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

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This article reexamines the argument that targeted programs increase pro-incumbent voting by persuading beneficiaries to cast ballots against their first partisan choice. The evidence comes from the randomized component of Progresa, the pioneering Mexican conditional cash transfer (CCT) program. Experimental data show that early enrollment in the program led to substantive increases in voter turnout and in the incumbent's vote share in the 2000 presidential election. The experiment also reveals that opposition parties' vote shares were unaffected by the program. Thus, the electoral bonus generated by CCTs may be best explained by a mobilizing rather than persuasive mechanism. These findings are difficult to reconcile with the notion that the electoral effects of CCTs are a result of prospective concerns triggered by threats of program discontinuation or endogenous program enrollment. Instead, the evidence in this article suggests that CCTs' mobilizing effects are compatible with programmatic politics.

Do targeted government programs increase pro-incumbent voting? One strand of literature contends that the introduction of government welfare programs offers ample opportunities for incumbents to claim the credit for positive program results.[1] Another strand of literature typically assumes that strategic allocations of public resources effectively sway voters in favor of the incumbent.[2] It seems likely, therefore, that government programs targeted to a specific subset of the population raise support for the incumbent. However, the empirical record remains mixed.[3] Moreover, most theories linking government expenditures to electoral outcomes predict that voters change their vote choice in response to distribution, while for the most part ignoring potential mobilizing effects.

This article provides evidence that a targeted government program can help the incumbent party mobilize its supporters, in a case where a traditionally clientelistic party shifted away from discretionary to programmatic spending. The evidence comes from *Progresa* (now *Oportunidades*), the Mexican conditional cash transfer (CCT) program. This government transfer is broadly

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[1]See, for example, Pierson (1996).

[2]For an insightful review of this literature, see Cox (2010).

[3]To name just a few examples, Cerda and Vergara (2008) estimate that in Chile throughout the 1990s direct welfare payments were electorally profitable, and Manacorda et al. (2010) find that a cash transfer in Uruguay generated support for the incumbent party even after the discontinuation of the program. On the other hand, Levitt and Snyder (1997) estimate that in the United States, expenditures such as social security, Medicare, low-income housing payments, and veterans' retirement benefits produce no electoral payoffs. Even within countries, targeted programs produce inconsistent electoral returns. In Argentina, a program that granted benefits to the unemployed improved the electoral performance of the *peronismo*, but not that of the *radicalismo* (Nazareno, Stokes, and Brusco 2006).

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representative of a government antipoverty targeted program that appears across many contexts.[4] While speculations about CCTs' pro-incumbent effects abound in the media,[5] scholarly work on the subject is inconclusive, as I describe in more detail below (Cornelius 2004; Díaz-Cayeros, Estevez, and Magaloni 2007, 2009; Green 2006; Zucco 2010).

The empirical conundrum that has bedeviled previous attempts to uncover the causal effect of targeted government programs is that if incumbents strategically funnel resources to areas where they are electorally vulnerable (or strong), then estimates of electoral returns are biased downward (or upward). If we could observe the process that incumbents use to allocate resources, then accounting for it would be enough. However, in most cases we can only approximate the process with some error. Thus, the concern that remains is that an unobserved omitted variable biases the estimations (Levitt and Snyder 1997). In addition, nonrecipients' responses to targeted programs (Humphreys and Weinstein 2009) and social desirability response bias when estimating the effects of CCT with survey data can render invalid inferences.[6]

To overcome these challenges, I take advantage of the fact that Progresa's randomized impact evaluation offers exogenous variation in the duration of exposure to program benefits. The experiment, together with data on election outcomes at the polling precinct level, reveal that assignment to early program enrollment led to a 7% increase in voter turnout and a 9% increase in incumbent vote share in the 2000 presidential election. The experiment also reveals that exposure to program benefits had no influence on support for opposition parties. Together, these findings lend evidence to Progresa's pro-incumbent mobilizing effects.

Although seemingly a relatively narrow issue, Progresa's effect on electoral behavior among the experimental groups speaks to the broader question of how compatible targeted government programs are with democracy. Existing research has staked out contradictory claims in this regard. Some argue that targeted programs persuade recipients to change their vote choice for programmatic reasons, such as retrospective voting (Díaz-Cayeros, Estevez, and Magaloni 2009). Others, however, posit that targeted programs are detrimental to democracy because they perpetuate a clientelist linkage between incumbents and recipients. In particular, the concern is beneficiaries of targeted programs may be persuaded to vote against their preferences in response to threats of program discontinuation (Cornelius 2004; Schedler 2000).

More consistent with the experimental findings than retrospective voting or clientelism is that the longer the duration of the program, the greater recipients' exposure to program benefits and the more opportunities the incumbent has to take credit for positive program results (Mayhew 1974). Although this mechanism gives an active role to the incumbent party, it is still closer to programmatic politics than clientelism. To further test that prospective considerations triggered by threats are not responsible for Progresa's mobilizing effects, I show that neither of the experimental groups was prioritized in political parties' territorial campaigns. That the mechanism behind Progresa's mobilization of voters is on the programmatic politics end is something not easily confirmed by previous evidence.

