Do Conditional Cash Transfers Affect Electoral Behavior? Evidence from a Randomized Experiment in Mexico

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Abstract

What are the electoral consequences of tackling poverty with Conditional Cash Transfer programs? I present evidence that cash transfers contingent upon investments on children's education, nutrition and health foster pro-incumbent support and increase turnout. The evidence comes from the pioneering Progresa, the Mexican CCT. Experimental data shows that full enrollment in the program twenty-one months before the 2000 presidential election, compared to enrollment in the program six months prior to the election, lead to an increase in turnout of seven percent and an increase in incumbent vote share of sixteen percent. A comparison between experimental precincts and precincts excluded from the program by the time of the election using propensity score methods reveals that the longer exposure to the program is responsible for the increases in turnout and incumbent vote share. These results are not consistent with theories of clientelism and vote buying. Instead, the data suggests that Progresa increased turnout and incumbent vote share mainly due to retrospective voting.

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I. Introduction

During the last 15 years, poverty relief programs have undergone significant transformation throughout the developing world. The transition started in the late-1990s when some countries in Latin America adopted Conditional Cash Transfer programs (CCT) to mitigate the social costs of macroeconomic adjustment. CCT are highly redistributive and targeted policies aimed at increasing access of low-income groups to education and health services (Mares and Carnes 2009). CCT innovative feature is that transfers are contingent upon investments on children's education, health and nutrition. Although these new social policies were first introduced by pro-free market governments in Mexico and Brazil, [2] currently at least thirty governments from all sides of the ideological spectrum have adopted a CCT, including Argentina, Chile, Colombia, Dominican Republic, Ecuador, El Salvador, Honduras, Jamaica, Nicaragua, Paraguay and Peru. Outside of Latin America, Turkey, Nigeria, Burkina Faso, Kenia, Yemen, Indonesia India, Cambodia, Bangladesh, Macedonia, Pakistian, Philippines and even New York City have their own CCT (Fiszbein and Schady 2009). The primacy of CCT is reflected in the World Bank's decision to lend USD$2.4 billion to start or expand CCT operations in response to the current financial crisis (World Bank 2009).

This transformation of antipoverty programs raises fundamental questions about the electoral effects of government spending. Are CCT recipients turning out to vote at higher rates than non-recipients? Are CCT persuading its recipients to cast a ballot in favor of the incumbent? Speculations about CCT electoral returns have been present in the media after the presidential elections in Ecuador, Peru, Trinidad and Tobago (BBC 2009), and, most notoriously, Mexico and Brazil (The Washington Post 2006, Lindert and Vincensini 2008). If these speculations are right, CCT not only have positive effects on household consumption, children's health and school enrollment (Fiszbein and Schady 2009), but have the attractive feature of mobilizing and nudging recipients in favor of the incumbent.

Uncovering the effect of enrollment in a CCT on a person's likelihood of turning out to

[2] under the administrations of President Ernesto Zedillo and Fernando Henrique Cardoso in Mexico and Brazil, respectively.

vote, or their vote choice, presents some challenges. Program recipients are unlike program non-recipients in observable, and perhaps unobservable ways. Thus comparing recipients and non-recipients will conflate the impact of the program with pre-existing differences, such as poverty, that also affect electoral behavior. Without exogenous variation in program enrollment, an empirical analysis could establish a correlation between this type of social policy and participation, at best. Moreover, since most CCT operate in countries where vote buying and clientelism are prevalent, the possibility that funds are allocated using electoral criteria not only compromises causal inference, but reinforces the view in some policy and academic circles that regardless of the specific characteristics of a program, targeting the poor is part of a political strategy to win votes (Tobin and Kosack 2009). Often social policies are indiscriminately classified as an exchange between a politician and a voter whereby material favors are offered in return for political support (Colburn 2002). Along with this classification comes the assumption that such funds are an effective strategy to win votes but are a threat to democracy. For instance, talking about the Mexican CCT, Cornelius (2004) concluded that the only explanation for a CCT recipient to cast her vote in favor of the incumbent party was that she must have felt coerced or threatened. Yet in the distributive politics literature, the impact of voting on the allocation of government spending receives more attention than the reverse. Thus, the extent to which, and the reasons why, CCT shape electoral behavior remain unknown.

To overcome these challenges, I employ a unique experiment conducted in the early stages of the Mexican Education, Health, and Nutrition Program (Progresa), one of the first and largest CCT. [3] Progresa is often cited in international forums as an example of a successful CCT program. The empirical strategy in this article draws on the fact that 505 villages were randomly treated either twenty-one months or six months before the 2000 presidential election. The long and short durations of the treatment offer the opportunity to estimate the causal effect of the program on turnout and the direction of the vote since enrollment in either group is not correlated to other factors that shape voting behavior,

[3]With the change in federal administration in 2000, the program was renamed Oportunidades. In this paper I will refer to it as Progresa.

such as poverty or past voting behavior.

The experimental data reveals that early enrollment in Progresa lead to a seven percent increase in turnout and a sixteen percent increase in incumbent vote share. A comparison using propensity score methods between the experimental precincts and a third group of precincts excluded from the program by the time of the election shows that the increases in turnout and incumbent support are driven by the longer exposure to the program while the shorter exposure had no effect.

Progresa's electoral returns could be explained through various channels, including conventional explanations based on clientelism, differential cost of voting among recipients and non-recipients and retrospective voting. The design of the experiment allows to disentangle some of the alternative explanations for these patters because the two treatment groups received cash transfers, but the longer exposure to the program translated into health and education improvements. The data suggest that clientelism does not account for the increase in turnout and incumbent gains observed among recipients. Instead, the results are compatible with retrospective voting.

Rigorous empirical study of CCTs electoral returns is just beginning. Diaz-Cayeros et al. (2006) and Zucco (2008), for instance, use methods of covariate adjustment (matching and regression) to calculate the electoral returns of the Mexican and Brazilian CCT, respectively. The reliability of the estimates based on these methods, however, rest on the assumption that the inclusion of covariates makes program enrollment exogenous. Green (2005) and Manacorda et al. (2010) calculate the electoral returns of the Mexican and Uruguayan CCTs using regression discontinuity designs. With these designs, electoral returns can be calculated by comparing villages around the eligibility threshold. This empirical strategy, however, faces the challenge that around the discontinuity point data may be sparse. While expanding the interval around the eligibility threshold would increase precision, it would also increase the probability of bias (Green et al 2009). This article is the first to provide estimates of CCT electoral returns using experimental data.

This article's evidence of a pro-incumbent effect contributes to a growing literature

on social policy in the developing world, where our understanding of the variation in the design of social protection and the political factors behind them remains limited (Mares and Carnes 2009 94). It is evident, however, that CCT ability to foster support for the incumbent explains, in part, the fact that left and right leaning governments have found this particular type of social policy attractive. More generally, this article speaks to the literature on the politics of welfare programs, where there is no consensus on the relationship between enrollment in welfare and political participation. For instance, the US experience shows that enrollment in some welfare programs leads to an increase in political participation presumably to protect or expand benefits (Campbell 2003, Mettler and Soss 2004), but enrollment in other programs stigmatizes recipients and lowers their sense of internal efficacy (Soss 1999).

