to those that fall just above the threshold and which are therefore ineligible.\(^2\)

While the administration of CCTs can often overcome the issue of non-random fiscal transfer distribution, this may not always be the case. In Mexico, Takahashi (2007) considers that it is possible that the type of partisanship and the level of competition could vary between municipalities, which could influence the distribution of Progresa. He uses the Laakso-Taagepera index at the municipal level to control for the effective number of parties contesting the election. He uses pre-program municipal-level electoral data and counts of beneficiary households by municipality in negative binomial regressions to estimate that the beneficiary household count is positively correlated with incumbent vote share in the previous election, and this causal effect is greater for municipalities in which the election is more competitive. His findings indicate support for the core-voter model, in which programs are targeted at districts with higher pre-existing levels of incumbent support. He also finds that the results were negative for two-party municipalities but positive for three-party municipalities, indicating

\(^2\) The exception to this general principle is Mexico’s Progresa. Progresa’s political implications have been studied, but mostly from the point of view of manipulation of program administration. Takahashi points out that the Zedillo administration’s strict means-tested measure of program eligibility would, in theory, disallow any such tinkering. The means-tested measure was developed using census data, with the final eligibility status confirmed by local assemblies. Takahashi finds that the percentage proportion of Progresa beneficiaries is positively correlated with the poverty level of the state, which supports the idea that this cash transfer is meant for the poorer segment of the population. However, the marginal rate of increase of beneficiaries with increasing marginality index (proportion living under specific unfavorable living conditions) is smaller in municipalities with above-medium income than in municipalities with below-medium income, which indicates some non-random variation in program assignment.
that the more competitive the election, the more likely it is that the municipality was favored in the assignment of Progresa. Therefore, non-random assignment of treatment is one of the pitfalls to avoid in empirical analysis of CCTs’ relationship to political outcomes. In our analysis of the Honduran case, we do not consider non-random assignment to be a source of bias, because of the manner in which they were chosen (described in section 4), and because treatment and control groups did not exhibit significant differences in political support patterns in the 1997 election.

One potential challenge in the empirical assessment of such programs is the number of simultaneous interventions. Program administrators often implement two or three different types of interventions at the same time for logistical reasons – not the least of which is the ability to reduce costs by marketing numerous interventions at once to the relevant communities. CCT programs generally include more than one type of intervention, for example with different transfers for meeting schooling requirements and health requirements, or with different interventions to increase the use and quality of services. It is therefore fairly straightforward to assess the effects of a program as a whole, but not as trivial to understand precisely what type of intervention has the greatest impact. As we will see in the program description of PRAF, a simultaneous supply-side intervention was planned, but not implemented.

Another potential issue is that the expectation of future benefits by the control group often dampens any real significant effects on support for the incumbent government. In an experimental evaluation, it is assumed that the control group is unaffected by the program. In CCT programs with experimental evaluations, the control groups are often individuals or districts that are eligible for the program but that do not receive treatment due to some randomized treatment methodology. Due to ethical considerations, such groups are often granted transfers after a sufficient evaluation time-period has passed. However, if individuals in the control group anticipate (or have been told) that the
incumbent will give them benefits a year or two in the future, they may respond by increasing their support for the incumbent pre-emptively, to ensure that they receive the benefits that they expect. This reduces the estimate of the direct effect of the program on the treatment group.

One of the downsides of generalizing CCTs as a form of a fiscal intervention is that people are more likely to be willing to pay (through taxes) to help people whom they see as helping themselves – i.e. abiding by the conditions – than they would be to contribute to other unconditional fiscal intervention schemes (Schady and Fiszbein, 2009). People living in eligible geographic regions but who are themselves individually ineligible for the program are therefore unlikely to punish the government that implements the CCT program by swinging their vote. This indicates that our results could underestimate the vote-buying effects of revenue-sharing, cash transfer and social protection mechanisms. We may need to be cautious about the validity of extrapolating our results to conclusions about other forms of fiscal intervention.

Manacorda et al. (2010) point out that we have to be cautious about the external validity of results involving empirical study of CCTs. They point out that poorer people who are more likely to receive cash transfers could have partisan political preferences towards parties that have traditionally encouraged wealth redistribution. In general, this means that results from empirical evaluations (both experimental and quasi-experimental) are not generalizable to other segments of the population, or to the population of the country as a whole. However, Anderson (1988) and Canache et al. (1994) point out that support for both of the traditionally powerful political parties in Honduras has been generally uniform across socio-economic class.
2.5 Conditional Cash Transfer Programs and Politics in Latin America

Using individual surveys, Manacorda, Miguel and Vígorito (2010) examine incumbent support using PANES’ assignment rule – which is based on a sharp cutoff in an underlying household eligibility (predicted income) score – in a regression-discontinuity design. They use post-program household survey data from 1,942 households to estimate that beneficiary households are 11-14 percentage points more likely to support the current government relative to the previous one. The advantage of their empirical design was, of course, the individual-level data used, which would be much more useful in indicating how individuals make political decisions. However, in order to do this, they had to sacrifice the use of actual voting data, and instead relied on a self-reported set of answers to survey questions. Survey responses could, potentially, differ widely from actual voting preferences, but the authors use Latinobarómetro (a Latin American political opinion survey) data to confirm that there is a correlation of 0.85 between voting intentions and actual voting behavior.

To confirm the validity of their model, they look for but do not find any discontinuity in household covariates such as gender, age and years of education, and in self-reported voter turnout. As in any government-administered program, there is the worry of non-random assignment around the cutoff, in the sense that the program could somehow be more likely to benefit those households with greater support for the governing party. This could encourage their core supporters to participate in the elections, or could be aimed at districts with a higher proportion of swing voters. However, the fact that program implementation was so highly correlated with eligibility (on the order of 99%) and that the formula for eligibility was determined after the administration of the survey makes this unlikely.

The key difference between Uruguay’s PANES and PRAF is that PANES was always advertised as a temporary program, indicating that the results of Manacorda et al. are likely to be even more conservative than those from PRAF. In Uruguay, they find not just an increase in incumbent support,
but a *persistent* increase till 2008, beyond the end of the program. Given that there is no long-lasting household income increase beyond the end of the program, it cannot be that this continued increase in incumbent support is income-driven. If this is present in PRAF data too, it has important implications for the way Honduran voters think and respond to fiscal interventions. This type of result could provide support for the adaptive and retrospective form of voting, in which partisanship is determined by incumbent performance, and not solely through political ideologies. This makes sense in the context of PANES since program beneficiaries had a consistently higher household income for the time period of the program. It could therefore be a form of reciprocative behavior on the part of voters, who tend to reward the incumbent for policies that benefit them.

Green (2006) uses a similar regression discontinuity strategy to evaluate the political effects of Progresa in Mexico using the population thresholds at which the program was administered, and estimates that the program had no effect on incumbent vote share. However, she does suggest that a possible reason for this is potential program manipulation, due to local politicians in ineligible municipalities just below thresholds putting pressure on the Progresa administration to consider including them in the program.

De La O’s work on Mexico’s Progresa arrives at the opposing conclusion – using a differences-in-differences model with 1994 as the baseline, she finds that full enrollment in the program twenty-one months before the 2000 presidential election, lead to an increase in turnout of seven percent and an increase in incumbent vote share of half a percentage point, corresponding to sixteen percent increase, relative to enrollment just 6 months prior to the election (the control group). This is yet another piece of the puzzle – clearly, the amount of time spent in the program affects the extent to which voting patterns change. In Honduras, PRAF was implemented in a generally tight timeline for the treatment group (since
payments were only made once in six months) and the control group did not begin receiving transfers until after the 2001 election, and so this is unlikely to be an issue.

Interestingly, De La O’s results indicate that with an increase in the vote share of the incumbent traditionally leftist PRI, there was no corresponding decrease in the vote share of PAN, the right-wing party, but there was a decrease in the vote share of the PRI’s main competitor, PRD, which was initially formed by key members of the PRI and some other left-wing politicians. This is a strong indicator that the changes in vote share were not ideological. They are unlikely to be related to a prospective voting model of behavior either, given that all politicians standing for election had promised to continue the program. Her conclusion is therefore that increased incumbent support was a reward for the benefit that voters derived from the program.

In a quasi-experimental empirical strategy, Nupia (2010) exploits the variation in municipal-level beneficiary rates for *Familias en Acción* in Colombia, and finds that a change in one percentage point in the FA eligibility rate results in an increase in the incumbent vote share by 0.5%. In order to control for potential simultaneity, he instruments the strength of the program in the municipality by the number of children in that municipality. He re-runs specifications while controlling for strength of ideological attachment to the incumbent party, and find, unsurprisingly, that the program has a larger effect on incumbent vote share in municipalities with stronger ideological alignment with the incumbent. Even so, the effect in municipalities with low and medium strength alignment is still positive, significant, and of the same magnitude as the overall effect. The natural conclusion to make is that people are willing to sacrifice ideology to reward incumbents for economic benefit. He makes a key assumption here – that variation in number of children is not positively correlated with poverty levels. Nupia explains that a higher poverty rate could give the incumbent a stronger incentive to put effort into expanding the program. If the proportion of children in the municipality is correlated with poverty levels, then it is by
extension correlated with the effort that the incumbent puts into expanding the program in that municipality. While Nupia acknowledges this potential source of bias, he does not address the issue, which calls into question the accuracy of his estimate.