This article makes three contributions to the existing literature. The first contribution is to the distributive politics literature. The assumption that voters are responsive to incumbents' tactical distribution finds some support in this analysis. However, as Cox (2010) and Nichter (2008) argue, mobilization effects are a central part of the story. The second contribution relates to the growing literature on social policy in the developing world, where our understanding of the variation in the design of social protection programs and the political factors behind them remains limited (Carnes and Mares 2009, 94). The findings here suggest that CCT programs' ability to foster support for the incumbent could explain, in part, the popularity of these social programs among left- and right-leaning governments alike. Finally, the article contributes to the vast literature on the effects of welfare programs on political participation, which has been primordially focused on the United States (Campbell 2003; Mettler and Soss 2004; Soss 1999; Soss, Hacker, and Mettler 2007).

Progresa: The Mexican CCT

Following the peso crisis of 1995, more than 16 million people fell into poverty in Mexico (Gil Díaz and Carstens 1996).[7] At that time, most of the funds available to tackle

[4]Currently, at least 30 governments around the world have adopted a CCT program similar to the Mexican example, including all governments in Latin America, except Venezuela and Cuba. Outside of Latin America, Turkey, Nigeria, Burkina Faso, Kenya, Yemen, Indonesia, Cambodia, Bangladesh, Macedonia, Pakistan, and the Philippines have their own CCT program (Fiszbein and Schady 2009).

[5]BBC (2009), *Washington Post* (2006) and Lindert and Vincensini (2008).

[6]For an insightful review of social desirability bias, as well as ways to circumvent it, see Gonzalez-Ocantos et al. (2012).

[7]The fraction of the population in the country living in poverty increased from 52% in 1994 to almost 69% in 1996. The poverty

poverty were unable to reach the poor. In fact, 75% of the total budget for poverty relief programs was then channeled to urban areas where nonpoor households captured most of the transfers (Levy 2006). In addition, scholars and policymakers agree that the most influential program in place when the crisis hit, the National Solidarity social fund (PRONASOL), was more effective at fostering support for the incumbent Institutional Revolutionary Party (PRI) than at reducing poverty (Skoufias 2005).[8]

Responding to this context, the administration of President Ernesto Zedillo launched Progresa in 1997. The program consists of three complementary components: a cash transfer, primarily intended to subsidize food expenditure, delivered directly to the female heads of poor households; a scholarship, intended to compensate for the opportunity cost of child labor, thus enabling children to stay in school;[9] and basic health care for all members of the household, with particular emphasis on preventive health care (Poder Ejecutivo Federal 1997).[10] All the components add up to an average transfer of US$35 per month, of which more than 85% is in cash. This is approximately 25% of the average poor, rural household income in the absence of the program (Levy 2006). Transfers are paid every two months. Progresa is conditional because receipt of benefits is contingent upon school attendance, regular medical checkups, and attendance at *pláticas* (meetings) related to health, hygiene, and nutrition issues.

The program is one of the largest efforts to improve the living conditions of impoverished children. Between 1997 and 2000, Progresa enrolled 2.6 million households. The program's budget in 2000 was about US$800 million or 0.2% of GDP a year (Gertler 2000).[11]

Three additional characteristics set Progresa apart from other poverty relief programs in Mexico. First, the program has clear and fixed criteria for determining eligibility based on poverty, and it is explicitly nonpartisan. 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.[12] Since 1998, all materials that reach recipients 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, 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, program designers created a new bureaucracy to operate the program. The agency, which is a satellite of the Ministry of Social Development, circumvented all intermediaries, including traditional and powerful mechanisms of federal money's distribution such as governors and the state branches of the Ministry of Social Development. Unlike previous administrators of prominent poverty relief programs who were mostly politicians, the first coordinator of Progresa was a scientist. Furthermore, provisions in the federal budget decree prohibit the use of the program to proselytize by any political party.

Finally, program designers delayed the inauguration of the program until one month after the 1997 midterm elections. Since then, budget decrees have included a prohibition to scale up the program six months prior to

lines were 2.09 and 1.54 2000-USD per day in urban and rural areas, respectively.

[8]For a review of the political economy of PRONASOL, see Álvarez and Mendoza Pichardo (1993); Bruhn (1996); Cornelius (2004); Dresser (1991); Fox (1994); Kaufman and Trejo (1997); Magaloni, Díaz Cayeros, and Estevez (2007); Molinar and Weldon (1994); Pérez Yarahuán (2005); and Soederberg (2001).

[9]The transfer comes with a nutritional supplement targeted to children between the ages of four months and two years and pregnant and lactating women. 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 preestablished upper amount.

[10]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 mellitus; and first aid for injuries (Parker and Teruel 2005).