This paper proceeds as follows. The following section describes the program. Section III includes the empirical strategy, data, and variables. Section IV presents the results from comparing the early and late treatment groups using the experimental data. Section V reports the results when comparing the experimental groups with a third control group weighted by the probability of being part of the experiment. Section VI explores the external validity of the experimental results with a municipal level analysis of the impact of program coverage on turnout and incumbent vote shares in 2000 and 2006. Section VII asks why would a CCT shape electoral behavior. I explore the alternative mechanisms behind Progresa's impact and set out an agenda for research in CCT electoral returns. Finally, Section VIII concludes.

II. Progresa

The peso crisis of 1995 caused the Mexican GDP to shrank by seven percent and private consumption by twelve percent. The population living in poverty increased from fifty-two percent in 1994 to almost sixty-nine percent in 1996. More than sixteen million people fell into poverty (Gil Díaz and Carstens 1996). The economic crisis hit the hardest in rural areas where poverty increased from thirty-seven to fifty-two percent, while in urban areas

it increased from ten to twenty-six percent.[4]

At that time, Mexico's federal government ran fifteen food subsidy programs: four were generalized and eleven were targeted at different urban and rural populations. These programs were operated by ten distinct ministries or agencies, and varied in coverage and size (Levy 2006). Despite the fact that poverty was especially prevalent in rural areas, seventy-five percent of the total budget for existing poverty relief programs was channeled to urban areas. "In fact, over half of the total budget was absorbed by the generalized bread and tortilla subsidies in the urban areas, where most of the income transfer was captured by non-poor households... In 1995 close to sixty percent of all poor rural families received no food support at all from government" (Levy 2006: 6).

In 1994, the most influential anti-poverty program in place, the National Solidarity Program (PRONASOL), was decentralized and used citizen participation as a central element in project selection, funding and implementation. This community based and demand driven program "achieved a high profile, however, was susceptible to local political influences" (Skoufias et al. 1999). Moreover, this program was characterized as a clientelistic program that fostered pro-PRI electoral dynamics without efficiently reducing poverty.[5]

Responding to this context, Progresa began operating in 1997 in the administration of President Ernesto Zedillo. The program delivers cash transfers to the female heads of poor households in rural communities with the objective of breaking the intergenerational transmission of poverty by promoting investments on children's education, health and nutrition. Unlike previous Mexican programs which funneled resources through the education and health sectors, Progresa aims to increase the demand for such services by transferring cash directly to households. The program consists of three complementary components: a cash transfer, primarily intended to subsidized food expenditure. This transfer comes with a nutritional supplement targeted to children between the ages of four months and two years and pregnant and lactating women; a scholarship, intended to compensate for the opportu-

[4]The poverty lines were 2.09 and 1.54 2000-USD per day in urban and rural areas, respectively.

[5]For a review of the political economy of PRONASOL see: Dresser 1991, Cornelius et al. 1994, Molinar and Weldon 1994, Fox 1994, Bejar et al. 1993, Bruhn 1996, Kaufman and Trejo 1997, Soederberg 2001, Diaz-Cayeros et al. 2002 and Pérez Yarahuán 2005.

nity cost of child labor, thus enabling children to stay in school; and basic health care for all members of the household with particular emphasis on preventive health care (Poder Ejecutivo Federal 1997) .[6] All the components add up to an average transfer of thirty-five US dollars per month, which represent approximately twenty-five percent of the average poor rural household income in the absence of the program. Of this amount, cash transfers from the education component represent fifty percent; cash transfers from the nutritional component thirty-six percent; in-kind food supplements, approximately four percent; and medicines and other services provided at the health clinics, ten percent. Thus, more than eighty-five percent of the benefits of the program are in cash. Transfers are paid every two months (Levy 2006).

The scholarship increases with the child's grade level to offset the greater opportunity cost of schooling for older children who are more likely to engage in household production or market work (Todd and Wolpin 2006). The transfer is slightly higher for girls who have lower secondary school enrollment rates. In its original design, Progresa grants were provided to children in third grade through secondary school. After 2001, grants were extended to high school levels. Thus, the total amount of the grant received depended on the number of children in the household as well as the gender and age of each child, but the transfer is capped at a pre-established upper amount.

Progresa is a conditional cash transfer because receipt of benefits is contingent upon school attendance (only three absences a month are allowed), regular medical check-ups and consultations at health care centers, and attendance to *pláticas* (meetings) where physicians, nurses and program officials talk to household members about health, hygiene and nutrition issues. The conditionalities are verified through the school and clinics records.

From September of 1997 to 2000, Progresa operated only in rural areas. After the right-wing party's (PAN) presidential victory in 2000, the program was continued in the rural areas and expanded to the semi-urban areas using parallel criteria to select recipients. In

[6] Interventions in the health service package include: basic sanitation, family planning, prenatal, childbirth and puerperal care, vaccinations, prevention and treatment of diarrhea, antiparasite treatment, prevention and treatment of respiratory infections, tuberculosis, high blood pressure and diabetes mellitusm and first aid for injuries (Parker and Teruel 2005).

2003, the program was extended to urban areas.

The program is one of the largest efforts to invest in poor children's education through the demand side. Between 1997 and 2000 Progresa enrolled 2.6 million households. In 1997, Progresa's budget was nineteen million dollars. By 2000, the budget increased to eight-hundred and thirty-five million dollars. Currently, it spends about US$2.5 billion a year (Gertler 2005).

Compared to previous programs based on general subsidy schemes, Progresa is more redistributive. In 1994, the highest and lowest income deciles benefited from the main food subsidy at almost the same rate, six and seven percent respectively. In contrast, the highest income decile does not benefit from Progresa's food component, whereas the lowest decile received thirty-five percent of it (Scott 2001).

Besides the innovative use of conditionalities, three characteristics set Progresa apart from other poverty relief programs in Mexico. First, Progresa's operational rules are explicitly non-partisan. The program has clear and fixed criteria for determining eligibility, as oppose to political considerations. These criteria are based on a geographical measure of poverty constructed from census data and household income surveys. The resources of the program and the formula to allocate them are described in detail in the federal budget, which is proposed by the executive but approved in the Chamber of Deputies.[7] The provisions in the federal budget decree explicitly prohibited the use of the program to proselytize by any political party. Since 1998, all documents, materials, and forms that reach recipients have been required to include the following text:

> We remind you that your participation in Progresa and receipt of benefits are in no way subject to affiliation with any specific political party or to voting for any specific candidate running for public office. No candidate is authorized to grant or withhold benefits under the program. Eligible beneficiary families will receive support if they show up for their doctor's visits and health education talks and if their children attend school regularly. Any person, organization,

[7] In 1997, the seventy-year ruling party (PRI) lost the majority in the Chamber of Deputies.

> or public servant that makes undue use of program resources will be reported to the competent authority and prosecuted under applicable legislation (Levy's translation 2006: 107)

Second, Progresa was designed first and then assigned a place in the federal bureaucracy. To administer Progresa a new autonomous agency was created. The agency is a satellite of the Ministry of Social Development. The agency is in charge of the implementation of the program all the way down to delivering cash transfers directly to beneficiaries. All intermediaries are circumvented, including traditional and powerful mechanisms of federal money's distribution such as governors and the state level bureaucracy of the Ministry of Social Development. The incentives of the new bureaucracy were shaped by a decree which established that using Progresa or any other social program for political reasons is a federal offense.