In contrast to Manacorda et al.’s findings in Uruguay, Bohn (2011) finds that in Brazil, votes cast by beneficiaries of *Bolsa Familia* did not play a key role in Lula de Silva’s re-election in 2006. She cites (1) the importance of concurrent social policies, (2) the fact that Lula’s bid to increase the breadth of his voting base began in 1994, long before he was in power, and (3) the fact that *Bolsa* beneficiaries tended to support him before the program was put in place as explanations for this conclusion. In conducting this study, she uses individual-level national surveys from as far back as 1989 to examine the characteristics and voting behavior of BF recipients. It should be noted that *Bolsa* was not geographically randomized in a way that would allow for a differences-in-differences or regression discontinuity strategy, unlike the work of De la O (2009) or Manacorda et al. (2009). Instead, Bohn analyzes the socioeconomic characteristics of Lula’s supporters in each of the electoral cycles to conclude that his support base showed gradual changes between 1989 and 2002, rather than a sharp change between 2002 and 2006. Similar probit models run specifically for the 2002 and 2006 elections show no significant effect of being a BF recipient on voting behavior, controlling for gender, age, income, education and region. Bohn’s paper considers several factors that could prove vital to electoral support for the incumbent, most of which involve analysis of not just the elections immediately preceding and succeeding the implementation of the social program, but also data from elections going back 15 years. However, without the possibility of an experimental or quasi-experimental method of evaluation, it is difficult to compare Bohn’s results directly with those of *Progresa* or PANES.

In summary, it is clear that the small pool of available literature has not come to a consensus on the effect of CCT programs on political outcomes. While all of the papers consider vote share of the
incumbent party as the outcome of interest, not all have found significant effects. Among those that have, the magnitudes of the effects vary widely. This is partly due to the different empirical strategies employed. Some use eligibility rules in quasi-experimental empirical designs, while others capitalize on random assignment of treatment to create comparable treatment and control groups. Programs also vary in size, generosity and reach, and therefore naturally have effects of different magnitude. More importantly, the political context in which the programs are administered and the types of voters most affected by the program vary from country to country.

It is therefore difficult to estimate the size or direction of an effect in the Honduran case. As described in the section on Honduras politics, there are a number of reasons to believe that Honduran voters are sensitive to fiscal interventions. However, the size of the transfer was small relative to those of other CCT programs, and much more infrequent. This could result in a smaller effect on vote share than estimated in other studies. We have few priors to guess at an effect in the Honduran case. Voter turnout in Honduras has always been relatively high, with a large proportion of the population displaying interest in the political process. This would suggest that a program like PRAF can do little to influence people to vote, since the people who would otherwise choose not to vote would be largely disinterested in election outcomes. However, Honduras’ experience in the 1993 election has shown that voter turnout is sensitive to the political climate, and this level of sensitivity may extend to the presence of social safety nets such as PRAF.
3. Honduras History and Politics

3.1 Poverty in Honduras

Honduras is one of Latin America’s poorest countries. Natural disasters and recent political struggles combined have taken their toll on Honduras’ economy and the welfare of its people. In 2002, 3.4% of rural households and 28.5% of urban households lived in extreme poverty (CEDLAS and the World Bank Database, 2011). For these families, their income was unable to cover the cost of basic necessities such as food. A 2001 national height-for-age survey of children found rates of chronic malnutrition of above 52% among 7-9 year olds in the southwest of the country (Honduras Ministry of Education, 2007). In these populations, children are often made to work in fields or travel door-to-door to make some income to help feed their families. School is out of the question, both because of the cost of books and other supplies, and because of the opportunity cost of the child’s labor. This is especially prevalent among older children, but also not uncommon among those aged 6-12. Health resources are either inaccessible or under-utilized since time spent away from work represents a high opportunity cost for the entire family.

This country of 8 million people, more than two-thirds of whom live below the poverty line, also faces a serious problem of inequality. In 1998, before the implementation of PRAF, the poorest 10% of the population received only 0.5% of national income (UNDP Country Evaluation, 2006). PRAF aimed to counter income inequality and promote human capital development in the poorest regions of the country with significant cash transfers.

PRAF is not the country’s first social safety net program. Programs that delivered food aid to the poor were implemented beginning in the 1950s, and reached over a quarter of the Honduran population (Moore, 2008). Certain supply-side interventions were put in place to improve the quality and efficiency
of social services such as healthcare, the largest of which was known as El Plan de Acción Nacional para el Desarrollo Humano, Infancia, y Juventud (PAN). Of all government interventions, PRAF is perhaps the best-known, and the longest-running program.

3. 2 Honduras’ political history

Honduras has traditionally had a two-party system, with the Partido Liberal and the Partido Nacional exchanging power from one election to the next. The parties date back to the turn of the 20th century, and the key ideological difference between them for many years was the issue of state involvement in the economy. In the past three decades, however, there has been little to choose between the two. Neither party has remained exclusively in power for more than two election terms since Honduras instituted free elections in 1980. The President is affiliated with one party or the other, and is a powerful figurehead in Honduran politics. However, the President cannot be re-elected for a successive term. There are three newer and smaller parties, the Partido Unificación Democrática, the Partido Demócrata de Honduras and the Partido Innovación y Unidad, which won between 1.8% and 2.3% of the vote in an average municipality in the 2001 elections.

The military officially gave up power in 1980, and general elections were held in the following year, 1981. Elections have been held every four years since then – in 1985, 1989, 1993, 1997, 2001 and 2005, and it is mandatory for all individuals between the ages of 18 and 70 to vote (Election Observation Mission Report, 1993). Elections have been considered free and fair (Election Observation Mission Report, 1993). There are three different levels of political officials elected in the same election period – the President, the Diputados (members of congress) and Mayors of municipalities. The logistics of voting have changed significantly between elections. Prior to 1993, all three elections were conducted in a single ballot, which means that choosing a party on the ballot would imply electing that party’s candidates for all levels of political office. In 1993, the column for each party on the single ballot was
divided into two subcolumns – one half for Presidente and the other half for Diputados. Voters would then put down one mark for the presidential candidate of their choice, and a second mark (potentially even for a candidate from a different party) for the Diputado of their choice. Voters could not vote in one of the elections (e.g. for Presidente) without voting in the other, since a single mark for a party would count towards both the Presidente and Diputados elections if no second mark was made. Corporaciones Municipales were elected on a separate ballot. 1997 was the first year in which split-ticket voting was used, with separate ballots for Presidente, Diputados and Corporaciones Municipales, allowing voters to choose, for example, the Partido Nacional representative as President but the Partido Liberal candidate as their municipality’s Mayor. They then also had the option of voting in one election without voting in another. Part of our analysis will examine how conditional cash transfers affect voting behavior in elections at different levels.

There are numerous reasons to think that the political background of Honduras may be conducive to fiscal interventions having significant effects on voting behavior. The Honduran public is not likely to base political decisions purely on the ideological stances of the various political parties. This is partly because the Partido Liberal and the Partido Nacional do not represent significant ideological differences (Kent and Barry, 1993; Acker, 1988; Rosenberg, 1995; Canache et al., 1994). This is supported by the results of Latinobarómetro’s public opinion survey in 1997 (which surveyed a representative sample of approximately 1000 individuals in Honduras), in which only 23.7% of those surveyed were close or fairly close to a particular political party, while the rest were either “sympathizers” or did not feel close to any particular party. This indicates a lower level of identification with a given party than the Latin American average (Latinobarómetro data), and the propensity for an individual’s vote to swing based on other factors like government-implemented social support programs. Bartolini and Mair (1990) and Roberts and Wibbels (1999) suggest the closer two political parties are in ideology, the greater the propensity for people to swing their votes from one party to the
other. There have been frequent exchanges of power between Honduras’ two key political parties, with
the Partido Nacional taking power in 1989 and 2001, and the Partido Liberal taking the Presidency in
other years.

Perhaps because of the relative lack of polarization by political ideology in Honduras, the
general public is supportive of the elective process (Rosenberg, 1995). In a 1992 Gallup opinion poll, 55%
of respondents stated that they felt elections had the power to effect change in the country. It is
therefore likely that fiscal interventions can be translated to observable changes in election outcomes.

Canache et al. (1994) point out a third factor that could compound the effect of the program on
incumbent vote share – they use multinomial logit models on public opinion prior to the 1989 election
to show that an individual’s vote for the incumbent Partido Liberal’s vote is significantly influenced by
what they term the “neighborhood political context” (the extent of support for the party in the
neighborhood) while the votes for the other parties are not. If this trend was present in 2001, living in a
municipality that benefited from PRAF could increase support for the incumbent party even among the
subset of the population that did not qualify for the program (because there were no young children or
expectant mothers in the household).

As an aside, the political situation in Honduras depends on numerous external factors. The
second half of the 1980s were a time of rising anti-American sentiment (Anderson, 1988) and by
extension, resentment against a government that was extremely economically dependent on the United
States. Some attribute Partido Liberal’s loss in the 1989 election to this sentiment. The elections of 1993
did not follow the same patterns as the others. In the 1980s, there was a trend of increasing support for
the two main political parties. While most Honduran elections were cleanly contested and garnered
strong voter participation, the proportion of abstentions in 1993 shot up to 35% from just 6% in the
previous election (Rosenberg, 1995). The 1993 election was one of the most controversial in the
country’s history. There were unprecedented amounts of mud-slinging against both candidates, with special emphasis on the allegedly pro-Communist nature of the Liberal Party candidate and the relations of the National Party candidate to the death squads (government-backed assassination squads) in the 1980s. The aberration represented by the 1993 election should not affect our empirical study, since we use 1997 election results as a baseline, and since we would expect any lingering effect on voting outcomes in treatment and control municipalities to be the same, because the PRAF transfers were randomly assigned.

Two external events that could affect voting patterns happen to fall within the timeframe of our study. The first occurred in January 1999, when the army officially came under civilian rule through a constitutional change that abolished the post of military commander-in-chief. Since this was a one-time national level change, and since treatment municipalities for the PRAF program were randomly selected, we would not expect this change to affect the treatment and control municipalities differently. It is therefore inconsequential to our empirical strategy.