[11]From September 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 semiurban areas using parallel criteria to select recipients. In 2003, the program was extended to urban areas.

[12]In 1997, the 70-year ruling party (PRI) lost the majority in the Chamber of Deputies.

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. 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). Compared to previous generalized subsidies, Progresa is more redistributive (Scott 2001). Regarding the program effects on children's 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). 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).

Calculating Progresa's Electoral Returns

Scholarly work on CCTs' electoral returns has made rapid progress over the last several years. The earliest and most common empirical approach relies on survey data. Drawing upon the Mexico 2000 Panel Study, Cornelius (2004) finds that respondents enrolled in Progresa were 12 and 26% more likely to vote for the incumbent (PRI) candidate than the right- and left-wing candidates, respectively. However, he finds no effect on turnout. Since then, CCTs' persuasive effects have dominated the literature. The Díaz-Cayeros, Estevez, and Magaloni (2007, 2009) analysis of the national exit polls fielded by the newspaper *Reforma* in 2000 and 2006 shows that voters enrolled in Progresa were 17 and 11% more likely to vote for the incumbent party in each election. They also find that program enrollment decreased the vote of the right-wing party in the 2000 election and decreased the vote of the left-wing candidate in the 2006 election. For the Brazilian CCT, Zucco (2010) uses the first Vox Populi survey in 2006 and finds that program enrollment increases the probability of voting for the incumbent by 30 and 43% among respondents in the two lowest brackets of income, respectively.

The appeal of using survey data is that a direct comparison can be made between recipients and nonrecipients. However, a variety of methodological challenges arise. Perhaps the most pressing issue is that self-reported turnout and vote choice are prone to social desirability response bias (Gonzalez-Ocantos et al. 2012). If program recipients are more eager to manifest support for the incumbent when responding to a survey than when casting a ballot, or are reluctant to declare that they did not turn out, then conformity bias leads to erroneous estimations.

Another concern with survey data relates to measurement error. Previous work is cognizant of the possibility that recipients are unlike nonrecipients. This is especially troublesome for the study of Progresa because the traits that set recipients apart, such as poverty, shape electoral behavior directly. Methods of covariate adjustment, like regression (Cornelius 2004) or propensity score matching (Díaz-Cayeros, Estevez, and Magaloni 2007, 2009; Zucco 2010), are the most prominent approach to avoid comparisons that conflate program impacts with preexisting differences. The remaining challenge is that traits such as income are likely measured with error in surveys. If noise in the measurement of income correlates with program enrollment, and also has a direct effect on electoral outcomes, then estimates will be biased. To mitigate this concern, Díaz-Cayeros, Estevez, and Magaloni (2007, 2009) include in the propensity score additional community-level characteristics.

Concerns regarding unbalanced observed demographics, however, pale before concerns about hidden bias and endogenous program enrollment. That clientelism abounds in countries where most CCTs operate exacerbates the concern that incumbents use unobserved strategic criteria to allocate program resources. Consequently, unobserved omitted variables and reverse causality remain problematic because "perfect balance on important covariates does not necessarily warrant causal claims" (Morgan and Winship 2007, 122). Even in cases when a rich set of covariates is available, if unobserved heterogeneity remains, then matching can lead to different results than an experimental benchmark (Arceneaux, Gerber, and Green 2010).

Estimates based on aggregate data so far have produced mixed results. Using propensity score matching in two cross-sections of data, Zucco (2010) finds that the Brazilian CCT increased incumbent vote share in 2002 by .12% and in 2006 by .2%. On the other hand, Green (2006) finds no effects of the Mexican CCT using a regression discontinuity design (RDD). In this innovative approach, a comparison is made between localities right below and above the program's eligibility threshold. The appeal of this design is that, because the threshold is arbitrary, localities around it are similar in many respects. The drawback is that villages close to the threshold are more populous and wealthier than the average eligible village.

Thus, it is unclear if the results can be extrapolated to all eligible villages.[13]

Using the Randomization

Progresa's randomized component offers exogenous variation in exposure to program benefits that allows circumventing the methodological challenges previously discussed. The randomized experiment was implemented in seven states where the program was first scaled up.[14] The sample selection process followed Progresa's targeting method closely. The first step was the selection of villages eligible for the program based on a poverty measure created with the 1990 census data and the 1995 partial census data. The poverty index was divided into five categories that go from very low poverty to 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. 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 fewer than 50 or more than 2,500 inhabitants were excluded. Finally, using Geographic Information System (GIS) software, remaining localities were grouped based on geographical proximity. Isolated localities were excluded from the selection process (Progresa 1998).[15]

Randomization was implemented at the village level.[16] Families in 320 villages were randomly selected to receive benefits in September 1998, whereas 186 villages were excluded from the program until January 2000 (Schultz 2001). There was a 60% probability of being assigned to the early treatment group and a 40% 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. In villages assigned to the late treatment group, none of the households received program benefits until January 2000 (Progresa, Methodological Note: General Rural, 2006). By the 2000 presidential election, villages in the early and late treatment groups had been assigned to treatment 21 months and six months, respectively.