Despite strong pressures from ministers who controlled the existing programs at that time, the coordinator of the Progresa agency was designated directly by the president. A committee made up of representatives from the ministries of Education, Health, and Social Development was also appointed; however, this committee had no capacity to compensate these ministries, either politically or financially. Unlike previous administrators of prominent poverty relief programs who were mostly politicians, the first coordinator of Progresa, Gómez de León, was a scientist. [8]

Finally, Progresa was insulated from the temptation to disproportionately increase the list of beneficiaries close to election time. Although the program was ready to be launched in January 1997, its operation was delayed until August, one month after the midterm elections of that year. This practice was continued by including in the budget decrees of

[8] Gómez de León was trained as a demographer on the Catholic University of Leuven, Harvard, and Princeton. Prior to Progresa, he had been the director of Conapo. And before that, he had coordinated the department of Demography of the Center for Economic and Demographic Studies at el Colegio de Mexico. Perhaps Gómez de León group's most important field experience was a birth control campaign that did not have the double objective of delivering resources to poor households and fostering political support for the incumbent party. As one of the members of his group explained: It was complicated enough to talk about sex and birth control methods to people in the rural areas, they did not wish to ask them for their vote on top of that (author's interview, Mexico city, August 2005).

2000 and 2003 a prohibition to include new beneficiaries in the program six months prior to election time. In sum, by adopting Progresa the executive decreased substantially its discretionary power to allocate social spending.

Progresa was the first social policy in Mexico evaluated through a randomized intervention. The design and implementation of the evaluation was in the hands of program officials in collaboration with the International Food Policy Research Institute. So far, the evidence is extremely positive. In terms of operation, the evaluation shows that the eligibility criteria described in the rules of operation predict actual enrollment in the program (Skoufias, Davis, and Vega 2001). Regarding the program effects on children well-being, the evaluation found that: "only after three years, poor Mexican children living in the rural areas where Progresa operates have increased their school enrollment, have more balanced diets and are receiving more medical attention" (Skoufias and McClafferty 2001, 3). More specifically, Progresa children aged one to five years have a twelve percent lower incidence of illness than non Progresa children (Gertler 2000) and eighteen percent lower incidence of anemia (Gertler and Boyce 2001) . Additionally, the program had positive effects on child growth. The impact of Progresa is equivalent to an increase of sixteen percent in mean growth rate per year (corresponding to one centimeter) for children enrolled in the program between twelve and thirty-six months of age (Behrman and Hoddinott 2000). The effects of the program are stronger among vulnerable subgroups. For instance, Progresa children aged 8-10 years whose mothers had no education grew an additional 1.5 cm (Fernald et al 2009). Beyond health indicators, enrollment in the program is also associated with a decrease in children's behavioral problems (Fernald et al 2009).

Although the program was designed to target children, adults in Progresa households are healthier too when compared to adults in non Progresa households (Gertler 2000). Adults in Progresa households experienced a significant reduction in the number of days of difficulty with daily activities and in the number of days in bed due to illness (Gertler and Boyce 2001). Finally, Progresa was found to have an effect over household expenditure patterns. Households in the program, spend thirteen percent more income on food than

households out of the program. Moreover, the additional food expenditure increased the acquisition of fruits, vegetables, meats and other animal products. Thus, dietary quality increased (Hoddinott et al. 2000).

In terms of education, enrollment rates in primary school were already high before the program and enrollment rates often fall dramatically in secondary school especially for girls. Thus Progresa's largest impact was reported on children who entered secondary school. For girls the increase in enrollment rates was twenty percent, and for boys it was ten percent (Parker and Teruel 2005). This additional education means that children, when becoming adults, will have higher permanent income by approximately 8 percent (Schultz 2001). Furthermore, the increase in enrollment due to Progresa is higher than the increase that would have been produced by the construction of additional secondary schools which is estimated to be 0.46 for girls and 0.34 for boys (Coady 2000).

III. Calculating Progresa's Electoral Returns

Estimating the effects of enrollment in Progresa on support for the then ruling party (PRI) presents some challenges. Program recipients are unlike program non recipients in observable (and perhaps unobservable ways). Thus comparing people enrolled in the program and people not enrolled will conflate the impact of the program with pre-existing differences. This is specially troublesome for the study of Progresa because the characteristics that set program recipients apart, such as poverty, are traits that also affect electoral behavior. After all, in 1997 the historic correlation between poverty, rural residence, and support for the PRI remained strong. To asses the effects of Progresa on electoral behavior, this article uses the randomized component of the program implemented in its early phases originally designed to evaluate program effects on schooling, nutrition and health. The random allocation of treatment provides a justification for causal inference (Green and Gerber 2003).

Randomization.

The program evaluation was carried out by program officials in collaboration with the International Food Policy Research Institute (IFPRI) between 1998 and 2000. The ran-

domized experiment was implemented in seven states where the program was first scaled up.[9] The experimental sample was drawn from the universe of eligible villages within these states. The sample selection process followed Progresa's targeting method closely. The first step was the selection of villages eligible to the program based on a poverty measure created with the 1990 census data and the 1995 partial census data. This poverty measure took into account educational levels, access to potable water and electricity, sewage coverage, etc. A threshold for eligibility was established allowing it to vary by broad geographical regions. The poverty index was divided into five categories that go from very low poverty to low, medium, high, and very high. Localities deemed to have a high or very high degree of poverty were considered priorities to be included in the program.

The second step was a result of the program's conditionalities. Since the program required the use of school and health services, additional criteria for selecting the experimental sample was based on geographic location, distance between localities, and the existence of health and school infrastructure. Those localities with access to school and health services or with available roads when the services were not located in the same community were considered eligible. In addition, localities with less than fifty or more than 2500 inhabitants were excluded. Finally, using Geographic Information System software, remaining localities were grouped based on geographical proximity using a routine that identified relatively isolated communities. The isolated localities were also excluded from the selection process (Progresa, 1998).

Randomization was implemented at the village level using a stratified sample by population. The final sample was drawn from a universe of 6,396 eligible villages. Families in 320 villages were randomly selected to receive benefits on September 1998, whereas 186

[9]The states are: Guerrero, Hidalgo, Michoacán, Querétaro, Puebla, San Luis Potosí, and Veracruz. The selection of these seven states responded largely to logistical and financial restrictions. The exclusion of two of the poorest states in the country from both the experiment and the first phases of the program deserves a few words. In the case of Chiapas, 1720 villages lacked data from the 1995 Partial Census probably because of the uprising of the guerrilla movement the previous year. In Oaxaca, political considerations prevented the implementation of the experiment. As the director of the program lamented: "In the early stages of Progresa, we could not make the program work in Oaxaca, our representatives ended up hurt most of the time when trying to do their job. We had to change our team because it was completely subordinated to the governor's interest at that time." (Author's interview, Mexico City, August 2005).

villages were excluded from the program until January 2000 (Schultz 2001). There was a sixty percent probability of being assigned to the early treatment group and a forty percent probability of being assigned to the late treatment group. In villages assigned to early treatment, all eligible households within each village, identified by the Household Socio-economic Characteristics Survey(ENCASEH), were offered enrollment in Progresa and normally accepted. In villages assigned to the late treatment group, none of the households received program benefits or services until January 2000 (Progresa, Methodological note: General Rural, 2006). By the 2000 presidential election, villages in the early and late treatment groups had been under treatment twenty-one months and six months, respectively.[10]