The other is Hurricane Mitch. Hurricane Mitch hit Honduras in late October 1998, and caused massive damage estimated to cost US$ 5.4 billion. 19,000 people lost their lives and 2.7 million people their homes (BBC, 2000). The path that Mitch took meant that the Southwest of the country suffered some of the worst damage from it. Government response was disappointing, mostly due to the crippling amounts of debt that the government already faced at the time (Oxfam Briefing, 1998). In the words of one BBC report written six months after the tragedy, “the Government of Honduras had not helped yet.” Morris et al. (2003) point out that the amount of aid received was small relative to the amount of damage done by Mitch, and that while the probability of receiving aid was negatively correlated with wealth and positively correlated with the loss in assets, the amount of aid received was not. This could signal inadequacies in the way aid was delivered to those affected. Given that some PRAF recipients
suffered great loss from the hurricane, frustration with the indecisive response from the incumbent government could have affected support for the party. The implications of Mitch for our study are detailed in Section 4.4.
4. The PRAF-II Program

4.1 Goals and Aims of PRAF

PRAF-I was implemented in 1990, and extended indefinitely in 1992, primarily with funding by the Inter-American Development Bank (IDB). The program supposedly imposed health and education conditions on recipients to foster human capital development in poorer regions, while the cash transfers themselves aimed to allow the poorest citizens to consume beyond critical poverty levels. However, Moore (2008) states that according to PRAF employees, conditions were never imposed. There were also significant administrative flaws with the program – for instance, many beneficiaries found that the vouchers issued them as part of PRAF-I could not be redeemed in stores for their full value.

The IDB’s criticism of PRAF-I’s transfer leakages to wealthier individuals, and its failure to address supply-side deficiencies led to the formulation of a new phase of the program, PRAF-II. PRAF-II was implemented as an addition to PRAF-I, which would retain its original structure and continue as planned. The primary goal of PRAF-II was the accumulation of human capital among the poorest communities through conditional cash transfers. These transfers would offset the implicit and explicit costs of schooling for children aged 6-12 to school and health transfers for expectant mothers and young children. More concretely, PRAF-II hoped to improve the school performance of children 6 - 12 years of age from the poorest municipalities in Honduras, and to improve the health and nutrition of pregnant women and children less than three years of age.

4.2 Initial Implementation of PRAF-II

PRAF-II hoped to implement both demand- and supply-side interventions – a key change from PRAF-I. The demand-side interventions were health transfers of US$ 40-50 for pregnant women and
children under the age of five (up to a maximum of two per household) who followed a regular schedule of health checkups (around five a year), and school transfers of US$ 50-60 per child (up to a total of three children per household) to families who sent children aged 6-12 years to school. Only children who had not as yet completed fourth grade were eligible for the transfer. The size of the health transfer was approximately a third of a basic food basket (Moore, 2008), while the school intervention was the sum of direct and implicit costs of sending a child to school (IFPRI, 1999). Supply-side interventions were sums of money granted to primary schools and health centers upon beginning a process to improve the quality of their services and community participation. The school interventions were to be given directly to Parent Associations, who then had the authority to decide how the funds would be used in the local schools. Health interventions involved training a community representative to diagnose children’s health problems and refer them to health clinics if necessary. However, most supply-side interventions were not in place by 2002 – only 17% of health transfers and 7% of education transfers were complete by then (Moore, 2008). Galiani et al. show that there was no discernible difference in schooling and health outcomes between groups of municipalities that received the supply-side transfers and groups of municipalities that did not (controlling for presence of a demand-side transfer). We therefore consider municipalities that benefited from PRAF to be those that received the demand-side intervention.

The 1997 National Census of Heights of First-Grade Students provided a distribution for height-for-age statistics across the country, and this data was then chosen to identify the 70 poorest municipalities. Stunting is an indicator of long-term malnutrition, which is closely related to chronic poverty. Between May and July 1999, IFPRI formulated an “Expenditure and Livelihoods Survey”, administered in the 180 poorest municipalities of the country.\(^3\) This survey helped quantify the costs (both implicit and explicit) of preventive health services for expectant mothers and school attendance

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\(^3\) The survey was used to develop a concrete algorithm to predict a household’s income. Instead of actual income, predicted income was used as part of the assignment rule due to the cyclical nature of income in the most rural, agriculture-dependent areas. However, the income rule was never applied, and all households in the municipality could be considered eligible if the municipality itself were deemed eligible.
for children aged 6-12. The questions used in the survey were developed over time by IPFRI and had been previously tested both in rural Honduras and in Mexico, as part of an evaluation of Progresa.\(^4\)

Figure 1: Eligible and Treated Municipalities

The 70 eligible municipalities were stratified into five blocks of 14 municipalities each, based on Height-for-Age Z-score. We will refer to the block with the lowest average HAZ score as block 1 and the block with the highest average HAZ score as block 5. Each of the 14 municipalities in each income stratum (or block) was randomly assigned to one of four groups, G1 – G4. For each stratum comprised of 14 municipalities, balls of four different colors were placed in a box, and a child chose a particular ball for each municipality. The size of the aperture in the box was small enough that the color of the ball could not be discerned before it was pulled out of the box, to prevent any tampering with the process. The color of the ball determined the group, G1-G4, into which the municipality would be placed. For each stratum, there were four municipalities in each group, G1, G2 and G3, and two in G4.

\(^4\) The survey forms used are provided in the Primer Informe de PRAF II.
At the end of the randomization process, there were 20 municipalities each in G1, G2 and G3 and 10 in G4, and each group comprised municipalities from all five HAZ strata. G1 and G2 were both allocated demand-side interventions, G1 and G3 were both allocated supply-side interventions, and G4 was a control group scheduled to receive transfers after 2001. Since the supply-side interventions were not implemented (Galiani and McEwan, 2011), essentially G1 and G2 (a total of 40 municipalities) received a demand-side intervention and G3 and G4 (a total of 30 municipalities) served as a control group. Once municipalities were chosen to receive the transfers, people who moved from a non-treatment municipality to a treatment municipality were not eligible to receive the transfer. Transfer recipients had to be residents of treatment municipalities at the time of the random draw. This removes any doubt about selective migration potentially biasing results. The program was advertised as an indefinite-period program, and all eligible municipalities were aware that they had been selected.

PRAF-II was implemented to benefit 20,000 homes through the maternal and infant health interventions for children under five years of age, and an additional 12,000 homes and 22,000 students through the educational interventions. Upon receipt of program funds from the IBD, the program executor had a contractual obligation to conduct a mid-term evaluation and a final evaluation of the project. Given that PRAF had set aside a large portion of its budget for monitoring and evaluations, these evaluations were conducted in detail. The evaluations suggested some positive effects, at least on the education side. These are detailed in the literature review. The program cost, as a whole, US$ 11 million. Funds were distributed as vouchers, which could be used as cash anywhere in the country. This would happen upon verification that the conditions imposed by the program had been complied with, but it is not clear that the conditions were strictly imposed (Moore, 2008).
4.3 Timeline of the Intervention

The following diagram illustrates all the pertinent events in the time period between the baseline election (1997) and the post-treatment election (2001). These include political events (blue), non-political events that could have a political impact (brown) and implementation of the program (green). It also includes the Honduran census (yellow), which is the source of many of our control variables.

Carlos Facusse of the Partido Liberal took office in January 1998. The elections held in November 1997 provided a strong baseline set of voting data. While the Encuesta de Hogares was implemented in 1997, the plans for the program were not complete until after the elections. We can therefore expect that the program would have had no effect on voting behavior in that election. In January 1999, the post of military commander-in-chief was abolished and the military came under the constitutional control of
the President. We would expect that this national-level change affected all 70 municipalities to a similar degree. Eligible municipalities were notified in October 1999, and the random draw of 40 treatment municipalities occurred late that month. Vouchers were distributed in six-month intervals since then. By November 2001, the next election in the country, the program was well under way and two sets of transfers had already been made to the treatment groups. Interestingly, election campaigns began in earnest around a month after the first set of transfers, which may have been good timing for the incumbents. Therefore, in November 2001, there was no doubt that the program had been successfully implemented, although its future was unclear. In the 2001 elections, a change of power brought President Ricardo Maduro of the Partido Nacional into office.

4.4 Hurricane Mitch

Hurricane Mitch ripped through Honduras between October 22nd and November 5th 1998, destroying crops and homes and causing millions of dollars’ worth of damage. On a macroeconomic scale, the influx of aid and emergency funding after Mitch caused a 28% appreciation of the lempira, which added to the Honduran economy’s woes (PAHO, UN Millennium Development Goals). It is worth noting that poorer families suffered a greater loss in income as a proportion of household expenditures (Morris et al., 2002). It is possible that the government’s slow response to Mitch, and the limited aid available for those most devastated by the hurricane affected Hondurans’ support for the incumbent government. Major sources of aid were church groups and NGOs, rather than governmental organizations. Mitch affected different municipios and entire departamentos differently and its effects should therefore be accounted for in our identification strategy. We have used the census survey data aggregated at the municipal level to construct an “effect of Mitch” variable, with the assumption that the effect of Mitch on changes in households’ voting behavior depended only upon the amount of
damage they faced, which would in turn determine the extent to which they were directly affected by
the government’s lack of response.