Program officials expected that an impartial program evaluation helped the program to survive the change in federal administration in 2000. However, it was also clear that delaying enrollment of eligible villages for the sake of the evaluation was politically sensitive. To avoid confrontations, program officials waited to publicize the evaluation until December 2000. Media reactions proved that concerns were justified, as the evaluation was criticized on ethical and budgetary grounds (Parker and Teruel 2005). Media exposure, however, came after the presidential election, so it did not alter information available to experimental groups during the period of study of this article.

Data

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. To take advantage of the random assignment, I overlayed the 506 experimental villages to the smallest unit of outcome measure for which census, program, and electoral data roughly coincide: the *sección electoral* (precinct).[17]

[13]This problem is exacerbated by the restriction of the sample to either localities that correspond one-to-one to electoral precincts or localities that were contained in two electoral precincts. More generally, RDD faces a trade-off between precision and bias because 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). Also, RDD's assumption is that unobservable variables that affect voting behavior are not discontinuous functions at the eligibility threshold.

[14]The states are Guerrero, Hidalgo, Michoacán, Querétaro, Puebla, San Luis Potosí, and Veracruz. The selection of states corresponded 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, 1,720 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).

[15]According to the program's operational rules, after eligible households had been identified, the list of beneficiaries was meant to be presented to community assemblies, and their feedback should have been 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, Davis, and Behrman 1999, iv).

[16]The methodological note of the evaluation mentions that the sample was stratified by population. However, the details of such population strata are not included, and none of the evaluation, data sets, or articles using the evaluation include them.

[17]The Federal Electoral Institute (IFE) and INEGI use different identifiers for states, municipalities, and villages. All merges were

Neither villages nor precincts have fixed population size, and generally they do not correspond one-to-one.[18]

Because the units of assignment to treatment and outcome measure do not overlap perfectly, the aggregation of villages into precincts brought in villages that were originally excluded from the experiment. Precincts in the sample contained six villages on average. Thus, experimental precincts were more populous than experimental villages, but they were very similar in terms of poverty, with villages having an average poverty of 4.66 and precincts of 4.58 on a scale that goes from 1 to 5.[19] Summary statistics of villages and precincts are presented in Table 1.

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 systematic 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. The data support these claims. When comparing the baseline characteristics of the early and late treatment groups at the precinct level, there are no statistically significant differences between them in poverty, population, population living in an eligible village, and number of villages.

If Progresa's experimental sample was a discretionary allocation strategy, as opposed to a random allocation, its distribution would reveal some form of electoral bias. The data give no evidence of this as there are no statistically significant differences between early and late treatment groups in terms of preprogram electoral behavior (column 3 of Table 2). To further test the validity of randomization, I estimated a logistic regression to predict early treatment based on turnout in 1994, vote shares of the three largest parties in 1994, average poverty, and population in 1995. As expected, none of the baseline characteristics is statistically different from zero, and the chi square is nonsignificant ($p = .29$). Similarly, baseline demographics and the chi square remain nonsignificant after the inclusion of a fixed effect by the number of villages in the precinct ($p = .49$).

Beyond balance in baseline covariates, random assignment means that each entity in the study has an equal chance to be in a particular treatment or control condition (Druckman et al. 2010). In the original experiment, each village had the same probability of being part of the early treatment group. To see the consequences of the aggregation of villages into precincts for the probability of

TABLE 1 Descriptive Statistics

Panel A: Villages in the Random Assignment		
N = 502		
	1995	2000
Average population	260	254
Average population above or 18 years old	132	131
Average poverty	4.66	
Panel B: Electoral Precincts		
N = 462		
	1995	2000
Average population	1977	2065
Average number of voters	1055	1099
Average poverty	4.58	
Average share of eligible population	0.87	
	1994	2000
Turnout	0.65	0.67
PRI vote share	0.42	0.37
PAN vote share	0.05	0.14
PRD vote share	0.10	0.12

Note: Poverty and population measures taken from CONAPO 1995 and 2000. Electoral data taken from *Atlas Electoral de México*, IFE 1991–2000.

done first by hand based on the village names taken from the ENCASEH (1997). I corroborated the match by overlaying the geographical boundaries of the electoral precincts and the position of the villages with GIS. Out of the 506 villages in the experiment, GIS located only 27 in a different precinct due to their proximity to the electoral precinct boundary. I kept the manual coding for these 27 villages. Four of the experimental villages were not found in the IFE records. The remaining experimental villages were located in 465 precincts. Ten of these precincts contained special voting booths where by law out-of-precinct voters can cast a ballot (in these precincts, turnout often exceeded 100%).