Basing the evaluation of a social program on a randomized intervention was not without its problems. On one hand, program officials expected that results based on a rigorous impartial evaluation would improve the chances that the program survived the change in federal administration in 2000. On the other hand, delaying the enrollment of a subgroup of eligible villages was a politically risky decision. Internally, program evaluators argued that budget restrictions prevented them from enrolling into the program all eligible households simultaneously. Thus, one fair way to decide the order in which families would be enrolled was to do it randomly. Yet to avoid confrontations that could jeopardize the program and the experiment, program officials publicized the evaluation until December 2000. At this point the evaluation was complete, and the results were made public. Some of the reactions in the media justified program officials' concerns. The evaluation was criticized on a prominent Mexican newspaper (*Reforma)*, first, on ethical grounds and second, on budgetary grounds (Parker and Teruel 2005). This attention in the media, however, came after the July 2000 presidential election. Thus, it did not altered the information available to the early and late treatment groups by the time of the election.

Data.

[10]According to the program's operational rules, after eligible households had been identified, the list of beneficiaries was presented to community assemblies. Their feedback was used to correct any inclusion or exclusion errors. Yet, this stage of selection was in practice irrelevant both for the experiment and the large scale operation of the program. As of 2000 "the number of households whose selection into Progresa was disputed at this stage of the selection process was minute (0.1 percent of the total number of selected households)" (Skoufias et al. 1999).

As in many countries, in Mexico, election results are not reported at the village level. Instead, election outcomes are reported at levels defined by the electoral law. Thus, to take advantage of the random assignment to treatment, I overlayed the 505 experimental villages to the smallest unit of outcome measure for which census, program, and electoral data roughly coincide: the *sección electoral* (precinct).[11] Neither villages nor precincts have fixed population size and generally they do not correspond one to one.

Because the unit of assignment to treatment and the unit of outcome measure do not overlap perfectly, the aggregation of villages into precincts brought in villages that were originally excluded from the experimental sample. [12] The 421 precincts that correspond to the experimental villages were in average more populous. In terms of poverty, however, the experimental villages and precincts remain very similar, with villages having an average poverty of 4.66 and precincts of 4.61 on a scale that goes from 1 to 5. The average share of a precinct's population living in an eligible village was .90. [13] To denote treatment status at the village level, a dummy variable taking the value of one when the village is part of the early treatment group and zero otherwise is sufficient. At the precinct level, however, (because precincts are more populous than villages) treatment status must be accompanied by a fractional response variable that captures the density of the treatment. Summary statistics of villages and precincts are presented in Table 1.

[11] IFE and INEGI use different identifiers for states, municipalities and villages so all merges were done by hand based on the villages names. Names of the villages were taken from the evaluation survey, ENCASEH 1997. Out of the 505 villages in the experiment, eight of them disappeared by the time of the 2000 Census. The remaining experimental villages were located in 465 precincts.

[12] The precincts with experimental villages included 3500 additional villages. INEGI records did not include 440 of the additional villages. For this reason, eighteen precincts had insufficient information to calculate their population and their poverty levels. Additionally five precincts disappeared after the 1996 redistricting process. Thus, these precincts were excluded from the analysis. In order to minimize measurement error, I retain precincts with 3000 inhabitants or less. This restriction eliminates 21 precincts that are outliers in terms of poverty levels and population size. As robustness checks, I also estimated all the models of the following sections using the full sample of precincts, I get practically identical results. Among the remaining precincts, forty-two corresponded one-to-one to their experimental village. Twenty precincts had between seventy-five and ninety-nine percent of their population living in randomly assigned villages. Forty-one precincts had between fifty-one and seventy-five percent of their population living in randomly assigned villages. One hundred and nine precincts had between twenty-six and fifty percent of their population living in randomly assigned villages. Finally, two hundred fifty-three precincts had between zero and twenty-five percent of their population living in an experimental village.

[13] To replicate the eligibility criteria I used the same Poverty Index (1995) used by program officials. Following the original randomization process, I defined a village as eligible if it scored a four or higher in the measure of poverty and had a population larger than 50 but smaller than 2500 inhabitants.

For the original randomization, Behrman and Todd (1999) show that villages in the late treatment group are a valid counterfactual for villages in the early treatment group as there are no systematical differences between them in terms of population size, age distribution, education levels, access to health services, and income. Once villages are aggregated into precincts, randomization still implies that assignment to receive early Progresa benefits is exogenous and, in principle, the baseline characteristics of the early and late treatment group should be balanced so that the late treatment group remains a reliable counterfactual for the early treatment group.

The data supports these claims. When comparing the baseline characteristics of the early and late treatment groups at the precinct level, there is no statistically significant differences between them in terms of demographic traits such as poverty, population, population living in an eligible village, even at the 10% significance level.

If Progresa was a vote buying strategy, its distribution would reveal some form of electoral bias. The data gives no evidence of this as there are no differences between early and late treatment groups in terms of pre-program electoral behavior (Table 2, column 3). The balance of pre-program traits remains even after taking into account the treatment status and the fraction of a precinct's voting age population living in randomized villages (Table 2, column 4).

Measurement

In the following analysis there are four dependent variables: turnout and party vote shares for each of the three largest parties (PRI, PAN, and PRD). Turnout is calculated as total number of votes as a share of potential voters living in the precinct. Potential voters refers to people with eighteen years or older. Similarly, vote shares are calculated as total number of votes for a given party as a share of potential voters in the precinct. The reason to measure these variables with respect to potential voters, as opposed to with respect to registered voters, is that the program asked the female head of the household for an identification. Since one of the most commonly used identification cards is the one issued by the Electoral Institute, then enrollment in the program could have the automatic

effect of increasing the number of registered voters.[14]

The main independent variables are *treatment*, which is a dummy variable that takes the value of one when the precinct includes villages from the early treatment group and zeros when the precinct includes villages in the late treatment group. *Dosage* is a continuous variable that indicates the share of a precinct's voters that live in randomly assigned villages. When *treatment* = 1, then *treatment* * *dosage* is the fraction of the precinct that was treated early.

As Table 2 shows, the baseline characteristics of precincts in the early and late treatment are balanced. Even so, the analysis in the next sections includes three types of covariate adjustments. First, all models include population changes and share of the precinct's population eligible to the program to take into account that the original randomization was stratified by population. [15] Second, as a robustness check, some of the specifications include state fixed effects. This set of variables take into account the fact that the education and health sectors are operated locally. Thus even if Progresa's cash transfers were standardized, the quality of the education and health services varied greatly from state to state. Finally, some specifications include the party at the local government in 1994 and 2000 to account for any differences caused by local political dynamics even though local government were circumscribed by Progresa.[16] Mostly, the effect of including covariates is to increase the precision of the estimates.

IV.Results.

Because of randomization, consistent estimates of Progresa's causal effect can be calculated by a difference-in-difference estimator. In this case, because there are only two time periods, 1994 and 2000, a fixed effects and a first differences model produce identical results. [17]

[14]The electoral data is compiled by IFE and reported in the Atlas of Federal Elections 1991-2000 and Statistics of the 2003 Federal Election.