Mitch could pose two challenges to our empirical strategy. The first is that if the amount of
damage caused by Mitch is somehow correlated with treatment, then there is at least one additional
factor that differentiates the treatment from the control group of municipalities, which biases our
experimental estimates. However, the summary statistics provided in Appendix 1 indicate that at least
one proxy for the damage caused by Hurricane Mitch did not vary between treatment and control
municipalities, on average. In addition, the path of the hurricane that is shown in figure 2, together with
the map of treatment and control municipalities supports this point. Treatment and control
municipalities are interspersed with each other, and do not exist as distinct geographic ‘clumps’ of
municipalities. Hence, it is likely that if one treatment municipality were hit badly by Mitch, its neighbor,
perhaps a control municipality, would be hit equally badly.

Our second issue is that Mitch poses a potential threat to external validity. Given that the census
and 2001 election data we use our study are collected in a period of time where Honduras is likely to
have been re-building itself after the damage from Mitch, our estimates of treatment effects from PRAF
could be dampened. If effects from Mitch are strongly negative for the incumbent, then this could mask
any smaller but significant effect of PRAF on vote shares. It seems possible that this is the case, given
that a simple regression of incumbent vote share on our proxy for damage caused by Mitch shows
strongly significant and negative effects of Mitch on incumbent vote share on the order of 20-30
percentage points in the Presidencial and Diputado elections, which is robust to the inclusion of control
variables from the 2001 census. In the context of an effect that size, and given estimates in other studies
on changes in vote share caused by CCT programs, it may simply be that any positive effect of the
program is offset to much larger degree by the negative impact of Mitch.
In general, assuming a homogenous effect of the civilian control of armed forces and Hurricane Mitch on incumbent support across treatment and control groups, it seems likely that PRAF treatment municipalities will exhibit greater incumbent support. The issue of voter turnout remains ambiguous, since Honduran voter participation has proved itself sensitive to political climate, while little research has been done on socioeconomic situation and inclination to vote.
5. Data

According to Honduran law, voting data is released by the Tribuno Superior Electoral. All the municipal-level voting data is available online at the TSE website, with the exception of 1997, which is only available at the departmento level. The 1997 municipal-level voting data are available in the 1997 Election Handbook released by the TSE. Voting data include the number of votes cast for each of the five parties on the ballot and in most years, the number of abstentions. It allows us to calculate incumbent vote share in each municipality in the following way:

$$PL \text{ Vote Share}_j = \frac{\text{Total Number of votes for PL in municipality } j}{\text{Total number of valid votes in municipality } j}$$

where the total number of valid votes is the sum of the votes cast for each of the five contesting parties.

Voter participation, another outcome of interest, can be calculated by taking the ratio of the total number of votes cast (valid and invalid) to the number of people eligible to vote (from the census data).

In evaluating PRAF, Morris et al. (2004) and Glewwe et al. (2004) use data from PRAF baseline surveys. One key issue with the evaluation of PRAF is the fact that the baseline survey was conducted between August and October 2000 for treatment households and between November and December of the same year for control households. November and December are key months for the coffee harvest which may lead some bias in the baseline measures for the evaluation, because the demographics of those who responded to the survey in the busy harvest months may have been fundamentally different from those who responded in August.

To eliminate any potential issues of bias in our controls, we use control variables from the 2001 census, which was conducted just 8 months after the first round of PRAF transfers, and prior to the election of 2001. Given the short time between the implementation of the program and the census, we can assume that the census accurately captures a baseline set of individual and household control...
variables. In addition, the census data give us an accurate estimate of the number of individuals in a municipality eligible to vote who are living in households that are eligible for the program (conditional on meeting program requirements), which we can then control for as a measure of program intensity in a given municipality. This is used as our measure of treatment in some experimental specifications. The control variables summarized in A1 are from municipal-level aggregation of the 2001 national census.
6. Empirical Strategy

6.1 Experimental Estimates based on Randomization of Treatment

Like most literature in this area, we measure political outcomes using municipal-level vote-share data (generally the vote share of the incumbent, although we will also consider the vote share of the main opposition party). We also use another dependent variable, voter participation, to test whether the program may have perhaps induced some voters to show their support for the incumbent party, perhaps in expectation of continued benefits. This is unique to this study, since previous evaluations of the political effects of conditional cash transfers have not studied voter participation outcomes.

The timeframe of Honduran national elections and the initial demand-side transfer payments from PRAF allow for a differences-in-differences approach. The difference in change in vote share and voter participation between 1997 and 2001 in treatment and control municipalities isolates the effect of the program on these outcomes, assuming that the two groups are otherwise homogenous. Prior work (Glewwe et al., 2004; Galiani et al., 2011) suggests that randomization was effective, and our summary statistics suggest the same. Given the method of selection of treatment municipalities, it is very unlikely that municipalities given the treatment differed fundamentally in any way from those that did not.

To formalize the strategy further, we are estimating the following equation:

\[ Y_{2001j} = \beta_0 + \beta_1 \text{Treatment}_j + \beta_2 Y_{1997j} + \beta' X'_j + \epsilon_j \]

where \( Y_{2001} \) and \( Y_{1997} \) represent our political outcomes in the respective election years, \( \text{Treatment}_j = 1 \) if the municipality \( j \) was one of the original 40 to benefit from PRAF funds and 0 if it was one of the 30 municipalities that were eligible but did not receive treatment, and \( X' \) is a vector of control variables created by aggregating the Honduran census. These control variables include the number of individuals...
eligible to vote, the proportion of voters living in a household with children under the age of 12 (which would give an estimate of the proportion of households in a given municipality who could be allocated the transfer upon compliance with PRAF conditions), a variable that estimates the average probability of a relative in the household migrating after Hurricane Mitch (which approximately quantifies the effect of Mitch in that municipality), average education and literacy of people in each municipality, and proportions of females and indigenous people in the municipality. These particular controls are used particularly to test for balance between treatment and control groups, to ensure the validity of our empirical strategies. We also include a set of block dummy variables in all specifications to control for differential effects among income strata. A table of summary statistics is provided in A1. None of the control variables show significant differences in means at the 10% level. This is further indicative of effective randomization among the eligible municipalities.

In order to test for the size and nature of any income-based disparities in the effect of the program, specifications are repeated with an interaction term of treatment with the dummy variable for each block, or poverty stratum, rather than a straightforward ‘treatment’ variable. This divides the effect of the program by municipality. The equation we estimate is then as follows:

\[ Y_{2001j} = \beta_0 + \beta_1 \text{Treatment} \times \text{Block 1}_{j} + \beta_2 \text{Treatment} \times \text{Block 2}_{j} + \beta_3 \text{Treatment} \times \text{Block 3}_{j} + \beta_4 \text{Treatment} \times \text{Block 4}_{j} + \beta_5 \text{Treatment} \times \text{Block 5}_{j} + \beta_6 Y_{1997j} + \beta' X'_{j} + \epsilon_{j} \]

This allows us to identify the source of any overall effect we might observe. Results are provided in table A3.

6.2 Eligibility Cutoff Regression Discontinuity Design

The selection of the eligible municipalities using the average First Grade HAZ score can be used to implement a regression discontinuity design as a robustness check. The 70 eligible municipalities were chosen because they fell below the cutoff HAZ score of -2.304. Since the score itself was randomly
selected (based on the expected scope of the program and its budget) we would expect municipalities that fell close to the cutoff to be essentially similar to ones that missed it narrowly and were thus ineligible for PRAF. Comparing changes in political outcomes at the margin would allow us to estimate a local average treatment effect at the cutoff. Since not all eligible municipalities received the treatment between 1999 and 2001, the discontinuity is a ‘fuzzy’ one, where the probability of receiving treatment does not increase precisely from 0 to 1 at the cutoff. The discontinuity at the cutoff is estimated using the following equations:

First Stage: \( \text{Treatment}_j = \alpha_0 + \alpha_1 \text{Eligibility}_j + f(\text{HAZ}_j) + \nu_j \)

Reduced Form: \( \text{Y}_{2001\_j} = \beta_0 + \beta_1 \text{Eligibility}_j + f(\text{HAZ}_j) + \beta_2 \text{Y}_{1997\_j} + \epsilon_j \),

where Eligibility = 1 if the municipality is one of the 70 below the cutoff average HAZ score of -2.304, and Treatment = 1 if the municipality was one of the 40 that received program benefits in the 1999-2001 period. The causal effect of the program can then be estimated by the Wald estimator, \( \frac{\hat{\beta}_1}{\hat{\alpha}_1} \). We conduct the analysis on vote shares of the incumbent and the key opposition party. In our specification, we also control for a lag of the 1997 vote share of the party in the reduced form equation to control for some measure of prior baseline support.

This addresses one potential source of bias in the randomization strategy. Since all eligible municipalities were aware of their status, and since the timeline of the program was indefinite, it is possible that non-treatment eligible municipalities were more inclined to vote for the incumbent in anticipation of the benefit being extended to the eligible but as yet untreated municipalities. Voters may have felt that with the program already under way, there would be a lower risk of its cancellation if the incumbent were to remain in power. This robustness check allows us to control for any potential positive effects on eligible non-treatment municipalities that anticipate treatment. Significant
differences between the two types of estimates could then be attributed to prospective voting by voters who expect to receive the transfers in the future.

This strategy hinges upon the assumption that municipalities are unable to self-select into the program (i.e. manipulate the cutoff score). If municipalities with aggregate HAZ scores slightly above the cutoff were indeed able to somehow receive the transfers even though they are ineligible, the cutoff score would be negated. Furthermore, municipalities that self-select into the program may differ fundamentally from those that do not engage in score manipulation, which implies that selection into treatment is not “as good as random.” In the case of PRAF, however, the HAZ score was the result of direct observation of first-grade children, who would not have been able to individually manipulate their own HAZ scores. In addition, the survey of first-graders took place well before PRAF-II was planned and implemented, which negates the risk of institution-level cutoff score manipulation. Therefore, regression discontinuity estimates should give us an unbiased estimate of the effect of PRAF-II on municipalities in the region of the cutoff HAZ score.