[18] I excluded from the analysis three precincts that were clear outliers in terms of population and the number of villages. For example, one of the precincts (located in Veracruz) has 66 villages, whereas the rest of the sample has on average six villages. Another precinct (located in Michoacan) has a population of 550,473 inhabitants, whereas the rest of the precincts have an average population of 2,065 inhabitants. Including these outliers in the analysis, however, does not change the results (estimations with outliers are available upon request). Additionally, five precincts belonged to districts that were affected by the 1996 redistricting process; thus, they have no election results in 1994.

[19] Population data come from the Census of Population and Housing (1990, 2000) and the partial census (1995) produced by INEGI. 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 2,500 inhabitants.

TABLE 2 Baseline Characteristics (Means and Standard Deviations)

	Early	Late	Difference
Poverty	4.57	4.59	0.01
			(0.04)
Population	2040.10	1851.35	−188.49
			(651.98)
Population eligible	0.88	0.85	−0.03
			(0.03)
Number of villages	6.08	6.37	0.29
			(0.37)
Randomly assigned villages = 1	0.90	0.90	0.002
			(0.02)
Randomly assigned villages = 2	0.09	0.08	−0.006
			(0.02)
Turnout 1994	0.65	0.64	−0.01
			(0.03)
PRI vote share 1994	0.43	0.41	−0.02
			(0.02)
PAN vote share 1994	0.05	0.06	0.01
			(0.009)
PRD vote share 1994	0.10	0.09	−0.004
			(0.012)

Note: The third column reports the difference in means between the late and early groups. Standard errors in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

treatment, consider the hypothetical example where 10 villages form part of the experiment. One set of villages corresponds one-to-one into precincts; another set corresponds two-to-one. Each village has a 50% chance of being assigned to the treatment. In the first set of hypothetical precincts, this means that 50% are in treatment and 50% are in control. In the second set, 25% are in control, 50% are in half-treatment, and 25% are in treatment. Precincts in the two sets have different probabilities of treatment, but these differences are captured perfectly by the set to which they belong. Moreover, for precincts in the second set there are two alternative treatments available: treatment and half-treatment, but this can also be captured by the set precincts belong to. Thus, taking into account the sets that villages belong to allows for appropriate estimations.

Going back to Progresa's experiment, the aggregation of villages into precincts influences the probability of treatment in two ways. First, 90% of precincts assigned to early treatment, as well as 90% of precincts assigned to late treatment, included only one experimental village. The remaining 10% of precincts included two experimental villages.[20] Second, the number of villages in the experimental precincts varies. To take into account that these two factors influence the probability of treatment, the analysis throughout includes fixed effects by the number of villages, and I split precincts into two groups based on whether they include one or two randomized villages. For ease of presentation, in the main text I include the results based on the 90% of the sample, and in the online appendix, I include the analysis pertaining to the remaining 10% of the precincts.

Finally, the aggregation of villages into precincts together with the rollout of the program outside of the experiment present a challenge that is analogous to the standard issue of experimental crossover. In experimental work, it is common that not all subjects assigned to one treatment regime accept it. In this application, because of the rollout of the program, not all households in precincts assigned to early treatment were enrolled early, and not all households in precincts assigned to late treatment were enrolled late (more details on this are discussed in the next section). To take into account these crossovers (and any failure to treat at the village level), the next section presents first the intent-to-treat (ITT) estimates of assignment to early treatment, which involve the purest experimental comparison. ITT is a robust way to analyze experimental data; however, it tends to produce conservative estimates of the effect of an intervention because crossovers from one treatment condition to the another dilute the effect (Dunning and Hyde 2010). I then present instrumental variable estimations of the effect of early coverage, where the instrument for early coverage is the random assignment to early treatment.

Experimental Results

In the following analysis, there are four dependent variables: turnout, incumbent (PRI) vote share, and party vote shares for the two largest opposition parties (National Action Party [PAN] and Party of the Democratic Revolution [PRD]) in 2000. Turnout is calculated as total number of votes as a share of the voting-age population (18 years or older). Similarly, vote shares are calculated as total number of votes for a given party as a share of the voting-age population in the precinct. The reason to measure all outcome variables with respect to potential voters, as opposed to registered voters, is that the program

[20]Two precincts in the late treatment group included three experimental villages, and since there is no counterfactual in the early treatment group, these two precincts are not included in the analysis.