[15]Population data comes from the Census of Population and Housing (1990, 2000) and the Partial Census (1995) produced by the National Statistics Agency (INEGI).

[16]Data on local elections was collected from the Center for Research on Development (CIDAC).

[17]I estimate a first-difference regression model. Turnout in a precinct can be written as:

$$y_{it} = \beta_0 + c_i + \beta_1(treatment) + \beta_2(dosage) + \beta_3(treatment * dosage) + \theta_2(year_2) + u_t \quad (1)$$

Table 3 presents the results when turnout is the dependent variable. To start, the effect of being in the early treatment group, as oppose to the late treatment group, is increasing in its dosage. In other words, the interaction of early enrollment in Progresa and dosage has a positive and significant effect on turnout. Once the party in control of the municipality and state fixed effects are included, the estimates suggest that full enrollment in the program twenty-one months before the election lead to an .05 percentage point increase in turnout (adding the main and interaction effects), significant at the five percent level (Column 3). Since in the late treatment group only 65 percent of people older than 18 voted, the effect of full enrollment in the early treatment group represents a 7 percent increase in voter turnout.

Did Progresa foster pro-incumbent support? Table 4 displays the results when party vote shares are the dependent variables. As with turnout, the interaction of early enrollment in Progresa and dosage has a positive and significant effect on incumbent vote share. Full enrollment in the program twenty-one months before election time, compared to enrollment in the program six months before the election, lead to an .05 percentage point increase in PRI vote share, significant at the ten percent level (Column 3). Because in the late treatment group only 36 percent of people older than 18 casted a ballot in favor of the incumbent, the effect of full enrollment in the late treatment group represents a 16 percent increase.

Turning to the causal effect of the program on the vote share of the two main opposition parties in 2000, Progresa had no effect on the right wing party, PAN (Column 6), but early enrollment in the program lead to a .05 decrease on PRD's vote share, significant at the

where y_{it} is turnout, β_0 is the intercept, *treatment* and *Dosage* are defined as described above. c_i captures unobserved characteristics of the precinct that are constant over time, and $year_2$ is a dummy variable that equals one for the post-program year, 2000. By doing a first-difference transformation, the unobserved but constant characteristics (c_i) are eliminated and the intercept gets differenced away.

$$\Delta y = \theta_2 + \beta_1(treatment) + \beta_2(dosage) + \beta_3(treatment * dosage) + \Delta u_{it} \quad (2)$$

where $\Delta y = y_{it} - y_{it-1}$. Treatment, dosage and their interaction are not differenced because in 1994 no precinct was incorporated in the program, so the change in this share is simply the share in 2000. θ_2 becomes the intercept in the differenced equation. The estimate of main interest is β_3. After estimating equation (3) with changes in turnout as the dependent variable, I estimated a specification equivalent to equation (3) but now define $\Delta y = y_{it} - y_{it-1}$ as the difference in vote share between 2000 and 1994. I estimated one equation for each party: PRI, PAN, and PRD.

one percent level (Column 9). Since only 9 percent of people older than 18 voted for the PRD, full enrollment in Progresa twenty-one months before the election lead to a decrease in PRD vote share of 41 percent.

V. Comparing the randomized groups to a third group excluded from the program

The previous section documents a robust positive difference in turnout and incumbent vote share between the early and late treatment groups. These results could suggests that Progresa had a mobilizing and a persuasive effect. In other words, voters who benefited from the program longer, participated at higher rates in the 2000 presidential election and voted at higher rates for the PRI than voters who benefited from the program a shorter period of time. The plausibility of this interpretation rests on whether voters in the late treatment group were unaware of their participation on Progresa's evaluation. Had they realized that program benefits were withheld for the sake of evaluation, the results in the previous section would also be compatible with the program leading to a decrease in turnout and incumbent vote share among the late treatment group due to their desire to punish the incumbent party for the randomization outcome.

There are a couple of issues related to the evaluation design and program operation that make the latter story worth while to consider. First, to measure health and education outcomes, the evaluation carried out repeated interviews in the early and late groups. Interviewers had the mandate of not revealing that the surveys were related to Progresa (Parker and Teruel 2005), yet the frequency of the interviews was extraordinary. Second, Progresa was scaled up in parallel to the evaluation. By the beginning of the experiment, the total number of beneficiaries was less than four hundred thousand. By the time the late group was enrolled in the program, there were 2 million households. It is plausible that the program expansion was noticed by the late treatment group (Parker and Teruel 2005). In such case, the late treatment group could have inferred that they were the losers of the randomization. Thus results in the previous section are insufficient to determine if the early treatment group turned out more or the late treatment group turned out less. Similarly,

the interpretation regarding changes in incumbent vote share is inconclusive. Moreover, the estimates in the previous section are illustrative when comparing the short and long duration of the treatment, yet they do not shed light to the effect of Progresa on recipients compared to non-recipients.

A way to determine if the early treatment turned out more or the late treatment turned out less, is to find a reliable control group that was not enrolled in the program at the time of the 2000 presidential election and was not affected by the experimental evaluation. The original experimental groups were taken from the universe of 6,396 eligible villages. From these population, 320 villages were randomly selected to be enrolled in the early treatment group, 186 were randomly enrolled in the late treatment group and, by default, 5891 eligible villages were randomly left out of the experiment. If the program had not been scaled up, then this third group of villages would have been a random control group. Progresa, however, was scaled up and eligible villages in this third group began to receive program benefits at different times. Thus, the group of eligible villages that by the time of the 2000 election were not enrolled in the program is not random. Following Hastings et al (2007), however, it is possible to estimate the causal impact of Progresa using an OLS model, weighted by a nonparametric estimate of the selection probability, or propensity score, to improve the balance of baseline characteristics between the experimental groups and the third non-random control group (Hirano, Imbens, and Ridder 2003, Hastings et al. 2007).

To construct this third group, I followed a similar procedure than before. First, I matched all eligible villages to their electoral precincts. Second, I kept in the analysis precincts that had not received program benefits by the time of the 2000 presidential election. Finally, to be consistent, I kept precincts with 3000 inhabitants or less.

Table 5 presents baseline characteristics of randomized and third groups and the regression adjusted difference between them. Precincts in the third group are comparable to precincts in the experiment in terms of poverty levels, share of the population living in eligible villages, PRI vote share and PAN vote share. Precincts in the third group are,

however, less populous than precincts in the experiment, have less turnout and vote less for the left-wing party (Column 3). These differences indicate an unbalance in baseline traits. To balance the distribution of baseline traits, the probability that a precinct is in the randomized group (P), as oppose to the third group, was calculated as a function of baseline characteristics. Then, observations in the third group are weighted by $P/(1-P)$. The probability weights correct the unbalance in observable baseline characteristics. Column 4 shows that in the re-weighted sample, there are no statistically significant differences between the randomized and third groups.

With these weights it is now possible to estimate the effect of early and late treatment relative to a control group. Table 6 presents the results of the weighted estimated of the effect of the early and late enrollment compared to precincts not enrolled in the the program by the time of the 2000 presidential election. As before, the interaction of early treatment and its dosage leads to a positive and significant increase in turnout and incumbent vote share and a decrease in PRD vote share. However, the voting behavior of the late treatment group is the same as the third group. The interaction between late treatment and its dosage is statistically insignificant across all columns.