This strategy would be straightforward to implement, except for the fact that the average HAZ score was not directly observed for any municipalities other than the 70 experimental municipalities that were eligible for the program. The 1997 height census, upon which the HAZ score for the 70 municipalities is calculated, records only three municipal-level variables for the non-experimental municipalities: (1) the proportion of children in a municipality with HAZ scores less than -3, (2) the proportion with HAZ scores between -3 and -2, and (3) the number of surveyed first-graders (Secretaría de Educación, 1997).

In order to estimate a HAZ score for all 298 municipalities, we follow Galiani and McEwan’s (2011) approach of regressing HAZ on the two proportions described above, and use the regression estimates to predict $\hat{HAZ}$. We then use this predicted score in all the regression equations described
above. As a check for the accuracy of the prediction, the correlation between HAZ and $\bar{HAZ}$ for the eligible municipalities (for all of which HAZ was directly observed in the dataset) is 0.96. The fact that we have to use a predicted value rather than a true value of HAZ implies that eligibility does not change sharply at the cutoff, as can be seen in the first figure in A5.

We visually test the fit of the data by conducting the following: we put municipalities in bins of width 0.05 by $\bar{HAZ}$ score, and calculate means of dependent variables for each bin, to smooth the data. In A5, we provide the plots of the bin means against $\bar{HAZ}$ score, and look for a potential discontinuity in the fitted line, which appears in almost all plots, for both incumbent and opposition vote shares. The functional form of $f(\text{HAZ})$ is chosen to be a quadratic linear spline ($\text{HAZ} \times \text{eligibility}^2$) that allows for two different quadratic fitted curves to be estimated on either side of the discontinuity. This then ensures that no spurious discontinuity that occurs due to a slope-change will be incorrectly interpreted as causal. A quadratic spline is chosen because it addresses more of the noise in the data than a linear spline would, while still allowing us sufficient power to estimate the discontinuity using a relatively small bandwidth of data around the threshold of eligibility, which minimizes bias.

The next step is to run the regression analysis, the results of which are shown in A6. In order to ensure the best fit possible for the data on either side of the HAZ cutoff, analyses are repeated with more flexible cubic functional form specifications, and results prove robust to the change in functional form specification.
7. Results and Discussion

7.1 Experimental Estimates

7.1.1. Incumbent Vote Share

Table A1 presents basic experimental results. Panel A examines the effect of the program on incumbent vote share. Some specifications also include the interaction term of proportion of voters eligible for the program (scaled at the mean) and treatment. In the presidential election, estimates of the cash transfers on vote share are negative, but less than 1.0 percentage point and insignificant. This seems to indicate that the program had no effect on presidential vote share. This makes intuitive sense for a few reasons; (a) beneficiaries are unlikely to attribute the program directly to action taken at the presidential level, since the original PRAF scheme had been in place for nearly a decade under its own governmental arm (b) the program was implemented in conjunction with an external organization, the IDB and (c) Presidents cannot be re-elected in Honduras, and so the incumbent president who served from 1997 till 2001, Carlos Facusse, could not be rewarded by votes for the PL candidate for the 2001 election.

Panel A also reflects that incumbent vote shares in the Diputados and Corporaciones Municipales elections are negatively affected by cash transfers. However, in the case of the election of the Diputados, the effect is 1.0 percentage point or lower, and its magnitude and sign are not robust to the inclusion of control variables from the census. Specifications 7 and 8, which include the census controls, in fact indicate a 0.1-0.2 percentage point increase in PL vote share caused by the cash transfer program (and in specification 8, this is at the mean proportion of eligible voters).

Addition of the interaction term does not affect the estimate of program effects at the mean proportion of voters eligible for the program, but the coefficients on the interaction terms are negative.
for the presidential and congress elections. This indicates that the higher the proportion of beneficiaries of the program in the municipality, the more negative the overall effect on incumbent vote share. While still statistically insignificant, this is suggestive of the possibility that the program hurt rather than helped public opinion of the incumbent government. It contradicts our hypothesis that the beneficiaries from the program would show increased support for the Partido Liberal, either as a reward for the benefits or in anticipation of future benefits, and that this trend would be particularly in municipalities with a higher number of beneficiaries.

With regard to the Corporaciones Municipales election, the negative effect of the program is even more pronounced than in the higher-level elections, and is estimated to be 2.81 percentage points. However, its significance is not robust to the inclusion of control variables. In specifications with interaction terms included, the effect of the program at the mean is of a similar magnitude, 2.38 to 2.48 percentage points, but still insignificant.

In comparing the three elections, it is clear that the effect of the program on incumbent vote share is somewhat negative. The magnitudes of the coefficient on the treatment dummy are also indicative of the fact that the program has a greater effect on vote shares in the more localized elections for mayors. This would make sense for a program administered at the local level, where politicians who are more frequently directly in contact with the population are more likely to reap any kind of political reward (both prospective and retrospective) from voters. Furthermore, the Pedersen index of Honduran election volatility, which covers the period from 1980-1997, has a value of 7.9 for mayoral elections and 6.2 for presidential elections, which suggests that presidential elections are in general less susceptible to swing votes than municipal elections (Roberts et al., 1999). This supports two aspects of our findings – the lower magnitude of any effect of the program in the presidential election relative to congress and
mayoral elections, and the stronger correlation of 2001 vote share with 1997 vote share in the presidential election relative to the other two elections.

The next issue of concern is why our results are negative, and why they are insignificant. There are a number of possible reasons for this. De la O’s experimental results from Progresa in Mexico conclude that the program resulted in a 0.5 percentage point increase in vote share for the incumbent party, which is an effect of relatively small magnitude. It is not surprising that the less-generous PRAF transfer did not result in a significant increase in incumbent vote share in beneficiary municipalities relative to control municipalities. As a measure of transfer generosity, Progresa transfers represented 21.8% of pre-transfer consumption for Mexican families, while PRAF represented only 7% of pre-transfer consumption for Honduran families (Schady and Fiszbein, 2009). PRAF transfers also occurred only once every six months, compared with Progresa transfers which were distributed once every two months (Levy, 2006).

The size of the PRAF transfer was designed to compensate families for the direct and implicit costs of health checkups and school attendance, but there is little evidence that the transfers and the resulting increases in school attendance and the regularity of health checkups improved students’ achievement or reduced the incidence of diarrhea, stunting or malnutrition (IDB, 2007). This may be part of the reason that Hondurans did not consider the transfer significant enough to motivate them to support the Partido Liberal over the four other political parties contesting the 2001 election.

The size of the transfers was also not sufficient to change poverty dynamics in the eligible municipalities. A low growth rate of income per capita, combined with a smaller effect on poverty in the lowest income deciles in the years after the program was implemented, indicates that households eligible for PRAF still found themselves in poverty. According to the IDB, initial asset levels, rather than income or consumption levels, are key determinants of transitioning between being poor and non-poor.
in Honduras (IDB, 2007), and fiscal transfers are unable to address this. This could explain the lack of effect of PRAF on PL vote share in the 2001 election.

A second possible explanation for the insignificance of the PL share estimates is that the negative impact of Hurricane Mitch on incumbent vote share and other major political shifts, such as the military coming under civilian control, may have overshadowed programs such as PRAF. The poorest segment of the population was the group most devastated by Hurricane Mitch, and focus on reconstruction efforts could very well have taken precedence over receiving sporadic cash transfers from the government for that group of citizens, which may mean that both treatment and control municipalities did not, on average, change their voting behavior in response to PRAF.

The key opposition party, the *Partido Nacional*, had also promised to continue the program, if elected. In particular, the program evaluation was set to run at least until 2002. Since voters would expect to derive the same economic benefit from voting for either the incumbent or the key opposition party, under a prospective voting model, we would not see any vote swinging from the key opposition party to the incumbent. This could further explain the insignificance of the results.

For a more complete analysis of these issues, we consider the same specifications, but using vote share data from the key opposition party, the *Partido Nacional*.

### 7.1.2 Main Opposition Party Vote Share

Before analyzing the estimates, it is important to understand the opposition party’s links to PRAF. Before the elections of 2001, the National Party had already committed to ensuring the continuity of PRAF if elected. Therefore, there would be little scope for prospective voters to swing their votes away from the National Party toward the Liberal Party. However, it is also unlikely that the *Partido*
Nacional would be rewarded simply for extending the same transfers that the incumbent party had already been delivering for around a year prior to the 2001 elections.

Panel B of table A2 shows an interesting phenomenon, however. Far from losing votes to the incumbent that was responsible for the benefits program, support for the opposition party appears to actually increase as a result of the program. The gains in opposition vote share are of magnitude approximately 1.1 percentage points in the presidential election, 2.5-2.8 percentage points in the Diputados election, and 3.5-3.7 percentage points in the Corporaciones Municipales election. The magnitude and significance of the estimates are, however, not robust to the inclusion of census controls. In particular, the magnitude of the estimates decreases by approximately 1.0 percentage point and becomes insignificant in every election, with the introduction of controls. The magnitudes of the effect of the program at each level of election follow the same general trend as the effects observed on incumbent vote share (though they are much larger), indicating that similar arguments regarding patterns of volatility in different levels of elections apply here, as well.

From the sign of the coefficients, it seems that contrary to our hypothesis, the program executed by the Partido Liberal to provide the poorest group of Hondurans with cash transfers caused increased support for the Partido Nacional. The fact that the estimated increase in PN share is greater than the estimated decrease in PL share in each election, together with our results on voter participation (section 7.1.4) indicate that the added support for the main opposition party is due not only to incumbent supporters in the prior election swinging their support to the opposition, but also from supporters of smaller peripheral parties (PDCH, PINU and PUDH) swinging their vote to the opposition in treatment municipalities, and new voters choosing to support the Partido Nacional.