TABLE 3 Impact of Progresa on Turnout and Party Vote Shares

ITT Estimates of the Assignment to Early versus Late Treatment	(1) Turnout	(2) PRI	(3) PAN	(4) PRD
Treatment	0.053*	0.037**	0.007	0.002
	(0.030)	(0.015)	(0.012)	(0.014)
Constant	0.580***	0.233***	0.191***	0.166**
	(0.172)	(0.086)	(0.072)	(0.074)
Controls	yes	yes	yes	yes
# Villages fixed effects	yes	yes	yes	yes
Observations	417	417	417	417
R-squared	0.116	0.288	0.197	0.318
IV Estimates of Early Progresa Coverage	**Turnout**	**PRI**	**PAN**	**PRD**
Early Progresa	0.156*	0.108**	0.021	0.006
	(0.087)	(0.045)	(0.035)	(0.040)
Constant	0.702***	0.414***	0.146**	0.140**
	(0.154)	(0.080)	(0.069)	(0.068)
Controls	yes	yes	yes	yes
# Villages fixed effects	yes	yes	yes	yes
Observations	417	417	417	417
R-squared	0.095	0.275	0.192	0.317

Note: In the upper panel, this table presents the intent-to-treat estimates of early versus late assignment to treatment. In the lower panel, the table presents the IV estimates of early Progresa coverage on turnout and party vote shares. The instrument is the assignment to early or late treatment. All columns include number of villages fixed effects and the following controls: poverty in 1995, population 1995, total votes 1994 and votes for the PRI, PAN, and PRD in 1994. Robust standard errors in parentheses. The appendix displays the estimates when no baseline controls are included. The estimates are robust to the exclusion of controls. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

asked the female head of the household for an identification card. Since the most common identification card is issued by the Electoral Institute, enrollment in the program could have the automatic effect of increasing the number of registered voters.[21]

Assignment to treatment status is captured by the dummy variable *Treatment*, which takes the value of one when the precinct includes a village assigned to early treatment and zero when the precinct includes a village assigned to late treatment. Because of randomization, consistent estimates of the effects of assignment to different durations of Progresa's benefits, or the ITT effect, can be calculated as the average outcome for the early treatment group minus the average outcome for the late treatment group. For the moment, the ITT analysis leaves aside the question of compliance. The upper panel of Table 3 presents the estimates of the effect of assignment to early treatment starting with turnout in column 1 and the party vote shares in columns 2–4. The specifications include number of villages fixed effects to take into account the aggregation of villages into precincts as explained earlier. The models also include a set of baseline covariates (poverty, population, total number of votes, votes cast for the PRI, PAN, and PRD).

The effect of being assigned to the early treatment group, as opposed to the late treatment group, on turnout is positive and statistically significant at the 10% level on a two-sided test. Assignment to early treatment leads to a 5 percentage point increase in turnout. Since base turnout in the late treatment group was 64%, the effect of assignment to early treatment represents a 7% increase in turnout.

Column 2 in Table 3 displays the results when PRI vote share is the dependent variable. As with turnout, assignment to early enrollment in Progresa had a positive and significant effect on incumbent support. A precinct assigned to be enrolled in the program 21 months before election time led to a 3.7 percentage point increase in PRI vote share, significant at the 5% level in a two-sided

[21]The electoral data are compiled by IFE and reported in the Atlas of Federal Elections 1991–2000, and Statistics of the 2003 Federal Election.

test. Because in the late treatment group base support for the incumbent was 41%, the effect of early enrollment represents a 9% increase. Turning to the causal effect of the program on opposition parties' support, Progresa had no statistically significant effect on either the right- or left-wing parties (columns 3 and 4).

In the supplemental material for the article available online, I include various robustness checks. First, I present all estimates without controls. Because treatment is orthogonal to baseline characteristics, excluding controls has a minimal effect on the estimates. Second, following Tomz, Tucker, and Wittenberg (2002), I show that results are robust to different specifications that take into account the compositional nature of the outcome variables. Third, I include a model where the dependent variable is the Marginal Vote Differential, calculated as in Arceneaux (2005). This statistic captures CCTs' potential mobilizing effect among incumbent and opposition supporters. It is reassuring that all estimates point in the same direction as the results presented here.

To estimate the effect of early enrollment in Progresa, as opposed to assignment, I use an instrumental variable estimator where random assignment is an instrument for early enrollment. Outside of the experiment, program coverage expanded according to program rules giving priority to the poorest eligible villages. Because precincts in the early and late treatment groups had an equal number of eligible villages, the experiment created a substantive difference in terms of early coverage between these two groups. In the early treatment group, 86% of precincts' households receiving program benefits in July 2000 were enrolled early. In contrast, in the late treatment group only 52% of households were enrolled early in the program.[22]

The first stage of the IV estimation corroborates that assignment to treatment is a valid instrument for early Progresa coverage. *Treatment* has a positive and statistically significant effect at the 1% level on early coverage. The magnitude of the effect is substantive (being assigned to early treatment increases early coverage by 34%). The F test of *Treatment* of 153 suggests that the instrument is far from weak (Staiger and Stock 1997; Stock and Yogo 2002). The lower panel of Table 3 shows the IV estimates. The effect of early coverage is three times greater than the ITT effect. Similarly, the effect of early Progresa coverage on the incumbent vote share is more than double compared to the ITT estimate. Finally, the IV estimates show no effect of Progresa coverage on opposition parties' vote shares.

[22]Early coverage is calculated as the share of households enrolled during the first four phases of the program's expansion with respect to the total households in the program by the 11th phase of the expansion. In practice, among precincts in the early treatment group, enrollment grew faster in the fourth and fifth expansions, which closely coincide with the experiment.