These estimates corroborate that early enrollment in the program increases turnout and incumbent vote share. On the other hand, these estimates suggest that enrollment in the late treatment group had no statistically significant effect in electoral behavior relative to being a non-recipient.

VI. External validity and Progresa's effect in 2006

The empirical strategy of this article has as main advantage that the random assignment of early and late treatments justifies the causal inference of program effects. Yet this article confronts similar challenges to other studies that rely on experimental data. First, the population of Progresa's evaluation is of substantive interest because we are interested in finding program effects in poor precincts. The experimental sample, however, was taken from a population of eligible villages in seven out of thirty-one Mexican states (excluding Mexico city where the program does not operate). Thus, concerns about the external validity of

the results are in order. Second, Progresa's randomized component may have operated differently than the large scale program. For instance, the evaluation could have foster greater efficiency in program operation. As noted by Deaton (2009) "there is no guarantee that the policy tested by the randomized control trail will have the same effects as in the trial"(42). Finally, Progresa's experiment is better suited to study short-term effect. Since both early and late treatment groups were enrolled in the program, the differences in electoral behavior due to the program are bounded to disappear. Therefore, the experimental data cannot answer wether Progresa had the same mobilization and pro-incumbent effect in the 2006 presidential election when the incumbent party was the PAN.

To explore wether the program had similar effects outside of the experimental sample and to find a preliminary estimate of its effect in the 2006 presidential election, this section explores the relationship between Progresa and electoral behavior using two cross-sections of municipal level data. Table 7 presents the results from regressions where the dependent variables are, starting in Column 2, turnout in 2000, incumbent vote share (PRI) in 2000, turnout in 2006, and incumbent vote share (PAN) in 2006. All these regressions include state fixed effects and a vector of census demographic controls (population, illiteracy rate, share of households without sewage, share of households without electricity, share of households without safe water and share of households without firm floor). As with the experimental data, with the municipal level data the estimates suggest that Progresa lead to an .08 percentage point increase in turnout and a .15 percentage point increase in incumbent vote share in 2000. The magnitudes of the estimates from the country-wide data are close enough to the estimates from the experimental data to corroborate the external validity of the latter.

With respect to Progresa's effect in 2006, the mobilization and pro-incumbent effects remains positive and significant at the 1 percent level, albeit the magnitude of the effect is weaker. Compared to the .08 percentage point increase in turnout in 2000, in 2006 Progresa lead to a .03 percent point increase, and compared to the .15 percent point increase in PRI vote share in 2000, in 2006 Progresa lead to a more modest .034 percent point increase in

PAN vote share.

VII. Discussion

The argument that poverty relief programs demobilize recipients due to the stigma that comes from collecting public assistance (Soss 1999), finds no support in the data. Rather, Progresa recipients turn out to vote at higher rates than non-recipients of comparable wealth. Most surprisingly, the analysis suggests that longer exposure to Progresa benefits leads to higher electoral participation and higher incumbent vote shares. The increase in turnout is comparable to the average effect of canvassing and other voter mobilization efforts in US elections (Gerber and Green 2000). Progresa's effect is also comparable to loosing in a school choice lottery in the US (Hastings 2007). In terms of the increase in vote share, the estimates show a positive effect. The magnitude of the effects seem at first modest, however, when put into context the effect is relevant. Consider for instance the margin of victory in the last two presidential elections in Mexico. While in 2000, six percentage points separated the winner (PAN) and the second place (PRI), in the 2006 elections the PAN's margin of victory over the PRD was less than half percentage point.

The evidence also suggest that part of the additional support for the PRI in 2000, came at the expense of the left-wing party but the right-wing party was unaffected. This is reasonable, because, in its early years, Progresa operated mainly in rural areas where the PAN had no base of support. Indeed, the continuation of Progresa under the PANista administration seems to have improved modestly the right-wing party's support among low income sectors, as reflected in the estimations of the effect of Progresa's coverage in the 2006 presidential election.

Why Progresa had such electoral returns? There are several of Progresa's components and traits that could be responsible for the increased turnout and pro-incumbent effects. Proving a particular mechanism, however, is a daunting task. On one hand, one of Progresa's strengths is that its operation is standardized. This means that there is no exogenous variation in program components to evaluate them separately. At the household level, where there is variation in exposure to components, this variation is likely to be product

of a systematic selection process. For instance, households that fail to bring their children to medical check ups but keep them at school, benefit from the scholarship and not the nutrition-related cash stippled. This type of household, however, is likely to be different to households that comply with all conditionalities. Moreover, this difference could be related to political outcomes in some way, rendering causal estimates implausible. Ideally to get a better sense of the mechanism, we could design experimental interventions where different combinations of Progresa's components are randomly manipulated. Alas, not only the current coverage of Progresa makes this research strategy difficult to implement, but changing program traits for the sake of evaluating different causal mechanisms, even in the field of education and health, has not been feasible (Todd and Wolpin 2006). In this section, I begin to eliminate some of the possible mechanisms behind Progresa's electoral effects that seem to be inconsistent with this article's data and discuss some other channels that seem to be at work.

To explain Progresa's electoral dynamics, the cash transfer is perhaps the most obvious mechanism. When putting together cash and votes in the same sentence, inevitably vote buying and clientelism come to mind. Indeed, accusations along these lines were not uncommon in the beginning of the 2000 presidential race. Progresa was described by some in the left-wing party as a "disguised twin of Pronasol that has as its main goal the perpetuation of electoral clientelism among the poorest" (*Servicio Universal de Noticias*, August 8, 1997). More specifically, opposition parties accused the PRI of threatening program recipients to discontinue the program if they failed to cast a ballot in favor of the party.

Is vote buying responsible for the increased turnout and incumbent vote share? In order to answer this question, the first thing to note is that program benefits were explicitly unconditional on vote choice. Second, uncertainty about the program's survival was substantially reduced by the public commitment made by all candidates to continue and enlarge the program if elected, despite their negative reactions in the outset of the presidential race (De La O, 2007). Third, by skipping traditional distribution channels (clientelism or machine politics), the program turn old styles of politics on their head. As a party broker displaced

by a program lamented: "What the [Progresa] staff don't get is that they have to let us decide who enters and who exits the program, otherwise, we can't punish people who didn't vote for us". [18] The influx of federal resources straight to voters' pockets challenged the monopoly of local power holders. Thus, if anything, Progresa weakened clientelist ties (De La O 2007). Fourth, if threats to discontinue the program lead to increased turnout and incumbent vote share, then the length of time a person is enrolled in the program should be irrelevant. This claim finds no support in the data. Finally, vote buying is an unlikely determinant of the differences in electoral behavior between the early and late treatment groups because the list of program beneficiaries was kept confidential until 2002 when the Access to Information Law was enacted. Thus, party brokers did not have fine grained information that would allow them to target the early and late treatment groups.

An alternative is that the cumulative cash explains such pattern. Note, however, that if this is the case, then the mechanism at work gets farther away from vote-buying. Party machines target poor voters who are most responsive to their gifts (Stokes 2005). Due to the longer exposure to program benefits, the early treatment group is healthier, more educated and has had additional disposable income available for longer. Thus, if given the choice, party machines would target not the early but the late treatment group. In turn, turnout and incumbent vote share should have been higher in the late treatment group. Empirically, this is not the case.