This unexpected result may be caused by two factors: If the program did not live up to beneficiaries’ expectations, it is possible that this was more evident to individuals in the treatment
municipalities than those in control municipalities. This would have caused some veteran voters in
treatment municipalities to shift support from the Partido Liberal and other smaller parties to the
Partido Nacional, and a greater proportion of new voters to choose to support the Partido Nacional,
particularly if they felt that the opposition party would be able to administer the program more
effectively. This is a reasonable conclusion considering the following issues that PRAF faced in the 1997-
2001 period (Moore, 2008):

(1) PRAF beneficiaries found it difficult to actually get access to the cash transfer. Transfers were
also handled by PRAF employees rather than official banking systems, and distribution was
therefore fairly inefficient.

(2) Transfers were distributed only twice a year, and therefore families found it difficult to budget
for these infrequent sums of money, which, even when received, would not contribute
significantly to their disposable income.

If voters felt that the National Party would be able to address these issues effectively, this may
explain the positive effect of the program on the vote share of the opposition in the 2001 election. The
effect of hurricane Mitch may have made the inadequacy of the program even more apparent, because
of the dire need for financial aid to rebuild communities after the hurricane.

One of the key conclusions we can draw from the incumbent and key opposition party vote
share results is that program beneficiaries are prospective voters. A lack of significant effect of the
program on incumbent party vote shares indicates that the incumbent party received no reward for
having implemented the program. Instead, beneficiaries showed increased support for the Partido
Nacional, which was not involved in the administration of the program. Beneficiaries must have
expected that the Partido Nacional could do a more effective job of governance, and assuming that
treatment and control municipalities were entirely similar except for receipt of transfers, this 
expectation can be causally linked to the administration of PRAF.

7.1.3 Experimental Estimates Separated by Block

In order to identify the source of our positive effect on opposition vote share, we separate the 
effect of the program on vote share by poverty level. Table A3 shows experimental effects on both 
parties’ vote shares, split by HAZ-score stratum, or block. Because of the nature of the selection into the 
municipalities, treatment and control municipalities are balanced across blocks, with four municipalities 
from each of the five strata receiving cash transfers. Here, block 1 municipalities had the lowest average 
HAZ scores, and block 5 municipalities had the highest average HAZ scores.

This analysis indicates that the program causes a decrease in PL vote share of 7.31 percentage 
points in the presidential election, 8.28 percentage points in the congressional elections and 8.77 
percentage points in the mayoral elections in block 5, but no significant effects in other blocks. These 
results are robust to the inclusion of controls. There is a corresponding increase in PN vote share of 6.37, 
6.98 and 8.26 percentage points in the presidential, congressional and mayoral elections respectively, in 
block 5 municipalities. These estimates are both statistically significant and robust to the inclusion of 
controls.

This is perhaps not surprising, given that the cash transfer amounts were deliberately set fairly 
low so that richer households would be deterred from collecting them, and the block with the highest 
HAZ scores would have a larger proportion of richer households for whom the transfers would not have 
been as significant. In contrast, for the poorest municipalities, the transfers would have been significant 
for almost all the households in the municipality, which would then translate into a less negative effect 
for the incumbent party.
7.1.4 Voter Participation

The effect of the program on voter participation is positive in the Presidential and Diputados elections, and negative in the Corporaciones Municipales election. However, all of the specifications estimate an effect of less than 1.0 percentage point, and all the estimates are insignificant. This suggests that the program had no effect on voter participation. Honduras has had a tradition of high voter participation in elections, and therefore those who do not vote probably do so for reasons that cannot be overcome by a relatively small cash transfer that just covers the costs of meeting the conditions imposed on beneficiaries. In the 2001 election, voter participation was at an average of 85-86% in municipalities eligible for the program.

7.2 Regression Discontinuity Estimates

7.2.1 Vote Shares

The regression discontinuity analysis assumes that municipalities that have average HAZ scores close to but above the threshold of eligibility are comparable to those that have average HAZ scores close to and below the threshold of eligibility (and are therefore eligible for the PRAF transfers). To estimate the discontinuity, we fit a piecewise spline on either side of the discontinuity, and estimate the size of the gap between the two pieces at the threshold.

Given the level of noise in the data, the quadratic spline proves a better fit to the data than the linear spline. Having a better fit to the data implies that the gap between the two pieces of the spline at the threshold provides a more accurate estimate of the true effect of the program. We allow the slopes of the splines to vary on either side of the discontinuity, so that the estimated effects account for potential changes in the slope of the line at the threshold. In A5, we present visual evidence of the
presence of a discontinuity in vote share at the threshold of eligibility of the normalized $\hat{H}AZ$ score, at 0. According to the scatter plots, there is a positive effect of the program on incumbent vote share at the threshold in the Presidential and Diputados elections, and what could be an insignificant effect in the Corporaciones Municipales election. Scatter plots using opposition vote shares as the dependent variable show a negative effect of the program in all three elections.

The next step is to estimate the precise size of the discontinuity in vote share using regression analysis, and the results of this are provided in A6. A bandwidth of 0.25 is chosen because it is the one that is small enough to minimize bias, but large enough to allow significant results from the visual analysis to be replicated.

There is a positive effect of receiving the cash transfers on PL vote share of 8.08, 13.88 and 7.61 percentage points in the presidential, Diputados and Corporaciones Municipales elections respectively. This result is significant for the national elections but insignificant at the local level. The large and positive effect indicates that voters in municipalities close to the threshold who received the transfers were significantly more likely to vote for the incumbent party than those who were close to the threshold but were ineligible.

This analysis compares individuals who were ineligible for the PRAF transfers to those who were eligible, and received the transfers. The contrast between this and the experimental results is perhaps indicative of the fact that there was some positive effect of the program on support for the incumbent of those who were eligible but did not receive the transfers, which resulted in an insignificant experimental estimate of the effect of the program on PL share. If this is so, it supports the idea that voters in the eligible group are prospective voters who consider potential future benefits in making political choices. For instance, people who were eligible but did not receive the transfers may have anticipated receiving the transfers at some point in the following four years (perhaps after the
evaluation was scheduled to be completed), and therefore increased their support for the incumbent party in anticipation of future benefit.

However, this explanation cannot account for the fact that the key opposition party also promised to continue the program. Then, a more likely explanation of this result is one in which voters in the PRAF-eligible municipalities that did not receive transfers nevertheless took the implementation of the program as a signal of the Partido Liberal’s interest in providing support to their communities, and therefore increased their support for the PL in the 2001 election.

The fact that the shift in support is concentrated at a different level of election in these results than in the experimental results is also interesting. In experimental results, the largest effects were felt in the mayoral elections, while here, the largest effects are felt at the congressional level. A potential reason for this is that voters who received transfers may think differently about the origin of the program than voters who did not. For instance, beneficiaries may have thought of the program as being a local one, since program administration was carried out at a municipal level (visiting door-to-door, pamphlets, etc.), while others who were not administered the program may have thought of it on a larger national scale since they may have received information about the program mainly through the press.

We then consider the effects of the program on the opposition party vote share at the threshold of eligibility. We estimate that receiving transfers caused a decrease in PN vote share of 7.83 percentage points in the presidential elections, with smaller positive estimates of 0.70 and 1.73 percentage points in the Diputados and Corporaciones Municipales elections respectively. While the presidential election estimate mirrors an increase of similar magnitude and significance in PL share, the latter two are insignificant, suggesting that the PN vote share in these two levels of election remained generally unaffected by the program.
If we consider our previous explanation that all positive effects on PN vote share in our experimental estimates were a result of poor program administration on the part of the incumbent party, then it is possible that this effect mitigated some of the decrease in PN share due to increased support for the PL that we see at the threshold. The overall effect of the program at the threshold would then be insignificant, as we see here.

7.2.2 Voter Participation

A similar discontinuity analysis was carried out using voter participation rates in the three elections in 2001, but the effect of the program on participation seemed to be 0, robust to bandwidth and spline specification. This is similar to the experimental results, probably for the same reasons explained in 7.1.4.
8. Robustness Checks and Extensions to Strategy

8.1 Presence of Persistent Effects

Experimental results motivate the question of whether increased support for the National Party persisted till the next election, in 2005. Voting data from 2005 tend to suggest that it did (table A7). The presence of a persistent effect would indicate that our experimental results are not simply the result of a spurious correlation. The presence of significant effects both in 2001 and 2005 are indications of voters’ focus on future benefits rather than rewards or punishments for past actions of a political party, which ties in well with Partido Nacional benefiting from PL’s poor administration of PRAF-II.

The source of the persistent effect is a natural extension to the argument that PRAF beneficiaries were responding to the poor implementation of the program, and the insufficient transfer amounts in their voting patterns in the 2001 election. The persistent and now significant positive effect of the program on the Partido Nacional vote share in 2005 suggests that PN lived up to the promises it had made prior to the 2001 election to run the program better. Increases in PN’s vote share that were insignificant in the 2001 election are now significant.

This is perhaps reflective of two major changes that occurred between 2001 and 2005 in the administration of PRAF. In a fairly public move, President Ricardo Maduro of the National Party, who was elected in 2001, laid off all PRAF employees. While the concrete benefits of this are unclear, such a signal may have contributed to a perception that the program was under new and therefore better administration. A program redesign in 2002 and 2003 saw the introduction of a new, more transparent formal banking system to deliver transfers to beneficiaries. The PRAF program began working with Banco Nacional de Desarrollo Agrícola (BANDESA), which did not have complete coverage in PRAF areas, in 2003 and with Banco Hondureño del Café (BANHCAFÉ) in 2004, which improved the efficiency of
transfer distribution. BANHCAFE also provided buses for beneficiaries to travel to the bank, and on occasion provided food for people waiting to receive their transfers (Moore, 2008).