Why Did Progresa Affect Electoral Outcomes?

Why did early enrollment in the program have such pro-incumbent mobilizing effects? Proving a particular mechanism is a daunting task. Instead of attempting to do so, I provide a discussion of mechanisms that seem to be at odds with the experimental evidence and discuss some other channels that seem to be at work.

When combining cash and votes in the same sentence, inevitably vote buying comes to mind. Is vote buying then responsible for Progresa's electoral returns? Stokes succinctly explains why it is difficult to answer this question: "Both vote buying and programmatic mobilization entail exchanges; in both, parties can be thought as paying a price per vote" (2007, 6). Vote buying, however, is distinct in at least three ways. First, "politicians [buying votes] target a range of benefits only to individuals who have already delivered, or who promise to deliver their electoral support to their partisan benefactor" (Kitschelt and Wilkinson 2007, 10). In contrast, politicians engage in programmatic mobilization when "they devise policy packages knowing that they are likely to benefit particular groups of voters, and that this in turn will make it more likely in general that members of these groups will vote for the party . . . but the party does not have the precise knowledge of who in the target constituency will vote for them" (10). In this first dimension, the experimental results are closer to programmatic mobilization than clientelism because we know with certainty that program benefits were explicitly noncontingent upon recipients' vote choice.

The second distinctive feature of vote buying is that the exchange is accompanied by sanctioning of voters who defect from the politician's partisan camp (Kitschelt and Wilkinson 2007). There are two equally detrimental corollaries to this feature. Clientelistic parties can punish voters who fail to vote by excluding them from the flow of goods or services (Stokes 2007), and parties can simply threaten voters to guarantee their compliance. Regarding the former, I have argued elsewhere that the regularized operation of the program in the hands of a new agency that circumvented governors, state delegates, and mayors prevented party brokers from effectively punishing program recipients who voted against the incumbent. Thus, if anything, Progresa eroded brokers' ability to sanction voters (De La O 2007).

Table 4 Impact of Assignment to Early and Late Treatment on Number of Party Observers

Variables	(1) Total	(2) PRI	(3) PAN	(4) PRD
Treatment	−0.478	−0.167	−0.031	−0.125
	(0.471)	(0.190)	(0.197)	(0.151)
Constant	11.245***	4.635***	3.401***	3.044***
	(0.372)	(0.155)	(0.157)	(0.124)
# Villages fixed effects	yes	yes	yes	yes
Observations	420	420	402	411
R-squared	0.140	0.116	0.089	0.120

Note: This table presents the intent-to-treat effects of early versus late treatment on the number of party observers at the polling precinct in the 2000 election. Robust standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

The experiment allows me to say something more specific regarding the second corollary. Both the early and late treatment groups received cash from the federal government. Therefore, both groups were susceptible to threats of program discontinuation. Yet, turnout is higher in the early treatment group. For this pattern to be compatible with a prospective story based on threats, we would have to assume that the incumbent party was able and, more importantly, willing to use fine-grained information to prioritize the 320 villages in one group and not the 186 villages in the other. Although it is not impossible, it is highly unlikely that a party that could target one group would not target the other because the experimental villages represent only .5% of the total villages in the country. Thus, threats are unlikely to explain the experimental results.

Table 4 lends additional support to this claim. I collected information on the number of party observers present at the polling station in the 2000 election. Party observers are a finite resource that parties allocate across polling stations.[23] If precincts in the early treatment group were a priority for the incumbent party, then we would expect to see a higher number of observers in those areas. Table 4 shows that this is not the case; precincts assigned to early and late enrollment had the same number of party observers.[24]

Finally, it is well established that clientelistic parties target poor voters because they are most responsive (Stokes 2007). Due to the longer exposure to program benefits, the early treatment group was healthier and had additional disposable income for longer. Thus, the clientelistic party would not target the better-off early treatment group, but the more vulnerable late treatment group. If the party followed such strategy, it was clearly unsuccessful since we know empirically that the better-off group cast more ballots in favor of the incumbent.

Other theories are compatible with the mobilization result, but fall short to explain Progresa's pro-incumbent effects. For example, a resource model predicts that participation is increasing with income (Brady, Verba, and Schlozman 1995). In Mexico, for decades high participation was a feature of poor and rural regions, but by Progresa's time, turnout patterns resembled more closely those of established democracies with more affluent people participating more in elections (Klesner and Lawson 2001). Thus, a resource model is compatible with the turnout result. However, if only an income effect was at work, we would expect that the more affluent a voter, the higher his or her sympathy for the conservative party.[25] This argument finds no support in the data.