A resource model of participation would suggest that it is increasing in income (Brady et al 1995). Although for decades high participation was a feature of poor and rural regions of the country, by Progresa's time, turnout patterns resembled more closely those of establish democracies with more affluent people participating more in elections (Klesner and Lawson 2001). Thus, a resource model would explain the higher turnout rate among the early treatment group. If electoral returns were explained by an income effect, however, we would expect that the more affluent a voter, the higher their sympathy for the conservative party. [19] This argument finds no support in the data. Thus, it is unlikely that a resource

[18] Author's interview with Party Broker, Mexico City, August 2005.

[19] Several studies document a positive link between income and support for the conservative party in

model on its own would account for these patterns.

Other theories of participation based on rational choice emphasize that the probability of turning out to cast a ballot decreases when the cost increases (Riker and Ordeshook 1968). If Progresa recipients face lower costs of casting a ballot, then this would explain the pattern observed. Indeed, Progresa's requirement that women head of the household present an official identification may have decreased the cost of participation for female recipients. The Federal Electoral Institute issues the most widely accepted official identity card in the country when people register to vote. In this sense, if enrollment in Progresa fosters registration to vote, then the cost of participation are lower for recipients relative to non-recipients of similar wealth. This cost-based theory would suggest that Progresa's effects are mainly to be found among the female heads of households. In the data, however, early and late treatment groups were enrolled in the program by election time. Moreover, because the groups were randomly selected, there is no reason to suspect that women in the early treatment group registered to vote at higher rates relative to women in the late treatment group. This analysis does not preclude the possibility that differential costs associated with the identity card explain part of the difference in turnout and vote shares between the early group and the third group not enrolled in the program by the time of the presidential election.

Theories based on social psychology suggest that unconditional gifts foster reciprocity (Landry et al. 2009). Progresa is one among few Mexican examples where federal government's discretionary power was voluntarily retrenched. While Progresa benefits were conditional on investments in children's education and health, the transfers were not tied to vote choice. More over, the longer the exposure to the program, the more credible its non-electoral nature. If poverty based targeting, as opposed to electoral targeting, fostered reciprocity, this could explain the differences in electoral behavior between the experimental groups. Evidence on this front is unavailable for the Mexican case. Research on the Brazil-

Mexico (Dominguez and McCann 1996, Moreno 2003, Dominguez and Lawson 2004). Gelman and Cortina (2009) also find that the PAN did better in richer states than in poorer states in the 1994, 2000 and 2006 elections).

ian CCT, however, suggests that program recipients voted at higher rates for incumbent mayors who were perceived as managing the program less politically with fewer program resources going to the non-poor (de Janvry, Finan and Sadoulet, 2006).

Closely related to the cost-based theories, an explanation based on differential health outcomes between the early and late treatment groups could explain the differences in electoral behavior (Blatman 2009). Indeed, among Progresa households both adults and children were healthier (Gertler and Boyce 2001). This means that the probability that bad health impeded participation was higher for the late treatment group. Note, however, that if the mechanism at work is related to health improvements, a simpler retrospective voting explanation could account for differences in electoral behavior too. Indeed, the positive outcomes generated by the bundle of Progresa components suggest that at the election day, voters in the early treatment group had more reasons to approve the performance of the incumbent party relative to the late treatment group. Survey-based evidence lends support to this claim. The Mexico 2000 Panel Study asked respondents about their presidential approval rates at three different points in the campaign. Throughout the campaign the approval rate of President Zedillo's work was higher among survey respondents who lived in rural areas and were enrolled in Progresa than among respondents who were equally poor, lived in rural areas, but were not enrolled in the program. The distance between the approval rates of these two groups was twelve percentage points in the beginning of the campaign (February) and twenty in the last month of the campaign (June).

VIII. Conclusion

This article provides evidence on the electoral returns of CCT by using a unique experiment that randomized program enrollment across eligible villages. The results suggest that the Mexican CCT lead to an increase in turnout and incumbent vote shares. The analysis also reveals that the longer exposure to Progresa benefits leads to higher electoral participation and higher incumbent vote shares. The data also suggest that part of the additional support for the incumbent in 2000, came at the expense of the left-wing party but the right-wing party was unaffected. This is reasonable, because, in its early years, Progresa

operated mainly in rural areas where the PAN had no substantial presence. Indeed, the continuation of Progresa under the PANista administration seems to have improved modestly the right-wing party's support among low income sectors, as reflected in the estimations of the effect of Progresa coverage in the 2006 presidential election.

The results in this article have some limitations. First, the data at hand allowed for the estimation of Progresa's effects on electoral behavior. Yet casting a ballot is only one manifestation of political participation. Some of the programs traits may well shape other dimensions of political participation. For instance, the condition to attend regular *plácticas* (information sessions) at the health center, gave women a venue to meet on regular basis. Several women recipients mentioned that, although it took some time for them to feel comfortable, discussions about local needs were not uncommon in these meetings. Indeed, in some cases, women presented these needs to the local government.[20] An additional program's trait that could shape political behavior is the selection in each village of three women that are in charge of informing the community about events related to the program. Despite the lack of formal organization, this network has allowed women, in some instances, to enter politics.[21].

Second, in this article I have shown that the results from the experimental data are likely to apply to the country. Yet the data is only suited to speak to the Mexican case. Even so, scattered through out the literature is evidence suggesting that other conditional cash transfer programs have pro-incumbent effects of similar magnitudes (Zucco 2008, Manacorda et al 2009). This suggest that there is some external validity of this article's results.

Third, this article is designed to study the short-term effects of the program. In the long term, program effects may disappear once the program is institutionalized. Alternatively, the effects could be sensitive to changes in program operation. Future research can address how stable are the electoral returns over time.

Finally, regarding the reasons behind the Mexican CCT electoral returns, this article find little support for explanations based on clientelism and vote buying. An explanation

[20] Author's interview with program recipients, Tlaxcala 2005.
[21] Author's interview with program recipients, Estado de Mexico 2002.

based on retrospective voting seems more likely to be at work. Despite the inconclusiveness on this regard, this article suggest one important general lesson. Conditional cash transfers are compatible with healthy democratic habits, such as participating in elections and have the attractive feature of fostering pro-incumbent support.

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Table 1: Descriptive Statistics

Panel A: Villages in the random assignment		
N=501		
	1995	2000
Average population	260	254
	(192)	(197)
Average population above or 18 years old	132	131
	(95)	(96)
Average Poverty	4.669	
	(.470)	
Panel B: Electoral precincts		
N=421		
	1995	2000
Average population	1194	1235
	(666)	(727)
Average number of voters	605	623
	(338)	(365)
Average Poverty	4.611	
	(0.455)	
Average share of eligible population	0.903	
	(0.220)	
	1994	2000
Turnout	0.637	0.656
	(0.161)	(0.145)
PRI vote share	0.418	0.367
	(0.157)	(0.140)
PAN vote share	0.050	0.128
	(0.055)	(0.103)
PRD vote share	0.099	0.128
	(0.114)	(0.119)

Notes: Poverty and population measures taken from CONAPO 1995 and 2000. Electoral data taken from *Atlas Electoral de México*, IFE 1991-2000. The original number of villages in the experiment was 506, I lost 4 villages because they were not found in the IFE records. The original number of precincts with randomized villages was 465. I excluded from the analysis 18 precincts with insufficient census data, and 26 precincts with population larger than 3000.