In A8, we break these results down by poverty block to test if most of the effect is due to block 5 municipalities, as initial experimental estimates in A3 seemed to show. This does indeed seem to be the case. The estimates of the effect on incumbent vote share in the three elections are slightly smaller in magnitude in 2005 than in 2001. However the effect of the program on PL share remains negative across all three elections and the program has a larger negative impact for the incumbent in block 5 relative to other blocks. We note here that while the magnitudes of the estimates are still relatively large (6.58 percentage points in the presidential election, 3.32 percentage points in the congress election and 7.71 percentage points in the mayoral election), coefficients are largely insignificant. This suggests a conservative conclusion that the program had no effect on PL vote share in 2005, even in block 5 municipalities. However, it is an encouraging sign that an examination of persistent effects results in negative coefficients on treatment, just as our initial analysis (using 2001 data) did.

The same analysis was conducted on Partido Nacional vote share. Here, the estimates for the effect of the program are generally positive (suggesting similar conclusions to those from 2001 data) but vary widely in magnitude and are generally insignificant. The insignificance of the estimates for both parties’ vote shares could simply be a result of lack of power in the regressions due to small sample size (only eight municipalities in each poverty stratum received transfers), but the estimates of program effect generally follow trends observed in 2001.

A similar analysis was not done for voter participation due to lack of accurate data on the number of registered voters for the 2005 election.
8.2 Inclusion of Lagged Vote Share from All Elections

In a robustness check, basic experimental results were repeated using lagged 1997 dependent variables for every election in every specification, to provide additional variation. For example, PL vote share in 1997 for presidential, congress and mayoral elections were all included in testing the effect of the program on PL vote share in 2001 in the presidential election. Results were robust to these additional lagged variables.

8.3 Testing Fragility of Block 5 Estimates

In a test of fragility of basic experimental results (when broken down by block), regressions are re-estimated with each of the block 5 municipalities eliminated in turn, to test whether the strong and significant negative effects of the program on incumbent vote share in block 5 municipalities is driven by a small number of outliers. This is a potential issue given that each poverty stratum has only 14 municipalities.

It seems that one municipality in particular is driving the positive effect of the program on opposition vote share in block 5 municipalities. When the Gualala municipality in the Santa Barbara district is excluded, the size of the positive block 5 effect for the National Party decreases to 4.48 percentage points in the Corporaciones Municipales election, and becomes insignificant. The effect is of a similar magnitude in the Diputados election, but there is no change in our estimates for the effect of the program on PN vote share in block 5 municipalities in the presidential election. Gualala was eligible for the transfers but did not receive them. The Gualala case is a particular exception because it has traditionally been a strongly liberal municipality (with the PL receiving more than 75% of the votes), and
so including it as a control municipality would have most likely exacerbated the effect of the cash transfers on PN vote share.

However, the overall experimental results are unaffected by the exclusion of Gualala from all specifications. This result indicates that the effect of the program on vote shares in block 5 municipalities is not as significantly different from the effect of the program in other blocks. However, all the municipalities taken together in the experimental evaluation do indicate a positive effect of the program on *Partido Nacional* vote share, as we concluded earlier. What now changes about our conclusion is that this effect is not driven mainly by block 5 municipalities, but by all blocks.
9. Conclusions

Cash transfer programs have been implemented in a variety of ways in different countries in
Latin America. Fundamentally, they aim to ease short-term poverty by supplementing the income of the
most impoverished. At the same time, the enforcement of conditions related to health and education is
expected to promote human capital development. Healthier, better educated people are naturally more
productive in the labor force. Economists have capitalized on some of the targeting and self-evaluation
rules of CCT programs to test whether the programs are achieving their goals, and have found positive
effects of the programs on schooling and education in a number of Latin American countries. We use
the same empirical strategies, but to test political response to such programs.

In general, testing the effect of fiscal interventions on politics is tricky. Fiscal interventions may
themselves be distributed according to political leaning (districts that show more support for
incumbents may be preferentially allocated cash transfers, for example), and certain types of fiscal
interventions (federal funding, for example) are not directly distributed to households. However, the
administration of CCTs have allowed for two different types of studies to be carried out: (1) In some
cases, the cash transfers are randomly allocated to some of the eligible households or geographic
regions, which allows us to compare political outcomes in recipient households or regions with
outcomes from identical eligible (but non-recipient) households or regions. (2) Most CCT programs are
targeted to poor households, and the threshold of eligibility for the household or municipality is
generally set arbitrarily according to the severity of poverty in the country and the amount of funding
available for the program. Municipalities that are very close to the threshold are therefore as good as
randomly assigned into the eligible group and the ineligible group, and therefore a comparison of
political outcomes in the eligible and ineligible municipalities close to the threshold can be causally
attributed to the program. In this paper, we carry out both types of study.
We look at evidence from PRAF-II, a conditional cash transfer implemented in Honduras in 1999, and use two empirical methods to estimate its effect on three political outcomes of interest – the incumbent party’s vote share, the key opposition party’s vote share, and overall voter participation. Evaluations of the political impact of CCTs in Mexico, Brazil, Colombia and Uruguay have had mixed results, and it is therefore difficult to hypothesize the magnitude of an effect in the Honduran case. Honduras’ political history suggests that we may find that both eligibility and actual transfer receipt cause voters to swing their votes from opposition parties towards the incumbent, leading to positive and significant estimates in both strategies.

However, experimental results indicate that there has been no effect or perhaps even a slightly negative effect on incumbent party vote share, and a positive and significant effect on support for the opposition. The opposition also appears to gain some votes from people who previously voted for fringe parties. Prior work using both of our empirical strategies has estimated that Mexico’s Progresa has had insignificant or negligible effects on incumbents, while the Progresa transfer was in fact more generous than the PRAF-II transfer that we evaluate. The first part of our conclusion is therefore unsurprising. However, the fact that the opposition party benefited from a program implemented by the incumbent government suggests perhaps that there were some issues with program implementation that cost the incumbent party significant support. Moore (2008) points out that the infrequency of transfers, in particular, may have been a frustrating issue for people who were expecting that the transfers would significantly supplement their consumption. Access to transfers was also an issue before the PRAF administration teamed up with banks that had branches in rural areas.

Experimental conclusions indicate two things: (1) Voters consider future economic benefit in their decision-making, that is, they follow prospective rather than retrospective models of voting behavior. This is clear from the fact that the key opposition party benefits from a program that they did
not implement, and because it gain votes from former supporters of both the incumbent and the fringe party in the 2001 election. The fact that the program has persistent effects on National Party vote share is also suggestive of this. (2) The costs or benefits of the program to vote share for either party tended to be larger in the more localized mayoral elections than the presidential and congress elections, indicating that program attribution is perhaps more localized. This makes intuitive sense for a program that was only implemented in select municipalities, rather than on a national level. We estimate the experimental effect by poverty stratum to identify the source of the effect, but find from our fragility analysis that the effect is not concentrated in any one poverty stratum.

Our regression discontinuity estimates suggest that relative to those who were ineligible, those who received the transfers did show increased support for the incumbent, which contradicts Green’s findings in Mexico using the same empirical strategy. It is difficult to directly compare our experimental results with our discontinuity estimates, since the two evaluations compare two different groups of voters. One way to synthesize the two is the following: In Honduras, perhaps eligibility for the program was a key motivator of support for the incumbent, while some inadequacies of the actual implementation of the program hurt the incumbent government. Eligibility for the program could have been an important driver of political support, particularly in the aftermath of hurricane Mitch, since eligible municipalities would have needed as much fiscal support from the government as possible for rebuilding efforts. The experimental results could then derive from disappointing inefficiencies in program implementation. This theory fits in well with the fact that PRAF underwent significant changes in the 2001 to 2005 period, and that the positive effect of the program for the Partido Nacional persisted until 2005.

There are certainly avenues for more work in this area. Within Honduras, interviews in the field with people who benefited from PRAF could provide some useful information about people’s perception
of the program. One clear limitation of this study is that it proposes but cannot pinpoint reasons for the interesting political effects we estimate. The program has continued to expand in the last decade, so some more recent evaluations may also be helpful (though such evaluations will have to work around the 2009 coup and relative instability of Honduras’ political arena). In addition, evaluations using political opinion surveys rather than municipality-level aggregated voting results could better identify the effect of the program, and identify spillover effects to non-beneficiaries within beneficiary municipality. There are also numerous other types of fiscal transfer programs around the world that have yet to be studied in terms of their political impact. These could also provide an interesting context for estimation of political effects of fiscal interventions.
10. References

Articles


**Books and Other Publications**


**Official Documents**

International Food Policy Research Institute (IFPRI). “*Primer Informe de PRAF II*”