Rational choice theories emphasize that the probability of turning out to vote decreases when the cost increases (Riker and Ordeshook 1968). It is possible that women enrolled in Progresa faced lower costs of voting, because to register in the program they were required to present an official identification card. The Federal Electoral

[23]Party observers data come from IFE. Total party observers have a mean of 11 and standard deviation of 5. In 2000, the PRI had the highest number of observers with a mean of 5 and a standard deviation of 2, followed by the PAN with a mean of 3.4 (std. dev. 2), and finally, the PRD had on average three party observers (std. dev. 1.5).

[24]In addition, it is unlikely that the party state delegations could target Progresa recipients because municipal governments did not have access to the list of program beneficiaries. This list was kept confidential until 2002, when the Access to Information Law was enacted.

[25]Several studies document a positive link between income and support for the conservative party in Mexico (Domínguez and Lawson 2004; Domínguez and McCann 1996; Moreno 2003). Cortina, Blanco, and Gelman (2009) also find that the PAN did better in richer states than in poorer states in the 1994, 2000, and 2006 elections.

Institute (IFE) issues a widely accepted identity card, so perhaps enrollment in Progresa fostered registration to vote, and women in the early treatment group had more time to process the IFE identity card. Alternatively, the better health of children among the early treatment group perhaps led to less impediments for women (and men) in this group to turn out. Yet, it is unlikely that a resource model or a cost-based explanation on its own accounts for the full set of experimental results.

Retrospective voting theories, as incarnated by Fiorina (1981), posit that evaluations of a party's recent performance should elicit a change in overall party evaluations. Over time, the argument follows, these evaluations shape voters' party identification. Retrospective voting then is conceptually associated with swings in party identification. This article is not well suited to test this mechanism because the data are aggregated, so I cannot test if the longer exposure of program benefits changed partisan affiliations. Yet, the fact that the program had stronger mobilizing effects, compared to persuasive effects, suggests that retrospective voting (through its effect on party identification) is not the main driver behind the findings.

A simpler explanation for the results is that from the point of view of recipients, the longer the duration of program benefits, the more desirable the program is. From the point of view of incumbents, the longer the duration of program benefits, the more opportunities to claim the credit for something that voters consider desirable. Following Mayhew's seminal work, "an actor who believes that a member [the incumbent] can make pleasing things happen will no doubt wish to keep him in office so that he can make pleasing things happen in the future"(1974, 53). The experimental results are consistent with this explanation.

One of the things that perhaps recipients liked about the program, and that took time to materialize, was precisely that benefits were not contingent upon vote choice. In Brazil, for example, CCT recipients voted at higher rates for incumbent mayors who were perceived as managing the program less politically and with fewer program resources going to the nonpoor (de Janvry, Finan, and Sadoulet 2006). This would be compatible with a credit claiming explanation, as well as with social psychology theories that suggest that unconditional gifts foster reciprocity (Landry et al. 2009).

In sum, if we had a continuum of possible mechanisms in which clientelism is at one end and programmatic politics is at the other, I hope the previous discussion has convincingly shown that Progresa's effects are closer to the programmatic end.

Conclusion

This article provides evidence on the electoral returns of the Mexican CCT by analyzing a unique randomized variation in the duration of program benefits across eligible villages. The findings suggest that the targeted program led to an increase in voter turnout and incumbent vote shares. While previous work focuses on CCT persuasive effects, this article shows that the CCT pro-incumbent effects are mainly explained by a mobilizing mechanism.

These findings have some limitations. First, the experiment 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 program's traits may well shape other dimensions of political participation. For instance, the condition to attend regular *plácticas* at the health center gave women a venue to meet on a regular basis. 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.[26] An additional trait of the program that could shape political behavior is the selection in each village of three women to be the bridge between the community and program staff. Despite the lack of formal organization, this network has allowed women, in some instances, to enter politics.[27]

Second, this article confronts external validity challenges similar to those that other studies that rely on experimental data face, or for that matter, studies that rely on case studies. However, the population of Progresa's evaluation is of substantive interest because ultimately we want to know if programmatic politics allows parties to retain, and foster, the support of the poor. The findings corroborate that the positive correlation found in previous studies between CCT enrollment and incumbents' vote shares is in fact causally driven by the program. However, in this case the electoral bonus is not explained by CCTs' ability to win the hearts and minds of the opposition's supporters. Instead, CCTs foster support for incumbents by mobilizing recipients.

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 the electoral returns are over time.

[26] Author's interview with program recipients, Tlaxcala (2005).

[27] Author's interview with program recipients, Estado de Mexico (2002).

Finally, regarding the reasons behind the Mexican CCT electoral returns, this article find little support for explanations based on clientelism. An explanation based on programmatic politics, and credit claiming, seems more likely to be at work. Despite the inconclusiveness on this regard, this article suggests one important general lesson. Programs targeted at individuals, when operated in a programmatic way, 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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Supporting Information

Additional supporting information may be found in the online version of this article:

SI.1: Taking into account the compositional nature of the dependent variable, and the Marginal Vote Differential

SI.2: Main estimates without controls

SI.3: Estimates among precincts with two experimental villages

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