Table 2: Baseline Characteristics

	Early	Late	Regression adjusted difference: Treatment	Regression adjusted difference: treatment X dosage
Poverty	4.602	4.632	-0.029	0.091
			(0.047)	(0.165)
Population	1159	1253	-93.052	122.459
			(68.937)	(201.159)
Population eligible	.911	.889	0.021	-0.088
			(0.022)	(0.077)
Turnout	.642	.634	-0.006	-0.062
			(0.035)	(0.058)
PRI vote share	.426	.407	0.012	-0.025
			(0.025)	(0.055)
PAN vote share	.049	.054	-0.013	-0.008
			(0.009)	(0.020)
PRD vote share	.101	.098	0.000	-0.004
			(0.013)	(0.041)

Notes: Regression adjusted difference reports the coefficient on wether the precinct was assigned to early or late treatment from separate regressions with each variable in the first column as the dependent variable. Standard errors in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

Table 3: The impact of early versus late treatment on turnout

Treatment X dosage	0.066	0.064*	0.081**
	(0.041)	(0.027)	(0.023)
Treatment	-0.030*	-0.029*	-0.033**
	(0.018)	(0.013)	(0.011)
Dosage	-0.015	-0.025	-0.030
	(0.035)	(0.027)	(0.028)
Population change	-0.366***	-0.358***	-0.361***
	(0.040)	(0.035)	(0.034)
Population eligible	0.022	0.009	-0.001
	(0.026)	(0.018)	(0.019)
Party in municipality 1994			Yes
Party in municipality 2000			Yes
State Fixed Effects		Yes	Yes
Constant	0.008	0.023	0.038*
	(0.026)	(0.020)	(0.018)
Observations	421	421	421
R^2	0.20	0.23	0.27

Notes: OLS estimations. Standard errors in parentheses. The second and third column's standard errors adjust for clustering at the state level.*** p<0.01, ** p<0.05, * p<0.1

Table 4: The impact of early versus late treatment on vote shares

	PRI	PRI	PRI	PAN	PAN	PAN	PRD	PRD	PRD
Treatment X dosage	0.103**	0.085*	0.094*	0.005	0.019	0.021	-0.069**	-0.070***	-0.068***
	(0.046)	(0.040)	(0.040)	(0.029)	(0.023)	(0.023)	(0.031)	(0.016)	(0.015)
Treatment	-0.042**	-0.038*	-0.041**	-0.006	-0.007	-0.008	0.016	0.019**	0.019***
	(0.020)	(0.017)	(0.015)	(0.013)	(0.010)	(0.011)	(0.013)	(0.006)	(0.004)
Dosage	-0.066*	-0.065*	-0.066**	-0.018	-0.022	-0.032	0.046*	0.042**	0.049***
	(0.039)	(0.027)	(0.025)	(0.025)	(0.025)	(0.023)	(0.026)	(0.015)	(0.011)
Population change	-0.282***	-0.308***	-0.303***	-0.072**	-0.063	-0.049	-0.069**	-0.063	-0.080
	(0.045)	(0.043)	(0.041)	(0.028)	(0.035)	(0.036)	(0.030)	(0.059)	(0.053)
Population eligible	0.065**	0.047	0.042	-0.031*	-0.032	-0.022	-0.003	-0.011	-0.022
	(0.029)	(0.033)	(0.029)	(0.018)	(0.020)	(0.020)	(0.019)	(0.022)	(0.024)
Party in municipality 1994			YES			YES			YES
Party in municipality 2000			YES			YES			YES
State fixed effects		YES	YES		YES	YES		YES	YES
Constant	-0.083***	-0.066**	-0.059**	0.114***	0.114***	0.108***	0.021	0.028	0.036
	(0.028)	(0.027)	(0.023)	(0.018)	(0.019)	(0.018)	(0.019)	(0.017)	(0.021)
Observations	421	421	421	421	421	421	421	421	421
R^2	0.11	0.20	0.23	0.03	0.24	0.31	0.03	0.05	0.12

Notes: OLS estimations. Standard errors in parentheses. The second and third column's standard errors adjust for clustering at the state level.*** p<0.01, ** p<0.05, * p<0.1

Table 5: Baseline characteristics of randomized and status-quo groups

	Randomized	Status-quo	Regression adjusted difference	Regression adjusted difference with probability weights
Poverty	4.612	4.554	.058	.081
			(.044)	(.072)
Population	1190.248	695.407	494.840***	-78.859
			(55.065)	(138.993)
Population eligible	.904	.940	-.036	.050
			(.027)	(.089)
Turnout	.640	.672	-.032**	.003
			(.0153)	(.023)
PRI vote share	.420	.423	-.003	.015
			(.015)	(.022)
PAN vote share	.050	.057	-.006	-0.004
			(.005)	(.007)
PRD vote share	.100	.170	-.069***	-.008
			(.011)	(.011)

Notes: Regression adjusted difference reports the coefficient on wether the precinct was part of the experiment or was not enrolled in the program by the election day from separate regressions with each variable in the first column as the dependent variable. Standard errors in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

Table 6: Weighted results of early and late treatment versus status quo group

	Turnout	PRI	PAN	PRD
Early Treatment	0.005	-0.015	-0.030	0.136**
	(0.028)	(0.031)	(0.023)	(0.057)
Dosage	-0.024	-0.063*	-0.003	0.166**
	(0.030)	(0.037)	(0.022)	(0.076)
Early X dosage	0.094***	0.105**	0.004	-0.175**
	(0.035)	(0.043)	(0.026)	(0.075)
Late Treatment	0.029	0.026	-0.029	0.115**
	(0.030)	(0.033)	(0.024)	(0.057)
Late X dosage	0.048	0.005	0.013	-0.093
	(0.052)	(0.055)	(0.032)	(0.081)
Constant	-0.029	-0.111***	0.137***	-0.100*
	(0.018)	(0.023)	(0.016)	(0.055)
Observations	614	614	614	614
R^2	0.09	0.16	0.07	0.25

Notes: Robust standard errors in parentheses. Regressions include the following controls: population change and population eligible to the program. *** p<0.01, ** p<0.05, * p<0.1

Table 7: Impact of Progresa using municipal level data

	Turnout 2000	PRI 2000	Turnout 2006	PAN 2006
Progresa coverage 2000	0.084***	0.151***		
	(0.007)	(0.023)		
Progresa coverage 2006			0.030***	0.034**
			(0.009)	(0.014)
Constant	0.653***	0.227***	0.185***	0.049***
	(0.023)	(0.029)	(0.048)	(0.013)
Observations	2326	2326	2372	2372
R^2	0.42	0.47	0.69	0.81

Notes: OLS estimations with state fixed effects. Standard errors adjust for clustering at the state level. Regressions include the following controls: population, illiteracy rate, share of households (hh) without sewage, share of hh without electricity, share of hh without safe water, share of hh without firm floor. *** p<0.01, ** p<0.05, * p<0.1