**Electoral Data**


11. Appendix

A1 Summary Statistics

<table>
<thead>
<tr>
<th>Independent and Control Variables</th>
<th>Treatment</th>
<th>Control</th>
<th>P-value of test</th>
</tr>
</thead>
<tbody>
<tr>
<td>Number of municipalities</td>
<td>40</td>
<td>30</td>
<td></td>
</tr>
<tr>
<td>Proportion 18 and over eligible for program</td>
<td>0.772</td>
<td>0.755</td>
<td>0.139</td>
</tr>
<tr>
<td>Proportion literate</td>
<td>0.630</td>
<td>0.618</td>
<td>0.567</td>
</tr>
<tr>
<td>Average number of years of school</td>
<td>2.816</td>
<td>2.690</td>
<td>0.441</td>
</tr>
<tr>
<td>Lenca</td>
<td>0.278</td>
<td>0.290</td>
<td>0.835</td>
</tr>
<tr>
<td>Proportion of individuals relocated due to Mitch</td>
<td>0.018</td>
<td>0.013</td>
<td>0.253</td>
</tr>
<tr>
<td>Proportion Female</td>
<td>0.496</td>
<td>0.495</td>
<td>0.837</td>
</tr>
<tr>
<td>Proportion with access to computer</td>
<td>0.003</td>
<td>0.002</td>
<td>0.676</td>
</tr>
<tr>
<td>Proportion with access to television</td>
<td>0.093</td>
<td>0.102</td>
<td>0.706</td>
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<tr>
<td>Age</td>
<td>37.369</td>
<td>37.584</td>
<td>0.520</td>
</tr>
</tbody>
</table>

Year 1997

Corporaciones Municipales

| PL Vote Share | 0.477 | 0.468 | 0.694 |
| PN Vote Share | 0.461 | 0.463 | 0.924 |
| Voter Participation | 0.770 | 0.775 | 0.878 |

Diputados

| PL Vote Share | 0.457 | 0.457 | 0.977 |
| PN Vote Share | 0.483 | 0.477 | 0.769 |
| Voter Participation | 0.756 | 0.774 | 0.612 |

Presidente

| PL Vote Share | 0.459 | 0.465 | 0.804 |
| PN Vote Share | 0.492 | 0.486 | 0.799 |
| Voter Participation | 0.767 | 0.787 | 0.553 |

Dependent Variables

Year 2001

Corporaciones Municipales

| PL Vote Share | 0.451 | 0.476 | 0.175 |
| PN Vote Share | 0.501 | 0.465 | 0.0379** |
| Voter Participation | 0.851 | 0.861 | 0.782 |

Diputados

| PL Vote Share | 0.425 | 0.436 | 0.613 |
| PN Vote Share | 0.504 | 0.472 | 0.1189* |
| Voter Participation | 0.851 | 0.861 | 0.777 |

Presidente

| PL Vote Share | 0.444 | 0.456 | 0.535 |
| PN Vote Share | 0.518 | 0.503 | 0.417 |
| Voter Participation | 0.850 | 0.861 | 0.769 |

Source: 2001 Honduras Census, 1997 and 2001 Honduras election data, and author’s calculations
Notes: *** indicates statistical significance at 1%, ** at 5%, and * at 10%.
P-values reported are taken from t-tests of differences in means of variables between the two groups of municipalities.
## A2 Experimental Estimates – Vote Share

### Panel A

**Dependent Variable: PL Vote Share in 2001**

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>Presidencial</th>
<th>Diputados</th>
<th>Corporaciones Municipales</th>
</tr>
</thead>
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<tr>
<td></td>
<td>1</td>
<td>2</td>
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<td>Treatment</td>
<td>-0.00773</td>
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<td>-0.000948</td>
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<tr>
<td></td>
<td>(0.00873)</td>
<td>(0.00903)</td>
<td>(0.00994)</td>
</tr>
<tr>
<td>Treatment * Proportion Voters Eligible</td>
<td>0.0234</td>
<td>-0.0583</td>
<td>0.0577</td>
</tr>
<tr>
<td></td>
<td>(0.210)</td>
<td>(0.229)</td>
<td>(0.337)</td>
</tr>
<tr>
<td>Proportion of Voters Eligible</td>
<td>-0.0619</td>
<td>-0.181</td>
<td>-0.126</td>
</tr>
<tr>
<td></td>
<td>(0.130)</td>
<td>(0.311)</td>
<td>(0.206)</td>
</tr>
<tr>
<td>PL Share, 1997 C Election</td>
<td>0.448***</td>
<td>0.436***</td>
<td>0.464***</td>
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<tr>
<td></td>
<td>(0.0878)</td>
<td>(0.0881)</td>
<td>(0.0956)</td>
</tr>
<tr>
<td>PL Share, 1997 D Election</td>
<td>0.714***</td>
<td>0.707***</td>
<td>0.727***</td>
</tr>
<tr>
<td></td>
<td>(0.0820)</td>
<td>(0.0836)</td>
<td>(0.0870)</td>
</tr>
<tr>
<td>PL Share, 1997 P Election</td>
<td>0.730***</td>
<td>0.724***</td>
<td>0.734***</td>
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<tr>
<td></td>
<td>(0.0518)</td>
<td>(0.0538)</td>
<td>(0.0566)</td>
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<td>Additional Controls?</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
</tr>
<tr>
<td>Observations</td>
<td>70</td>
<td>70</td>
<td>70</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.803</td>
<td>0.804</td>
<td>0.833</td>
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</table>

Robust standard errors in parentheses

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

All specifications control for block dummies and include a constant term. Proportion eligible was rescaled at the mean. Additional controls include all aggregated census variables presented in A1.

Running regressions with additional lagged controls for vote share in other elections did not affect results.
Panel B

**Dependent Variable: PN Vote Share in 2001**

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>Presidencial</th>
<th>Diputados</th>
<th>Corporaciones Municipales</th>
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<tr>
<td><strong>1</strong></td>
<td>0.0109</td>
<td>0.0277**</td>
<td>0.0370**</td>
</tr>
<tr>
<td><strong>2</strong></td>
<td>0.0111</td>
<td>0.0252*</td>
<td>0.0356**</td>
</tr>
<tr>
<td><strong>3</strong></td>
<td>0.00292</td>
<td>0.0118</td>
<td>0.0258</td>
</tr>
<tr>
<td><strong>4</strong></td>
<td>0.00252</td>
<td>0.0132</td>
<td>0.0277</td>
</tr>
<tr>
<td><strong>VARIABLES 1 2 3 4 5 6 7 8 9 10 11 12</strong></td>
<td></td>
<td></td>
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<tr>
<td>Treatment</td>
<td>(0.00841)</td>
<td>(0.0133)</td>
<td>(0.0149)</td>
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<td>Treatment * Proportion Voters Eligible</td>
<td>-0.0999</td>
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<td>-0.524</td>
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<td>Proportion of Voters Eligible</td>
<td>(0.202)</td>
<td>(0.308)</td>
<td>(0.350)</td>
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<td>PN Share, 1997 C Election</td>
<td>0.0278</td>
<td>0.379**</td>
<td>0.305</td>
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<td>PN Share, 1997 D Election</td>
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<td>PN Share, 1997 P Election</td>
<td>0.0278</td>
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<td>Additional Controls?</td>
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<td>Observations</td>
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<td>R-squared</td>
<td>0.807</td>
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<td>0.339</td>
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Robust standard errors in parentheses

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

All specifications control for block dummies and include a constant term. Proportion eligible was rescaled at the mean. Additional controls include all aggregated census variables presented in A1.

Running regressions with additional lagged controls for vote share in other elections did not affect results.
## A3 Experimental Estimates by Poverty Block

### Panel A

**Dependent Variable: PL Vote Share in 2001**

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>Presidencial</th>
<th></th>
<th></th>
<th></th>
<th>Corporaciones</th>
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Robust standard errors in parentheses

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

All specifications control for block dummies and include a constant term.

Additional controls include all aggregated census variables presented in A1.
### Panel B

**Dependent Variable: PN Vote Share in 2001**

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<td>0.664***</td>
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Robust standard errors in parentheses

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

All specifications control for block dummies and include a constant term.

Additional controls include all aggregated census variables presented in A1.
A4 Experimental Estimates – Voter Participation

Dependent Variable: Voter Participation in 2001

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</table>

Robust standard errors in parentheses

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

All specifications control for block dummies and cluster by block.

All specifications also include a constant term. Additional controls are aggregated municipal-level census variables listed in A1.

Voter Participation was calculated using 2001 census data on number of individuals over the age of 18 living in the municipality.
A5 Regression Discontinuity Descriptive Figures – Vote Share

Notes: Plotted points are means of the dependent variable taken across municipalities sorted into bins of width 0.05 by predicted HAZ score. Predicted HAZ score is rescaled so that the threshold of eligibility is 0.
## A6 Regression Discontinuity Estimates – Vote Share

**Bandwidth: 0.25**

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<td>Treatment Presidente Diputados Corporaciones Municipales</td>
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Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1
## A7 Persistent Effects – Vote Share

### Panel A

**Dependent Variable: PL Vote Share in 2005**

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Robust standard errors in parentheses

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

All specifications control for block dummies and include a constant term. Proportion eligible was rescaled at the mean. Additional controls include all aggregated census variables presented in A1.

Running regressions with additional lagged controls for vote share in other elections did not affect results.
### Panel B

#### Dependent Variable: PN Vote Share in 2005

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<td>0.681</td>
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<td>0.412</td>
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<td>0.225</td>
<td>0.236</td>
<td>0.418</td>
<td>0.445</td>
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</table>

Robust standard errors in parentheses

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

All specifications control for block dummies and include a constant term. Proportion eligible was rescaled at the mean. Additional controls include all aggregated census variables presented in A1.

Running regressions with additional lagged controls for vote share in other elections did not affect results.
## A8 Persistent Effects by Poverty Block

### Panel A

**Dependent Variable: PL Vote Share in 2005**

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Robust standard errors in parentheses

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

All specifications control for block dummies and include a constant term. Proportion eligible was rescaled at the mean. Additional controls include all aggregated census variables presented in A1.
### Panel B

**Dependent Variable: PN Vote Share in 2005**

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Robust standard errors in parentheses

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

All specifications control for block dummies and include a constant term. Proportion eligible was rescaled at the mean.

Additional controls include all aggregated census variables presented in A1.